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Abstract. Intellectual and professional reforms in evolutionary studies between 1935 and 1950 included substantial expansion, diversification, and realignment of community infrastructure. Theodosius Dobzhansky, Julian Huxley and Alfred Emerson organized the Society for the Study of Speciation at the 1939 AAAS Columbus meeting as one response (among many coming into place) to concerns about 'isolation' and 'lack of contact' among speciation workers worried about 'dispersed' and 'scattered' resources in this newly robust 'borderline' domain. Simply constructed, the SSS sought neither the radical reorganization of specialities nor the creation of some new discipline. Instead, it was designed to facilitate: to simplify exchange of information and to provide a minimally invasive avenue for connecting disparate researchers. Emerson served as SSS secretary and was its principal agent. After publishing one block of publications, however, the SSS became 'quiescent'. Anxious to promote his own agenda, Ernst Mayr tried to manoeuvre around Emerson in an effort to revitalize the project. After meeting impediments, he moved his efforts elsewhere. The SSS was too short-lived to merit a claim for major impact within the community; however, it reveals important features of community activity during the synthesis period and stands in contrast to later efforts by George Simpson, Dobzhansky, and Mayr.

Evolutionary studies underwent a renaissance in the 1930s. Nowhere was this expansion greater than in the study of mechanisms for speciation – the result of new techniques, key conceptual developments and increased research opportunities.[1] As research programmes grew, investigators reached beyond their established disciplinary networks for additional analytical tools, information and expertise. Throughout the decade, this reaching outward found expression within existing community infrastructures: joint meetings of societies and associations, co-operative arrangements and overlapping subsections. For most needs, these proved sufficient.

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1 Many cases could be cited. Mid-1930s examples include E. Anderson, 'The species problem in *Iris*', *Annals of the Missouri Botanical Garden* (1936), **23**, 457–509; T. Dobzhansky, '*Drosophila miranda*, a new species', *Genetics* (1935), **21**, 377–91; A. Kinsey, 'The origin of higher categories in *Cynips*', *Indiana University Publications*, *Science Series* (1936), **4**, 1–334; G. Simpson, 'The Fort Union of the Crazy Mountain Field, Montana, and its mammalian faunas', *Bulletin United States National Museum* (1937), **169**, pp. i–x, 1–287; and K. Wiegand, 'A taxonomist's experience with hybrids in the wild', *Science* (1935), **81**, 161–6. For historical discussions, see, for example, W. Provine, 'Origins of the genetics of natural populations series', in *Dobzhansky's Genetics of Natural Populations*, *I–XLIII* (ed. R. Lewontin, J. Moore, W. Provine and B. Wallace), New York, 1981, 5–83. It is a mistake to think that mathematical population genetics offered the only cutting edge in evolutionary studies at mid-decade.

By the end of the 1930s, however, these existing structures seemed to fail. Fearing an inability to track innovations elsewhere, specialists increasingly spoke about 'isolation' and 'lack of contact'. They complained about 'dispersed', 'scattered' literature slowing down their progress and preventing them from keeping up to date. A speciation worker needed, it seemed, to spend all day (time they did not have) in an excellent library (a resource many did not possess) to follow current developments in relevant domains. Never mind unfamiliar technical vocabularies or the problems associated with recognizing implications. Never mind announcing one's own results and general conclusions to those outside the usual audience of related specialists. Part of the immediate acclaim for Dobzhansky's book, *Genetics and the Origin of Species* (1937), came from its compact survey of gene and chromosome phenomena relevant to speciation processes. It met a market demand for summary and translation.

This paper discusses the formation and operation of the Society for the Study of Speciation (hereafter SSS), a 1939 response to calls for improved community infrastructure by those studying mechanisms of species formation. Simply constructed, this organization sought neither the radical reorganization of existing specialities nor the creation of a new discipline. Instead, it was designed to facilitate: to simplify exchange of information, to increase interaction, to update publication lists and to provide a forum for co-ordinated research and critical evaluation. Debates about this organization and its future revealed contrasting visions of what was needed and what ought to be accomplished.

This study fits into a larger revision in the historiography of the synthesis period of evolutionary studies. Standard accounts represent the 1930s and 1940s as a period of robust theory development, when a comprehensive and integrated modern approach to evolution came into use.[2] By comprehensive, this theory was supposed to apply to all known evolutionary phenomena. By integrated, it was supposed to enrol elements from many specialities within biology while simultaneously reconciling their differences and setting everyone on a common theoretical footing.

Revisionist historians raise several basic complaints about standard accounts. First, standard accounts narrowly construe scientific activity as the construction of abstract theory, then follow its evolution in the search for conceptual 'revolutions' and theory reconciliations. Second, these accounts are dominated by narrative frameworks crafted by partisans in debates about evolutionary theory and lack critical distance or historiographic self-awareness. Third, standard accounts place extraordinary emphasis on a small core of actors and a small subset of their communications. They deliver a narrative of magisterial 'architects' and 'classics', not a meaningful analysis of biographies, contexts, or communities. Fourth, standard accounts so completely emphasize the fact of consensus and its self-evident need that they offer poor analyses of the nature of 'synthesis', the senses in which actors disagreed among themselves, and the social or intellectual conditions

2 Foundations in synthesis historiography include W. Provine, *The Origin of Theoretical Population Genetics*, Chicago, 1971; W. Provine, 'The role of mathematical population geneticists in the evolutionary synthesis of the 1930s and 1940s', *Studies in the History of Biology* (1978), **2**, 167–92; W. Provine, *Sewall Wright and Evolutionary Biology*, Chicago, 1986; E. Mayr and W. Provine (eds.), *The Evolutionary Synthesis: Perspectives on the Unification of Science*, Cambridge, MA, 1980; E. Mayr, *The Growth of Biological Thought*, Cambridge, MA, 1982; E. Mayr, 'Controversies in retrospect', *Oxford Surveys in Evolutionary Biology* (1992), **8**, 1–34.

motivating consensus formation and its preservation. In short, standard accounts leave much to be desired.

My contributions to this revisionist literature emphasize community and research infrastructure – journals, societies, communication networks, the ecology of institutions and communities and so on – and the interaction between infrastructure and actors pursuing their individual intellectual and social agendas.[3] The goal has been to give richer meanings to concepts of professional communities and to introduce a local terrain to the landscape of those communities at work in the synthesis period.

This paper discusses the formation and operation of the Society for the Study of Speciation during its brief existence. First, I discuss its formation, foregrounding several elements of the specific historical moment in late 1939 that gave momentum to its launch. Second, I discuss the projects undertaken by SSS secretary Alfred Emerson, as a way of assessing Emerson's own emphases as well as the diversity of interests within the wider speciation community. Despite strong initial enthusiasm, the SSS was short-lived. Third, I discuss causes for this unexpected disintegration and how one active partisan in the community, Ernst Mayr, manoeuvred to revitalize and transfer control of the project. Frustrated by his lack of success, Mayr took his organizational energies elsewhere. Close analysis of the SSS offers a case study of community infrastructure in the biological sciences. It also raises important points about the nature and heterogeneity of the synthesis projects.

To institute an informal information service

Organization of the SSS – at the 1939 annual meeting of the American Association for the Advancement of Science (AAAS) in Columbus, Ohio – was a simple, minor affair. So much so, it missed mention in the meeting's minutes as reported in *Science*.[4] There had been correspondence beforehand about taking some kind of action, but the actual initiation was largely impromptu. Ernst Mayr later described the setting as merely a 'conference between [Julian] Huxley and [Theodosius] Dobzhansky' at the Columbus meetings that he too 'sat in at', in which the basic scheme was drawn up.[5] Several days later, still in Columbus, some combination of this trio approached Alfred Emerson, the University of Chicago ecological entomologist who was sympathetic to the idea of forming a collective project, with the goal

3 J. Cain, 'Common problems and cooperative solutions: organizational activities in evolutionary studies, 1937–1946', *Isis* (1993), **84**, 1–25; J. Cain, 'Ernst Mayr as *community* architect: launching the Society for the Study of Evolution and the journal *Evolution*', *Biology and Philosophy* (1994), **9**, 387–427; J. Cain, 'For the 'promotion' and 'integration' of various fields: first years of *Evolution*, 1947–1949', *Archives of Natural History*, forthcoming; and J. Cain, 'Here is a field that requires joint counselling: Ernst Mayr and the committee on common problems of genetics and paleontology, 1942–1947', *Studies in History and Philosophy of Biological and Biomedical Sciences*, forthcoming.

4 G. Baitsell, E. G. Butler, E. M. Cory, C. Mickel and O. R. McCoy, [Reports of 1939 meetings for AAAS Section (F) and associated societies], *Science* (1940), **91**, 110–19.

5 Mayr to Emerson, 26 March 1940, folder 75, Papers of Ernst Mayr, collection HUG (FP) 14.7, Professional Correspondence, 1931–1952, Harvard University Archives, Cambridge, MA (hereafter Mayr-Harvard). On Emerson's role, also see Emerson to Colleagues, 18 March 1940, in folder 'Emerson, R. A.', Papers of Leslie Dunn, collection B-D917 (hereafter Dunn), American Philosophical Society Library, Philadelphia, PA (hereafter

of implementing some kind of plan. They hoped Emerson could be persuaded to take charge, especially as neither Huxley nor Dobzhansky wanted to take on the administrative burdens of actually organizing or running the group.[6]

These principals agreed among themselves that there now existed in speciation studies 'an informal co-operative group of scientists willing to pass information from one to the other'. Something was needed, they argued, to simplify further exchange within that group and to enrol others in the sharing process. Emerson agreed to take on the job. 'The need was felt by many students of speciation for a greater degree of integration between the various fields', he wrote when announcing the group's creation.

> Those contributing to an understanding of the factors influencing speciation are often in fields and institutions which have little direct contact with those who are attacking the problem from somewhat different angles and are using different techniques ... The general object of the Society [will be] to institute an informal information service which will tend to correlate the various approaches.[7]

As these principal 'students of speciation' crafted their plan, they knew what they wanted. The goal was assuredly not to create a new primary professional association, in competition with the Genetics Society of America, the Ecological Society of America, the American Ornithologists' Union or the Society of Vertebrate Paleontology. This was not primarily about identity formation; they did not consider the precise boundaries and missions for the group. Instead, the plan was to create an informal network. Its organization should be kept loose. Most collective business – what little they expected – could be done by an official administrative secretary. The sole purpose of the association would be to simplify communication: announcements, discussions and exchanges of information. These principals knew each was operating with roughly similar ideas about coverage and knew each had roughly the same centre in mind. This uniformity in vision, displayed in the SSS's first announcements, was both directive and inclusive:

> The major field of interest is the dynamics of the origin of species. Obviously the analysis of the facts of speciation involve the study of divergence of populations classified as subgroups within the species. Therefore studies of the origin of local populations, races and sub-species are necessary parts of the study of speciation. Also many factors may be studied and verified through analysis of the evolution and stability of the higher taxonomic categories. There should be no limitation on the inclusion of any phase of evolution that contributes to an understanding of the central problem of the origin of species.
>
> The major factor complexes may be termed hereditary variation, isolation, and selection. These may be subdivided into various types and mechanisms and numerous illustrative examples among plants and animals may be given. The recognized fields of Bacteriology, Botany, Zoology and Anthropology have long been interested in the species problem. The biological sciences which

APS). Mayr later said Carl Epling also attended the conference with Huxley. See 'History of the Society for the Study of Evolution', folder 'SSE History', Papers of Ernst Mayr, collection B/M451, APS Library (hereafter Mayr-APS). Correspondence prior to the meeting between Huxley, Dobzhansky, and any other principal on organizing this group has since been lost.

6 For reasons that are unclear, they chose not to ask Mayr if he would be interested in the job. Probably, seeing him in 1939, Mayr lacked centrality and reputation.

7 A. Emerson, [Excerpts in 'Evolution news'], *American Naturalist* (1941), **75**, 86–9.

> obviously are making contributions to speciation and general evolution include Morphology, Cytology, Genetics, Biogeography, Ecology, Paleontology, Physical Anthropology, Comparative Psychology, Comparative Physiology, Embryology, Population Biology and Taxonomy.[8]

Implementing these plans – actually forming a group and organizing its activities – fell into Emerson's hands as society 'secretary'. At the Columbus AAAS meeting he either agreed or volunteered[9] to serve indefinitely, or at least until 'the permanence of this society is assured' and 'a system of election by members' could be instituted.[10] As sole officer (other than an *ad hoc* 'executive committee' that had no stated function), Emerson was responsible for the 'general organization of the group' and the 'publication of information'.

Emerson's interest and participation was not surprising. Throughout his career, Emerson had been an active investigator of speciation questions, especially in his research on termite nesting behaviour.[11] He also earned a reputation as a strong advocate of interdisciplinary co-operation, both in his capacity as editor of *Ecology* (1930–9) and in book reviews he contributed to that journal. Commenting on Robson and Richards's *Variation of Animals in Nature* (1936), for instance, Emerson noted that 'the time seems ripe for the first comprehensive analysis of modern knowledge of evolutionary events which brings the recent discoveries of the taxonomists, animal geographers, ecologists and geneticists into balanced relationship'.[12] Emerson regularly used book reviews to introduce and assimilate work from outside specialities into ecology. One of his techniques was to note the absence of an ecological perspective in a book under review, then suggest how the project would be enhanced by what he called a more 'integrative approach'.[13] With this background, Emerson seemed a good – indeed an obvious – choice to run the group.

Julian Huxley and the British 'Association'

Despite the press of global events around that autumn, the Columbus meeting of the AAAS in December 1939 was a timely setting for infrastructural expansion in speciation studies. Calls for increased integration and mutual exchange of information had been growing in previous years, and no existing forum seemed to accommodate the needs of the many constituencies vying for inclusion. Of course, needs alone cannot cause action. Several specific factors converged in Columbus to add strong momentum for action and provided

8 Emerson, op. cit. (7), 87–8.

9 Emerson said he 'was approached' by Huxley and Dobzhansky 'with the suggestion that he organise a co-operative association of individuals'. In Emerson to Colleagues, 18 March 1940, Dunn.

10 For this informal society, membership was open to 'anyone interested' who volunteered themselves and paid the small membership fee. Emerson, op. cit. (7), also see Emerson to Colleagues, 18 March 1940, Dunn.

11 E. Wilson and C. Michener, 'Alfred Edwards Emerson', *Biographical Memoirs of the National Academy of Science* (1983), **53**, 159–75; G. Mitman, *The State of Nature: Ecology, Community, and American Social Thought*, 1900–1950, Chicago, 1992.

12 A. Emerson, 'Speciation', *Ecology* (1937), **18**, 152–4, 153.

13 Forgotten in discussions of evolutionary theorizing on the American scene are works such as W. C. Allee, A. E. Emerson, O. Park, Th. Park and K. P. Schmidt, *Principles of Animal Ecology*, Philadelphia, 1949.

simple, easy-to-choose solutions for the problems at hand, solutions that could be instituted with little effort, threatened no disruptions and offered obvious benefits.

One of these factors was Julian Huxley.[14] By the late 1930s, Huxley had long been a vocal advocate for co-operation and co-ordination across disciplinary boundaries, choosing his own label as a 'general biologist'.[15] Crucially, Huxley was part of a faction within the Zoological Society of London intent on moving its orientation away from gentlemen and lady amateurs and towards professional, 'modern' research biology, concerned only with the 'purely scientific aspects of zoology'.[16] For reasons described elsewhere, these reform efforts met stiff resistance, and relatively early in his tenure as secretary of the Society, Huxley was forced to abandon the idea of major reform.[17] By 1936 he was searching for alternative means and more accommodating constituencies, quickly finding one in plans to create an Association for the Study of Systematics in Relation to General Biology (hereafter Association).

Launched by London-based researchers, the Association came together in mid-1936. Its leadership included John Scott Gilmour and William Bertram Turrill (Royal Botanical Gardens, Kew) and Hampton Wildman Parker (British Museum, Natural History). Huxley introduced himself into their project, adding his characteristic flourish and profile. Intending primarily to facilitate experimental taxonomy, Association organizers claimed several functions for the group: exploring the relation of phylogeny to cytogenetic and taxonomic data, studying criteria for taxonomic categories and increasing uniformity of usage, relating taxonomy to principles of general biology and co-ordinating research between amateurs and professionals as well as between speciation workers and taxonomists. Given the constraints of that moment in history, Association committees managed considerable activity (though historians largely have yet to recognize this productivity).[18]

Best known among Association projects was the 1940 volume *The New Systematics*,

14 On Julian Huxley in this period, begin with R. Clark, *The Huxleys*, London, 1968, especially 254–68; C. K. Waters and A. Van Helden, *Julian Huxley*, *Biologist and Statesman of Science*, Houston, 1992; Cain, 'Common problems', op. cit. (3).

15 After becoming Secretary to the Zoological Society of London in 1935, Huxley aggressively used that position to focus the collective attention of specialists towards 'general' biological problems, such as behaviour and evolution. Previously he played important roles in the organizational efforts of British ornithology, experimental biology and animal behaviour as well as in national planning efforts in Britain and abroad. For example, Huxley to Wells, May/June 1934, Papers of Julian Sorell Huxley, Woodson Research Center, Rice University, Houston, TX (hereafter Huxley), box 11, folder 5. In 1936 Huxley helped arrange a session for the Zoological Section of the British Association for the Advancement of Science, designed to help integrate genetics, ecology, and selection. J. Huxley, 'Natural selection and evolutionary progress', *Nature* (1936), **138**, 571–3, 603–5; Anonymous, 'Genetics and ecology in relation to selection', *Nature* (1936), **138**, 748–9.

16 See e.g. Gray to Huxley, 2 February 1936, Huxley, box 12, folder 1, and Hardy to Huxley, 29 February 1940, Huxley, box 14, folder 1. Importantly, while Huxley was working to impose a 'modern' standard of academic zoology on the Zoological Society, many Fellows feared he was degrading the scientific value of the zoological gardens. This is discussed briefly by Clark, op. cit. (14).

17 Relevant correspondence in Huxley and in the archives of the Zoological Society of London; Clark, op. cit. (14) presents a basic sketch of events.

18 M. P. Winsor, 'The English debate on taxonomy and phylogeny, 1937–1940', (Unpublished manuscript); and Cain, 'Common problems', op. cit. (3).

edited by Huxley. No mere anthology, this collection instantiated the Association’s programme of co-operation and co-ordination in the pursuit of common interests. The goal was not simply to celebrate the infusion of experimental taxonomy into systematics but also to promote cross-disciplinary researches on divergence and isolation and relate them to taxonomic concepts and evolutionary mechanisms. ‘It would probably be no exaggeration’, the ichthyologist Carl Hubbs wrote in his review of *The New Systematics*, ‘to call this [book] the outstanding evolutionary treatise of the decade, perhaps of the century. The approach is thoroughly scientific; the command of basic information amazing; the synthesis of disciplines masterly’.[19]

Late in 1939 Huxley toured the United States, making headlines as he campaigned for American intervention in Europe and discussed ‘post-war settlement’.[20] Huxley made a point of attending the Columbus AAAS meeting; he was an invited speaker. Huxley’s presence in Columbus energized those pressing for the organization of speciation workers. He promoted the Association’s work, boasting about successes in sponsored projects such as *The New Systematics* (then in press) and the ten sessions devoted to ‘genetics in relation to evolution and systematics’ at the Seventh International Genetics Congress held in Edinburgh several months earlier.[21]

Huxley enthusiastically supported what he thought would be an American complement to the (what he now thought of as his) Association. He knew Dobzhansky and Emerson were behind the project. These were researchers with the means and the professional profiles necessary to implement whatever plan they devised, and Huxley – ever an ideas man – had every reason to take their interest seriously.[22]

In the historical moment of the December 1939 AAAS meetings, Huxley’s support also lent certification to the American project. As a recognized leader of a similar and successful effort in Britain, he demonstrated such a project could have a recognized constituency and could produce valuable results. He offered a model to follow. If Huxley did all he later took credit for, he gave the American project the direction and definition it needed to crystallize. If nothing else, his attendance added useful momentum to a project just then coming into shape. Indicative of this sense of Anglo-American connection, when Emerson first suggested a name for the project, it was ‘the Association for the Study of Speciation’.[23]

19 C. Hubbs, [Review of *The New Systematics*], *American Naturalist* (1941), **75**, 172–6. Also see reviews by A. Emerson, [Review of *The New Systematics*], *Botanical Gazette* (1940), **102**, 412; A. Emerson (ed.), [*News bulletin*]. *Society for the Study of Speciation*, Chicago, 1941, 17–29; A. Emerson, ‘Taxonomy and ecology’, *Ecology* (1941), **22**, 213–15. Ironically, other reviewers complained that the range of disciplines co-ordinated by the Association was overly narrow. R. F. Poulson, for example, angrily complained about the ‘almost complete neglect of recent immunological and serological research’. R. F. Poulson, [Review of *The New Systematics*], *American Journal of Science* (1941), **239**, 239–40.

20 On Huxley’s trip, see correspondence in Huxley, box 13, folder 6, and box 14, folders 14–15, plus his personal diary for ‘1940’.

21 Conference proceedings are D. Jones (ed.), *Proceedings of the Sixth International Congress of Genetics*, *Menasha*, *WI*, 1932; and R. Punnett (ed.), *Proceedings of the Seventh International Genetical Congress*, Cambridge, 1941. Also see J. Huxley, ‘Evolutionary genetics’, in ibid., 157–64. Other symposia are listed in the Association’s annual reports.

22 The evidence is not clear as to whether Huxley brought the suggestion of forming a group to the Americans or they approached him to support a project already conceived.

23 Emerson to Colleagues, 18 March 1940, Dunn.

Dobzhansky and the Speciation Symposium

With high praise for *Genetics and the Origin of Species* (1937) and growing recognition for his work on the genetics and ecology of natural *Drosophila* populations, Dobzhansky had became a recognized leader of speciation studies on the American scene.[24] His ability to draw together elements from many specialities and craft them into an integrated, visionary whole drew considerable attention. He was also a pioneer, carving out a research programme like no other – one that tapped into some of the most active areas of attention. Charismatic, full of energy and charming, Dobzhansky moved with ease through professional circles far away from those customary for a narrow specialist. He left many 'fans' in his wake – Ernst Mayr was one[25] – and was heavily recruited in the late 1930s by universities anxious to develop programmes in evolutionary genetics. That Dobzhansky was calling for integration and co-operation among speciation workers made an impact. If anyone could build on the sense that now was a time ripe for concerted action, it was him.

Normally at AAAS meetings, general symposia complemented the many technical sessions. For the zoology and botany sections, these symposia tended to revolve around fundamental problems or current hot topics in the field (what counted as such was determined by the officer asked to organize the session). Some symposia, such as the one sponsored annually by the American Society of Naturalists, were prestigious affairs and carried numerous co-sponsoring societies. Acting on behalf of the Genetics Society of America, Dobzhansky had been asked to organize the Naturalists' Symposium at the Columbus meeting. He chose 'speciation' as its topic.

'It should be a very interesting symposium!' Dobzhansky told participants once he had finished preparations.[26] He advertised the session as 'a critical review of recent work on the important biological problem of speciation and its relation to evolution'. It included a major address by Sewall Wright on the breeding structure of populations and the effects of population size on speciation. Mayr also gave a major address, summarizing speciation processes in birds. Additional talks were provided by Lee Dice, who described ongoing research in *Peromyscus*, and Warren Spencer, who identified a series of divergences in *Drosophila* populations. Dobzhansky himself closed the session with a survey of the concept of evolutionary divergence, making the point that speciation was only one stage on a continuum of diversification into higher taxonomic groups.[27] According to meeting minutes and later reports, attendance at the symposium was 'unusually large'.[28]

24 On Dobzhansky in the 1930s, see Provine, op. cit. (1); M. Adams (ed.), *The Evolution of Theodosius Dobzhansky*, Princeton, 1994; and R. Kohler, *Lords of the Fly: Drosophila Genetics and the Experimental Life*, Chicago, 1994, 250–93.

25 On Mayr's early friendship with Dobzhansky, see Cain, 'Here is a field', op. cit. (3).

26 Dobzhansky to Mayr, 18 October 1939, Correspondence between Ernst Mayr and Theodosius Dobzhansky, APS Library (hereafter Mayr/Dob), folder '1937–1947'.

27 The December symposium included the following papers: Sewall Wright, 'Breeding structure of populations in relation to speciation'; Ernst Mayr, 'Speciation phenomena in birds'; Lee Dice, 'Speciation in *Peromyscus*'; Warren Spencer, 'Levels of divergence in *Drosophila* speciation'; and Theodosius Dobzhansky, 'Speciation as a stage in evolutionary divergence'. All papers appear in *American Naturalist* (1940), **74**.

28 No numerical count of those in attendance has been located.

In their presentations, participants agreed on their general enterprise and its basic foundations. They spoke in positive terms about future prospects and a strong, shared sense that their work was placing them on the right track. Adding to these themes, each speaker claimed special insight into crucial dimensions of their common problems. For instance, in describing a new, six-step model for species formation, Mayr told the audience that geneticists could contribute principally to two of these steps, that ecologists could contribute to three others and that systematists (such as Mayr) had something to say about each step.

This well-attended, highly positive symposium put the problem of speciation under a prominent spotlight for naturalists attending the Columbus meeting. Sitting in the audience, Huxley and Emerson certainly would have witnessed that. Picking up this enthusiasm, it was easy for planners of the new facilitating organization to transfer that momentum into committed action for co-operative exchange. This was the time to act.

Richard Goldschmidt and the Silliman Lectures

Also important to the historical moment of December 1939 for speciation workers were Richard Goldschmidt's Silliman Lectures delivered at Yale University several weeks prior to the AAAS meeting. For more than a decade, Goldschmidt had been a prominent defender of solely genetic explanations for the origin of species. His theory emphasized the importance of major changes in the germ plasm and of heritable changes in developmental sequences for producing large-scale, discontinuous changes in morphology. Sometimes these changes produced differences so sharp they isolated the recipient from the parent species. If that recipient proved adapted to its surroundings and managed to survive competition, a new species would result. This theory – dubbed 'macromutation' – was highly controversial both within and without the genetics community.[29] For speciation workers increasingly focused on process and mechanism, Goldschmidt's work had obvious relevance. The problem was that principal organizers in speciation studies thought Goldschmidt could not be more wrong.

Each speaker in Dobzhansky's symposium presented materials directly challenging

29 Synthesis historians are still a long way from fully understanding Goldschmidt's place in the evolutionist community, his self-imposed role as outsider, his attributed role as foil, or the value of the combined Goldschmidt/Schindewolf synthesis of macromutationist theory. Dietrich's recent discussion is the most thorough and solid in detail, though his general conclusion (that Goldschmidt 'set an agenda and provided a common target' thereby becoming a 'significant factor in the formation of the evolutionary synthesis') exceeds the evidence. M. Dietrich, 'Richard Goldschmidt's "heresies" and the evolutionary synthesis', *Journal of the History of Biology* (1995), **28**, 431–61. Dietrich's assessment, however, is not nearly as overstated as Mayr suggests (based on his own recollections). E. Mayr, 'Goldschmidt and the evolutionary synthesis: a response', *Journal of the History of Biology* (1997), **30**, 31–3. Gould exaggerates the 'heretic' case for reasons more to do with forwarding his own claims about evolutionary processes than any sense of duty to the historical record. S. Gould, 'The uses of heresy: an introduction to Richard Goldschmidt's *The Material Basis of Evolution*', in R. Goldschmidt, *The Material Basis of Evolution*, New Haven, CT, [1940] 1982 reprint, pp. xiii–xlii. A solid context for Goldschmidt is provided by J. Harwood, *Styles of Scientific Thought: The German Genetics Community, 1900–1933*, Chicago, 1993. Any discussion of macromutation must engage comments, such as Mayr's, that prominent colleagues (especially those at the American Museum such as Robert Cushman Murphy and G. K. Noble) were 'mutationists' (Mayr to Simpson, 29 March 1974, folder 'Simpson, G. G. #1', Mayr-APS) and must consider early discussions among synthesis principals about the nature of hereditary mutations (Mayr/Dob).

Goldschmidt's thinking, though his views were not discussed explicitly. Among the core problems these speakers had with Goldschmidt's view was that it provided no role for the kinds of geographic or genetic variation they were studying in natural populations. Neither did it take into account a population's ecology, known modes of isolation and divergence, or any of the other features of population dynamics these researchers were coming to believe stood at the heart of speciation processes. To them, species were populations that had slowly diverged, and had become isolated, from other populations of a parent species. Species formation was a continuous process – populations to subspecies to species – not the product of macromutations. From all they knew about speciation processes, the mechanisms Goldschmidt proposed were completely off the mark.[30]

Nevertheless, Goldschmidt's views certainly were present in Columbus. Mayr had attended Goldschmidt's Silliman lectures several weeks before the AAAS meeting, despite the fact that he did not expect to hear 'anything new'.[31] Goldschmidt had also given Mayr a copy of his book manuscript for comments.[32] Respectful of this senior colleague (and like Mayr, a German émigré) but unhappy with the macromutation theory, Mayr explained, 'You believe in very large steps that completely revolutionize the species and I believe in steps that are so small that they completely escape the attention of the laboratory experimentalist… I am afraid that you haven't convinced me yet that geographical variation has nothing to do with speciation'.[33] In Columbus Mayr reported on what he had heard and read from Goldschmidt.

Knowing that Goldschmidt was actively expanding and publicizing his views motivated organizers of the speciation society in a minor, but not trivial, way. It would be far too strong to argue that the foundation of the SSS was a concerted effort to rally opposition against Goldschmidt or to mark him as a heretic. Instead, Goldschmidt's activity irritated those wanting to move speciation studies in a different direction. Knowing that his conclusions were soon to appear in a major book (competing with Dobzhansky's) must have frustrated them more.

Goldschmidt's activities reminded those pushing for increased communication that misconceptions needed to be discussed and dispelled: geneticists had more sophisticated views of nature than those of the macromutationist; speciation involved more than genetic processes; and spectators of the new speciation movement needed to know that Goldschmidt's was a decidedly minority view. There was no point calling for increased co-operation and exchange of information if participants picked up the wrong ideas from genetics. The presence of such misleading information meant some kind of response was needed so that the importers of genetics information had a way to differentiate right thinking from wrong thinking in an area outside their expertise. For those pushing to increase information exchange among speciation workers, it must have seemed that everyone would be best served if 'current' and 'reliable' information was more easily available within the market-place of ideas.

30 Mayr described Goldschmidt's view as 'wrong' as early as 1935. See Mayr to Dobzhansky, 25 November 1935, Mayr-Harvard, folder 22.

31 Mayr to D. Lack, 8 December 1939, Mayr-Harvard, folder 62.

32 These lectures were the core of Goldschmidt's 1940 book, see Goldschmidt, *Material Basis*, op. cit. (29).

33 Mayr to Goldschmidt, 13 December 1939, Mayr-Harvard, folder 58.

Figure 1. Emerson's classification scheme for bibliographies produced by the Society for the Study of Speciation. (From materials relating to Society for the Study of Evolution, collection B/M451S 1991 1027 ms, American Philosophical Society Library, folder 'early letters'.)

G. General Papers dealing with many factors
I. Distinctions between species and other categories.
- **A. Morphological distinctions.**
  - **1. Embryological and Developmental distinctions.**
- **B. Cytological distinctions.**
- **C. Genetic distinctions.**
- **D. Physiological distinctions.**
- **E. Psychological (Behavioristic) distinctions.**
- **F. Ecological distinctions.**
- **G. Geographic distinctions.**
- **H. Stratigraphic (Fossil sequence) distinctions.**

II. Causes of Variation.
- **A. Gene Mutation.**
- **B. Recombination.**
- **C. Genome (Chromosome) mutation.**
- **D. Non-Mendelian (cytoplasmic) inheritance.**
- **E. Paedomorphosis. Neoteny.**
- **F. Orthogenesis. Heterogony. Heterochrony. Allometry.**
- **G. Recapitulation. Vestigial Structures. Stability of Type.**
- **H. Lamarckian evolution. The inheritance of acquired somatic characters.**

III. Isolation.
- **A. Topographical isolation.**
- **B. Spatial isolation.**
- **C. Time isolation.**
- **D. Ecological isolation.**
- **E. Seasonal isolation.**
- **F. Physiological isolation.**
  - **1. Behavior (Psychological) isolation.**
  - **2. Physiological isolation (proper).**
  - **3. Mechanical isolation.**
  - **4. Gamete Sterility (Infertility).**
  - **5. Embryological impairment (Inviability).**
  - **6. Adult sterility.**
  - **7. Parthenogenesis. Apomixis.**
  - **8. Asexual reproduction.**
  - **9. Population waves.**
  - **10. Migration pressure.**

IV. Natural Selection.
- **A. Overproduction.**
- **B. Competition.**
- **C. Preadaptation.**
- **D. Degenerative evolution.**
- **E. Convergence.**
- **F. New habitats (radiation).**
- **G. Physical and chemical factor correlation.**
- **H. Biotic factor correlation.**
- **I. Cyclomorphosis.**
- **J. Sexual characters. Sex ratios. Sexual selection.**
- **K. Population units.**

V. Artificial Selection.

Regular contact with 'anyone interested'

Following decisions in Columbus, the SSS fell onto Emerson's desk. His first step was to distribute a questionnaire announcing the group's formation, seeking interested parties and asking for a first round of news and commentary. He posted this in March 1940, telling colleagues that this 'loose organization of co-operating members' would sponsor 'the publication of a booklet about twice a year for spreading pertinent information concerning bibliographical citations and notes from various members and laboratories'.[34] Emerson wanted specialists to send him correspondence about their research: 'notes concerning original work, critical comments upon the work of others, and news items of interest to the group'. To this correspondence he would add a few pieces of his own or what he had solicited from others, then distribute it to 'anyone interested'.

Despite strong initial interest, more than a year elapsed from Emerson's original notice before he posted the first bulletin in March 1941.[35] As expected, the twenty-nine page news bulletin was an informal product – mimeographed and without even a cover page or masthead. It included 'notes and comments' from a few dozen researchers – really a hodgepodge of queries, statements and suggestions for research projects excerpted from incoming letters. It also contained roughly thirty 'communications from laboratories, organizations, and individuals' describing ongoing research and a set of comments collectively described as a 'discussion from members concerning [a] statement of the objectives of the Society for the study of speciation'. This last section included words of encouragement and suggestions about the SSS's domain.

In its original form, the bulletin bore strong resemblance to other news bulletins that principals were well acquainted with, such as the *Maize Genetics Cooperation News Letter* (popularly called the 'corn letter', begun in 1932), the *Drosophila Information Service Bulletin* (1934) and the *Society of Vertebrate Paleontology News Bulletin* (1940). For the first issue, Emerson added notes about the scope and organization of the society – these were reprinted in *American Naturalist*.[36] Plus, as an example of the kind of dialogue he hoped to facilitate, Emerson included a deliberately provocative, 5900-word

34 Emerson to Colleagues, 18 March 1940, Dunn (this announcement is misfiled under *Ralph* Emerson and not *Alfred* Emerson). This announcement comes close to what Huxley claimed to Gilmour, saying that he (Huxley) had arranged for the

> issuing of a mimeographed bulletin, twice yearly, giving lists of the more important papers on zoological and botanical systematics which would interest workers in cytogenetics, ecology, and other branches of biology, and vice versa, together with notes and queries, notes on methods, etc. Prof. A. E. Emerson is undertaking the work, with the aid of strong local and general committees.

Attached note on Huxley to Gilmour, 12 March 1940, Huxley, box 14, folder 2.

35 Though undated, external evidence places the bulletin's release in March 1941. The only reaction to the bulletin located to date is in March 1941. See Mayr to Emerson, 31 March 1941, Mayr-Harvard, folder 75; and correspondence with Huxley and colleagues in June 1941, Huxley, box 15, folder 3. A copy of Emerson's news bulletin for the Society, op. cit. (19), is in APS. An original is in the author's possession. For the three 1941 publications of the Society, see J. Cain (ed.), *Regular Contact with Anyone Interested: Documents of the Society for the Study of Speciation, 1941*, Winona, MN, 1999.

36 Emerson, op. cit. (7).

review of *The New Systematics*, then becoming available in the United States, describing the work as 'an important contribution to the difficult task of welding many techniques of investigation pertaining to the central problem of evolutionary dynamics'.[37]

Accompanying his first bulletin was Emerson's initial instalment of the bibliography on speciation and an address-list of members. Emerson wanted to publish bibliographies related to all aspects of the SSS's domain. He planned to search the literature himself but wanted members to contribute citations of their own, too. For intellectual control, Emerson developed a key (Figure 1). Along the same lines, Emerson printed the bibliography 'in such a manner that the items may be cut out and pasted on catalogue cards if desired'. Listing 1250 entries, this bibliography offered a sample of the speciation literature during the late 1930s. For some subjects it was reasonably comprehensive; for others, it was extremely thin.

Although Emerson had put considerable energy into this bibliography, he recognized its shortcomings. Some contributors sent in their entire publications list regardless of their relevance to speciation. Others were overly selective. In the bulletin, he reminded members that 'the society is formed to pass information among members, but the members should take the responsibility for placing such information in the hands of the secretary'. First on his desired list of contributions were 'titles of important publications with appropriate bibliographical citations … These should include important items missed in the former lists'.[38]

Emerson's address-list included 374 individuals. A study of a forty percent random sample of this membership suggests their basic demographics. In age, the membership (entirely male) was skewed significantly towards the early to middle portions of careers,[39] though it otherwise followed a roughly normal distribution. A majority of members in the sample were members of the AAAS (sixty-eight percent). Membership of other umbrella organizations most frequently included the American Society of Zoologists (twenty-seven percent), the Genetics Society of America (twenty-four percent), the American Society of Naturalists (twenty-two percent), the Botanical Society of America (eighteen percent) and the Ecological Society of America (eighteen percent); overlap was high in these memberships.[40] Member specialities clustered in roughly the same way membership of umbrella organizations did. The vast majority claimed expertise as taxonomists, though organismic taxonomists (ornithologists, mammalogists, botanists and so on) outnumbered geneticists and cytologists roughly two to one. Of additional note is the unexpectedly high frequency of taxonomic entomologists and the substantial additional range of research

37 Emerson, News Bulletin, op. cit. (19), 18.

38 Emerson, News Bulletin, op. cit. (19), 29.

39 The age distribution followed the following sequence: younger than thirty, 8 percent; between thirty and forty, 35 percent; between forty and fifty, 23 percent; between fifty and sixty, 21 percent; sixty and over, 13 percent.

40 Membership in other organizations was determined using those identified by persons listed in *American Men of Science*, 7th ed. (1944). An important point to note is that memberships in these organizations overlapped significantly and sometimes clustered strongly. For example, given membership in the speciation group and the Botanical Society of America, chances are high that the person was also a member of the Genetics Society of America.

specialities represented in the sample – from palaeontologists to horticulturists, bacteriologists to physical anthropologists. These demographics basically match the skewed distribution for the 'executive committee', where two geneticists and two cytologists were outnumbered by seven zoologists and one palaeontologist. The membership sample was well distributed across the United States, with a small number also from abroad.

Judging by the size and diversity of this initial group and the enthusiasm in the first bulletin, the SSS seemed off to a solid start. All indications suggested strong momentum, high participation and an eager audience. With the SSS in place and Emerson running the bulletin, speciation workers could look forward to increased communication and years of productive interaction. Such optimism proved unwarranted, however. The SSS quickly fell apart.

Problems with implementation

Though it might have begun with considerable momentum, the SSS proved short-lived. Emerson's first bulletin was the only one ever produced. The bibliography stopped, too, and the address-list never underwent revision. Shortly after the first bulletin appeared, the SSS could be described, at best, as 'quiescent'.

The causes of this collapse were various and convergent. When the SSS was founded, Emerson agreed to serve as secretary – to be 'responsible for the general organization of the group' and the 'publication of information for distribution'.[41] But Emerson was a busy man in those years, and other priorities forced the society to be placed low on his list. In addition to extensive teaching and research, he was expanding his concepts of social co-ordination and the superorganism – this in the context of the global rise of fascism and considerable debate over the proper role of individuals in a society. Emerson was also ending his second term as editor of *Ecology*, writing innumerable commentaries and reviews, as well as serving several other professional societies in administrative capacities. On top of this and whatever duties he took on during national mobilization, Emerson carried a heavy administrative load in the University of Chicago's active ecology and zoology groups. He had also recently begun a major collaborative book project.[42] 'I have too much to do', Emerson regularly complained.[43]

Emerson's complaints about workload seep into the first bulletin. Although he asked for and tried to respond to ideas from members, 'a number of suggestions, although laudable, involve more time than the secretary can devote to this undertaking'.[44] Always working 'within the limits of his time', Emerson also apologized when the abstracting project proved too involved. 'Time from other activities did not permit a more adequate classification or abstracting of the literature which amounted to more than was

41 Emerson, News Bulletin, op. cit. (19), 1.

42 G. Mitman, 'From population to society: the cooperative metaphors of W. C. Allee and A. E. Emerson', *Journal of the History of Biology* (1988), **21**, 173–94, and Mitman, *State of Nature*, op. cit. (11).

43 Emerson to Simpson, 6 November 1946, George Gaylord Simpson Papers, manuscript collection 31. APS Library, folder 'Emerson, A. E'.

44 Emerson, News Bulletin, op. cit. (19), 2.

anticipated.’ He apologized for the ‘numerous typographical errors’ that could not be ‘adequately proof-read in the time available’.[45] After the first bulletin had been posted and comments came back to Emerson, he found he could not keep up with demand. ‘The difficulties [facing the SSS] are purely a matter of the time involved’, he explained.[46] Emerson was overwhelmed, and it showed.

Precisely how and how much national mobilization and America’s entry into the 1939–45 war affected SSS plans more generally are not clear. However, many specialists halted research programmes or adapted them to relate somehow to the wartime situation. Some entered military service or took on supporting roles, leaving their scientific careers temporarily on hold. Simpson, for example, served as an intelligence officer in North Africa and Italy. Dobzhansky travelled to Brazil in 1943 as part of a good-neighbour programme designed to thwart German influence in Latin America. Later, Mayr produced bird-watching manuals for servicemen in the South Pacific. For the SSS, Emerson expressed frustration with his lack of success recruiting good materials for the bulletin as members delayed work and shifted to other projects. With this disruption and dislocation going on everywhere around him, it is likely that Emerson followed the same strategy: putting the SSS and its publications on hold and setting himself to more pressing work.

Besides timing, another problem inherent in this fledgeling group was its extremely wide range of interests. Watching from England, William Turrill noticed this, suggesting to Huxley that ‘our American friends are finding the title of their Society too narrow’.[47] The wide range – from bacteriology to physical anthropology, from *Drosophila* salivary gland chromosomes to Pleistocene glaciation – made it difficult for any secretary to maintain a clear focus for the SSS or to preserve the sense of unity and common purpose. Too many constituencies pulled the society in too many directions. As described in the first bulletin, the group’s founders hoped to create a communication network for those interested in the ‘dynamics of the origin of species’. Emerson and other Society principals understood this to mean the mechanics of speciation processes, such as were discussed in the symposium Dobzhansky organized in Columbus. But others had different interpretations of what the SSS offered. The physical anthropologists wanted to discuss how best to distinguish human races. The bacteriologists wanted operational species concepts for the test-tube environment. Some members wanted to discuss specific phylogenies; others wanted to examine the origins of particular adaptations. Breadth stretched the identity of the infant SSS to its breaking point.

A large number of taxonomists with expertise in the morphology and geographical distribution of particular plant or animal groups joined the SSS, though they seem to have had little interest in the dynamics of speciation processes *per se*. Their interest in joining – based on sample investigations of their careers and on comments published in Emerson’s news bulletin – involved the new systematics (*sensu* Huxley) more than basic investigations of speciation mechanics. For the most part, systematists in the SSS simply wanted techniques to improve their classifying. Rather than investigating processes and

45 Emerson, News Bulletin, op. cit. (19), 29.
46 Emerson to Mayr, 30 April 1941, Mayr-Harvard, folder 75.
47 Turrill to Huxley, 12 June 1941, Huxley, box 15, folder 3.

mechanisms of species formation or hoping this new group would create some sort of sub-speciality within the larger field of speciation or evolution studies, these taxonomists asked questions such as: how can we define a species, subspecies or variety with less subjectivity?; how much and what kinds of divergence offer diagnostic tools when identifying subspecies?; how do we integrate cytological and genetic results with the morphological criteria we already use for taxa?[48] Although complex issues in themselves, these taxonomists regarded such questions as something other than 'theoretical' issues relating to species formation.

This cluster of interests among systematists was different in important ways from that of the SSS principals. The principals wanted to extend taxonomic work and extract from it information about biological processes and mechanisms. In many ways, the systematists' cluster would have been wholly satisfied by an organization more like that which the Association in Britain was evolving into.[49] They would not have been well served with the SSS as planned at the Columbus meeting and implemented by Emerson.

Criticisms of the news bulletin added to overall dissatisfaction. Anxious to facilitate research in this area as best he could, Huxley circulated copies of the bulletin and bibliography to colleagues in the Association soon after these publications arrived in London. Uniformly, those colleagues complained about the inadequacy of the bibliography compared with the *Zoological Record*, published regularly by the Zoological Society of London. *Zoological Record* was the international standard for taxonomic literature. Both Owain Richards and John Smart complained to Huxley that they thought the Americans were making poor use of it. Smart announced his intention to work on this problem after the war. Also sceptical of the basic function of the bibliography, Turrill managed a small, constructive note: 'I should not wish to be in any way unappreciative of Emerson's efforts – we have found some of his references useful already'.[50]

Trying times, a swamped editor, the heterogeneity of the group and this general dissatisfaction with the SSS's initial projects brought about an ungluing of the group.

'You ... have been active in constructive criticism'[51]

One of the North American speciation workers most eager to see the SSS succeed was Ernst Mayr. Experienced with changing infrastructure and knowledgeable about its importance for professional and intellectual change, Mayr had been encouraged by British success with

48 For a good demonstration of this perspective on the American scene, see the papers presented in the symposium on 'the relation between taxonomy and speciation', organized by Melville Hatch for the 1940 AAAS meeting in Seattle. Papers in that session were delivered by Hatch, Joshua Bailey, Jens Clausen, Robert Usinger and H. J. Muller and were published in *American Naturalist* (1940), **75**. Mayr discusses these different groups in the introduction to E. Mayr, *Systematics and the Origin of Species*: *From the Viewpoint of a Zoologist*, New York, 1942.

49 Winsor, op. cit. (18).

50 Turrill to Huxley, 12 June 1941, Huxley, box 15, folder 3. Also in this folder is other correspondence in reply to Huxley's circulation of the SSS material.

51 Emerson wrote this about Mayr, see Emerson to Mayr, 30 April 1941, Mayr-Harvard, folder 75.

the Association and had vaguely explored the issue of an American complement with Huxley prior to the Columbus AAAS meeting in 1939.[52] He also 'sat in' at the original discussion between Huxley and Dobzhansky where the idea for the SSS was 'hatched'.[53]

Mayr, a passionate defender of the importance of systematics in biology, strongly believed that some of biology's cutting-edge issues would benefit greatly from information already well known to his systematist colleagues. While geneticists, by and large, were just getting interested in the variation of natural populations, Mayr complained, taxonomists had been studying that subject for decades. For every example of genetic variation across geographical areas, Mayr could point to dozens of cases where morphology did the same – where populations diverged into races, races diverged into subspecies, subspecies diverged into species. Geneticists needed to consult taxonomists in this regard, and taxonomists needed to make their work more accessible to interested geneticists so the interaction could proceed. Mayr wanted taxonomists involved in ongoing research at the cutting edge, such as speciation studies, and he wanted to make it easy for outsiders to reap the 'rich harvest of systematic data' Huxley described in *The New Systematics*.[54] In the late 1930s Mayr wanted to ensure systematics and biogeography had a strong presence in the current enthusiasm for speciation studies. This was one reason the new society was important to him: it was the facilitator he had been looking for since meeting Dobzhansky in 1935, and it was a route for him to promote his deliberately balanced programme in 'bird biology'.

Though not appointed to the original executive committee – at the time he was an associate curator of the Rothschild collection at the American Museum of Natural History in New York City with neither the recognition of Dobzhansky, Simpson and Anderson nor the proximity to Emerson characteristic of others on the committee – Mayr closely followed developments regarding the SSS. He responded quickly to Emerson's first notice in March 1940, promising support and assistance when 'I know how far these plans have crystallized'.[55] But it took Emerson a full year after this notice to create the first bulletin. Mayr was moving intellectually at a rapid pace at this point of his career, and for him, this delay was inexcusable. Moreover, the wait had not been for the better. Mayr was not pleased when he finally saw Emerson's bulletin and bibliography.

Buoyed by the strong sense of his own recent accomplishment – his Jesup lectures on systematics and the origin of species at Columbia University were just concluding when Emerson's material arrived in Spring 1941 – Mayr immediately undertook to improve the situation. Whether he thought the secretarial duties overwhelming or believed Emerson lacked the skills to accomplish them effectively, Mayr quickly manoeuvred to oust him as

52 Mayr to Emerson, 6 May 1941, Mayr-Harvard, folder 75. Also see correspondence between Mayr and Lack, Mayr-Harvard, folder 62, and Mayr to Seventy, 31 March 1939, Mayr-Harvard, folder 67. On Mayr's experience in organizational work for reform, see Cain, 'Here is a field,' op. cit. (3). On Mayr's biography, see essays in *Biology and Philosophy* (July 1994), **9**; and J. Cain, 'Mayr and bird biology', (submitted).

53 Mayr to Emerson, 26 March 1940, Mayr-Harvard, folder 75. From internal evidence in this letter it seems that Emerson did not attend this 'conference' but was later approached with the idea of the group. See also Emerson to Colleagues, 18 March 1940, Dunn.

54 J. Huxley (ed.), *The New Systematics*, Oxford, 1940, Foreword.

55 Mayr to Emerson, 26 March 1940, Mayr-Harvard, folder 75.

SSS secretary.[56] For reasons that are unclear, Mayr chose to ask A. Glenn Richards if he would take over the job.

Richards, an entomologist at the University of Pennsylvania since 1939 and primarily interested in experimental insect morphology and embryology, had spent a year (1936–7) at the American Museum. During the first part of this stay, Dobzhansky delivered his 1936 Jesup lectures at Columbia University. At some time during the year – probably starting at Dobzhansky's lectures – Mayr and Richards met and became friends. At the University of Pennsylvania in 1941, this thirty-year-old, up-and-coming speciation worker (and systematist) also found himself occasionally involved in one of Philadelphia's other scientific activities, production of the *Biological Abstracts*. Mayr knew Richards had experience with editing and abstracting. He seemed an ideal candidate for the task of improving the SSS bulletin and its bibliographies.[57]

Richards was enthusiastic at first. 'After … talking it over with various people here I am ready to undertake the job if you can make such arrangements with the [executive] committee', he told Mayr in early March 1941.[58] Unsure of what route to prefer, Richards knew the path must include two important steps: consulting the executive committee on 'general and basic questions' and making sure the 'best means' were taken 'not to affront Emerson and yet get results'. Logistically, Richards told Mayr, he would need money for mimeographing and for stenographic work; his department had few resources and no financing to offer. Taking over the newsletter would be time-consuming, too. 'One warning everyone has given me is not to undertake so much that my work or research is seriously interfered with … Naturally I cannot undertake to do it all myself.' Richards wanted to see the questionnaires Emerson had collected the year before so he could identify those 'who have expressed interest and what suggestions have been made'.

Richards also discussed the abstracting project with John Flynn, editor-in-chief of *Biological Abstracts*. Flynn offered to absorb that work into a new section of his reference volumes. Richards, who had firsthand experience of section-editing for Flynn, told Mayr, 'There seem to me to be several obvious and pertinent objections to this but it may well be held in mind as an adjunct to the bibliographic service'. If the SSS did nothing more than provide an abstracting service, then *Biological Abstracts* offered more 'advantages' than 'disadvantages'. 'Perhaps I am too optimistic', Richards confidently reassured Mayr, 'but I would like to see the Society be more than that'.

After hearing from Richards, Mayr consulted Dobzhansky further about changing the SSS and replacing Emerson with someone who would produce 'results'. Records of those discussions have not survived. No doubt these were in person, and no doubt Dobzhansky told Mayr that replacing Emerson would be both difficult and unwise.[59] Emerson was

56 At this point Mayr was not on the executive committee.

57 From his own role as a contributor, Mayr was familiar with the potential importance of *Biological Abstracts* in professional and intellectual reform. See Cain, 'Mayr and bird biology', op. cit. (52).

58 Richards to Mayr (cc. Dobzhansky), 10 March 1941, Mayr-Harvard, folder 86. The previous correspondence from Mayr to Richards is lost.

59 Both Dobzhansky and Mayr lived and worked on New York City's upper west side, seeing each other frequently in this period. Mayr visited Columbia University regularly, and Dobzhansky was a familiar visitor to the American Museum. Further, Mayr's 1941 Jesup lectures at Columbia were under way in this month – March 1941 – offering regular opportunities for informal discussion.

central to the enterprise, and any refurbishment of the SSS had to keep him in the centre. Mayr communicated this news to Richards.

'I have no objection to letting the matter drop', Richards wrote to Mayr in reply.

> I also have no intention of writing Emerson suggesting I horn into 'his' party. On the other hand, if you and the others feel it might be profitable to have me serve as Emerson's assistant, I am willing to have you write him suggesting that such can be done if he wishes. It seems to me that you could do this with less danger of affronting him than I.[60]

As for the bulletin, Richards thought it 'all right' in present form and the use of critical reviews was 'good' and 'worth retaining and even amplifying'. But he was still highly critical of the bibliography – woefully incomplete, Richards complained, as it seemed to involve no careful choosing. 'Having scanned these 1,249 [*sic*] references, I am even more dubious of their value, and feel that if it is to be the chief function of the Society it would be best to work via *Biological Abstracts*'. Richards now was advocating that the SSS create and edit a section for Flynn's series, negotiating beforehand issues such as how its indexing system would accommodate the interdisciplinary nature of their work (one of Mayr's major complaints about Flynn's project). Given that the perennially troubled *Biological Abstracts* seemed finally on the upswing – 'enlarging, improving and getting financial security' – Richards suggested, 'I feel they could do a better job of this especially if we put as much work into *Biological Abstracts* as went into the preparation of [Emerson's] issue'. A section on speciation would take care of the sweeping project associated with producing a bibliography. This would relieve the SSS secretary so other work could be done.

After hearing from Richards, Mayr crafted a letter to Emerson. He did not reveal his scheming to replace the secretary. Describing the bulletin not only as a 'great pleasure' with 'so much useful information' that it served as a signal 'to the outside world' that 'the students of speciation are beginning to co-operate more closely with each other', Mayr nevertheless did not shy away from telling Emerson at least some of the problems he was having with SSS publications. 'On page 29 you asked for criticism and suggestions, and I am sure that you will welcome the following remarks'.[61]

'The point that strikes me (and everybody else I have talked to) most', Mayr explained to Emerson, 'is that the bringing out of a truly informative and complete Bulletin is more than any one person can handle'. Mayr asked for subdivision. Appoint someone to follow speciation work regarding freshwater organisms, for example; another for marine organisms. Ask someone to review recent advances in specific research areas. Arrange for 'review symposia of small papers'. 'I have made sure that several of us [local workers] are willing to co-operate with you if you want to delegate to us part of the work. So please do not hesitate to let us know if you want some specific job done.'

As for the bibliography, merely listing papers was inadequate; there was no means to sort this list unless 'you have a large library at your elbow where you can check up [on] what each paper contains'. Asking people to volunteer titles was not productive either.

60 Richards to Mayr, 20 March 1941, Mayr-Harvard, folder 86.
61 Mayr to Emerson, 31 March 1941, Mayr-Harvard, folder 75.

> Of every [paper] that I read I am sure that you probably have read it also, and then I do not bother to send [the title] to you. If, on the other hand, I had the job of gathering all the information on speciation in island birds [Mayr's research speciality] or some such job, I would painstakingly gather all such information.

As provided, most of the papers listed seemed to 'have only the slightest connection with speciation', while the 'truly important papers' were 'not sufficiently emphasized'. 'I wonder', Mayr inserted plainly,

> whether it would not be possible to incorporate the bibliography with *Biological Abstracts* by adding ... a separate section on speciation, and listing in it all the papers of interest to us. The bibliographic section of the Speciation Bulletin would then be able to concentrate on a detailed, both informative and critical, discussion of some 80 or 100 papers that are primarily concerned with speciation.

It was a month before Emerson replied.[62] 'Your comments on our recent mimeographed material were about the best we received in the way of constructive comment', he told Mayr. The source of the problems, Emerson explained, was simply time – time to organize the material and time to recruit information from the membership. Perhaps misunderstanding Mayr's suggestions for assistance and obviously with a view to his available resources, he turned down everything Mayr had proposed: 'I cannot take more time in personally writing every [member], but must rely upon the members to send me their material voluntarily'. Instead, members who volunteered material had to 'trust me to edit and get [their contributions] out within the confines of our budget and capacity to handle such matters'. Emerson seemed uninterested at this point in becoming the kind of active editor Mayr envisioned. 'We should be delighted to receive any information from you' for the bulletins, Emerson told Mayr, clearly hinting that he was not going to orchestrate larger projects like those Mayr proposed. If Mayr wanted to create the summaries, that would be up to him.

Probably Emerson was focusing so directly on the constraints of his publications that he missed Mayr's larger point about delegation. Neither did Emerson have the luxury – as he would have called it – of time to search the country trying to recruit prospective authors of major reviews or to involve himself in lengthy negotiations with Flynn about *Biological Abstracts*. This focus is suggested later in Emerson's reply to Mayr. Dobzhansky (and perhaps others) had asked Emerson to place Mayr on the executive committee. Casually, Emerson wrote to Mayr, 'I hereby tender you this appointment if you feel in the mood'. Describing the office, Emerson continued, 'So far I have asked little work for the members, but I would appreciate more voluntary contributions than have been forthcoming'. Emerson hoped Mayr could oblige.[63] With Mayr scheming to replace the secretary, the irony here is inescapable.

Mayr's reply to Emerson was cordial, though clearly restrained: 'I appreciate the honour of being appointed a member of the Executive Committee and I shall be delighted to accept. The speciation society has been close to my heart for a considerable time.'[64] Not pushing Emerson again, he only spoke in general terms about eventually constituting the

62 Emerson to Mayr, 30 April 1941, Mayr-Harvard, folder 75.
63 Emerson to Mayr, 30 April 1941, Mayr-Harvard, folder 75.
64 Mayr to Emerson, 6 May 1941, Mayr-Harvard, folder 75.

SSS ‘on a more formal basis’ and creating a ‘journal devoted to the publication of general papers on systematics and speciation’. ‘I don’t think there is any other kind of literature in the field of biology as scattered as that of speciation and general systematics.’

Mayr was furious in a letter to Richards, written the same day.[65] Quoting Emerson’s letter at length, he frothed, ‘In other words, he wants to continue as he has done in the past and not reorganize the set-up as several of us had suggested’. Without Emerson’s co-operation, Mayr knew that refurbishing the SSS now was impossible. ‘Under the circumstances there is nothing that can be done and I am sorry that I caused you so much trouble … I still believe that we can eventually improve the situation, but it is obvious that the time for this has not yet come’.

When Mayr later described to friends what had caused the SSS to go ‘defunct’, he blamed Emerson’s ‘lack of initiative’.[66] Before long, Mayr was offered an opportunity to work on a parallel project, chairing the genetics section of the National Research Council’s Committee on Common Problems of Genetics and Paleontology. He jumped at the chance, thereby bypassing Emerson and the unravelling SSS.[67]

A short-lived organization

The Society for the Study of Speciation was begun as an informal, utilitarian addition to community infrastructure. The SSS was too short-lived for historians to claim for it major impacts. That said, it remains central to the history of infrastructure in evolutionary studies during the synthesis period for several reasons.

First, consider the question of continuity with other groups. Superficial histories of synthesis-period organizations place the SSS as ancestor to the Society for the Study of Evolution (SSE, formed in 1946) and the journal *Evolution*.[68] For instance, when Mayr and Simpson fashioned a historical legacy for their later community-organizing, they claimed this ancestry, using its precedent as evidence of popular demand and legitimacy for their new infrastructure projects. However, claims to continuity can be sustained only in a minimal sense, and they obscure important discontinuities. Minimally, Emerson transferred to the SSE a small balance remaining in the SSS treasury. He also sent along the address-list for recruitment. For a brief period in 1945–6, Emerson and some of the ‘membership’ spoke of continuity, and at the SSE organization meeting in mid-1946 they spoke of a ‘rebirth’ of the speciation group.

But to emphasize continuity between the speciation and evolution societies obscures the breaks. Most importantly, after Mayr usurped the role of organizer (beginning within the Committee on Common Problems and building with the SSE) he took pains to make clear that his product was not Emerson’s product. Supported by the momentum of the

65 Mayr to Richards, 6 May 1941, Mayr-Harvard, folder 86.

66 Mayr to Huxley, 3 January 1946, Materials relating to Society for the Study of Evolution, collection B/M451S 1991 1027 ms, APS. Unless otherwise noted, the folder is ‘Early letters’ (hereafter SSE Archive).

67 J. Cain, ‘Common problems,’ op. cit. (3).

68 Emerson later claimed to have a ‘certain paternal pride’ in the speciation society and protested Mayr’s claim that he was simply a ‘midwife’ for the group. See Emerson to Just, 28 March 1952, folder ‘SSE-History and Related Materials’, SSE Archive.

Committee on Common Problems, Mayr worked to produce formal networks of communication – in the form of professional journals and sanctioned programmes at professional meetings – over informal ones. He also sought a more specific direction for his projects: 'All papers [published in *Evolution*] must deal with evolutionary factors and forces. Papers that deal with other subjects, as for instance straight taxonomy, nomenclature, or with the mechanics of inheritance, are not considered to fall within the scope of the new journal'.[69] Many of the constituent interests expressed in Emerson's 1941 news bulletin simply fell outside Mayr's expressed domain. If they failed to involve process and mechanism, those interests would be out of place. Most importantly, Mayr had little interest in merely exchanging information. His interests were synthetic and constructive. Communication and dialogue were first steps, but Mayr looked past these. He wanted theory-building, discussions of implications and rational integration (keeping in mind his sense of proper 'balance' across specialities). In itself, general swapping about seemed pointless to Mayr.

A second historically important feature of the SSS involves the rhetoric of co-operation and exchange. Officially, the SSS was created primarily for practical reasons: to facilitate communication and the transfer of information; to provide a vantage point for tracking developments; and to provide a means for critical dialogue and assessment. The SSS was also intended to serve community functions, providing a means for bringing together people who worked within fundamentally different research contexts but who shared interests in the same kinds of research problems. Underneath this rhetoric are some rather partisan and individual interests that should not be submerged.[70]

'Speciation workers' were a heterogeneous group in the late 1930s. Different constituencies associated with this identity sought different things from co-operation and exchange. Some wanted a forum for further developing the 'new systematics' (*sensu* Huxley) within nomenclature. Others were expanding their studies of singular processes, such as polyploidy or hybridization.[71] Some speciation workers were concerned with these only in terms of their own research programmes. Others saw in the Society a mechanism for introducing such new techniques and epistemic standards into their circles of colleagues. Still others looked to the Society for a balancing mechanism – so that in the rush towards cytological and genetic mechanisms and techniques, decades of international (and especially for Mayr, German) traditions of solid morphological work were not passed

69 Mayr to Council, 7 November 1946, in Mayr-APS, folder 'SSE History'.

70 Here perspective analysis is beneficial. J. Cain, 'A matter of perspective: disparate voices in the evolutionary synthesis', (submitted).

71 Joel Hagen and Keith Vernon have separately discussed the powerful analytical methods developed by the mid-1930s that allowed geneticists and cytologists to follow divergence and isolation at the hereditary level. J. Hagen, 'Experimental taxonomy, 1930–1950: the impact of cytology, ecology, and genetics on ideas of biological classification', Oregon State University, Ph.D. dissertation, 1981, UMI NO AAI8128569; J. Hagen, 'Experimentalists and naturalists in twentieth-century botany: experimental taxonomy, 1920–1950', *Journal of the History of Biology* (1984), **17**, 249–70; and K. Vernon, 'Desperately seeking status: evolutionary systematics and the taxonomists' search for respectability', *BJHS* (1993), **26**, 207–27. Claims by speciation workers that these methods created objective means for determining taxonomic rank were extreme manifestations of a widespread impression that these methods provided significant new tools for studying systematic groups and for investigating evolutionary processes.

over and forgotten.[72] When Mayr claimed the Society was signalling 'to the outside world' that 'the students of speciation are beginning to co-operate more closely with each other', he was asserting a growing respect for those he identified with more than he was claiming real integration for the community or their theories.[73]

On the rhetoric of exchange, a key point to note involves the function of surveillance. With the sharp expansion of interest in species studies and speciation mechanisms during the 1930s, one problem involved the subject's breadth. As Emerson's bibliography demonstrated, so much work was under way in so many specialities that no one could possibly manage to keep their fingers on the pulse of such variety. What was new? What was important? What developments in one domain (such as isolating mechanisms related to chromosome behaviour in plant systems) might have relevance for another (such as the same in *Drosophila*). Acting with the interests of general theorists, the principals should also be seen as motivated by their own need for access to current information. They had no time to read many bodies of specialist literature, much less to learn the technical vocabularies and baseline information necessary for separating insights from masses of information. What they needed were systems for 'exchange' whereby workers in domains distant from their own would circulate their basic interests, data, conclusions and questions. This circulation would provide a means for surveillance: an efficient mechanism for keeping 'in touch' with activity that allowed for direct contact and detailed knowledge-transfer later. Nothing destroys prospects for a 'general' theory quicker than ignorance of developments in an area of supposed application.

Historians of the synthesis period might see in the SSS a mechanism for 'disciplining' knowledge as part of the social and intellectual construction of academic fields.[74] This would be a mistake; it is too strong. Aside from the fact that it was active for so short a time, the informal structure of the SSS and its utilitarian products were well suited to communicating information and surveillance, but poorly suited to oversight, direction management, demonstration to outsiders or enforcement. And with so loose a membership base, plan for action, bibliographic net and project interest, the SSS seemed poorly equipped for consensus definition. At the start, there were no expressed intentions of producing position statements or policies, no plans for meetings, no sense of a need for regulation. Self-consciousness certainly manifested itself. 'Speciation workers' were coming to identify themselves loosely as a collective having common research interests in mechanisms and processes alongside other common theoretical problems. This was part of a general transition from product to process – from zoology and botany to biology – in life sciences of the twentieth century. Though one step in the transition, the SSS was not

72 Mayr's partisan position is discussed in Vernon, op. cit. (71) and J. Cain, 'Mayr as community architect', op. cit. (3). In her representation of Mayr's role organizing evolutionists and editing journals, Smocovitis misses this point. V. B. Smocovitis, 'Disciplining evolutionary biology: Ernst Mayr and the founding of the Society for the Study of Evolution and *Evolution* (1939–1950)', *Evolution* (1994), **48**, 1–8; 'Organizing evolution: founding the Society for the Study of Evolution (1939–1950)', *Journal of the History of Biology* (1994), **27**, 241–309.

73 Mayr to Emerson, 31 March 1941, Mayr-Harvard, folder 75. In the 1950s, Mayr more politely attributed the failure of the Society to 'war conditions'. See 'History of the Society for the Study of Evolution', Mayr-APS, folder 'SSE History'.

74 T. Lenoir, *Instituting Science: The Cultural Production of Scientific Disciplines*, Stanford, 1997; Smocovitis, op. cit. (72).

enough for the meaningful social or intellectual cohesion implied by terms such as 'field', or 'discipline'. Historians can afford to be more cautious on this point.

So strong a focus on the SSS would also risk missing substantial infrastructural activity beforehand and elsewhere. Some of this activity was formal. Obvious examples include the Association in Britain, parts of the revived Columbia Biological Series, and the various speciation symposia at annual AAAS meetings. Others were informal. The biosystematics group (roughly twenty-five people) in the San Francisco Bay area had been meeting informally since 1935.[75] A group of ecologists and zoologists around Emerson regularly met in the Chicago area.[76] Mayr ran an informal reading 'seminar' at the Linnaean Society of New York.[77] Other informal groups are identified in the notes of Emerson's 1941 bulletin. The SSS was part of this larger process of expansion in speciation studies and part of a larger process of professional and intellectual change underway in evolutionary studies. Though significant, the SSS need not be the cornerstone upon which synthesis historians build everything that comes later.

75 See Jens Clausen's note in Emerson's news bulletin, op. cit. (19), 5. On the biosystematics group, see Hagen, op. cit. (71), as well as V. B. Smocovitis, 'Botany and the evolutionary synthesis: the life and work of G. Ledyard Stebbins', Cornell University, Ph.D. dissertation, 1988, UMI NO AAI8900923.

76 Mitman, *State of Nature*, op. cit. (11).

77 Cain, 'Mayr and bird biology', op. cit. (52). Mayr's Linnaean Society work is placed firmly into the context of American ornithology by M. Barrow, Jr., *A Passion for Birds: American Ornithology after Audubon*, Princeton, 1998.