

Launching the Society of Systematic Zoology in 1947

Joe Cain, J.Cain@ucl.ac.uk

Department of Science and Technology Studies, University College London

Introduction

Thirty years after the fact, American entomologist Richard Blackwelder (1909–2001) was still angry. In an article celebrating twenty-five years since the launch of *Systematic Zoology*, Blackwelder barely held his tongue. “Our era started in an atmosphere of controversy. There was difference of opinion on basic ways to look at systematics. And when it came time to form an organization to further the science of systematics, there was strong difference of opinion on how this was to be done.” (Blackwelder, 1977: 109). This split, Blackwelder explained, led to the formation of two societies rather than one. In Blackwelder’s account, his side had tried to be civil and flexible. The other side’s “refusal” to co-operate, however, led to a reluctant divorce. Rejected, his side went their own way in 1947, organizing the Society of Systematic Zoology (SSZ).

This paper examines the SSZ’s launch. The basic outline of events is recited frequently — normally deriving from Blackwelder (1977: 109–112) or Hull (1988: 106–107). These accounts lack detail and context. They also fail to draw from archival material now available. This paper aims to fill these omissions. Why did the society form when it did? Who was at its center? How do these efforts fit into the larger context of American life sciences in the aftermath of World War II? Most important, I challenge Blackwelder’s account of the society’s founding. Blackwelder entered the society rather late in the organizing process. His motivations (genuine though they were for him) were only part of a more complex picture. The conflict that motivated him seems peripheral to the aims and objectives stated by those at the first steps forward.

Organizing the society, 1946–1947

The SSZ organized during the December 1947 meetings of the American Association for the Advancement of Science (AAAS) in Chicago, Illinois, USA. Leading this project were Waldo LaSalle Schmitt¹ and George Wharton². The decision to organize was not spontaneous. Schmitt and Wharton worked for more than a year to produce it. Most participants had been invited to join via a circular distributed four months before. The long process was completed with the approval of the permanent constitution in September 1948.

According to Blackwelder’s legend, a misunderstanding provided the crucial spark. A year and a half before the SSZ’s formal launch in Chicago, Schmitt and Wharton attended the organization meeting for another group, the Society for the Study of Evolution (SSE).³ Prominent systematists led this organizing effort (e.g., Ernst Mayr, Alfred Emerson, George Gaylord Simpson). Others strongly backed it (e.g., Carl Hubbs). There seems to have been a general impression before their meeting that the proposed society would lean heavily towards the interests of systematists or that it would work to place systematics at the center of the biological sciences.⁴ But organizers of the evolution group disappointed those with this impression. They had a fairly clear idea where they wanted to steer the new society. First, they wanted it to focus on the analysis of evolution: its patterns, processes, and mechanisms. Second, they wanted to use it as a lever for elevating the status of evolutionary studies in their home disciplines. These relatively narrow interests would come at the expense of other developmental issues within those home disciplines.⁵

As the legend continues, Schmitt rose at the organizing meeting of the SSE during their discussion of terms of reference. He asked for a broadening of the new society's domain to cover all of systematics (i.e., nomenclature, rules, collections management, etc.) and not simply mechanisms of evolution. This supposedly provoked an extended argument with the organizers, who complained such an expansion in their goals would dilute their more specific interest in evolutionary processes. As Schmitt's argument stalled, Wharton supposedly tugged on his jacket. He asked Schmitt to sit down and not to worry. They could start their own group along the lines Schmitt was suggesting.⁶

The first record of Schmitt and Wharton taking action towards a society for systematists dates to April 1947, twelve months after the SSE's launch. While looking ahead to a set of meetings in the summer that would bring Wharton and other sympathetic colleagues to Washington, DC, Schmitt returned to the question of an organization for zoological taxonomy.⁷ For reasons he never explained, Schmitt found an organizational model in the American Society of Plant Taxonomists, founded in 1935 (Schofield, 1998). He adapted their corporate documents and asked Wharton to consider if this approach provided a reasonable starting point. Wharton replied on 1 April 1947.

The constitution of the Plant Taxonomists seems to be readily adaptable to a society of taxonomic zoologists. I would suggest the title for our society "Union of Taxonomic Zoologists." The purpose of the society might be expressed simply as follows: Purpose-To advance the study of taxonomic zoology and the welfare of taxonomic zoologists by providing a medium for the expression of the needs, ideals, achievements, and purposes of its memberships. ... In preparation for this meeting it would be desirable to contact taxonomists who are interested in the project at as many museums and universities as possible in order to line up membership in their respective institutions.⁸

Wharton cautioned against moving too quickly. "At the meeting in Washington this summer a nucleus of an organization could certainly be started," he suggested. However, no formal action should be taken (such as naming charter members, adopting a constitution, or electing officers) least colleagues elsewhere "identify the society too closely with Washington" — i.e., with the USNM. Wharton feared this might hinder their intentions for a "truly national" character to the group.⁹ Schmitt quickly agreed on the slow pace. "We should do as the Society for the Study of Evolution did, forming only a tentative organization and leaving the formal organization for the AAAS meeting in December."¹⁰ The kernel of a plan agreed, these two busy men set their organizing aside for other, more pressing, work.

A four-month break in correspondence separates this basic arrangement with the next substantive action recorded.¹¹ Schmitt picked up his correspondence with Wharton again in early August 1947. Apparently, the "tentative organization" had not come to fruition over the summer, so Schmitt's focus moved to the upcoming AAAS meetings in December. On 7 August, Schmitt sent Wharton the draft for a circular "we might mail out to interested parties" together with a draft constitution.¹² As previously agreed, the constitution was co-opted largely from the plant taxonomists. Schmitt asked Wharton to be critical of the text, however, and to think hard about the circular because he wanted to appear certain of their intentions once going public. Schmitt also passed on to Wharton informal comments from Loren Woods (Chicago Museum of Natural History), who suggested the circular be "more emphatic about the fact that there is no other society to serve the interests and viewpoints of the taxonomic workers as such." This might have been an effort to distance their plans from the evolutionist society; however, if Schmitt meant this, he left references to the evolutionists in the sub-text.

In his August 1947 correspondence with Wharton, Schmitt also worried about securing an enthusiastic and competent secretary-treasurer for the administration of their society. With many years experience watching people in such roles, Schmitt knew the importance of this position for a new organization. He also knew the burdens it would impose on the volunteer.¹³ "If we are to launch the society, and I am anxious to do so," Schmitt told Wharton, "someone

will have to volunteer and I will do that now, unless we can find someone more willing to take over.”¹⁴

Considering who to invite for their organizing meeting, Schmitt proposed going through several of the most recent numbers of *Zoological Record* and *Biological Abstracts* for names, as “this would reach the more active workers and in that way get spread around by word of mouth...” Needing to keep tight control of costs, Schmitt was forced to rely on informal transmission of their news: “...we will scarcely be able to circularize the full membership of each society that may be made up largely of taxonomists as desirable as this might be.” Schmitt concluded his letter to Wharton by affirming Wharton’s concern about signatories on the circular. Wharton had suggested these signatories be recruited with a careful eye towards representing various specialties and regions. Still anxious to avoid impressions of a Washington bias to the group, Schmitt agreed.¹⁵

Wharton’s reply on 18 August was enthusiastic and filled with suggestions on how best to “spread the word”. He added rhetorical flourishes to their circular. He also affirmed again Schmitt’s suggestion about copying the evolutionists, proposing “we form an organization committee this Christmas and have the formal adoption of a constitution and election of officers a year from now.”¹⁶ The two men agreed, then set to work recruiting signatories for the circular.¹⁷ Schmitt approached candidates himself.

Enthusiastic responses by possible signatories encouraged these men on. Alexander Petrunkevitch (Osborn Zoological Laboratory, Yale University) must have been overwhelmingly supportive in his reply. His letter brought Schmitt to tell Wharton, “Doesn’t [his] response make you feel like cheering?” Replies sometimes also proposed additional scope for the future society. Petrunkevitch’s letter must have included the suggestion of publishing a journal. “I like the sound of a ‘Journal of Systematic Zoology’,” Schmitt told Wharton. “That is something to dream about and without question for the future.”¹⁸

Their circular was ready in early September 1947. Schmitt and Wharton chose five signatories, themselves included. The others were Carl Frederick William Muesebeck (US Department of Agriculture economic entomologist), Karl Patterson Schmidt (Chicago Museum of Natural History herpetologist), and Arthur Sperry Pearse (Duke University parasitologist). Schmitt distributed 500 copies of their circular according to their previously agreed strategy, a draft constitution accompanied each circular.¹⁹ The title of their circular left no doubt as to their goal.²⁰ Because of its importance, their two-page circular is quoted here in full.

Proposal for a Society of Systematic Zoologists [8 Sept 1947]

Dear Colleague:

The world is still in a ferment; times are still troublesome. We are all of us busy beyond words, and perhaps do not wish to be bothered even with things of very direct concern to us as taxonomic workers.

Where in all the welter of proposals for the advancement of science and of science legislation, passed or pending, has the importance of the knowledge of the kinds of animals received its due recognition? In the Vannevar Bush report, “Science, the Endless Frontier,” the “description of species of animals, plants, and minerals” has been grouped with the establishment of standards for hormones, drugs, and X-ray therapy, as background research. Pure research, on the other hand, is defined as “research without specific practical ends” resulting “in general knowledge and understanding of nature and its laws.” We may have our own opinions as to which type of research our labors as systematists are to be classed. But are individual personal opinions enough to make our legislators, other scientific colleagues, and the laymen aware of both the needs and the worthwhileness of taxonomic study?

The plant taxonomists saw the light some eleven years ago and founded the American Society of Plant Taxonomists. So successful has the society been in advancing the common interests of the taxonomic botanists that they now have a flourishing society of more than 600 members, American and foreign.

The fact that there is no similar society to serve the interests and viewpoint of the taxonomic workers in zoology has prompted this letter. We, the undersigned, feel that there is a definite need for a Society of Systematic Zoologists and that such an organization can, and will, be of mutual benefit to its members and make a distinct contribution to the welfare and future of systematic zoology.

Its purpose and activities as we envision them would be:

First, to provide recognized official support for the point of view of the systematic zoologists so that their needs, as well as their contributions to zoology, may become better known to workers in other fields of science, and to the public in general.

Second, to serve as a clearing house for workers in systematic zoology, for the exchange of pertinent information and concepts, the identification of specimens, and the advancement of the study of systematic zoology.

Third, to compile and issue a directory of its members, including their special field of study, the groups of animals which they are willing to identify, and the faunal areas or regions in which they may be especially interested. [page 2]

Fourth, to publish a news letter at least once a year giving news: of current taxonomic research work for facilitating cooperation and avoiding duplication of effort; of material desired for the furtherance of specific studies; of expeditions in progress or planned that may be of service to systematists, or may be seeking trained taxonomic personnel; and of such other matters, domestic or foreign, as may be of value to systematic workers.

Fifth, to cooperate with existing societies whose membership may be in part or largely made up of individuals interested in systematic zoology in the support of projects of special interest to systematic zoologists.

Sixth, to provide government agencies and other establishments having need of experts or specialists in systematic zoology with names of those best able to serve them, and

Last, but not least, to hold an annual meeting in conjunction with other zoological societies, perhaps in the nature of a luncheon and business meeting, without the presentation of scientific papers.

If you are interested in the formation of a society along the lines suggested, will you not kindly let either Dr Waldo L. Schmitt [postal address omitted] or Dr. G. W. Wharton [postal address omitted] have your frank reaction to our proposal, enclosing, if you please, a carbon of your letter. An organization meeting will be held during the Chicago meeting of the AAAS. The Association has arranged for us to have the use of Parlor D, Congress Hotel, at 10 am, Monday, December 29, and will announce this fact in their official program. For purposes of discussion at that meeting, we append what is largely a paraphrase of the constitution of the American Society of Plant Taxonomists.

Because of practical limitations we have not been able to send this letter to everyone interested in systematic zoology. We should, therefore, appreciate

it if you would “pass the word along” to others so that as many as possible can cooperate in this endeavor.²¹

In their two-page proposed constitution, Schmitt and Wharton identified the new society’s object as “to promote the interests of taxonomy and systematic zoology for both invertebrate and vertebrate animals, museum and experimental work, and the scientific welfare of its members.” Anyone “sincerely interested” in these objects and who “has done serious work in systematic zoology” was eligible to join. The society’s governing Council would have the job of deciding which applicants fit these criteria.

Analysis: themes motivating a sense of need

Schmitt and Wharton developed four major themes in their 8 September 1947 circular. These are key for understanding why the SSZ launched when it did. This section breaks from the narrative about organization to analyze the perceptions of need expressed in this circular.

1. Serving the work of day-to-day taxonomy

Though it might be seen now as a rather generic call for fellowship, Schmitt and Wharton’s circular points to a general absence of infrastructure within systematics in the immediate post-war years. This lack of support involved several dimensions of day-to-day work.

First, there was the return to peace. Schmitt and Wharton’s circular contains a call to re-establish contact. A great deal had changed since the start of the war. Careers had changed as a result of national service and shifting research opportunities to support the war effort. Institutions had shifted their missions. Research programs had grown and absorbed new techniques and new demands. Disruptions to the normal flow of causal communication — caused, for instance, by the cancellation of meetings owing to travel restrictions — meant many felt out-of-contact with other members of their specialties. Some of Schmitt and Wharton’s efforts to organize no doubt simply sought a mechanism for re-establishing contact with old friends, catching up on professional and technical developments, collectively assessing developments over the past half decade, and re-launching the work of their particular specialties.

Moreover, the return to peace meant a return to work. In terms of absolute load, curatorial work in the post-war period was rather more busy than usual. Collections in many museums were being returned, unpacked, and resorted. (This was especially true of large institutions along the American eastern seaboard where collections had been evacuated.) To this, a general shortage of technical labor slowed progress returning curation to pre-war levels. Collections and research also had been expanding rapidly in areas with potential military and medical value or where service personnel had been stationed overseas. Systematists associated with teaching institutions also saw large increases in student populations and training demands. At the very least, systematic workers in the immediate post-war years were extremely busy professionals.

One sub-text in Schmitt and Wharton’s call to organize was a concern over the rapidly expanding scale of science in the previous decade. A national commitment for science and technology facilitated a change in its scale. AAAS membership rose sharply following the war: from twenty-seven thousand in 1945 to forty-seven thousand in 1950 (Wolfe, 1989, Lewenstein, 1999). The number of new societies and publications also ballooned (Price, 1961, Bates, 1965). In the life sciences, this had the effect that an expert in one specialty now stood little chance of using informal networks to monitor relevant activity in both their own and related specialties.²² In this context, calls for directories and annual meetings, newsletters and notices of activities sought to provide workers with tools for benign surveillance. Through these mechanisms, “duplication of effort” could be avoided, “matters...of value” could be traced, and a “clearing house” approach could work to bring disconnected activities or untapped collections to the service of interested specialists. Schmitt and Wharton’s efforts

were by no means unusual in the immediate post-war years. Scientists in every discipline were struggling to keep up with the torrent of activity and change.

Another sub-text in Schmitt and Wharton's call involved concerns about nomenclatural practice. It's hard to recreate the sense of uncertainty over zoological nomenclature during the immediate post-war years. In 1947 the most recent International Congress of Zoology (XII, 1935 in Lisbon) was more than a decade in the past. Another was expected (Paris, July 1948), but details were vague during most of 1947.²³ With European institutions far from recovered and East-West co-operation increasingly curtailed, Americans felt a keen nervousness about the current state of play. This is a complicated subject in its own right, but the key point to remember is that it was by no means clear in the mid 1940s how nomenclatural disputes could be adjudicated. Local bodies, such as the USNM's "Committee on Zoological Nomenclature," also tried to assert national and international influence as a way to fill this vacuum (e.g. Blackwelder et al., 1947). Moreover, arguments regarding nomenclature required considerable skill as a litigator. International conventions existed in a netherworld between the 1905 *Règles* (Rudeval, 1905) and a long series of Opinions issued by the International Commission on Zoological Nomenclature.²⁴ Numerous efforts to summarize the current state of the rules appeared following the Lisbon congress. This reflects the degree of confusion around the bundle of poorly digested case law.

Promoting an official platform for American expertise was one step towards stabilizing the situation. Schmitt's plans for the SSZ included committees to do precisely this. As an aside, this paralleled events in Britain, where the revival of the Association for the Study of Systematics in Relation to General Biology and its consolidation into the Systematics Association in 1946 can be understood as another effort to get the systematics community back in its feet (anonymous, 1948).

2. Advancing the principles of systematic zoology

Schmitt and Wharton's emphasis on "principles" underscored two themes active in the post-war context. One involved a fundamental transition in American life sciences.

As professional groups reassembled in the immediate post-war years, some undertook fundamental reassessments of priorities, methods, and principles. In the life sciences, this furthered a process of disciplinary realignment underway since the start of the century involving a transition from object to process-focused work, or from zoology to biology.²⁵ In this realignment, new specialties were represented as transcending traditional disciplines grounded in the comparative affinities of organisms (such as between mammalogy, ornithology, ichthyology, and so on) and as building on functional or process affinities (such as genetics, developmental biology, ecology, endocrinology, behavior, and evolution). The extraction of a discipline-transcending, principle-based biological specialty of systematics (sometimes "systematic zoology" or "systematic biology") from organism-focused taxonomic specialties came rather late in the overall sequence of realignments in the life sciences. Nevertheless, it represented one stage in this much broader shift in interests and emphasis. The SSZ provided a community infrastructure for promoters of this realignment. It helped them draw attention not only to extracted principles within systematics but also to the extracting process itself.

On this point, when *Systematic Zoology* first appeared in 1952, contributions were solicited especially with reference to "principles and the application of principles of wide implication and general interest in any phase of systematics, such as comparative anatomy, zoogeography, paleontology, taxonomy, classification, evolution, or genetics ..."²⁶ Blackwelder later emphasized:

...*Systematic zoology* is not intended for the publication of descriptive papers. The Council sees its field as the philosophic aspects of systematics, its principles and problems, as well as news of systematists, their institutions, courses, and publications. Systematic material is not completely ruled out,

but it must be subordinate to the discussion of principles. (Blackwelder, 1952: 92)

A second theme in Schmitt and Wharton's emphasis on principles focused on the consideration of particular principles in themselves. Blackwelder's many retellings of the SSZ launch place conflict over principles at the very center of Schmitt and Wharton's motives. In his founding narrative, these three lined up on one side of the conflict. On the other side were evolutionists of the SSE with their phylogenetic approach, especially Mayr and Simpson.²⁷

Despite Blackwelder's later representations, Schmitt and Wharton's original call for the SSZ did not express a desire to promote any particular systematic principles or philosophy over any other. Interestingly, they neither identified these differences in their circular nor constructed a sense of conflict in which their side needs organization. Schmitt was not openly hostile to the evolutionists.²⁸ He maintained good relations with Mayr throughout his career. He also encouraged the evolutionist society during its organizing phase, even writing a strong letter in support of Mayr and Simpson's proposal to support the journal, *Evolution*.²⁹ He was asked to assist with a 1949 membership campaign for the evolutionist society.³⁰ Even in deep disagreements with Simpson, Schmitt is cordial. When discussing his intellectual differences with the evolutionists, Schmitt presented them as a matter having little to do with him. He regularly excluded himself from assessments of quality — apologizing for “not being an evolutionist...” when asked to consider issues of evolutionary taxonomy. Ignoring the presence of a conflict allowed Schmitt to disagree with their approach and maintain his own emphasis on other methodologies without having to strain his working relationships with increasingly prominent colleagues.³¹

Some proponents of different approaches certainly made their conflicts known (e.g., Winsor, 1995) and considerable discussion took place among systematists about the relative strengths of various approaches and how their results could be best reconciled (e.g. Huxley, 1940). Moreover, polarization and affiliation into tribal groups played an important role in SSZ activities after the mid 1950s (Hull, 1988). But conflict and polarization were not Schmitt and Wharton's style. Their calls for the “exchange of pertinent information and concepts” and for “the advancement of the study of systematic zoology” represent an interest in discussing all aspects of systematics within an overarching pluralism. Though each man had strong views on these matters, they chose to step back from tribalism to consider larger disciplinary issues. They promoted this approach in their circular.

Too much analytical focus on the conflict between supporters and detractors of evolutionary taxonomy obscures developments in systematic principles during the 1930s that cut across this divide and that were just beginning to assert wide influence as the international situation deteriorated. Most important, “experimental taxonomy” brought important changes to systematics in inter-war America. Overall, its appeal involved the promise of greater objectivity and consistency in determinations, plus the hope that criteria could be chosen for keys that somehow were more fundamental to understanding the kind of organism on hand. As “experimental” techniques became increasingly fashionable, researchers used this label rather loosely. By the late 1930s, “experimental” practice in taxonomy included several general areas of activity:

1. use of characters accessible through laboratory — such as biochemistry, serology, and karyology (chromosome banding patterns, chromosome numbers), and quantitative methods
2. use of characters whose extent of phenotypic variability can be tested through experimental manipulation or crossing
3. use of new categories of characters that seemed more biological and functional than morphological and structural — such as those involving behavior, physiology, or ecology
4. use of criteria derived from experimental tests (such as hybridization trials or crosses) and measurements of process (such as chromosome/gene flow)
5. use of tools to express confidence on determinations (such as inferential statistics)

Hagen (1984) provided a general introduction to some of these dimensions; most are rather poorly studied by historians. Some of the most widely recognized exemplars of these methods included work by Babcock's team on *Crepis* (Babcock and Stebbins, 1938, Babcock et al., 1942), Kinsey's (1936) work on *Cynips*, Miller's (1941) studies of *Junco*, Anderson's (1949) hybrid work on *Tradescantia* and *Iris*, Patterson's (1941) field work with *Drosophila*, and Hubbs' (1940) hybridization experiments in fishes. Simpson and Roe's (1939) *Quantitative Zoology* pressed the utility of inferential statistics.

Evolutionary taxonomists — anxious in the 1930s to create more solid epistemic foundations — absorbed many of these “experimental” techniques as a way to solve persistent complaints from other biologists about the objectivity and testability of their work. This was expressed most obviously in efforts to promote “biological” species concepts (Dobzhansky, 1937, Mayr, 1942) and in attempts to document “evolution in action” through the identification of subspecies, Rassenkries, and clines (Mayr, 1941, Huxley, 1939a and 1939b, Rensch, 1929). Huxley's (1940) excitement for a “new systematics” derived largely from a sense of elation over the union between experimental and evolutionary themes in systematics.

Evolutionary systematists weren't the only ones appropriating developments in “experimental” taxonomy. Blackwelder and Boyden (1952) provide an competing example.³² These experts represented themselves as practical, atheoretical taxonomists also anxious to promote experimental techniques as part of a broad programme to set systematics on more secure epistemic foundations. Methods developed by experimental taxonomists held great promise for improving the quality of all classifications. Experimental data played increasing prominent — then dominant — roles in taxonomic determinations (just as the role of DNA sequencing has done since the 1990s). But this work needed to take place, Blackwelder and Boyden argued, independently from the work of establishing evolutionary connections or studying evolutionary processes. Taxonomic categories should only reflect degrees of similarities and differences, groupings should not build on phylogenetic connections, and nomenclature need not reflect the degree of evolutionary association. Breaking the connection between phylogeny and taxonomy removed one layer of hypotheses and put systematic work on the best possible evidential grounds.

With “experimental” techniques on the rise, more researchers coming into the subject, and the prospect of improved senses of objectivity and testability, this was an exciting time for systematists — opportunities for considerable advancement seemed on the horizon. In calling for an organization to consider systematic principles, Schmitt and Wharton might have been far more interested in considering the value and implications of new “experimental” techniques for systematic work. All taxonomists would be worrying about these issues in the post-war years and considering how to balance their relative value for day-to-day work.

3. Offering a service role to those needing taxonomic expertise

Schmitt and Wharton's emphasis on service to those requiring the special skills of taxonomists tapped into two themes regarding human resources and expertise.

On one hand, a role channeling queries about expertise reflects an approach to securing personnel that became common during the war and continued into the 1950s and 1960s.³³ These efforts to rationalize and centralize procurement of expertise offered efficiency gains while also breaking away from other likely gatekeepers, such as the National Academy of Sciences or the AAAS. This was an easy service to offer. In correspondence about SSZ committees, Schmitt later suggested directories — listing the groups each member felt qualified to consult about — as the most suitable means for developing this service. Blackwelder produced the first directory in 1949.³⁴

On another hand, this emphasis on promoting service roles spoke directly to many potential members. Taxonomists rarely lack a service role in their employment.³⁵ More specifically, Wharton assisted the US Navy in pest identification and control during the war.³⁶ Schmitt watched innumerable consultancies underway at the US National Museum.³⁷ In post-war

America, professional societies of biologists with service roles dwarfed those with predominantly academic interests.³⁸ Close to SSZ organizers, the interest in mixing practical diagnostic taxonomy together discussions of systematic principles also arose in the only newsletter produced by the Society for the Study of Speciation in 1940.³⁹

A service role became increasingly important for naturalists in their relations to other life sciences. Colleagues trained less in subjects defined by particular organisms (such as mammalogy, entomology, or comparative anatomy) and more on biological subjects. They lacked taxonomic expertise and subsequently earned scathing comments. "My blood boils," Horton Hobbs, Jr. (University of Virginia, crustacean taxonomist and ecologist) complained to Schmitt, "at the way some of the physiologists and anatomists disregard (I feel certain through ignorance on their part) even the basic rules of Zoological Nomenclature...and even worse, slave away on the intricacies of some organism about which they haven't the slightest idea as to its identity, let alone its affinities. Don't let me get started on that."⁴⁰ Whether or not the service was wanted by these colleagues, supporters of taxonomic work increasingly promoted consultancy as a way to add value to their own role in the life sciences. Thus, when taxonomists were unable to compete institutionally with other biologists they could tie themselves squarely to programs supporting work in the service of medicine, public health, genetics, ecology, oceanography, and so on. This use of "service" rhetoric also was a common tactic used by museum directors. It allowed them to claim high value for their institutions' collections and personnel while also offering research frameworks designed for modern science and the public interest (e.g., Parr, 1939 and 1959).

4. Representing systematics within the sciences

A great deal of competition and jostling for position took place between specialties in the life sciences during the post-war years. In part, this continued disputes arising from on-going disciplinary realignments beginning before the war. It also reflected growing unease with the increasing influence of disciplines such as genetics, biochemistry, cellular biology, and molecular biology. These communities not only succeeded in acquiring growing percentages of the overall resources available to life scientists, but they also increasingly displaced other specialties in universities and research institutions. This meant shifts in the distribution of staff, space, money, students, and opportunities. Along with many others in the naturalist tradition, systematists were acutely nervous about their status within the life sciences, and a great deal of discussion took place regarding marginalization.⁴¹

Schmitt and Wharton's call for organization certainly would have been understood as an attempt to stake a claim within this context. Their claim to "represent" implied an effort to fight back. Calls to unify and to project a common front were extremely common in the life sciences during the post-war years.⁴² Thus, Dobzhansky quipped that "nothing in biology makes sense except in the light of evolution," and James Watson asserted that there was "only one biology...and it is molecular biology." Schmitt and Wharton's call simply was a bit more verbose: "It is a major aim of [this] Society to bring about a recognition of the importance of systematics in zoology. This does not require justification ... but ignorance of [its] contributions and the extent of the service has prevented some zoologists ... from recognizing the fundamental nature of systematics and its indispensableness to many other fields."

Much later, Blackwelder recalled the SSZ leadership considered a more overarching role:

At the beginning the S.S.Z. was looked upon in part as an association of societies. This was because the field of taxonomy never had been united, and there were already two dozen specialty societies ... A program to invite affiliation of these societies was planned but never really got started. At about this time, the American Institute of Biological Sciences [AIBS] was formed...and it appeared that his function of affiliation might be better handled there. In time, it became obvious that the [AIBS] could not enlist the

taxonomic societies of zoology, but by this time the plan had been dropped by the SSZ.⁴³

Mayr was involved in a long-running parallel campaign to improve the status of systematics. "After all," he asserted in his 1942 *Systematics and the Origin of Species*,

it must not be forgotten that the average taxonomist is more than a mere caretaker of a collection. In most cases he collects his own material, he studies it in the field, and develops thereby the techniques and point of view of the ecologist. Furthermore, most of the younger systematics have had a thorough training in various branches of biology, including genetics. This experience, both in field and laboratory, gives the well-trained systematist an excellent background for more ambitious studies. It is only natural that he cannot and will not content himself with being merely a servant to some other branch of biology. Systematics is for him a full-fledged science. (Mayr, 1942: 9)

At times his focus concerned the "viewpoint" of the systematist within evolutionary studies. At other times it ranged across many other elements of the life sciences and combined promoting both systematics and evolutionary studies within the life sciences. In fact, Mayr had taken steps toward organizing systematists for these goals several years before Schmitt and Wharton. Early in January 1944 he made discrete enquiries on the matter. Writing to Carl Hubbs, for instance, Mayr explained

I have been asked by a number of colleagues to organize a society of animal taxonomists. It seems doubtful whether it is wise to start another society. At the same time it is true that the taxonomists are about the most disorganized lot of scientists.

Such a society might do a great deal to increase the prestige of taxonomy, particularly if it can manage to publish a journal which embodies the most valuable generalizations resulting from taxonomic work. I have been corresponding with [Julian] Huxley, and he has encouraged me in this matter.

The time is extremely unfavorable to start an active organization. On the other hand it might be worth while to discuss these plans in an informal way and then go ahead with a well crystallized plan after the completion of the war. What do you think of this matter? Don't you think it would be a good idea to get the entomologists, ornithologists, etc. together to discuss taxonomic principles?⁴⁴

During the post-war jockeying for position, Mayr expressed concerns similar to Schmitt and Wharton. The "time has come," Mayr confided to a colleague, "to bring taxonomists back to the fold of general biology. The truth is that the experimental biologists have had for many years the opinion that taxonomists were not biologists."⁴⁵ To Schmitt, Mayr was sufficiently comfortable to be blunt:

...the production of papers of general interest by taxonomists is practically a survival necessity. We must impress on general biologists that taxonomic work is not merely a clerk's job but real, genuine biology. Furthermore, we must impress on them that it yields results that are inaccessible to any other branch of biology. I think we have made a good beginning and taxonomy enjoys a greater prestige now than it had for a generation or more. We have to keep at it to improve our position, otherwise we will slide back again.⁴⁶

Ever zealous, Mayr went so far as to complain to Schmitt about how "regrettable" it was that the USNM was "not more prominent in this movement of bringing together taxonomy with the other biological sciences."⁴⁷ (Given Schmitt and Wharton's work for a systematics society, Mayr's criticism seems misplaced.)

The self-conscious concern over the status of systematics within biology sometimes produced a backlash. For instance, it led Frank Pitelka doubted the wisdom of declarations about the discipline's important or place in the scientific community, complaining:

Why should systematists as a group be self-conscious, apologetic, or defensive? Notwithstanding the current extent of antagonism toward systematics, the fact remains that such antagonisms, when expressed, are more a measure of a man's breadth as a biologist than an indictment of systematics. Whatever traumata any one of us individually may have experienced, our prestige and stature will be the outcome of what we are and do, not what we tell someone we are and are trying to do.⁴⁸

Instead, he predicted, hard work would be recognized in its own right. "I think the professional ego of systematists is safe, and the less said about it publicly, the better."⁴⁹

This jostling for position in the post-war years also was motivated by debates over how best to structure federal support for scientific research. The spark igniting a frantic round of such jostling was the publication of Vannevar Bush's (1945) *Science: The Endless Frontier* (Zachary, 1997, Reingold, 1991). Bush defended the need for liberal funding of basic research in the sciences as the best strategy for long term results. "It provides scientific capital," he argued, for both foreseen and unforeseeable national problems (Bush, 1945: 13). He called for substantial funding of universities, government science agencies, and other research institutions. Paradoxically, Bush defended the need for heavy investment in basic research around notions of service to the nation. He argued, for example, medical progress often was due to discoveries in "remote and unexpected fields of medicine and the underlying sciences." In this approach, Bush arranged underlying basic science along lines feeding into medicine. Thus, much of basic chemistry and physics and all of anatomy, biochemistry, physiology, pharmacology, bacteriology, pathology, parasitology, and so on were set into subordinate relations.

Schmitt and Wharton's circular identified Bush's work specifically. Systematics ranked very near the bottom of Bush's priorities. When Bush's report became the focal point for national debates over a possible national science foundation, academic biologists reacted defensively and their maneuvering grew intense (Appel, 2000). In response, advocates in many specialties argued for two actions: organization and lobbying. Centrality became a claim most specialties asserted. Unity around central themes became the party line. Schmitt and Wharton's circular clearly asserted that systematics possessed both "needs" and "worthwhileness" when set against the backdrop of the sciences more generally. Organizing was designed to establish a unified voice and spokespeople to represent them as a constituency.

Strong support for Schmitt and Wharton

The writing campaign to organize systematists worked wonders. Schmitt and Wharton received 96 responses to their September 1947 circular. They reported 72 as favorable. Most replies were shared between the two men and discussed in detail.⁵⁰ A few respondents sent extensive comments on possible goals and projects.⁵¹ Overall, the general tone of the replies reinforced their planning decisions. Wharton summarized the results plainly, "From the replies ... I am satisfied that the society can be formed."⁵²

Most supporters recognized the potential for inward looking service for systematists and outward looking service for wider communities. Concerns about marginalization were common, too, such as Horton Hobbs' letter (quoted above).⁵³ Henry Bronislaw Stenzel (University of Texas, invertebrate paleontologist specializing in crustacea) peppered enthusiasm with reminders for inclusion. "You are entirely right in stressing the needs of such a group and I heartily approve of your proposal. As a minor criticism I wish you would definitely include systematic paleontologists in the constitution. Most paleontologists have to

do a great deal of systematic work, perhaps more than those who work with living forms. At the same time a close co-ordination between those who work with fossils and those who work with living forms is desirable.”⁵⁴

With an eye on the national funding debates, annelid specialist John Percy Moore strongly supported what Schmitt and Wharton were attempting. “Taxonomy certainly needs to be vivified and justified in the eyes of the scientific world and of the sponsoring foundations, to whom it has been misrepresented.” He suggested “weighty arguments” in favor of the plan.

Taxonomy, the great grandmother (medicine her consort) of biological science and for centuries practically its dominant discipline, has been so largely brushed aside by the modern experimentalists, who would file it among the family genealogical records, that it is being largely deserted by most of the younger biologists, who are unable to bear up under that *unwanted feeling*.

Of course there has been some recent reversal of that attitude. The ecologists have continued to associate with taxonomists as good and useful pals and the geneticists (perhaps rather shamefacedly) have found that, outside of the purely cytological and hereditary fields, they are themselves microtaxonomists, dealing with mutations and populations and their relation to speciation. That is the significance of [the] formation of the Society for the Study of Evolution, and for the rise of the “new systematics.”

Taxonomy itself has broadened. Many systematists regard living organisms as complexes of physico-chemical properties rather than of static characters and at least theoretically look upon classification as an expression of *all* that is known about animals or plants, whether derived from anatomy, physiology, ecology, genetics, chemistry, *et al.*⁵⁵

By the end of September 1947, Schmitt and Wharton were planning a program for the December AAAS meeting (Chicago).⁵⁶ Their objective for that meeting was to appear certain in their goals and specific in the actions they proposed. In Chicago, they would seek a mandate to put this plan into effect. Schmitt was not shy about enlisting advice. For instance, he invited WH Camp (New York Botanical Garden and key to organizing the Society of Plant Taxonomists) to speak at the meeting. “Your organization is certainly the prototype of ours and we would love to have a few words from you. Here’s hoping!”⁵⁷

The demands of other business took Schmitt away from further organizing until late November 1947. Apologizing to Wharton for the break in his attentions, Schmitt presented ambitious plans for society activities from its very start. These plans, he suggested, would give direction. Even if these projects were set aside for the moment, Schmitt said, he wanted to catch the organizing momentum and steer it towards clearly defined goals. Thus, he suggested committees on policy and standards, cooperation with international committees of nomenclature and the *Zoological Record*, cooperation with systematic botanists, membership, publicity, the society’s newsletter, directory, and journal. He wanted a group to revise the constitution. He also proposed committees to promote systematics in schools and universities, to develop a textbook on practice and procedures, and to co-ordinate celebrations for the anniversary of Linnaeus in 1958.⁵⁸ Given his early enthusiasm for the idea of a journal and the ambition of his other committee plans, Schmitt now hesitated about prospects for a journal. “Several have suggested that a journal be published by the Society,” he reminded Wharton. “I can’t yet see eye to eye with them, but the matter should certainly be considered, perhaps only for the future.” Instead, Schmitt proposed creating a large committee charged with producing a newsletter. This was to ensure it was “representative of the country and the interests to be served.”⁵⁹ A journal could come later.

Schmitt put considerable effort into recruiting prospective members for possible committee roles. For example, he invited Norman Stoll (parasitologist at the Rockefeller Institute in

Princeton, NJ) to Chicago so he might help in the effort to “talk over” their proposal. “You will be a very essential man to our organization and we especially want you for a committee that we hope to form for cooperation with the International Committee [sic], of which you are a member.”⁶⁰

Though generally supportive of these efforts, Wharton again suggested Schmitt was moving too fast by enlisting committee members and their chairs.⁶¹ This pushed too hard and gave the impression of a one-man job. For one, they had only been empowered so far to launch the society, not administer it. Schmitt was initiating activities in the name of the society before its constitution had been formally approved and before the likely membership had been consulted. For another, naming and empanelling committees was the kind of work best left for society presidents. Though Schmitt said he only wanted to ensure things got underway, Wharton warned this approach might cause more problems than they solved.

Sixty-six people attended the organization meeting at the Chicago AAAS on 29 December 1947. Following their September circular, an announcement in *Science* read: “The Society of Systematic Zoologists has scheduled a preliminary meeting ... to consider the organization of a society of animal taxonomists.” (anonymous, 1947: 560) A morning session would be dedicated to questions of organization. An afternoon session was organized for a presentation by Arthur Francis Hemming about the upcoming meeting of the International Commission on Zoological Nomenclature at the 1948 XIII International Congress of Zoology in Paris.⁶²

All went pretty much to plan at the organization meeting.⁶³ Schmitt was elected president. Though supporters of Schmitt and Wharton meant this as a sign of their support, this caused a technical glitch. It meant Schmitt could not serve in the crucial role of secretary. Thus, Wharton was elected as secretary-treasurer until the Council could elect an alternative. The basic result of the meeting was a license for Schmitt and Wharton to proceed with the society’s organization, create its operating infrastructure, and undertake the formalities of its official constitution. The group also agreed to keep “charter” membership open to any person who paid their dues before the next (i.e., the first annual) meeting.⁶⁴

The SSZ held its first annual meeting in Washington, DC, on 12 September 1948.⁶⁵ The constitution was officially adopted.⁶⁶ Alexander Petrunkevitch (Osborn Zoological Laboratory, Yale University) was elected president, though he did not attend the meeting. Schmitt anticipated Petrunkevitch’s election. Before the meeting, Schmitt offered his vision for the next steps forward:

I wish that we might count on you for our September 12 breakfast meeting of the Society of Systematic Zoology. I have never forgotten your several suggestions on the improvement of the proposed Constitution, and especially the one regarding a journal of systematic zoology. I am certainly going to do my bit to get such a journal subsidized. I am sure that if we finance it for just a few years, two or three, it will make a place for itself. Besides discussions of nomenclatorial problems, systematic practice and proceedings, I should like to see such a journal instituted for the purpose of making the decisions of the International Commission more widely known than at present is possible. Furthermore, I would also like to see the first descriptions of new species on card form, letter size, for convenience in filing, issued as a supplement to this journal. This is something on the order of the card index to types of Devonian fossils put out by the Wagner Free Institute of Science [Philadelphia, PA]. No matter what may be said about a new species in subsequent monographic series, its first appearance should be on a standard reference card, letter size, because file cabinets for that size are standard, and the sheet is large enough to carry figure descriptions and whatever remarks an author may care to make. The institution of such a publication of new species would obviate the many little scrappy notes describing species scattered through many different and often obscure journals.⁶⁷

After the meeting, Schmitt delivered the news of his election to Petrunkevitch. “I write to tell you how happy I am that this is the case and I hope that you will have no hesitation in accepting the office. We need some one of mature judgment like yourself to guide us at least through our first definitive year.”⁶⁸

The SSZ was well and truly underway.

Where does Blackwelder’s narrative fit?

By all accounts, Richard Blackwelder⁶⁹ played a fundamental role in the SSZ’s first decade. He deserves credit not only for running the society as a going concern but also as a driving force behind many SSZ activities. He was involved in the launch of the *News Letter*, then *Systematic Zoology*. He created the society’s “book exhibit” and represented the SSZ in many settings.⁷⁰ Formally, he served as secretary, 1949–1960, then president in 1961. Blackwelder poured his heart into the SSZ.

Blackwelder enters the archival record for the SSZ in March 1948, after the launch was well underway.⁷¹ Following the December 1947 organization meeting, Schmitt and Wharton corresponded many colleagues in their efforts to build on the foundations created in Chicago. In February 1948 Wharton drafted a second circular updating colleagues on progress thus far and calling for charter members. He sent this draft to Schmitt for comments. At some point — it is not recorded in archival records — Schmitt passed this to Blackwelder for comment. He replied in a long letter, rewriting Wharton’s draft and raising many technical points about procedure for the two men to note. Blackwelder seems to have thought long and hard about this task. Closing his reply to Schmitt, Blackwelder offered “I can foresee great possibilities for the society and hope to make some definite suggestions for its activities as soon as it is going.”⁷² Schmitt forwarded these comments to Wharton. Though rejecting most of Blackwelder’s suggestions, Schmitt did not hesitate to convey a sense of potential value in his new recruit.

Dick Blackwelder is tremendously interested in all that we are trying to do with the Society for Systematic Zoologists. ... He wants to see the various nomenclature groups throughout the country coordinated in some fashion and feels that our Society is just the place for it, ... Because of his interest, I asked him to work over your draft ...⁷³

This seems to mark Blackwelder’s entrance into SSZ business. Blackwelder’s letter to Schmitt has a formality that suggests lack of familiarity. While Schmitt consulted Blackwelder further before December 1948, it seems to have been neither extensive nor central to Schmitt’s decisions about the SSZ. After December 1947, Schmitt listed Blackwelder as a prospective member for some of the committees he planned for the SSZ. No doubt Blackwelder occasionally interacted with Schmitt at the US National Museum: they worked in the same building. However, Blackwelder seems to have been brought into SSZ business after the launch took place. From there, of course, Blackwelder earned Schmitt’s confidence and their camaraderie developed. By the end of their careers, Blackwelder’s acknowledged the debt he owed Schmitt with a devotional biography (Blackwelder, 1979).

Blackwelder’s narrative of the SSZ’s launch placed at its center the tension between evolutionists and taxonomists. For him, this conflict motivated the original need for organization. The earliest version of Blackwelder’s narrative appeared in the draft copy for an SSZ recruiting brochure, circa 1951. The spark came at the founding meeting of the evolutionists’ group, he wrote. A request was made to broaden the scope of new society’s scope to include “other fundamental aspects of systematics,” but this suggestion was rejected. “That decision may have been wise, and undoubtedly resulted in a more unified purpose, but it also had the effect of shutting out those systematists whose primary interests were not in evolution as such.”⁷⁴ Blackwelder repeated this basic version of events for the rest of his life (e.g. Blackwelder, 1964, 1967: 329–372, 1977). He included it in an oral history

produced in 1978⁷⁵ and in a biography of Schmitt he published the following year (Blackwelder, 1979: 157).

Blackwelder's professional writing expressed the same polarization. His contribution to the first number of *Systematic Zoology*, with Alan Boyden, was a broadside against "phylogenists" within systematics. Their criticisms aimed specifically at Dobzhansky, Mayr, Simpson, and Huxley (Blackwelder and Boyden, 1952). Blackwelder (1964) claimed the "speciationists have stolen and twisted" his generation's revolution in systematics techniques and called for a return to an "omnispersive approach" that he later explained in detail (Blackwelder, 1967). Even his short guide to the classification of animals raised this conflict (Blackwelder, 1963). This polarization continued in private correspondence, where Blackwelder could be scathing. For instance, writing to Schmitt about a supportive paper soon to appear in the journal, Blackwelder confided:

Don't fail to read the lead article in *Systematic Zoology* this issue - by Borgmeier. I was afraid Brooks [John L Brooks, who edited the journal, 1952–1957] wouldn't accept it because it's so hard on the New Systematics people. Really rides over Mayr rough shod; he obviously deserves it. It's high time that somebody brought together the larger amount of disagreement with the popular fictions of these people who think that the business of systematics is to detect evolution at work. Incidentally, I can take a bow for Alan Boyden and me in the remarks of Borgmeier.⁷⁶

For a myriad of intellectual reasons Blackwelder disagreed with principles underlying evolutionary taxonomy. He certainly was not alone. However, for some reason — no doubt for complex reasons — he chose to represent this disagreement such that he needed to reject their project absolutely. Blackwelder created a self-identity based on this opposition and constructed his relation to evolutionary taxonomy in terms of bitter conflict and rivalry. This construction was more than rhetorical. It was personal.⁷⁷

Blackwelder tied this rivalry to the SSZ. He regularly compared his SSZ with the evolutionists and the SSE. This provided an out-group for defining the SSZ. Defending the "taxonomic side of systematics" gave purpose to his role as secretary. It explains why he chose to emphasize the way his side had been "shut out" from the organizing of the SSE. It also explains why his historical narrative about the SSZ's launch exaggerated this moment and polarized the issues in play far beyond what is supported by the archival evidence. Origin myths reinforce social bonds in important ways (Abir-Am and Elliott, 2000). Blackwelder put history to work for his own purpose.

Blackwelder's emphasis on conflict with evolutionary taxonomy haunted him from the start of his involvement with the SSZ. He suffered his first major losses during efforts to launch the new journal.

When SSZ officers finally decided in 1951 to launch a journal, Blackwelder headed the effort to secure seed money. They first turned to the American Philosophical Society (APS). This seemed an obvious choice at the time because the SSZ could follow the evolutionists who received an APS grant to launch *Evolution*.⁷⁸ When Blackwelder approached the APS for support with a similar proposal, he deliberately drew an analogy with the SSE and asked for equal treatment.⁷⁹ Blackwelder's plan was a bit more ambitious: the grant would provide for 64-page numbers six times a year produced with high quality illustrations. He arranged support from distinguished colleagues.⁸⁰ But the SSZ request was rejected. Blackwelder thought the reasons were suspicious. It didn't help that Simpson was an APS member and one who would have been an influential voice in the relevant committee. Simpson, who had yet to join the SSZ, declined to support Blackwelder's proposal within the APS.⁸¹ Twenty-five years later Blackwelder still expressed anger over the issue when he recalled how the proposal had been declined "with the explanation that the APS does not support publications (not ours anyway!)." (Blackwelder, 1977: 113)

Far worse, Blackwelder was unable to secure the editorship of *Systematic Zoology*. His role as “Editor *pro. tem.*” for the first number resulted from his desire to see the project launched and launched as he saw fit. Schmitt wanted to propose Blackwelder as permanent editor but hesitated, telling Frank Pitelka, “He is a most excellent secretary and a most indefatigable one. I know that he would make also a top-notch editor for the journal, but we cannot give him all the work that the Society needs to have done.”⁸² No doubt encouraged by Blackwelder after he was passed over, Alan Boyden put himself forward for the editorship as a way to keep their side on top.⁸³ However, the editorship went to John L. Brooks (Osborn Zoological Laboratory, Yale University). While not an overt “speciationist,” Brooks followed in the traditions of the Osborn Laboratory and advocated a phylogenetic approach to classification (e.g. Petrunkevitch, 1952b).

Brooks’ editorship began with the second number. Petrunkevitch (1952a) introduced his young colleague, stressing his credentials as a researcher with an “enviable past and a promising future.” Blackwelder (1952) added his own polite welcome. Under the surface the transition was fraught with conflict between these two men. Among other issues, Blackwelder insisted on a large space in the journal devoted to SSZ “news”. He would produce this (e.g. Blackwelder, 1952). This conflict became sufficiently acrimonious that senior colleagues felt the need to intervene.⁸⁴ Blackwelder’s difficulties with Brooks were not made easier by the preponderance of papers appearing in *Systematic Zoology* defending evolutionary taxonomy and classifications based on phylogenies (such as Huntington, 1952, Maslin, 1952, Myers, 1952, Stunkard, 1952, Van Cleave, 1952, the relative balance of content in the journal is examined by Hull, 1983).

Through the 1950s Blackwelder’s rivalry focused most directly on Mayr, and these two repeatedly clashed over who would create and lead the infrastructure within systematic zoology. This rivalry was unfortunate for Blackwelder. Mayr grew in prominence during the 1950s to become a senior statesman in the discipline. Blackwelder did not. This relative imbalance must have been frustrating for Blackwelder. His involvement with the SSZ — and his sense of the SSZ’s historical role in defending his cause — came to mean more and more.

Losing the editorship of *Systematic Zoology* was an early setback in this rivalry. It meant Blackwelder had failed to match Mayr’s largely single-handed success with *Evolution* (Cain, 2000a). To compensate, Blackwelder tried to make the most of the news section in the journal, then re-launched the *SSZ News Letter*, partly “because of the desire of the editor to remove this material from the journal and partly because of the desire of the Secretary-Treasurer to have more flexibility...”⁸⁵

In the early 1950s Mayr became disillusioned with the SSE and *Evolution*. The relative dominance of geneticists in the society frustrated his attempts to secure a “balance” with the concerns of evolutionary systematics. Moreover, while editor of *Evolution* (1946–1949), Mayr largely failed (despite considerable effort) to recruit papers from systematists representing an evolutionary perspective. This situation did not change under the next editor, Edwin Colbert. As Mayr turned away from *Evolution*, he turned towards *Systematic Zoology* and the increasing representation not only of phylogenetic approaches but also the speciation studies he sought most to promote. of evolution. Mayr joined the editorial board in 1953 just as his co-authored textbook on systematic principles appeared.⁸⁶

In 1953 Mayr also left his curatorial position at the American Museum of Natural History for an Agassiz professorship at Harvard University. This was an important personal transition for him: this offered the kind of independent, prestigious position he had long sought. As part of this move, Mayr fully committed himself to a statesman’s role representing systematics at a national level. Added momentum came with his election to the International Commission of Zoological Nomenclature (1954–1976).⁸⁷ Plus, in 1954 he was elected to the National Academy of Science. “Taxonomy has had a considerable rejuvenation during recent decades,” he wrote his Academy colleagues, “but we must continue to make every effort to broaden its basis even more. If we do not, the best among the students will go into other

fields. And there is still so much to be done..."⁸⁸ Mayr quickly involved himself with policy committees of the National Research Council and the American Institute of Biological Sciences, most importantly one focused on funding priorities for the preservation of biological materials.⁸⁹ Another focused on core biological concepts, and their report influenced policy decisions within the National Science Foundation (Appel, 2000). In 1961, Mayr replaced Romer as director of the Museum of Comparative Zoology.

In contrast, Blackwelder fared poorly. Though recognized for the considerable energy he spent on SSZ activities, he largely failed to move into leadership roles. Perhaps Blackwelder tied his own sense of identity too strongly to his role in the SSZ. This blurred the distinction between the Society's purposes and his own professional goals and intellectual interests.⁹⁰ In 1954 such blurred distinctions provoked serious questions about Blackwelder's commitment to his employer, the US National Museum, and questions about overuse of the institution's largess for the SSZ's gain. Though he adamantly denied any wrongdoing, Blackwelder was forced to resign over this matter.⁹¹ Four years of unrelated employment and failed attempts at fellowships followed before Blackwelder moved back on track, joining the expanding zoology department at Southern Illinois University in Carbondale. Blackwelder sorely needed the SSZ during this period if simply for his continuing professional identity.

The differences in careers were obvious to all. In 1955 Mayr proposed creating an international directory of systematists, using his connections with NSF to secure funding. When he consulted Schmitt on the matter, Mayr received the expected support, "...naturally [the idea] strikes a responsive chord here. There is no question about the need for such a list." But Schmitt peppered this support with a plea to assist his friend. He asked Mayr to give the project to Blackwelder, who could devote much time to it, use his established connections as SSZ Secretary, and see to the frequent updating. "I think that I am aware of Blackwelder's faults as well as anyone. There are many assignments which someone else could do better, but this one seems peculiarly suited to his capabilities."⁹² Mayr conceded. Blackwelder and his wife saw the project through (Blackwelder and Blackwelder, 1961).

Conclusion

This paper develops two historical points. First, it describes the launch of the SSZ, drawing primarily on previously unconsidered archival sources. This focuses attention on the efforts of Waldo LaSalle Schmitt and George Wharton to rebuild an infrastructure for American systematists following World War II. Close analysis of their September 1947 circular peels away successive layers of historical context. It provides an appreciation for the complex senses of need that shaped decision making at the time.

Second, this paper separates the launch of the SSZ from the founding narrative presented by Richard Blackwelder. Blackwelder came to SSZ business relatively late in the organizing process. While he played a key role in subsequent activity, Blackwelder's account of the launch was secondhand. In retelling that narrative, Blackwelder was heavily influenced by his own construction of the state of systematics in 1940s America. This emphasized rivalry and polarity. It set the SSZ into competition with evolutionary taxonomists and tied its success to that competition. Rather than complain about authenticity, the purpose of this analysis has been to contextualize Blackwelder's founding narrative. This origins myth says much about the man who promoted it and compares with other studies of the role of origins mythology in promoting social bonds within tribal communities in science (Abir-Am and Elliott, 2000).

No group came to dominate the SSZ in its first decade. Neither did a broad intellectual consensus form. Instead, the SSZ functioned as a "trading zone" in which members of different camps met and engaged in exchanges (Galison, 1997, 1999). As a symbol of community the SSZ also provided a boundary object that different groups could define in their own terms and for their own reasons but that all could support jointly in defense of a shared cause: the promotion of systematics as fundamental within the life sciences (Star and Griesemer, 1989). Appel (2000: 150) shows the importance of this joint voice for systematics.

For example, of all US federal monies supporting basic systematics in 1954, 61% derived from NSF. This was the agency most targeted by those a vision for unified action in systematics. Whether or not they were truly unified misses the point. Their efforts sowed the seeds for a new generation of American systematists. Ultimately that new generation pressed ahead to develop fundamental changes in systematic theory and practice.

Acknowledgements

Many thanks to David Williams for his work organizing this symposium. Thanks also to William Cox (Smithsonian Institution Archives) for considerable assistance as well as to the staffs at the American Philosophical Society Library and Scripps Institution of Oceanography. Glen Needham, James Keirans, and Michelle Drobik provided biographical information about George Wharton. This project was undertaken as part of a Research Fellowship from the Leverhulme Trust.

Archival collections

The following abbreviations are used in this paper: **Dunn Papers:** Leslie C. Dunn Papers, American Philosophical Society Library, Philadelphia, PA; **Hubbs Papers:** Carl Hubbs Papers. Archives of the Scripps Institution of Oceanography, University of California, San Diego, La Jolla, CA; **Mayr/Evolution:** Ernst Mayr/Society for the Study of Evolution Papers. Collection B/M451s, American Philosophical Society Library, Philadelphia, PA; **Schmitt Papers:** Waldo LaSalle Schmitt Papers, 1907–1978. Record Unit 7231. Smithsonian Institution Archives, Washington DC; **Simpson Papers:** George Gaylord Simpson Papers. American Philosophical Society Library, Philadelphia, PA. Unless otherwise noted, items are in series 1; **SSE Papers:** Materials Relating to Society for the Study of Evolution, collection B/M451S 1991 1027ms, American Philosophical Society Library, Philadelphia, PA; **SSZ Records:** Records of the Society of Systematic Zoology, 1947–1975. Record Unit 7226. Smithsonian Institution Archives, Washington DC.

Bibliography

- Abir-Am, P. G. and Elliott, C. A. (eds) (2000) *Commemorative practices in science: historical perspectives on the politics of collective memory*, University of Chicago Press for History of Science Society: Chicago.
- Allee, W. C., Emerson, A. E., Park, O., Park, T. and Schmidt, K. P. (1949) *Principles of animal ecology*, W. B. Saunders: Philadelphia, PA.
- Allen, G. (1975) *Life Sciences in the Twentieth Century*, John Wiley and Sons: New York.
- Anderson, E. (1949) *Introgressive hybridization*, John Wiley and Sons: New York.
- anonymous (1947) 'Programs of the AAAS Sections and the Societies', *Science*, 106: 558–566.
- anonymous (1948) 'The Systematics Association Annual Report VI. (1946–1947)', *Proceedings of the Linnean Society*, 160: i–iv.
- Appel, T. (2000) *Shaping Biology: the National Science Foundation and American Biological Research, 1945–1975*, Johns Hopkins University Press: Baltimore, MD.
- Babcock, E. B. and Stebbins, G. L. (1938) *The American Species of Crepis, Their Interrelationships and Distribution as Affected by Polyploidy and Apomixis*, Carnegie Institution of Washington: Washington, DC.
- Babcock, E. B., Stebbins, G. L. and Jenkins, J. A. (1942) 'Genetic Evolutionary Processes in Crepis', *The American Naturalist*, 76: 337–363.

- Ball, C. R. (1946) 'Why is Taxonomy Ill-supported?', *Science*, 103: 713–714.
- Bates, R. E. (1965) *Scientific Societies in the United States, 3rd edition*, Pergamon Press: Oxford.
- Beatty, J. (1994) 'The Proximate/Ulimate Distinction in the Multiple Careers of Ernst Mayr', *Biology and Philosophy*, 9: 333–356.
- Benson, K. R., Maienschein, J. and Rainger, R. (eds) (1991) *The Expansion of American Biology*, Rutgers University Press: New Brunswick, NJ.
- Blackwelder, R. E. (1952) 'SSZ News', *Systematic Zoology*, 1: 92–95.
- Blackwelder, R. E. (1963) *Classification of the Animal Kingdom*, Southern Illinois University Press: Carbondale, IL.
- Blackwelder, R. E. (1964) 'Phyletic and Phenetic versus Omnispective Classification', in Heywood, V. H. and McNeill, J. (ed)^(eds) *Phenetic and Phylogenetic Classification: A Symposium*, London: The Systematics Association, 17–28.
- Blackwelder, R. E. (1967) *Taxonomy: A Text and Reference Book*, John Wiley and Sons: New York.
- Blackwelder, R. E. (1977) 'Twenty Five Years of Taxonomy', *Systematic zoology*, 26: 107–137.
- Blackwelder, R. E. (1979) *Zest for Life, or Waldo Had a Pretty Good Run: The Life of Waldo LaSalle Schmitt*, Allen Press: Lawrence, KA.
- Blackwelder, R. E. (1990) *A Tolkien Thesaurus*, Garland: New York.
- Blackwelder, R. E. and Blackwelder, R. M. (1961) *Directory of Zoological Taxonomists of the World*, Southern Illinois University Press for the Society of Systematic Zoology: Carbondale, IL.
- Blackwelder, R. E. and Boyden, A. (1952) 'The Nature of Systematics', *Systematic Zoology*, 1: 26–33.
- Blackwelder, R. E., Knight, J. B. and Sabrosky, C. W. (1947) '[Letter in "Comments by Readers"]', *Science*, 106: 315–316.
- Borgmeier, T. (1957) 'Basic Questions of Systematics', *Systematic Zoology*, 6: 53–69.
- Boyden, A. (1953) 'Fifty Years of Systematic Serology', *Systematic Zoology*, 2: 18–30.
- Brockway, L. (1979) *Science and Colonial Expansion: the Role of the British Royal Botanic Gardens*, Academic Press: New York.
- Bush, V. (1945) *Science: The Endless Frontier. A Report to the President*, United States Government Printing Office: Washington, DC.
- Cain, J. (1994) 'Ernst Mayr as *Community Architect*: Launching the Society for the Study of Evolution and the Journal *Evolution*', *Biology and Philosophy*, 9: 387–427.
- Cain, J. (ed) (1999) *Regular Contact with Anyone Interested: Documents of the Society for the Study of Speciation, 1941*, Riverside Press: Winona, MN.
- Cain, J. (2000a) 'For the 'Promotion' and 'Integration' of Various Fields: First Years of *Evolution*, 1947–1949', *Archives of Natural History*, 27: 231–259.
- Cain, J. (2000b) 'Towards a 'Greater Degree of Integration': The Society for the Study of Speciation, 1939–1941', *British Journal for the History of Science*, 33: 85–108.

- Cain, J. (2002) 'Epistemic and Community Transition in American Evolutionary Studies: the 'Committee on Common Problems of Genetics, Paleontology, and Systematics' (1942–1949)', *Studies in History and Philosophy of Biological and Biomedical Sciences*,
- Cittadino, E. (1990) *Nature as Laboratory: Darwinian Plant Ecology in the German Empire, 1880–1900*, Cambridge University Press: Cambridge.
- Cole, W. H. (1958) *Serological and Biochemical Comparisons of Proteins*, Rutgers University Press: New Brunswick, NJ.
- Cox, W. (1983) *Guide to the Papers of Waldo LaSalle Schmitt, 1887–1977*, Smithsonian Institution Archives: Washington, DC.
- Dietrich, M. R. (1998) 'Paradox and Persuasion: Negotiating the Place of Molecular Evolution Within Evolutionary Biology', *Journal of the History of Biology*, 31: 85–111.
- Dobzhansky, T. (1937) *Genetics and the Origin of Species*, Columbia University Press: New York.
- Dupree, A. H. (1957) *Science and the Federal Government: A History of Policies and Activities to 1940*, Harvard University Press: Cambridge, MA.
- Dupree, A. H. (1963) 'Central Scientific Organization in the United States Government', *Minerva*, 1: 453–469.
- Dupree, A. H. (1972) 'The Great Instauration of 1940: The Organization of Scientific Research for War', in Holton, G. (ed)^(eds) *The Twentieth-Century Sciences: Studies in the Biography of Ideas*, New York: W.W. Norton and Company, 443–467.
- Emerson, A. (1937) 'Speciation', *Ecology*, 18: 152–154.
- Emerson, A. (1941) 'Taxonomy and Ecology', *Ecology*, 22: 213–215.
- Galison, P. (1997) *Image and Logic*, University of Chicago Press: Chicago, IL.
- Galison, P. (1999) 'Trading Zone: Coordinating Action and Belief', in Biagioli, M. (ed)^(eds) *Science Studies Reader*, New York: Routledge, 137–160.
- Greenberg, D. (1967) *The Politics of Pure Science: An Inquiry into the Relationship Between Science and Government in the United States*, New American Library: New York.
- Hagen, J. (1984) 'Experimentalists and Naturalists in Twentieth-Century Botany: Experimental Taxonomy, 1920–1950', *Journal of the History of Biology*, 17: 249–270.
- Hagen, J. (1992) *An Entangled Bank: the Origins of Ecosystem Ecology*, Rutgers University Press: New Brunswick, NJ.
- Heims, S. (1991) *Constructing a Social Science for Postwar America: The Cybernetics Group, 1946–1953*, MIT Press: Cambridge, MA.
- Hemming, F. (1948) 'Important Advances in Zoological Nomenclature Achieved at 13th International Congress of Zoology', *Science*, 108: 156–157.
- Holton, G. (ed) (1972) *The Twentieth-Century Science: Studies in the Biography of Ideas*, WW Norton: New York.
- Hubbs, C. L. (1940) 'Speciation of Fishes', *The American Naturalist*, 74: 198–211.
- Hull, D. (1983) 'Thirty-One Years of Systematic Zoology', *Systematic Zoology*, 32: 315–342.
- Hull, D. (1988) *Science as a Process: an Evolutionary Account of the Social and Conceptual Development of Science*, University of Chicago Press: Chicago.

- Huntington, C. E. (1952) 'Hybridization in the Purple Grackle, *Quiscalus quiscula*', *Systematic Zoology*, 1: 149–170.
- Huxley, J. (ed) (1940) *The New Systematics*, Clarendon Press: Oxford.
- Huxley, J. (1942) *Evolution: The Modern Synthesis*, Harper and Brothers: New York and London.
- Huxley, J. S. (1939a) 'Clines: an Auxiliary Method in Taxonomy', *Bijdragen tot de Dierkunde*, 27: 491–520.
- Huxley, J. S. (1939b) 'Ecology and Taxonomic Differentiation', *Journal of Ecology*, 27: 408–420.
- Kay, L. (1993) *The Molecular Vision of Life: Caltech, the Rockefeller Foundation, and the Rise of the New Biology*, Oxford University Press: New York.
- Keen, A. M. and Muller, S. W. (1948) *Procedure in Taxonomy, revised edition*, Stanford University Press: Stanford, CA.
- Kinsey, A. (1936) 'The Origin of Higher Categories in *Cynips*', *Indiana University Publications, Science Series*, 4: 1–334.
- Kleinman, K. (1999) 'His Own Synthesis: Corn, Edgar Anderson, and Evolutionary Theory in the 1940s', *Journal of the History of Biology*, 32: 293–320.
- Koerner, L. (1999) *Linnaeus: Nature and Nation*, Harvard University Press: Cambridge, MA.
- Lack, D. (1943) *The Life of the Robin*, Witherby: London.
- Larson, E. J. (2001) *Evolution's Workshop: God and Science on the Galapagos Islands*, Basic Books: New York.
- Lee, M. (1951) 'Detlev W. Bronk, Scientist', *Science*, 113: 143.
- Lewenstein, B. V. (1999) 'Shifting Science from People to Programs: AAAS in the Post War Years', in Kohlstedt, S. G., Sokal, M. M. and Lewenstein, B. V. (ed) (eds) *The Establishment of Science in America: 150 Years of the American Association for the Advancement of Science*, New Brunswick, NJ: Rutgers University Press, 103–165.
- Maienschein, J. (1991) *Transforming Traditions in American Biology, 1880–1915*, Johns Hopkins University Press: Baltimore, MD.
- Maslin, P. (1952) 'Morphological Criteria of Phyletic Relationships', *Systematic Zoology*, 1: 49–70.
- Mayr, E. (1941) 'The Origin of Gaps Between Species', *The Collecting Net*, 16: 137–143.
- Mayr, E. (1942) *Systematics and the Origin of Species*, Columbia University Press: New York.
- Mayr, E. (1969) *Principles of Systematic Zoology*, McGraw-Hill: New York.
- Mayr, E. (1992) 'Controversies in Retrospect', *Oxford Surveys in Evolutionary Biology*, 8: 1–34.
- Mayr, E., Linsley, E. G. and Usinger, R. L. (1953) *Methods and Principles of Systematic Zoology*, McGraw-Hill: New York; London.
- Melville, R. V. (1995) *Towards Stability in the names of Animals: a history of the International Commission on Zoological Nomenclature, 1895–1995*, International Trust for Zoological Nomenclature: London.

- Miller, A. (1941) 'Speciation in the Avian Genus *Junco*', *University of California Publications in Zoology*, 44: 173–434.
- Mitchell, R., Horn, D. J., Needham, G. R. and Welbourn, W. C. (1969) 'Dedication', in Needham, G. R., Mitchell, R., Horn, D. J. and Welbourn, W. C. (ed)^(eds) *Acarology IX (Symposia, Volume 2)*, Columbus, OH: The Ohio Biological Survey, xv–xvii.
- Myers, G. S. (1952) 'The Nature of Systematic Biology and of a Species Description', *Systematic Zoology*, 1: 106–111.
- Parr, A. (1939) 'On the Functions of the Natural History Museum', *Transactions of the New York Academy of Sciences*, 2nd series, 2: 44–58.
- Parr, A. (1959) *Mostly About Museums: From the Papers of A. E. Parr*, American Museum of Natural History: New York.
- Patterson, J. T. (1941) 'The *virilis* Group of *Drosophila* in Texas', *The American Naturalist*, 75: 523–539.
- Pauly, P. (1987) *Controlling Life: Jacques Loeb and the Engineering Ideal in Biology*, Oxford University Press: New York.
- Petrunkévitch, A. (1952a) 'Introducing the Editor', *Systematic Zoology*, 1: 71.
- Petrunkévitch, A. (1952b) 'Principles of Classification as Illustrated by Studies of Arachnida', *Systematic Zoology*, 1: 1–19.
- Price, D. d. S. (1961) *Science Since Babylon*, Yale University Press: New Haven.
- Price, D. d. S. (1963) *Little Science, Big Science*, Columbia University Press: New York.
- Rainger, R., Benson, K. and Maienschein, J. (eds) (1988) *The American Development of Biology*, University of Pennsylvania Press: Philadelphia.
- Reingold, N. (1991) 'Vannevar Bush's New Deal for Research; or, the Triumph of the Old Order', in Reingold, N. (ed)^(eds) *Science American Style*, New Brunswick, NJ: Rutgers University Press, 284–333.
- Rensch, B. (1929) *Das Prinzip Geographischer Rassenkreise und das Problem der Artbildung*, Gebrüder Borntraeger: Berlin.
- Rudeval, F. R. (1905) *Règles Internationales de la Nomenclature Zoologique*, private: Paris.
- Said, E. W. (1978) *Orientalism: Western Conceptions of the Orient*, Routledge and Kegan Paul: London.
- Schenk, E. T. and McMasters, J. H. (1936) *Procedure in Taxonomy*, Stanford University Press: Stanford, CA.
- Schofield, E. K. (1998) *A History of the American Society of Plant Taxonomists: The First Sixty Years - 1936 to 1996*, American Society of Plant Taxonomists: Laramie, WY.
- Shapley, H. (1950) 'Kirtley F. Mather: President-Elect, AAAS', *Science*, 111: 131–132.
- Simpson, G. G. (1937) 'The Fort Union of the Crazy Mountain field, Montana, and Its Mammalian Faunas', *Bulletin of the United States National Museum*, 169: 1–287.
- Simpson, G. G. (1944) *Tempo and Mode in Evolution*, Columbia University Press: New York.
- Simpson, G. G. (1945) 'The Principles of Classification and a Classification of Mammals', *Bulletin of the American Museum of Natural History*, 85: i–xvi, 1–350.
- Simpson, G. G. (1961) *Principles of Animal Taxonomy*, Columbia University Press: New York.

- Simpson, G. G. and Roe, A. (1939) *Quantitative Zoology: Numerical Concepts and Methods in the Study of Recent and Fossil Animals*, McGraw-Hill Book Co.: New York.
- Smocovitis, V. B. (1994) 'Organizing Evolution: Founding the Society for the Study of Evolution (1939–1950)', *Journal of the History of Biology*, 27: 241–309.
- Star, S. L. and Griesemer, J. (1989) 'Institutional Ecology, 'Translations' and Boundary Objects: Amateurs and Professionals in Berkeley's Museum of Vertebrate Zoology, 1907–1939', *Social Studies of Science*, 19: 387–420.
- Stunkard, H. W. (1952) 'Life Histories and Systematics of Parasitic Worms', *Systematic Zoology*, 2: 7–18.
- Thackery, A. (ed) (1992) *Science After '40*, University of Chicago Press for History of Science Society: Chicago.
- Traweek, S. (1988) *Beamtimes and Lifetimes: The World of High Energy Physics*, Harvard University Press: Cambridge, MA.
- Van Cleave, H. J. (1943) 'An Index to the Opinions Rendered by the International Commission on Zoological Nomenclature', *American Midland Naturalist*, 30: 223–240.
- Van Cleave, H. J. (1952) 'Speciation and Formation of Genera in Acanthocephala', *Systematic Zoology*, 1: 72–83.
- Vernon, K. (1993) 'Desperately Seeking Status: Evolutionary Systematics and the Taxonomists' Search for Respectability', *British Journal for the History of Science*, 26: 207–227.
- Wilson, E. O. (1994) *Naturalist*, Time Warner: New York.
- Winsor, M. P. (1995) 'The English Debate on Taxonomy and Phylogeny, 1937–1940', *History and Philosophy of the Life Sciences*, 17: 227–252.
- Wolfe, D. (1989) *Renewing a Scientific Society: the American Association for the Advancement of Science from World War II to 1970*, American Association for the Advancement of Science: Washington, DC.
- Zachary, G. P. (1997) *Endless Frontier: Vannevar Bush, Engineer of the American Century*, Free Press: New York.

Notes

- ¹ Waldo LaSalle Schmitt (1887–1977) was one of America’s most respected taxonomists (Blackwelder, 1979, Cox, 1983). A head curator at the United States National Museum (USNM), Schmitt’s expertise was crustacea. He had risen through the institution’s ranks since 1910. He also played important roles in fisheries research and studies of faunas around oceanic islands.
- ² George Willard Wharton, Jr. (1914–1990) built his career at Duke University, remaining in the zoology department after his PhD (1939) until 1953, when he moved to the University of Maryland (1953–1961), then Ohio State University (from 1961, becoming emeritus in 1976). After developing the Acarology Laboratory at Maryland and taking it to Ohio State, Wharton served as its official director (1969–1976) as well as guiding force (Mitchell et al., 1969). Wharton’s expertise was mites and ticks, with an emphasis on their systematics, ecology and parasitology.
- ³ Documents verify that Schmitt and Wharton attended the organisation meeting of the Society for the Study of Evolution society that took place on 30 March 1946 at the St Louis AAAS convention. Cain (1994: 401 and table 1) lists them in attendance and their signatures appear on the attendance sheet (Smocovitis, 1994: figure 1). Blackwelder was not present.
- ⁴ The impression that this new society might center around principles of systematics was not unreasonable given the reputations of those prominent in the organization for defending the values and virtue of systematics within biology: e.g., Mayr (1942), Emerson (1941, 1937 and later Allee et al., 1949), Kinsey (1936) and Simpson (1944 and 1945). Huxley’s (1940, 1942) conception of the “new systematics” and a “modern synthesis” of evolution similarly highlighted this emphasis. This impression also would have been bolstered by those following the advertised connections between the new evolution society and the short-lived Society for the Study of Speciation, organised in 1939 but functioning only for about a year. In the speciation society, the concerns of taxonomists formed a large part of the discussion (Cain, 2000b). Schmitt, Wharton, and Blackwelder were members of the speciation society.
- ⁵ Mayr’s mission at this particular moment was to raise the status and value of systematics within the life sciences, not to develop — as Schmitt wanted — professional infrastructure within systematics. As he explained to Schmitt in March 1948, Mayr saw a noble purpose in this outward-looking effort, “The trouble with us taxonomists is that we are so busy with all sorts of jobs we don’t get around to doing research, and when we do we usually don’t have enough leisure to produce a good, all-around biological paper but have to be satisfied more or less with taxonomic revision.” (Mayr to Schmitt, 9 March 1948, *Mayr/Evolution Papers*, folder “Schmitt”)
- ⁶ This legendary moment most likely did not happen as was repeated so often by Blackwelder. The minutes of the March 1946 SSE meeting record no such conversation between Schmitt and the organisers while noting several other discussions and identifying numerous speakers making comments. Schmitt is identified in this meeting’s minutes at two points, each to make motions: (1) that the society be named the “Society for the Study of Evolution” instead of several alternatives, and (2) that the society affiliate with the AAAS. Elsewhere in the minutes, Edgar Anderson is identified objecting to the purposes of the society as too narrow, preferring instead “studying systematics and species vigorously” under the general study of speciation. Mayr and Simpson objected that this would exclude paleontologists. This is a different point from what the SSZ legend

suggested, though it may have provoked a sidebar involving Schmitt and Wharton (either at the meeting or later) that was passed on to Blackwelder after the fact (which he may also have embellished). These minutes are in “Materials Relating to the Society for the Study of Evolution,” SSE Papers, box “Council Meetings”, folder “Minutes”. Schmitt became a charter member of SSE and was consulted in the initial organising (e.g., Mayr to Schmitt, 7 May 1946 Schmitt Papers, box 24, folder 9). Neither Wharton nor Blackwelder joined the SSE (see the July 1947 membership directory in Schmitt Papers, box 52, folder 2). Minutes of subsequent meetings of both society and council likewise record no conversation as Blackwelder describes. This conversation also may have taken place in another setting, or it might be a conflation of memories. Blackwelder (1979: 157), for example, places Schmitt’s confrontation at Boston in December 1946, but this cannot be true because Wharton was not recorded present at that meeting and the minutes show no discussion of the topic (while recording much discussion on other issues). In the same presentation, Blackwelder (1979: 157) states Schmitt’s comments lead to Robert L. Usinger volunteering to help. This could have happened only at the March 1946 meeting, where Usinger was present. In a *circa* 1951 printed brochure about the SSZ, Blackwelder states a group met “shortly afterward to organize another society.” (Brochure in Simpson Papers, folder “Society of Systematic Zoology”. Quote on p. 3.)

- ⁷ These meetings probably were for the Biological Society of Washington. In 1947, Schmitt served as the BSW’s president.
- ⁸ Wharton to Schmitt 1 April 1947, Schmitt Papers, box 60, folder “[SSZ]: Correspondence, T–Z”.
- ⁹ Wharton to Schmitt 1 April 1947, Schmitt Papers, box 60, folder “[SSZ]: Correspondence, T–Z”. The interest in inclusiveness might be a subtle comment against the leaders of the evolution society, who strongly clustered around only a few locations, especially New York and Berkeley (Cain, 2002).
- ¹⁰ Schmitt to Wharton 3 April 1947, Schmitt Papers, box 60, folder “[SSZ]: Correspondence, T–Z”.
- ¹¹ No records exist for what transpired in Washington during the summer meetings of Schmitt, Wharton and others. Schmitt certainly talked up the idea of a society but to whom and in what detail is not preserved. Important for context, the first issue of Mayr’s journal, *Evolution*, appear in mid-July 1947.
- ¹² Schmitt to Wharton 7 Aug 1947, Schmitt Papers, box 60, folder “[SSZ]: Correspondence, T–Z”.
- ¹³ As illustrations of his point, Schmitt could look at Mayr’s success with the Society for the Study of Evolution (Cain, 1994) and at Alfred Emerson as one overwhelmed in the Society for the Study of Speciation to the society’s detriment (Cain, 2000b).
- ¹⁴ Schmitt to Wharton 7 Aug 1947, Schmitt Papers, box 60, folder “[SSZ]: Correspondence, T–Z”.
- ¹⁵ Schmitt to Wharton 7 Aug 1947, Schmitt Papers, box 60, folder “[SSZ]: Correspondence, T–Z”.
- ¹⁶ Wharton to Schmitt 18 Aug 1947, Schmitt Papers, box 60, folder “[SSZ]: Correspondence, T–Z”. Schmitt clearly felt the stronger sense of urgency for organising. Wharton’s caution seems motivated by a desire for procedures that maximised inclusion.

- ¹⁷ For example, signatories are discussed in Schmitt to Wharton 29 Aug 1947, Schmitt Papers, box 60, folder “[SSZ]: Correspondence, T–Z”.
- ¹⁸ Schmitt to Wharton 29 Aug 1947, Schmitt Papers, box 60, folder “[SSZ]: Correspondence, T–Z”. Petrunkevitch’s original letter has not been located. Schmitt always credited Petrunkevitch with suggesting the idea of a journal, e.g., Schmitt to Petrunkevitch 21 September 1948, Schmitt Papers, box 60, folder “[SSZ]: Correspondence, M–P”. Also Schmitt to Pitelka 14 July 1950, in same folder, where Schmitt added, “He said we needed it to be able to maintain our place and position in the scientific world. Aside from all other considerations, it has a prestige value that cannot be gainsaid.”
- ¹⁹ The number of copies is stated Schmitt to WH Camp 29 Sept 1947, Schmitt Papers, box 60, folder “[SSZ]: Correspondence, A–E”. No directory of recipients has been located.
- ²⁰ “Zoologists” was the preferred reference, rather than “Zoology”. This point was discussed explicitly at the organization meeting in December 1947, when “Dr [E. Raymond] Hall moved that the name remain the Society of Systematic Zoologists for the present, and Dr [Thomas] Park amended the motion to the effect that the name be taken up by the committee who would prepare a constitution.” (minutes of 29 December 1947 meeting, in SSZ Records, box 24, folder: “1947 Organiz. Mtg.”) In correspondence through August 1948 (e.g., draft constitution in Hubbs Papers, box 34, folder 63), Schmitt and Wharton refer to the “Society of Systematic Zoologists”. The approved version of the constitution dated 13 September 1948 (copy in Hubbs Papers, box 34, folder 61) refers to the “Society of Systematic Zoology”.
- ²¹ “Proposal for a Society of Systematic Zoologists” 8 Sept 1947, Schmitt Papers, box 60, folder “SSZ: Memoranda, Minutes of Meetings, Election Ballots, Financial Records, Committee Reports, 1947–1959”. Another copy is located in Hubbs Papers, box 34, folder 60. This is the letter mentioned in (Blackwelder, 1977: 109). The first draft of this proposal, handwritten by Schmitt with modifications by Wharton, accompanies Wharton to Schmitt 14 August 1947, Schmitt Papers box 60, folder “[SSZ]: Correspondence, T–Z”.
- ²² In the early 1960s, Price (1963) identified this problem, noting the formation of “invisible colleges” and proposing their use to effectively manage disciplines.
- ²³ The situation had stabilised considerably by the end of 1948. The XIII International Congress took place in Paris (July 1948). A good deal of revision was done to Commission procedure and structure but the central goal was to reconstitute the international framework and stabilise working practices (Hemming, 1948). The whole *Bulletin of Zoological Nomenclature* 1950 number 4 was devoted to changes from the Paris meetings of the Commission (also see Melville, 1995: 52–55). Intensive work in the 1950s led to the first (1961) edition of the *International Code of Zoological Nomenclature*, the first complete and official revision to nomenclature rules since the *Règles*.
- ²⁴ The official statutes for zoological nomenclature in 1947 remained the 1905 *Règles Internationales de la Nomenclature Zoologique* (Rudeval, 1905), with the French original superseding German and English translations. Amendments to the *Règles* were adopted at nearly every international congress. News of subsequent Opinions rendered by the International Commission on Zoological Nomenclature peppered the specialist literature and were summarised in various compilations, such as Shenk and McMasters (1936) — revised by Keen and Miller (1948) — and Van Cleave (1943). In the period 1910–1936, the Commission issued 133 Opinions. Between 1939–1948, they issued 61 more (Melville, 1995). The *Bulletin of Zoological Nomenclature* began as part of an effort to

systematically record this information. As with all systems based on case law, differences in interpretation accumulated.

- ²⁵ Allen (1975) called this the “revolt from morphology”. The precise nature of this process has been debated extensively (e.g. Pauly, 1987, Rainger et al., 1988, Maienschein, 1991, Cittadino, 1990, Benson et al., 1991). On post-war changes to science, Holton (1972) and Thackery (1992) cover this subject generally; also see Heims (1991) for cybernetics; Hagen (1992) for ecology, and Allen (1975: 187–228) and Kay (1993) for molecular biology.
- ²⁶ This information is printed on cover 3 (inside back cover) of the journal’s volume 1, number 1.
- ²⁷ Cain (1994, 2000a, 2000b, 2002) examines the evolutionists’ programme in depth. Evolutionary taxonomy underwent considerable growth in popularity during the 1940s. It focused attention on intra-specific variation and mechanisms for speciation. It challenged taxonomy to reflect degrees of evolutionary relatedness and used nomenclature to imbed theories about the dynamics of divergence or isolation. Mayr (1942) and Simpson (1937, 1945) are the best remembered American advocates of this approach. Other prominent exemplars at the time included Kinsey’s (1936) studies of wasps, Emerson’s (1941, 1937) work on the phylogeny of termite nests, and Lack’s work on robins (Lack, 1943) and finches (Lack, 1943); among others. Hull (1983) shows a strong presence of evolutionary systematists in the earliest volumes of *Systematic Zoology*.
- ²⁸ Wharton’s views are difficult to assess as very little correspondence in his voice survives.
- ²⁹ Schmitt to Mayr 4 June 1946, Schmitt Papers, box 24, folder “Mayr, Ernst,” in which he explained to potential funders, “For us systematists evolution has been, throughout, a guiding principle and basic to all our ideas of classification and species. That there may yet be a journal in which the dynamics of evolution and of species will be freely discussed, where pertinent observations and the results of special investigations can be brought together for all who are interested sounds almost too good to be true.”
- ³⁰ Schmitt to Theodor Just 11 May 1949, Schmitt Papers, box 52, folder “Society for the Study of Evolution”. Schmitt told Just, “The last few numbers have been particularly good and, from my humble systematic point of view, most pertinent.”
- ³¹ See, for example, Schmitt to Mayr 7 June 1965, Schmitt Papers, box 24, folder “Mayr, Ernst”.
- ³² Boyden’s considerable expertise in serology served as background (see, e.g., Boyden, 1953, Cole, 1958).
- ³³ By the middle 1950s this would lead to the Scientific Manpower Commission (Bates, 1965: 195). The American Institute of Biological Sciences also became involved in manpower issues for the life sciences.
- ³⁴ “Systematic directory of members” (no date, nine pages, by Blackwelder) Schmitt Papers, box 60, folder: “SSE: Memoranda, Minutes of Meetings, Election Ballots, Financial Records, Committee Reports, 1947–1959”. A copy in Hubbs Papers, box 34, folder 61, is stamped as received 17 March 1949. A directory of members also appears in the first *News Letter*, dated 7 December 1949.
- ³⁵ Dupree (1972, 1963, 1957) discussed service at the federal level in America. Greenberg (1967) and Appel (2000) consider the political dimensions. This relation between science and the state was hardly new either to American systematics or to the twentieth century,

e.g., Koerner (1999) or Brockway (1979). Lewenstein (1999: 105) argued the language of service reflected an increasingly outward-looking language in American science during the post-war years as it became increasingly focused on improving human welfare. With Appel (2000) in mind, this language seems more to do with shifting patterns of patronage and the rhetoric or justifications those patrons found politically acceptable.

- ³⁶ The Ohio State University's "Faculty Information Service" 5 October 1968 biography of Wharton emphasized his service to practical problems in parasitology. Thanks to Michelle Drobik (University Archives at Ohio State University) for this item and other biographical material.
- ³⁷ Blackwelder (1979) described Schmitt's activities during the war.
- ³⁸ The American Institute of Biological Sciences compiled membership data on US biological societies, e.g., *AIBS Bulletin*, volume 1 (January 1951), page 10. The largest societies were in applied subjects: the Society of American Foresters (6,500 members), the Society of American Bacteriologists (4,800), American Dairy Science Association (2,700), and the American Association of Economic Entomologists (2,500). Twenty-four for the 43 organizations listed reported memberships higher than the SSZ.
- ³⁹ The newsletter is reproduced by Cain (1999) and discussed in Cain (2000b).
- ⁴⁰ "Horton" to Schmitt 4 Oct 1947, Schmitt Papers, box 60, folder "[SSZ]: Correspondence, F–L". Such resentment was common across the life sciences. For example, it underlies Mayr's often quoted initial approval of Dobzhansky as "here is finally a geneticist who talks like a naturalist" (Mayr, 1992: 2). In the 1940s, the rise of ecosystem ecology provoked a similar complaint about ignorance of specific organisms (Hagen, 1992), and these resembled complaints by stratigraphers in paleontology as well as experimenters in high energy theoretical physics (Traweek, 1988).
- ⁴¹ For example, Ball (1946). Vernon (1993) discusses this point generally. The concern over status is repeated in the 1950s with respect to funding within the US National Science Foundation (Appel, 2000) and in the 1960s in conflicts between cell and molecular biologists on the one hand and evolutionary, organismal, and ecological biologists on the other hand (Dietrich, 1998, Beatty, 1994). Wilson (1994: 218–237) described this tension as it manifested at Harvard University. Larson (2001) and Kleinman (1999) examine cases where naturalists adapted to the post-war changes in priorities and thrived.
- ⁴² In evolution, calls for unity are discussed by Cain (1994). In genetics, Ralph Cleland to "colleague" 20 July 1946, Dunn Papers, folder "National Academy of Sciences, #1". Cleland was working to build support for a 10 July 1946 petition to create a new section for genetics in the National Academy membership as a way to build "unity of spirit and purpose". Parr's unifying schemes centered on ecology and oceanography (Parr, 1939). Parallel calls came to unify academic biology as a whole (e.g., via American Institute of Biological Sciences, the American Society of Zoologists, and *The American Naturalist*) or through the medical sciences (e.g. National Society for Medical Research). This paralleled calls to unify science as a whole (e.g. Lee, 1951, Shapley, 1950).
- ⁴³ Blackwelder (1979: 158–159). Given his sense of rivalry amongst groups of systematists, this organising tactic represents an attempt to develop positional superiority (Said, 1978) in which his camp would stand to speak for all systematists.
- ⁴⁴ Mayr to Hubbs 6 January 1944, Hubbs Papers, box 35, folder 56. Mayr had complex reasons to suggest this plan. These focused on promoting systematics within the life sciences more than inward looking concerns of taxonomic practice. Hubbs' reply and a

follow-up letter from Mayr is located in the same folder. In Mayr's follow-up, he proposed a *Journal of General Systematics* and a "Society of Animal Taxonomists" (Mayr to Hubbs 15 January 1944, Hubbs Papers, folder 35, folder 56). When Mayr proposed launching the Society for the Study of Evolution several years later, he pressed for an emphasis on "evolutionary systematics" and an organization that sought "to bring the taxonomists and geneticists together" (e.g., Mayr to Hubbs 11 January 1945 Hubbs Papers, box 35, folder 56, and Mayr to Hubbs 23 September 1947, Hubbs Papers, box 34, folder 58. This period is examined from Mayr's perspective in Cain (2002, 1994).

- ⁴⁵ Mayr to Karl Schmidt, 26 April 1946, SSE Papers.
- ⁴⁶ Mayr to Waldo LaSalle Schmitt, 9 March 1948, *Mayr/Evolution* Papers, folder "Schmitt".
- ⁴⁷ Mayr to Schimtt, 17 February 1948, *Mayr/Evolution* Papers, folder "Schmitt".
- ⁴⁸ Frank A. Pitelka to Lee R. Dice 19 April 1950, Hubbs Papers, box 34, folder 62.
- ⁴⁹ Frank A. Pitelka to Lee R. Dice 19 April 1950, Hubbs Papers, box 34, folder 62.
- ⁵⁰ Wharton to Schmitt 3 Dec 1947, Schmitt Papers, box 60, folder "[SSZ]: Correspondence, T–Z".
- ⁵¹ For instance, Louis Hutchins sent seven pages of detailed suggestions, in Louis Hutchins to Schmitt 3 October 1947, Schmitt Papers, box 60, folder "[SSZ]: Correspondence, F–L". A short note of approval was written by Hubbs to Schmitt 30 September 1947, Hubbs Papers, box 34, folder 62.
- ⁵² Wharton to Schmitt 6 Oct 1947, Schmitt Papers, box 60, folder "[SSZ]: Correspondence, T–Z".
- ⁵³ "Horton" to Schmitt 4 Oct 1947, Schmitt Papers, box 60, folder "[SSZ]: Correspondence, F–L".
- ⁵⁴ Stenzel to Schmitt 29 Sept 1947, Schmitt Papers, box 60, folder "[SSZ]: Correspondence, R–S".
- ⁵⁵ J. Percy Moore to Schmitt 28 Sept 1947, Schmitt Papers, box 60, folder "[SSZ]: Correspondence, M–P".
- ⁵⁶ Wharton to Schmitt 6 Oct 1947, Schmitt Papers, box 60, folder "[SSZ]: Correspondence, T–Z".
- ⁵⁷ Schmitt to WH Camp 29 Sept 1947, Schmitt Papers, box 60, folder "[SSZ]: Correspondence, A–E".
- ⁵⁸ The year 1958 was one of several plausible bicentenaries for Linnaeus (Koerner, 1999). The first editions of his pamphlets *Systema naturae* (System of nature, 1735 first edition) and *Genera plantarum* (Genera of plants, 1737) used the sexual system, but his use of this approach preceded these publications. Linnaeus first used trivial names for species in *Species plantarum* (Species of plants, 1753) and in the tenth edition of *Systema naturae* (System of nature, 1758). Use of the year 1758 probably is meant to celebrate the adoption of trivial names and the consistent use of binomial nomenclature.
- ⁵⁹ Schmitt to Wharton 28 Nov 1947, Schmitt Papers, box 60, folder "[SSZ]: Correspondence, T–Z". A list of committees serving in the first year is located Schmitt Papers, box 60, folder "SSZ: Memoranda, Minutes of Meetings, Election Ballots, Financial Records, Committee Reports, 1947–1959". One person declined nomination to a committee chair.

Libbie Hyman was asked to run the “Committee on Improving Publication Sources” but declined. As Schmitt told her colleague Alexander Petrunkrvtch, “It was my hope that she would begin to break ground looking toward a periodical for systematic zoology...”
Schmitt to Petrunkrvtch 21 September 1948, Schmitt Papers, box 60, folder “[SSZ]: Correspondence, M–P”. Blackwelder (1977: 109–110) mentions some of this work though attributes its start to 1948 rather than 1947.

- ⁶⁰ Schmitt to Norman Stoll 28 Nov 1947, Schmitt Papers, box 60, folder “[SSZ]: Correspondence, R–S”. The reference is certainly to the International Commission on Zoological Nomenclature.
- ⁶¹ Wharton to Schmitt 3 Dec 1947, Schmitt Papers, box 60, folder “[SSZ]: Correspondence, T–Z”.
- ⁶² British lepidopterist and civil servant Arthur Francis Hemming (1893–1964) served as Secretary for the International Commission on Zoological Nomenclature since 1936 (Melville, 1995).
- ⁶³ Minutes for the December 1947 Chicago meeting were made by Wharton and are located in Schmitt Papers. box 24, folder “1947 Organiz. Mtg.”. They do not list attendees. Technically, the Society had not yet launched because they voted only to proceed with the organisation, not vote to approve the constitution. Schmitt and Wharton’s draft constitution was approved as a provisional structure for one year. The formalities are documented in extensive correspondence, e.g., Schmitt to Wharton 9 Jan 1948, Wharton to ER Hall 11 Jan 1948, and Schmitt to Wharton 6 Feb 1948, all in Schmitt Papers, box 60, folder “[SSZ]: Correspondence, T–Z”. Some of the first complaints about organisational decisions arrived before March. FG Walton Smith complained to Schmitt about lack of potential councillor as marine invertebrate specialist. Mayr complained about the nominating process for officers. See Schmitt to Wharton 9 March 1948, Schmitt Papers, box 60, folder “[SSZ]: Correspondence, T–Z”. A good summary of activity is provided by Wharton to “Dear Colleague” 1 April 1948 letter/circular, Schmitt Papers, box 60, folder “SSE: Memoranda, Minutes of Meetings, Election Ballots, Financial Records, Committee Reports, 1947–1959”.
- ⁶⁴ Membership lists for the society as of 10 May 1948 and 1 Dec 1948 are located Schmitt Papers, box 60, folder “SSE: Memoranda, Minutes of Meetings, Election Ballots, Financial Records, Committee Reports, 1947–1959”.
- ⁶⁵ In 1948, annual meetings of scientific societies associated with the AAAS broke with the tradition of convocation week at the end of December. This was because the AAAS celebrated its centennial in September and moved its annual convention accordingly: 13–17 September 1948 in Washington, DC. The new American Institute of Biological Sciences (AIBS) organised meetings of its charter biological societies in conjunction, held on 10–13 September 1948. The SSZ met within the AIBS meetings. Schmitt called the 12 September 1948 meeting “our definitive organization meeting” (Schmitt to Walter Necker 7 Sept 1948, Schmitt Papers, box 60, folder: “[SSZ] Correspondence M–P”).
- ⁶⁶ Schmitt Papers, box 60, folder “SSZ-Constitution and By-Laws” contains a constitution marked as the “original” copy and dated as adopted on 13 Sept 1948. The date may be an error, mistaken for the 12th. Details on the state of society business are summarised in a letter from Schmitt to Petrunkrvtch 21 September 1948, Schmitt Papers, box 60, folder “[SSZ]: Correspondence, M–P”. Some time after the meeting, Wharton circulated a mimeographed letter to members of the society describing the organisational process to date, announcing the formation of ten committees, and counting the number of members

at 345 (Wharton to Hubbs, no date but stamped as received 17 March 1949, Hubbs Papers, box 34, folder 61).

- ⁶⁷ Schmitt to Alexander Petrunkevitch 7 Sept 1948, Schmitt Papers, box 60, folder “[SSZ] Correspondence M–P”. Schmitt’s organisational efforts to date were represented in his “List of Committees serving prior to September 12, 1948,” Schmitt Papers, box 60, folder “SSE: Memoranda, Minutes of Meetings, Election Ballots, Financial Records, Committee Reports, 1947–1959”.
- ⁶⁸ Schmitt to Petrunkevitch 21 Sept 1948, Schmitt Papers, box 60, folder “[SSZ] Correspondence M–P”. Schmitt also offered more advice about appointing committees and putting them to work. He paid particular attention to council work regarding nomenclature. “No matter how uncertain our present dealings with nomenclatorial problems may be, it is better to be somewhat more conservative than radical, as Hemming seems to have become.” Petrunkevitch was slow to agree to the post, as is shown in correspondence as late as Schmitt to Petrunkevitch 15 Oct 1948, Schmitt Papers, box 60, folder “[SSZ] Correspondence M–P”.
- ⁶⁹ An expert in coleoptera, Richard Eliot Blackwelder (1909–2001) earned his PhD at Stanford University (1934), held a postdoctoral position at the US National Museum (1935–1938), then assistant curator at the American Museum of Natural History in New York (1938–1940), then returned to the USNM as assistant (1940–1942) and associate curator (1942–1954). Conflicts with management (largely over the relative balance of his time spent on SSZ business) led to his resignation from the USNM. Blackwelder moved to St John Fisher College in Rochester, NY (1956–1958), then to Southern Illinois University at Carbondale, rising through the professorial ranks (1958–1977, then emeritus). In his retirement, Blackwelder became a leading collector of Tolkieniana (Blackwelder, 1990, 1993 #41). He donated this substantial collection to Marquette University (which also holds the JRR Tolkien Papers).
- ⁷⁰ Blackwelder summarised the main points of his work in the SSZ in Blackwelder (1977: 110–114).
- ⁷¹ Wharton’s minutes of the December 1947 meeting do not identify Blackwelder as speaking and provide no list of attendees. This makes it impossible to say if Blackwelder was in attendance.
- ⁷² “Memo for Dr. Waldo L. Schmitt” from Blackwelder 6 March 1948 and Schmitt to Wharton 9 March 1948, both in Schmitt Papers, box 60, folder “[SSZ]: Correspondence, T–Z”. The original version of Wharton’s second circular is lost so it is impossible to learn much about what Blackwelder changed in his revision.
- ⁷³ “Memo for Dr. Waldo L. Schmitt” from Blackwelder 6 March 1948 and Schmitt to Wharton 9 March 1948, both in Schmitt Papers, box 60, folder “[SSZ]: Correspondence, T–Z”.
- ⁷⁴ The draft text is located in Hubbs Papers, box 34, folder 60. A copy of the printed brochure is located in Simpson Papers, folder “Society of Systematic Zoology”. Quote on p. 3 of printed version. Blackwelder published a similar version of events in his “Secretary’s Special Issue” of the *SSZ News Letter*, series 2, number 7, June 1957. A copy is located in SSZ Records, box 23.
- ⁷⁵ The oral history is deposited as Record Unit 9517, Smithsonian Institution Archives, Washington, DC. His discussion of the SSZ’s formation is on pp. 34–42.
- ⁷⁶ “Dick” to “Waldo”, 2 December 1957 in Schmitt Papers, folder “Blackwelder, R.E., 1949–1959”. The paper referred to is Borgmeier (1957).

- ⁷⁷ In contrast, Schmitt used a different representation, one that minimized the extent of their intellectual overlap and that provided for co-existence.
- ⁷⁸ Securing funds to launch *Evolution* was by no means simple (Cain, 1994). After failing elsewhere, Mayr and Simpson approached the American Philosophical Society in 1946 only because a colleague pointed out they had surplus funds not spent during the war. After some delicate negotiations and a campaign of supporting letters, the money was awarded. This support was unusual for the APS, who worried about the precedent it might set.
- ⁷⁹ A copy of the “Proposal to Establish a New Journal, Systematic Zoology, ...” accompanies Blackwelder to Simpson 27 July 1951 Simpson Papers, folder “Society of Systematic Zoology #1”.
- ⁸⁰ For example, Hubbs to Edward Conklin 3 Aug 1951, Schmitt Papers, box 60, folder: “[SSZ]-Correspondence F–L”. Hubbs argued: “That our Society is filling a real need and place is indicated by the success of its programs and by the fact that it has very rapidly grown to a membership of approximately 1000, with prospects for future growth. A real reason for the success of the meetings and of the Society is the feeling that there is need for an integrative association of specialists in different fields of systematic zoology. The trend of development in systematic zoology has been very strongly towards the broad general interpretive or ‘philosophical’ approach. An increasingly large number of workers, in our Society representing the majority in the dominant group, are concerned very much with the broad general definitely biological aspects of the science. There has been a strong integrative tie-in between systematic zoology and evolution, genetics, modern ecology, embryology, and other fields. My feeling is that it would be very good for biology in general to have established on an adequate basis a journal of ‘Systematic Zoology’ that would emphasize the broadly applicable and fundamental aspects of systematic work. This would make for a better understanding between zoologists in different fields and would certainly increase integration. The journal would certainly be mighty good for systematic zoologists, as it would tend to lead them further into fundamental thinking and fundamental research.” Other correspondence from Hubbs in this folder shows the extent of efforts to obtain this support.
- ⁸¹ Simpson to Blackwelder 21 August 1951, Simpson Papers, folder: “Society of Systematic Zoology”.
- ⁸² Schmitt to Pitelka 14 July 1950, Schmitt Papers, box 60, folder: “[SSZ]: Correspondence, M–P”. No doubt, Schmitt was aware of the growing concern in the museum about Blackwelder’s workload and the time he spent on SSZ activities.
- ⁸³ Boyden to Hubbs 22 January 1951, Hubbs Papers, box 34, folder 64.
- ⁸⁴ Alfred Romer (SSZ president in 1952) wrote Blackwelder to intervene on Brooks’ behalf in June 1952, “you have pretty much kept the editorial reins in your own hands and left him little leeway.” Romer tried to be polite but forcefully, “You have done so much for the Society, Dick, that I hate to appear to oppose in any way anything you wish.” But in Brooks, Romer saw “another guy that looks to be able to develop an equal enthusiasm and carry a chunk of the burden of Society work.” Schmitt rushed to Blackwelder’s defence, complaining that Brooks was inexperienced and acted with a rather strong hand. This led to meetings between Romer and Blackwelder, and Blackwelder and Brooks before the end of the month. In the end, Romer found reconciliation, “You know, Dick, I think that the fact we can have a bit of a dither of this rot is something to be pleased over rather than the contrary. It emphasizes the fact that in the Society and its Journal we

really have something worth getting excited about.” Romer summarised the situation best when writing to Schmitt: “In Dick and Brooks we have two young bucks so eager to do a good job that they were getting in each other’s hair.” Related correspondence is peppered throughout the Schmitt papers, box 60. For quotes, see Schmitt to Romer 6 June 1952, 10 June 1952, and 23 June 1952 Schmitt Papers, box 60, folder “[SSZ]: Correspondence, R–S”.

- ⁸⁵ Blackwelder’s comment in the 1 March 1953 *News Letter*, which was the first number of the second series.
- ⁸⁶ Mayr, Linsley, and Usinger (1953). This and Simpson (1961) upstaged Blackwelder, who wanted to write a book on general principles. He finally succeeding with Blackwelder (1967). Mayr replied with a revision to his earlier text (Mayr, 1969). By this time, the systematics community was well and truly polarised into tribal groups and both men were being upstaged by other issues (Hull, 1988).
- ⁸⁷ Also elected to the International Commission in 1954 was another advocate of the new systematics, Alden Miller. He served 1954–1966, and as president 1963–1965.
- ⁸⁸ Mayr to Hubbs 5 May 1954, Hubbs Papers, box 24, folder 60. The 1954 election included Mayr and Petrunkevitch, bringing Hubbs to calculate the presence of seven taxonomists (also including George Simpson, Alfred Romer, Alexander Wetmore, Remington Kellogg, and Carl Hubbs himself).
- ⁸⁹ Schmitt Papers, box 40, folder “American Institute of Biological Sciences,” covers 1950–1972, including “Report of the Committee on Systematic Biology of the [AIBS],” undated 24 page. Serving on this committee were Alfred Emerson, C.O. Erlanson (USDA), Th. Just (Chicago), Karl Schmidt, George Simpson, W.C. Steere (Stanford), Oswald Tippo (Yale), Victor Twitty (Stanford), and Ernst Mayr (Chair). This project related to the NSF in 1954 and discussed at length the possibility of creating a national center for systematic biology.
- ⁹⁰ Blackwelder belonged to no other major professional organisation in his career other than AAAS and SSZ. His affiliations are followed in successive editions of *American Men and Women of Science*. This sense of ownership is illustrated in his correspondence with Carl Hubbs during Hubbs’ presidency in 1951, see Hubbs Papers, box 34, folders 60–64.
- ⁹¹ Blackwelder’s resignation letter is Blackwelder to Leonard Carmichael 23 October 1954 in Schmitt Papers, box 3, folder “Blackwelder, R.E., 1949–1959”.
- ⁹² Schmitt to Mayr 10 November 1955, Schmitt Papers, box 24, folder “Mayr, Ernst”. Thinking ahead, Schmitt prepared Blackwelder for the possibility of a visit from Mayr with this idea in mind. “Don’t feel as I first did about it — that he was trying to take part of our original project [of a directory through the SSZ] and make something of it. I may be mistaken but, whether or no, I want you to be receptive so that at least you can explore the proposition. I would like you to take hold of it, perhaps insisting on enough money to make it worth your while.” (Schmitt to Blackwelder 14 Nov 1955 in Schmitt Papers, box 3, folder “Blackwelder, R.E., 1949–1959”)