Laboratories, Museums, and the Comparative Perspective: Alan A. Boyden's Quest for Objectivity in Serological Taxonomy, 1924–1962

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ABSTRACT

The rise of experimentation and the decline of natural history constitute the historiographic backbone to most narratives about the history of the life sciences in the twentieth century. As I argue here, however, natural history practices, such as the collection and comparison of data from numerous species, and experimental practices have actually converged throughout the century, giving rise to a new hybrid research culture which is essential to the contemporary life sciences. Looking at some examples of researchers who studied experimentally the relationships between organisms offers a unique window into how the norms, values, and practices of natural history entered the laboratory and, conversely, how the norms, values, and practices of experimentation transformed natural history. This paper concentrates on a largely overlooked episode in the history of the life sciences: the development of Alan A. Boyden's serological taxonomy. In the United States, from the late 1920s to the early 1960s, he was the most prominent advocate of this experimental approach in natural history. His quest for an objective method to understand the relationships among species, his creation of a serological museum where he could apply his comparative perspective, and his continued negotiations between natural historical and experimental traditions, illustrate the rise of a new hybrid research culture in the twentieth century. It also helps us solve a historiographic puzzle, namely how biological diversity

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The following abbreviations are used: AMNH, American Museum of Natural History; APS Archives, Gaylord Simpson Papers, American Philosophical Society, Philadelphia, PA, Ms. Coll. 31, Series I, Folder Alan A. Boyden; *JHB, Journal of the History of Biology*; *NYT*, *New York Times*; *PZ*, *Physiological Zoölogy*; Rutgers Archives, Special Collections and University Archives, Rutgers University Libraries; *SHPBBS*, *Studies in History and Philosophy of Biological and Biomedical Sciences*; *SMB*, *Serological Museum Bulletin.*

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become so central in the experimental life sciences, i.e., in a tradition which we generally understand as having focused on a few model organisms, and which relegated the study of biodiversity to naturalists and their museums.

KEY WORDS: natural history, experimentation, serology, systematics, genomics, model organism, George H. F. Nuttall, Edward T. Reichert, Alan A. Boyden

INTRODUCTION

The relationship between experimentalism and natural history has long been a vexing problem in the historiography of the life sciences.[1] The standard narrative focuses on the decline of natural history starting in the late nineteenth century and the corresponding expansion of experimentalism, which is understood to have dominated the life sciences in the twentieth century. The rise of molecular biology is used as one example to illustrate the triumph of the experimental approach to the production of biological knowledge. A number of authors have added nuance to this story by showing that natural history maintained a diminished but healthy position in the changing realm of the life sciences. They have shown, for example, that natural history was declining relatively but growing absolutely around 1900, due to the general expansion of biology's territory; or that natural history remained "alive and well" after 1900 but "primarily within museums," thus increasingly at the margins of the life sciences.[2] These amendments, however, have not fundamentally altered the debate, especially because

1. This narrative is the main theme of the classic works by William R. Coleman and Garland E. Allen. William R. Coleman, *Biology in the Nineteenth Century: Problems of Form, Function and Transformation* (Cambridge: Cambridge University Press, 1971); Garland E. Allen, *Life Science in the Twentieth Century* (Cambridge: Cambridge University Press, 1978). These works still largely influence contemporary historiography; see for example Peter J. Bowler and Iwan Rhys Morus, *Making Modern Science: A Historical Survey* (Chicago: University of Chicago Press, 2005).

2. See Lynn K. Nyhart, "Natural History and the 'New' Biology," in *Cultures of Natural History*, ed. Nicholas Jardine, James A. Secord, and Emma C. Spary (London: Cambridge University Press, 1996), 426–43, on 422; and Keith R. Benson, "From Museum Research to Laboratory Research: The Transformation of Natural History into Academic Biology," in *The American Development of Biology*, ed. Ronald Rainger, Keith R. Benson, and Jane Maienschein (Philadelphia: University of Pennsylvania Press, 1988), 77. Robert E. Kohler and Joel B. Hagen have most directly challenged this dichotomy; see Robert E. Kohler, *Landscapes and Labscapes: Exploring the Lab–Field Border in Biology* (Chicago: University of Chicago Press, 2002); Joel B. Hagen, "Experimentalists and Naturalists in 20th-Century Botany—Experimental Taxonomy, 1920–1950," *JHB* 17, no. 2 (1984): 249–70; and Joel B. Hagen, "Naturalists, Molecular Biology, and the Challenge of Molecular Evolution," *JHB* 32 (1999): 321–41.

they have tended to view the naturalist and experimentalist traditions as mutually exclusive. Here, I argue that it is more productive to consider these approaches as different but complementary "ways of knowing" that together compose the fabric of modern science.[3] This perspective makes it possible to reconsider the standard narrative of twentieth-century life science by examining the tensions that occurred within the experimental sciences and within natural history, including those between researchers in each area who adopted the natural historical way of knowing—its epistemic values, its material practices, and its moral economy—and those who aligned themselves with the experimental way of knowing.[4] There is no doubt that the experimental sciences gained much authority in the twentieth century, but that does not mean that their success was due solely to the experimental way of knowing.

One of the most distinctive aspects of the natural historical way of knowing is its reliance on the collection, comparison, and computation of biological facts from many species. The historiography of natural history has emphasized how, since the Renaissance, naturalists have produced knowledge by comparing natural facts from a great variety of organisms.[5] Natural history was certainly descriptive, as its critics argued, but more importantly, it was comparative. Collections—curiosity cabinets, botanical gardens, and natural history museums, for example—were tools which naturalists used to compare numerous

3. John V. Pickstone, *Ways of Knowing: A New History of Science, Technology and Medicine* (Manchester: Manchester University Press, 2000); John V. Pickstone, "Working Knowledges Before and After Circa 1800: Practices and Disciplines in the History of Science, Technology and Medicine," *Isis* 98 (2007): 489–516.

4. For the tensions within the experimental sciences, see Bruno J. Strasser, "Collecting and Experimenting: The Moral Economies of Biological Research, 1960s–1980s," Preprints of the *Max Planck Institute for the History of Science* 310 (2006): 105–23; Bruno J. Strasser, "GenBank: Natural History in the 21st Century?," *Science* 322 (2008): 537–38; Bruno J. Strasser, "Collecting, Comparing and Computing Sequences: The Making of Margaret O. Dayhoff's Atlas of Protein Sequences and Structure, 1954–1965," *JHB* (2010, in press); Bruno J. Strasser," Inventing GenBank: Natural History, Experimentation, and the Moral Economies of Biomedicine, 1979–1982" *Isis* (in press).

5. For a broad overview, see Paul Lawrence Farber, *Finding Order in Nature: The Naturalist Tradition from Linnaeus to E. O. Wilson* (Baltimore: Johns Hopkins University Press, 2000). On the importance of collections in the natural history perspective, see for example Brian W. Ogilvie, *The Science of Describing: Natural History in Renaissance Europe* (Chicago: University of Chicago Press, 2006); Paula Findlen, *Possessing Nature: Museums, Collecting, and Scientific Culture in Early Modern Italy* (Berkeley: University of California Press, 1994); Nicholas Jardine, James A. Secord, and Emma C. Spary, eds., *Cultures of Natural History* (New York: Cambridge University Press, 1996); Emma C. Spary, *Utopia's Garden: French Natural History from Old Regime to Revolution* (Chicago: University of Chicago Press, 2000); Jim Endersby, *Imperial Nature: Joseph Hooker and the Practices of Victorian Science* (Chicago: University of Chicago Press, 2008).

specimens, representing a variety of species. The comparison of specimens rested, in part, on subjective judgments, and naturalists valued their personal expertise, if not intuition, in carrying out these judgments. The experimental way of knowing, by contrast, relies on the use of instruments to produce phenomena in model organisms.[6] Instruments provided a way to approach an ideal of objectivity based on measurement, quantification, and precision.[7] Unlike naturalists, experimentalists focused on just one specie (or a very small number) over the course of their careers. This approach was expected to lead to universal biological knowledge because it was assumed that "what is true of *E. coli* is true of the elephant," as the molecular biologist Jacques Monod famously put it.[8]

I have argued elsewhere that the contemporary experimental life sciences have borrowed much more from the natural historical way of knowing than has previously been recognized.[9] The widespread collection, comparison, and computation of genomic data, for example, clearly belongs to the natural historical way of knowing, and modern databases such as GenBank, which contains DNA sequences from almost two hundred thousand species, share many

6. See for example, on mice, Karen A. Rader, *Making Mice: Standardizing Animals for American Biomedical Research, 1900–1955* (Princeton: Princeton University Press, 2004); on TMV, Angela N. H. Creager, *The Life of a Virus: Tobacco Mosaic Virus as an Experimental Model, 1930–1965* (Chicago: University of Chicago Press, 2002); on Drosophila, Robert E. Kohler, *Lords of the Fly: Drosophila Genetics and the Experimental Life* (Chicago: University of Chicago Press, 1994); on *C. elegans*, Soraya de Chadarevian, "Of Worms and Programmes: *Caenorhabditis Elegans* and the Study of Development," *SHPBBS* 29, no. 1 (1998): 81–105; on Arabidopsis, Sabina Leonelli, "Growing Weed, Producing Knowledge: An Epistemic History of Arabidopsis Thaliana," *History and Philosophy of the Life Sciences* 29, no. 2 (2007): 193–223; and more generally Adele E. Clarke and Joan H. Fujimura, eds., *The Right Tool for the Job: At Work in Twentieth-Century Life Sciences* (Princeton: Princeton University Press, 1992); and Angela N. H. Creager, Elizabeth Lunbeck, and M. Norton Wise, eds., *Science without Laws: Model Systems, Cases, Exemplary Narratives, Science and Cultural Theory* (Durham: Duke University Press, 2007), part I.

7. On objectivity, Lorraine Daston and Peter Galison, *Objectivity* (New York: Zone Books, 2007); on precision, M. Norton Wise, ed., *The Values of Precision* (Princeton: Princeton University Press, 1995); on quantification, Theodore M. Porter, *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life* (Princeton: Princeton University Press, 1995).

8. Jacques Monod and François Jacob, "General Conclusions: Teleonomic Mechanisms in Cellular Metabolism, Growth, and Differentiation," *Cold Spring Harbor Symposia on Quantitative Biology* 21 (1961): 389–401, on 363. On the origins of this expression, see Herbert C. Friedmann, "From 'Butyribacterium' to 'E. coli'—an Essay on Unity in Biochemistry," *Perspectives in Biology and Medicine* 47, no. 1 (2004): 47–66. In the earlier part of the twentieth century, knowledge derived from animal model organisms was believed to hold true only for animals, not plants and microbes, as would become the case in molecular biology starting in the late 1950s.

9. Strasser, "Collecting and Experimenting" (ref. 4); Strasser, "GenBank" (ref. 4); Strasser, "Collecting, Comparing, and Computing" (ref. 4); Strasser, "Inventing GenBank" (ref. 4).

characteristics with earlier natural history collections. In this paper, however, I examine some of the transformations that occurred within natural history as researchers tried to make it more experimental in the first half of the twentieth century. I focus specifically on how the classification and evolution of species came to be studied in laboratories at the biochemical level. Long before the rise of molecular evolution in the 1960s, which was predicated upon the idea that proteins could be considered "documents of evolutionary history," researchers turned to the biochemical properties of organisms to understand their systematic position and evolutionary history.[10] The few who did so in the first half of the twentieth century, such as George H. F. Nuttall in Cambridge, England, Edward T. Reichert in Philadelphia, and Alan A. Boyden in New Brunswick, New Jersey, were never able, unlike the molecular evolutionists who followed them, to challenge seriously more traditional systematists who based their work on morphological comparisons. But their stories offer a unique window into how the norms, values, and practices of the natural historical way of knowing entered the laboratory, and, conversely, how the norms, values, and practices of the experimental way of knowing transformed natural history.

These researchers contributed in making natural history more experimental and in promoting associated values such as objectivity, measurement, quantification, and precision among naturalists. In this sense, my narrative complements that of Robert E. Kohler, who has explored in great depth in his *Labscapes and Landscapes* how biologists transposed the experimental ideal from the laboratory to the field.[11] My key argument, however, is that a history of experimental systematics does not only show how experimentalist values took hold in natural history, it also illustrates how the natural historical way of knowing, based on collecting, comparing, and computing, came to be practiced in the laboratory. This conclusion follows up on Joel B. Hagen's call to go beyond the simple opposition between naturalists and experimental biologists by looking at some of the places where these two traditions coalesced, such as within the controversy over molecular evolution in the 1960s, or within experimental taxonomy in the 1920s and 1930s.[12]

This paper concentrates on a largely overlooked episode in the history of experimental taxonomy: the development of Alan A. Boyden's serological

10. Emile Zuckerkandl and Linus Pauling, "Molecules as Documents of Evolutionary History," *Journal of Theoretical Biology* 8 (1965): 357–66.

11. Kohler, *Landscapes and Labscapes* (ref. 2).

12. Hagen, ""Naturalists, Molecular Biology" (ref. 2); Hagen, "Experimentalists and Naturalists" (ref. 2).

taxonomy.[13] Although he did not invent the method, Boyden became its most vocal advocate among American life scientists from the late 1920s to the early 1960s. His quest for an objective method to understand the relationships among species, his creation of a serological museum where he could apply his comparative perspective, and his continued negotiations between natural historical and experimental traditions, illustrate the rise of a new hybrid research culture in the twentieth century.

MEASURING SPECIES, CIRCA 1900

Before Boyden began his long career as a serological taxonomist, George Henry Falkiner Nuttall (1862–1937) made one of the earliest attempts to systematically study the diversity of species at the biochemical level and to understand their taxonomic and phylogenetic relationships.[14] Born in San Francisco, Nuttall earned an MD at the University of California at Berkeley and a PhD from the University of Göttingen, before working at Johns Hopkins University and at Robert Koch's Hygienic Laboratory in Berlin. In 1899, he moved to Cambridge, England, where he eventually became a professor of biology and, in 1921, the first director of the Molteno Institute for Research in Parasitology.[15] His training in medical bacteriology had familiarized him with the problems of identifying microorganisms having no visible morphological differences as well as with the methods of serum therapy. This familiarity most likely informed his idea to apply the "precipitin reaction" to the study of animal taxonomy. This precipitate-forming reaction (hence the name) took place when serum (the fluid component of blood) from an animal of one species previously injected

13. Alan A. Boyden's postwar career has been briefly mentioned, but mainly as a critique of the work of Morris Goodman, in Edna Suárez-Díaz, "The Rhetoric of Informational Molecules: Authority and Promises in the Early Study of Molecular Evolution," *Science in Context* 20, no. 4 (2007): 649–77; Joel B. Hagen, "From Immunodiffusion Experiments to Sequence Analysis: Morris Goodman and the Origins of Molecular Anthropology," *JHB* (2010, in press). On the contribution of Mabel Boyden, Alan A. Boyden's wife, see Priska Gisler, "Collecting True Blue Blood: A Journey to the Heart of 1960s Biology," *Endeavour* 33, no. 3 (2009): 112–16.

14. George S. Graham-Smith, "George Henry Falkiner Nuttall (5 Jul 1862–16 Dec 1937)," *Journal of Hygiene* 38, no. 2 (1938): 129–40.

15. George S. Graham-Smith and David Keilin, "George Henry Falkiner Nuttall, 1862–1937," Obituary Notice of Fellows of the Royal Society 2, no. 7 (1939): 493–99. For his later work in parasitology, see Francis E. G. Cox, "George Henry Falkiner Nuttall and the Origins of Parasitology and Parasitology," *Parasitology* 136, no. 2 (2009): 1389–94.

with serum from a second species was brought, in vitro, into the presence of serum from the second or yet another species. Anti-dog serum, for example, was prepared by injecting dog serum into a rabbit. After the rabbit had produced an immunological reaction, its serum (anti-dog) was drawn and tested against serum from various species. The anti-dog serum reacted strongly with dog serum, less so with cat serum, and not at all with crab serum, for example. The reaction thus indicated how similar the blood antigens of two species were, and thus, since it was believed that blood antigens were inherited, how close genetically the species were to one another.[16]

Nuttall first used this technique for forensic purposes (i.e., identifying blood stains), before engaging in a large-scale study of the relationships among species, especially vertebrates.[17] He hoped this technique would allow him to "measure species," as he put it in 1902, and surpass the morphological comparisons which were plagued by the "subjective element" because taxonomists relied on personal judgments in evaluating "similarities of structure in existing forms."[18] For his work, Nuttall obtained the blood of hundreds of species from "seventy gentlemen," mainly naturalists working in natural history museums, public zoos, and colonial research institutions from around the world.[19] In a monograph published in 1904 and dedicated to Paul Ehrlich and Elie Metchinkoff, Nuttall described the results of 16,000 tests performed on 586 species.[20] (Fig. 1) He used

16. Karl Landsteiner has just published his paper on the inheritance of human blood groups in 1901. Louis K. Diamond, "The Story of Our Blood Groups," in *Blood, Pure and Eloquent*, ed. Maxwell M. Wintrobe (New York: McGraw-Hill Book Company, 1980), 691–717.

17. George H. F. Nuttall and Edgar M. Dinkelspiel, "Experiments upon the New Specific Test for Blood (Preliminary Note)," *British Medical Journal* 1 (1901): 1141; George H. F. Nuttall, "On the Formation of Specific Anti-Bodies in the Blood, Following Upon Treatment with the Sera of Different Animals," *American Naturalist* 35 (1901): 927–32; George H. F. Nuttall and Edgar M. Dinkelspiel, "On the Formation of Specific Anti-Bodies in the Blood Following Upon Treatment with the Sera of Different Animals Together with Their Use in Legal Medicine," *Journal of Hygiene* 1, no. 3 (1901): 367–87; George H. F. Nuttall, "A Further Note on the Biological Test for Blood and Its Importance in Zoological Classification," *British Medical Journal* 2 (1901): 669; George H. F. Nuttall, "The New Biological Test for Blood in Relation to Zoological Classification," *Proceedings of the Royal Society of London Series B–Biological Sciences* 69, no. 453 (1901): 150–53; George H. F. Nuttall, "Progress Report Upon the Biological Test for Blood as Applied to over 500 Bloods from Various Sources, Together with a Preliminary Note Upon a Method for Measuring the Degree of Reaction," *British Medical Journal* 1 (1902): 825–27.

18. Nuttall, "Progress Report" (ref. 17), 827; George Henry Falkiner Nuttall, *Blood Immunity and Blood Relationship: A Demonstration of Certain Blood–Relationships Amongst Animals by Means of the Precipitin Test for Blood* (Cambridge: Cambridge University Press, 1904), 1.

19. Ibid., 411–13.

20. Ibid., 213.

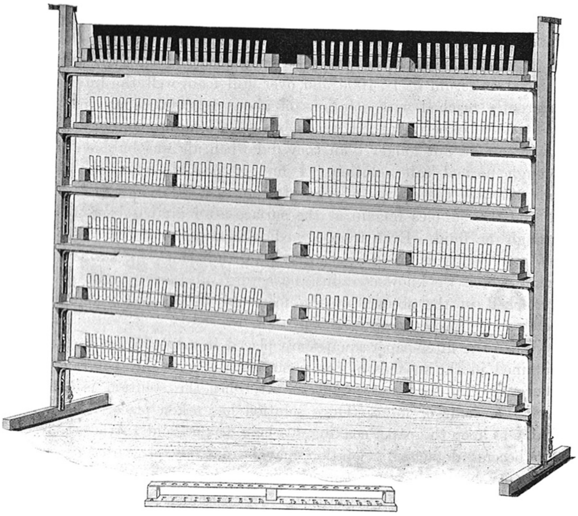

FIG. 1 George H. F. Nuttall's test-tube rack used for his serological studies. Each test tube contained an anti-serum and serum from different species. *Source*: Nuttall, *Blood Immunity* (ref. 18), 68.

the results of these experiments to draw qualitative, and sometimes quantitative, relationships among species. He expressed satisfaction that his results, in the case of primates, for example, confirmed the phylogenies established through morphological comparisons.[21] However, he also was confident enough to claim in some cases that serological results which contradicted morphological classifications were a better description of the order of nature. For example, he claimed that the horseshoe crab was more closely related to arachnids (spiders) than to crustaceans (crabs), whereas morphologists had grouped it, as its common name indicates, with the latter.[22] This work was widely discussed in the first three decades of the twentieth century, but often criticized by researchers who were unable to replicate his results.[23]

In Philadelphia, the physiologist Edward Tyson Reichert (1855–1931) embarked on a similar project, though he used a different technique.[24] After obtaining

21. Ibid., 2.

22. Ibid., 362.

23. For an overview of these critiques, see Alan A. Boyden, "Systematic Serology: A Critical Appreciation," *PZ* 15, no. 2 (1942): 109–45. The lack of standardization of the antisera, and the difficulty of visually determining if a precipitation had actually occurred, were some of the reasons for the inconsistent results.

24. Anonymous, "Reichert, Edward Tyson," in *Marquis Who Was Who in America 1607–1984*, see www.credoreference.com/ (last accessed 1 Mar 2009).

an MD at the University of Pennsylvania School of Medicine in 1879, he worked there on various projects in experimental physiology. Like Nuttall, he examined the properties of blood to compare species, but instead of observing immunological reactions, he focused on the crystallization patterns of hemoglobin, and compared these in different species. Similarities in crystallization patterns, as seen through the microscope, indicated to Reichert that the species were closely related. In the same spirit as Nuttall, he tried to make these comparisons quantitative by measuring crystal angles. He published his results, gathered beginning in 1902 and representing over one hundred species, in a massive volume in 1909, where he explained that "hemoglobin serves as an index … to differentiate genera, species, and individuals."[25] Reichert compared the results of his classifications based on crystal comparisons to traditional phylogenies based on anatomical comparisons, and was pleased to find that they yielded similar results. He later continued his studies by comparing starches from different organisms, publishing an equally massive volume in 1913, and which culminated in a comprehensive monograph summarizing his hemoglobin and starch studies in 1919.[26]

Like Nuttall, and like earlier naturalist collectors as well, Reichert met the challenge of gathering samples from numerous specimens by relying on a broad network of institutions and individuals. He obtained specimens, or sometimes just blood samples, from animals purchased on the food market, or from public aquariums (in New York), zoos (in Philadelphia, Washington, and New York), and slaughterhouses, but also from dealers, collectors, and fishermen.[27] A wide diversity of animals entered the laboratory, although not always in the best condition—he acknowledged that "most animals arrived in a state of putrefaction."[28] In the physiology laboratory, rabbits, dog, and humans were

25. Edward Tyson Reichert and Amos P. Brown, *The Differentiation and Specificity of Corresponding Proteins and Other Vital Substances in Relation to Biological Classification and Organic Evolution: The Crystallography of Hemoglobins* (Washington, DC: Carnegie Institution of Washington, 1909), iv.

26. Edward Tyson Reichert, *The Differentiation and Specificity of Starches in Relation to Genera, Species, etc; Stereochemistry Applied to Protoplasmic Processes and Products, and as a Strictly Scientific Basis for the Classification of Plants and Animals* (Washington, DC: Carnegie Institution of Washington, 1913); Edward Tyson Reichert, *A Biochemic Basis for the Study of Problems of Taxonomy, Heredity, Evolution, Etc., with Especial Reference to the Starches and Tissues of Parent-Stocks and Hybrid-Stocks and the Starches and Hemoglobins of Varieties, Species, and Genera* (Washington, DC: Carnegie Institution of Washington, 1919).

27. Reichert and Brown, *Differentiation and Specificity* (ref. 25), xvi–xvii.

28. Ibid., xvii.

common subjects, but the Indian python, Tasmanian wolf, or Venezuelan deer studied by Reichert were unusual sights, to say the least.

ALAN A. BOYDEN'S SEROLOGICAL SYSTEMATICS

The writings of Nuttall and Reichert, and especially of the former, became the main inspiration for the research of Alan A. Boyden (1897–1986). Unlike his two predecessors, who each made only a brief foray into experimental systematics, Boyden would spend his entire career in that area. He became the leading figure in serological systematics and evolution in the middle third of the twentieth century, further developing Nuttall's techniques and applying them to various problems of classifications and phylogeny. Perhaps most important, he believed that by bringing laboratory techniques to bear on classical problems of natural history, he could reform natural history by making it more quantitative, precise, and objective—the main epistemic values of the experimental sciences.

After obtaining a PhD in zoology at the University of Wisconsin in 1924, Boyden joined the Zoological Laboratory at Rutgers University, where he remained until his retirement in 1962.[29] At the outset of his research he was confident that the "precipitin reaction," used so efficiently by Nuttall, could be much improved in order to assess the relationships among species and draw conclusions about their phylogenies. Indeed, like Nuttall, he assumed in 1926 that the degree of reaction between the antiserum and various (blood) proteins was "in proportion to the degree of relationship of these proteins to each other."[30] Boyden's supervisor at Wisconsin, the zoologist Michael F. Guyer, who had applied this technique in his research on experimental evolution and advocated for its use in taxonomy and phylogeny, most likely inspired Boyden to pursue this line of research.[31] There was also wider interest that may have encouraged Boyden; at the same time, for example, the immunologist Karl Landsteiner

29. For Boyden's biographical data, see James G. Crowther, *Famous American Men of Science* (New York: Penguin Books, 1944), 189 and his personal file, Alan Arthur Boyden, "Faculty Data," 1 Jan 1942, "Faculty Biographical File," 1 Sep 1946 and "Faculty Biographical File," 20 Aug 1957, Rutgers Archives.

30. Alan A. Boyden, "The Precipitin Reaction in the Study of Animal Relationships," *Biological Bulletin* 50, no. 2 (1926): 73–107, on 75.

31. Michael F. Guyer, "Blood Reactions of Man and Animals," *Scientific Monthly* 21, no. 10 (1925): 145–46.

at the Rockefeller Institute was claiming, based on Nuttall and Reichert's work, that "one could roughly construct the zoological tree merely on the basis of precipitin reactions."[32]

Boyden first attempted to improve the reliability of the precipitin reaction, which had been challenged by numerous authors, especially in Germany, after Nuttall's publication. In order to do so, Boyden drew on earlier studies by researchers who had investigated the mechanisms of immunity and also on those who had used the precipitin reaction for forensic purposes, essentially to identify human blood stains, or as the basis of the Wasserman test. He began by using the "ring test" to find the minimum concentration of antiserum which would elicit a reaction (producing a visible ring) in a test tube, thus measuring the sensitivity of the antiserum to a given antigen.[33] He took the value of the homologous reaction (for example, anti-rabbit serum reacting with rabbit serum) as a normal value. The more different two species were, the less reactive the antiserum would be. Boyden thus generated numerical values that he could use to indicate distances between pairs of species. (Fig. 2) Even though the "ring test" was clearly quantitative, the visual determination of the lowest concentration producing a precipitate was still somewhat subject to interpretation.

Boyden applied variations of his "ring test" throughout the 1920s and 1930s, until in 1938 his colleague at Rutgers University, the physicist Raymond L. Libby, developed an instrument to measure the turbidity of solutions, which Boyden then used to assess more precisely and, he believed, more objectively the extent of the precipitin reaction.[34] Instead of visually inspecting the solution for the presence or absence of reaction, he could now automatically estimate the amount of the precipitate for a given concentration of antiserum. The rapidity of this test allowed him to make measurements over a whole range of concentrations and, after some calculations, to obtain a single value representing the strength of the antiserum reaction toward serum of another species, indicating

32. Karl Landsteiner, "Cell Antigens and Individual Specificity," *Journal of Immunology* 15, no. 6 (1928): 589–600, on 596.

33. Boyden, "The Precipitin Reaction" (ref. 30); Alan Boyden and Joseph G. Baier Jr, "A Rapid Quantitative Precipitin Technic," *Journal of Immunology* 17, no. 1 (1929): 29–37; Alan Boyden, "Precipitin Tests as a Basis for a Quantitative Phylogeny," *Proceedings of the Society for Experimental Biology and Medicine* 29, no. 8 (1932): 955–57; Alan A. Boyden, "Precipitins and Phylogeny in Animals," *American Naturalist* 68 (1934): 516–36.

34. For a description of the technique, see Alan A. Boyden and Ralph J. DeFalco, "Report on the Use of the Photronreflectometer in Serological Comparisons," *PZ* 16, no. 3 (1943):229–41.

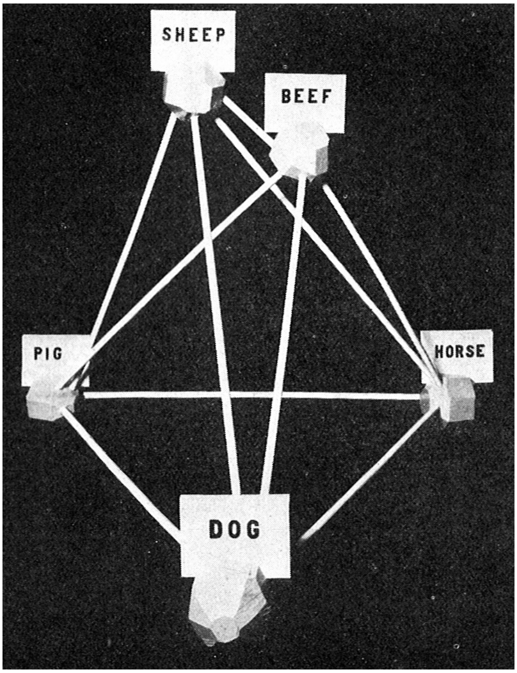

FIG. 2 Distances, represented in three dimensions, between different mammal species, as determined by Boyden's serological tests. *Source*: Boyden, "Precipitins and Phylogeny" (ref. 33), 524.

how closely related were the proteins of two organisms.[35] He spent many years trying to identify all possible sources of experimental error in order to improve the reproducibility of his method.[36]

Boyden's lifelong quest to improve the precipitin method was driven by his desire to make taxonomy (and eventually phylogeny) "entirely objective and independent of the interpretation of the observer."[37] Quantification was a means to achieve this goal, and precision was thus a necessary corollary. Serological methods would "yield measurements" of the degree of relationships among species, making the "study of relationships more exact [and] more scientific," a most desirable achievement for Boyden.[38] By contrast, Boyden, paraphrasing

35. Basically, Boyden took the integral of the titration curve. See Boyden, "Systematic Serology" (ref. 23), and Alan Boyden, "Serology and Animal Systematics," *American Naturalist* 77 (1943): 234–55.

36. See for example the summary in Boyden, "Systematic Serology" (ref. 23).

37. Boyden, "Precipitin Reaction" (ref. 30), 103.

38. Boyden, "Precipitins and Phylogeny" (ref. 33), 518.

his colleague G. Kingsley Noble, head of the Department of Experimental Biology at the American Museum of Natural History (AMNH), argued that "phylogenists and systematists sometimes appear to work on an instinctive basis, to 'feel' their way to their systematic groupings."[39]

Phylogenists and systematists relied mainly on morphological data—skins, bones, and fossils. Boyden's critique thus focused primarily on the problems associated with the use of morphological characters. Because naturalists had failed to develop a quantitative measure of morphological features, he argued, they necessarily depended on "interpretation as to what various structures may mean in descent." The problem with interpretation, for Boyden, was that it "differs with interpreters," resulting in "an endless difference of opinion as to the relationships of certain groups of animals necessitating countless 'revisions' of them."[40]

Boyden was sometimes quite dismissive of morphology, such as when he claimed that his method had "succeeded in giving us what a century or more of intensive morphological investigation has failed to provide, namely, a basis for a quantitative phylogeny."[41] He went so far as to ridicule taxonomists who were working exclusively with morphological characters when he wrote, referring to the rabbits he was using to produce serum in his experiments, that "so far, the rabbit has actually made fewer mistakes than man in the attempt to construct a natural system of classification."[42]

The objectivity of the serological method contrasted with the subjectivity of the morphological method for Boyden. He later explained, comparing the two approaches, that "systematic or comparative serology [relied on] relatively objective means … to reveal essential similarities and differences," whereas "systematic or comparative morphology [relied on] relatively subjective estimates of degree of structural similarity."[43] Over and over again, Boyden justified the use of serology over morphology to determine animal relationships by the fact that it was more quantitative and therefore more objective: "from the standpoint of objectivity alone, biochemical comparisons outrank most morphological descriptions."[44] Elsewhere, he would ask rhetorically, "What can be more objective than chemical description?"[45] Although Boyden acknowledged that

39. Boyden, "Systematic Serology" (ref. 23), 118.
40. Boyden, "Precipitin Reaction" (ref. 30), 102.
41. Boyden, "Precipitin Tests" (ref. 33), 957.
42. Boyden, "Serology and Animal Systematics" (ref. 35), 253.
43. Alan A. Boyden, *SMB* 2 (1949), 12.
44. Boyden, "Serology and Animal Systematics" (ref. 35), 241.
45. Boyden, "Systematic Serology" (ref. 23), 115.

in its present state serology was far from a perfect method, he argued that "biochemical evidence regarding the natures of organisms may ultimately outweigh all other bases of classification," even if at present "some 'dyed-in-the-wool' morphologist" still belittled its importance.[46]

In equating quantification with objectivity, and criticizing morphologists for their qualitative and therefore subjective approach, Boyden ignored the fact that some systematists claimed to be quantitative and subjective simultaneously.[47] In their 1939 *Quantitative Zoology*, for example, the paleontologist George Gaylord Simpson and the clinical psychologist Anne Roe emphasized numerical methods, and even the use of calculating machines, to classify organisms.[48] They presented simple statistical methods to analyze measurements, mainly the size of different skeleton parts of different specimens. Most importantly, they defined species by averaging the measurements of one or more characters present in a collection of specimens. At the same time, they recognized that systematics would always remain an art, and that the choice and weighing of the characters to be measured was somewhat subjective.[49] In a series of letters between Simpson and Boyden over the concept of homology (similarity due to common ancestry), Boyden rejected the concept because there was often no objective way to determine common ancestry independent of the reliance on mere similarity. Simpson, on the other hand, was unmoved by this difficulty. For him, the fact that homology was based "on theory and opinion rather than on objective fact" did not "invalidate it or make it less useful."[50] "Science certainly needs words to express opinions," he claimed unapologetically.[51] In 1953, the leading systematist Ernst Mayr and his co-authors took the same views and explained why taxonomy was both a science and an art: the "good doctor and the good taxonomist make their diagnoses by a skillful evaluation of symptoms

46. Ibid.

47. Joel B. Hagen, "The Statistical Frame of Mind in Systematic Biology from *Quantitative Zoology* to *Biometry*," *JHB* 36, no. 2 (2003): 353–84.

48. George Gaylord Simpson and Anne Roe, *Quantitative Zoology: Numerical Concepts and Methods in the Study of Recent and Fossil Animals* (New York, London: McGraw-Hill, 1939); on quantification in paleontology, see Léo F. Laporte, *George Gaylord Simpson: Paleontologist and Evolutionist* (New York: Columbia University Press, 2000), chap. 3.

49. Hagen, "Statistical Frame" (ref. 47), 358.

50. George Gaylord Simpson to Alan A. Boyden, 30 Oct 1944, George Gaylord Simpson Papers, American Philosophical Society, Philadelphia, Ms. Coll. 31, Series I.

51. George Gaylord Simpson to Alan A. Boyden, 23 Jan 1947, APS Archives. In addition, of course, Simpson, having the fossil record at his disposal, was on a safer ground to make claims about homologies than Boyden and any other systematists working only on present forms.

in the one case and of taxonomic characters in the other."[52] Similarly, the botanist William B. Turrill from Kew Gardens, himself an experimental taxonomist like Boyden, claimed as late as 1957 that "classifying is never entirely objective since the peculiarities and particularities of the human mind and of the individual taxonomist impose subjective elements on the result." More importantly, Turrill was unsure whether the lack of objectivity was even a problem for taxonomy: "How far the subjective element can be eliminated or controlled, or even how far it is desirable to attempt so to treat it, is debatable."[53] Boyden, on the other hand, having firmly adopted the objectivity ideal of the experimentalist, sought to eliminate the subjectivity involved in the choice and weighting of characters by relying on a single one: the immunological affinity of blood. He justified this decision based on the fact that this single biochemical trait, unlike morphological ones, was immune from environmental and developmental influences, and was thus singlehandedly sufficient to measure the genetic relationships between species.[54]

In his early work, Boyden took a more conciliatory view towards morphologists, and recognized that serology and morphology should be used in a "complementary manner as a check on morphological findings."[55] He felt that it would prove most useful in the classification of certain branches of the animal kingdom, especially invertebrates, where morphology had not yet succeeded in providing many answers. After having shared his enthusiasm about serological taxonomy with George Gaylord Simpson, the latter replied: "Naturally, I would make the reservation (as you also state) that its data are only to be added to, not to replace, those of morphological research."[56] Boyden was hopeful, however, that in the long run, serology would not be "limited to the confirming of previous morphological analysis of relationships," but would make contributions of its own. Indeed, in the field of crustacean systematics, for example, Boyden was able to propose an original classification based on his serological method, a fact recognized even by Mayr in 1953.[57]

52. Ernst Mayr, E. Gorton Linsley, and Robert L. Usinger, eds., *Methods and Principles of Systematic Zoology* (New York: McGraw-Hill, 1953), 106–07.

53. William Bertram Turrill, "The Subjective Element in Plant Taxonomy," *Bulletin du Jardin Botanique de l'État à Bruxelles* 27, no. 1 (1957): 1–8, on 8.

54. Boyden, "Systematic Serology" (ref. 23), 116.

55. Boyden, "Precipitins and Phylogeny" (ref. 33), 516.

56. George Gaylord Simpson to Alan A. Boyden, 21 Sep 1936, APS Archives.

57. Boyden, "Serology and Animal Systematics" (ref. 35); Mayr, *Methods and Principles* (ref. 52), 113.

The usefulness of classifications based on serological relations went beyond solving certain taxonomic problems of interest only to naturalists. For Boyden, a sound taxonomy was essential for all of experimental biology. Because so many species existed in nature (about one million, he estimated), it was impossible to perform "the same experiments on all of them." But this would be unnecessary once a natural classification was established which would make it possible to "generalize effectively about related or essentially similar organisms and eliminate the need for countless repetition of the same experiments."[58] Boyden was positioned ideally to make this point, being himself an experimentalist, and his argument resonated with concurrent efforts of systematists to regain prestige with regard to the experimental sciences. Indeed, between the 1940s and the 1960s, systematists such as Ernst Mayr, George Gaylord Simpson, and Richard E. Blackwelder took a number of steps, intellectual and institutional, toward "upgrading, improving, scientizing" their discipline.[59] They rethought the links between systematics, evolutionary theory, and population genetics (the "new systematics"), they introduced new kinds of empirical data (especially from experimental genetics, serology, cytology, and ecology), they created the Society of Systematic Zoology in 1947 and, in 1952, they launched the journal *Systematic Zoology*.[60]

Boyden's attempt to build a discipline of serological zoology was largely unsuccessful until after World War II. He remained the leader of the field, but a lonely one.[61] Numerous researchers, especially in Germany, used serological techniques

58. Boyden, "Systematic Serology" (ref. 23), 120.

59. Keith Vernon, "Desperately Seeking Status—Evolutionary Systematics and the Taxonomists Search for Respectability 1940–60," *British Journal for the History of Science* 26, no. 89 (1993): 207–227, on 209.

60. Even though they shared a common goal, there were sharp intellectual divisions among systematists, especially between traditional taxonomists and "new systematists." See David L. Hull, *Science as a Process* (Chicago: University of Chicago Press, 1988); and Joe Cain, "Launching the Society of Systematic Zoology in 1947," in *Milestones in Systematics*, ed. David M. Williams and Peter L. Forey (Boca Raton, FL: CRC Press, 2004), 19–48. The later development of numerical taxonomy is not considered here, but can be seen to be in continuity with this movement. See Keith Vernon, "The Founding of Numerical Taxonomy," *British Journal for the History of Science* 21, no. 69 (1988): 143–59; Joel B. Hagen, "The Introduction of Computers into Systematic Research in the United States During the 1960s," *SHPBBS* 32, no. 2 (2001): 291–314.

61. In the United States, until 1945, most of the systematic serologists working on animal taxonomy were colleagues of Boyden at Rutgers, most importantly Charles A. Leone, but also Ralph J. DeFalco and Douglas G. Gemeroy. For some exceptions, see the work of Raymond W. Wilhelmi at Woods Hole, who insisted like Boyden on the objectivity of the serological data. Raymond W. Wilhelmi, "Serological Relationships between the Mollusca and Other Invertebrates," *Biological Bulletin* 87, no. 1 (1944): 96–105. See also the work of Russell W. Cumley at the University of Texas. Russell W. Cumley, "Comparison of Serologic and Taxonomic Relationships

for taxonomic purposes, but mostly for only a limited period of time. The fact that Boyden was so feisty in reviewing their work, especially when it did not live up to his high technical standards, might have discouraged some from pursuing the matter further. Morphologists, unsurprisingly, remained unmoved by the serological approach, at least until the postwar period. By that time, the main systematists of the day, such as George Gaylord Simpson and Ernst Mayr, mentioned Boyden's work and the serological approach generally approvingly, if only in passing.[62] They praised the use of physiological characters, including serum, proteins, and other biochemical components, as useful complements to the analysis of morphological characters, such as measurements of bone lengths, but never as a replacement for the morphological approach.[63]

A MUSEUM IN A LABORATORY

After the war, Boyden embarked on a more ambitious plan to develop the field by creating, in 1948, a Serological Museum at Rutgers University and publishing

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of Drosophila Species," *Journal of the New York Entomological Society* 48, no. 3 (1940): 265–74. In the 1930s, serology seemed to have been more popular, especially in Germany. On the German context, in the field of plant taxonomy, see for example Carl Mez, "Die Bedeutung der Sero–Diagnostik für die stammesgeschichtliche Forschung," *Botanisches Archiv* 16 (1926): 1–23, and for a general review of serological taxonomy and phylogeny, see Albert Erhardt, "Die Verwandtschaftsbestimmungen Mittels der Immunitätsreaktionen in der Zoologie und ihr Wert für phylogenetische Untersuchungen," *Ergebnisse und Fortschritte der Zoologie* 7 (1931): 279–377. In the United States, see K. Starr Chester, "A Critique of Plant Serology, Part I: The Nature and Utilization of Phytoserological Procedures," *Quarterly Review of Biology* 12, no. 1 (1937): 19–64; K. Starr Chester, "A Critique of Plant Serology (Continued), Part II: Application of Serology to the Classification of Plants and the Identification of Plant Products," *Quarterly Review of Biology* 12, no. 2 (1937): 165–90; K. Starr Chester, "A Critique of Plant Serology (Concluded); Part III: Phytoserology in Medicine and General Biology Bibliography," *Quarterly Review of Biology* 12, no. 3 (1937): 294–321.

62. Boyden's work is not mentioned in Julian Huxley, ed., *The New Systematics* (Oxford: The Clarendon Press, 1940), nor in Ernst Mayr, *Systematics and the Origin of Species from the Viewpoint of a Zoologist* (New York: Columbia University Press, 1942) or George Gaylord Simpson, "The Principles of Classification and a Classification of Mammals," *Bulletin of the American Museum of Natural History* 85 (1945): 1–307. On the other hand, it is mentioned in passing in Mayr, *Methods and Principles* (ref. 52), and in George Gaylord Simpson, *Principles of Animal Taxonomy* (New York: Columbia University Press, 1961) and, on the other side of the systematist's community, in Richard E. Blackwelder, *Taxonomy: A Text and Reference Book* (New York: Wiley, 1967). On the debates between Blackwelder and Simpson, see Cain, "Launching the Society" (ref. 60).

63. Mayr, *Methods and Principles* (ref. 52), 110–14; George Gaylord Simpson, *Animal Taxonomy* (ref. 62), 73.

The Serological Museum Bulletin.[64] The move was institutionally unusual, to say the least. The boundaries between natural history and experimental biology, or, to put it differently, between the museum and the laboratory, had become increasingly blurred in the interwar period.[65] However, this had occurred primarily as a result of the burgeoning practice of field experimentation, or the creation of laboratories in natural history museums, not the creation of museums in laboratories. In 1928, for example, the AMNH had founded a Department of Experimental Biology to be headed by G. Kinsley Noble,[66] an initiative that had been supported by "the younger members" of the AMNH board of directors.[67] Naturalists often recognized the value of experimental methods, for social as well as intellectual reasons, but experimentalists rarely acknowledged those of natural history.

The creation of Boyden's Serological Museum within the Zoological Laboratory at Rutgers is a revealing and unusual case of how natural historical methods made their way into the laboratory. As a reporter for *Science* put it, the new institution represented a "unique kind of museum."[68] Its purpose was very similar to that of most natural history museums, and Boyden underlined this similarity. He insisted that proteins of the bodies of organisms were just as worthy of preservation and conservation as their "skins and skeletons," because they were just as characteristic.[69] The Serological Museum would complement other natural history museums which typically kept only the "innermost insides and outermost outsides" of animals, i.e., skeletons and skins, by preserving the

64. Anonymous, "Serological Museum of Rutgers University," *Nature* 161, no. 4090 (1948): 428; Anonymous, "New and Unique Kind of Museum," *Science* 107, no. 2774 (1948): 217.

65. For late nineteenth-century examples of continuities between laboratories and museums in Britain, see Alison Kraft and Samuel J. M. M. Alberti, "'Equal Though Different': Laboratories, Museums and the Institutional Development of Biology in Late-Victorian Northern England," *SHPBBS* 34, no. 2 (2003): 203–36; Sophie Forgan, "The Architecture of Display—Museums, Universities and Objects in 19th-Century Britain," *History of Science* 32, no. 96 (1994): 139–62.

66. On this episode, see Gregg Mitman and Richard W. Burkhardt Jr, "Struggling for Identity: The Study of Animal Behavior in America, 1930–1950," in *The American Expansion of Biology*, ed. Keith R. Benson, Ronald Rainger, and Jane Maienschein (New Brunswick: Rutgers University Press, 1987); Joseph Allen Cain, "Common Problems and Cooperative Solutions—Organizational Activity in Evolutionary Studies, 1936–1947," *Isis* 84, no. 1 (1993): 1–25, on 20.

67. American Museum of Natural History, *Sixtieth Annual Report of American Museum of Natural History* (New York: The Museum, 1929), 13; Noble and Boyden later collaborated on a serological study of amphibians, Noble's specialty. Alan Boyden and G. Kingsley Noble, "The Relationships of Some Common Amphibia as Determined by Serological Study," *American Museum Novitates* 606 (1933): 1–24.

68. Anonymous, "New and Unique" (ref. 64).

69. Ibid.

FIG. 3 Alan A. Boyden (left) and his collaborator Charles A. Leone (right) showing blood samples to a university official in the serological museum, 1951. *Source:* Rutgers Archives, Faculty Bio.

"chemical compounds that keep their life-processes going."[70] Boyden did not need the "great halls and showcases" of the traditional natural history museum in order to exhibit his objects of study, since "bottles of sera look much like each other"; what he needed were "adequate cold rooms for the preservation of these sera."[71] A Rockefeller Foundation grant made possible the construction of such a room in 1951, in a former coal bin.[72] (Fig. 3)

Like most natural history museums, Boyden's new institution meant to collect, classify, preserve, share, and study serum samples. And like most natural history collections, it was driven by the ideal of "comprehensiveness." The principal objective of the museum was to "build up by collection and exchange samples of as many kinds of protein as are obtainable from all kinds of organisms,

70. Anonymous, "Blood Proteins to be Kept in New, Special Museum," *Science News Letter* (12 Jun 1948): 377.

71. Alan A. Boyden, "Introductory Remarks," in *Serological and Biochemical Comparisons of Proteins*, ed. William H. Cole (New Brunswick: Rutgers University Press, 1958), 1.

72. George H. Holsten (Rutgers News Service), Press Release, 6–7 Apr 1951, Rutgers Archives, RG 04/A16, Box 101; and Anon, "Rutgers Unit Gets $5000," *NYT*, 7 Apr 1951.

young and old, healthy and diseased."[73] By 1950, the collection already held blood samples from more than four hundred species, and Boyden was hoping to receive and collect many more.[74] More than the sheer quantity of specimens, it was the completeness of the collection that mattered most to Boyden. When asked by a reporter, in 1960, how many samples he possessed, Boyden replied "we don't count," but estimated the number in the thousands. He summarized his approach by noting that he wanted to collect anything he could "get a hold of."[75] The most precious specimens were those that completed some aspect of the collection. In 1950, for example, as Rutgers University's press service proudly announced, the museum had received in a "securely-wrapped package, flown in by air express" the last specimen of blood serum that Boyden needed to complete the Museum's collection of sera from all the eighteen orders of mammals.[76] Indeed, blood from a flying lemur (which doesn't fly and isn't a lemur) had just been sent to Boyden from Madagascar. As the orders of mammal were completed, Boyden aimed for his next goal, namely, to "sample all of the 118 families which divide the eighteen orders."[77]

These blood samples, which looked all alike, were not for display, but to be used in serological experiments. Boyden envisioned his museum as a place for study, reflecting the Early Modern usage of the word as equivalent to the "studio."[78] Every new blood sample that was added to the collection made numerous new experiments possible, because it could be tested against all the previously collected samples. Blood from a new species thus created an entire new set of relationships within the collection, and potentially challenged the established relationships between the samples. In the comparative perspective, every additional element, instead of bringing the collection closer to completion, actually increased the number of possible comparisons which could be made. The collection was a tool to produce experimental knowledge about taxonomy, and the researchers used it in the same way that naturalists used collections in natural history museums, except that they made experiments, rather than visual comparisons.

Boyden's serological museum shared another characteristic of natural history museums: it was embedded in a complex network of collecting

73. Alan A. Boyden, *SMB* 1 (1948), 1.

74. Alan A. Boyden, *SMB* 5 (1950), 1.

75. Anonymous, "Plan to Lecture, 'Fish' Abroad," *Sunday Home News*, 4 Sep 1960.

76. Anonymous, "Triple Play," Jan 1950, RA, Newspaper Clippings, Folder Faculty Bio.

77. Ibid.

78. Boyden, "Introductory Remarks" (ref. 71), 1; Findlen, *Possessing Nature* (ref. 5), 48.

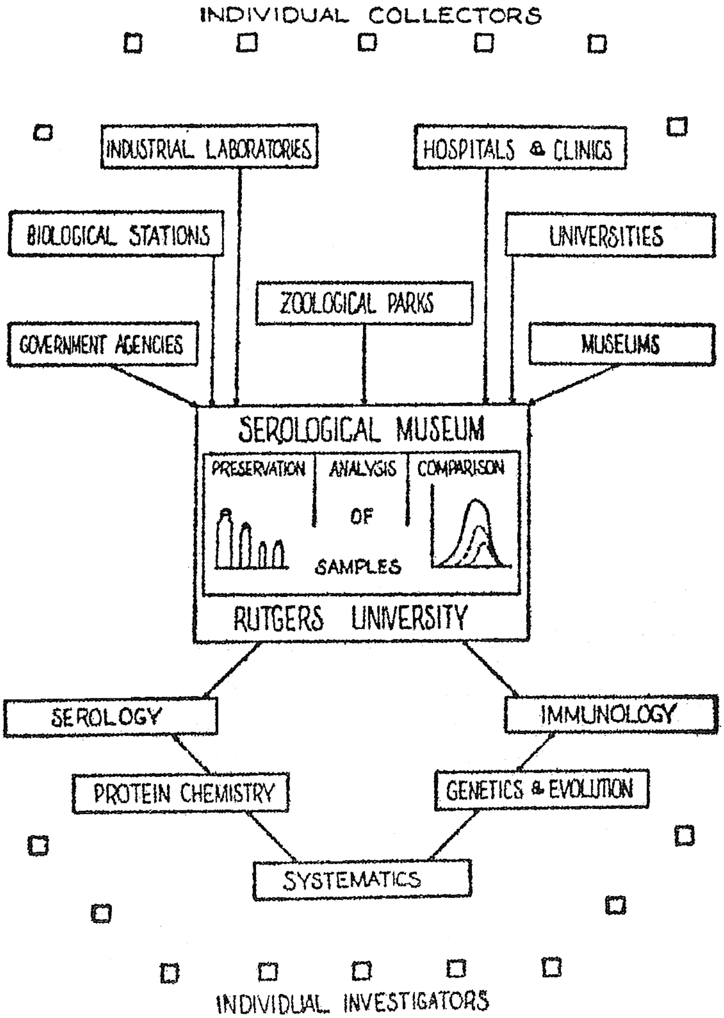

FIG. 4 Illustration representing the network of relationships between the Serological Museum and other institutions. *Source*: *SMB* 4 (1950), 1.

institutions.[79] (Fig. 4) Boyden had begun his serum collection with domestic animals—sheep, pig, cow, horse, and dog—which could be found in rural New Jersey, around Rutgers University. However, because these posed little challenge for the taxonomist or the evolutionary biologist, he quickly moved

79. See for example Endersby, *Imperial Nature* (ref. 5); and Richard W. Burkhardt Jr, "Naturalists' Practices and Nature's Empire: Paris and the Platypus, 1815–1833," *Pacific Science* 55 (2001): 327–41.

to expand the scope of his collection to include a more diverse range of species. For example, he collected blood of numerous organisms from zoos and marine stations from across the United States, from the New York Zoological Park to the San Diego Zoo, and from the Mount Desert Island Biological Laboratories in Maine to the Marine Biological Laboratory of the Carnegie Institution in Florida.[80] Aardvarks, anoas, giant pangolins, and warthogs were some of the more unusual species whose blood Boyden obtained from the San Diego Zoo for his museum.[81] Federal agencies, such as the U.S. Fish and Wildlife Service and the U.S. Biological Survey, provided samples of wild species living in the United States, such as the bison and the elk. Natural history museums, such as the American Museum of Natural History in New York, were also a rich source of blood samples. Boyden asked George Gaylord Simpson at the AMNH that "whenever expeditions are sent out . . . due consideration . . . be given to the matter of blood samples."[82]

The similarity of Boyden's effort with the operation of natural history museums also extends to one of the latter's most distinctive features, namely its reliance on naturalist collectors in the field. When Boyden inaugurated his museum, he invited the cooperation of "all naturalists, wherever they might be [and who] may be in a position to collect and contribute or exchange samples." He made clear that the very success of the museum depended "on the extent of the cooperative effort" of many individuals.[83] A few years after the museum opened, Boyden renewed his call in the journal *Science*, where he detailed how blood samples should be collected in the field. He appealed specifically to all those who were planning zoological collecting expeditions in remote regions. Boyden was aware that most of these naturalists would be "unequipped for refined serological collecting" and "inexperienced in the standard procedures."[84] He thus described a simple method, involving paper soaked with blood from wounds or from the "animal's carcass as it is being skinned." Additional amounts of blood could be obtained through "cutting open the heart and major vessels." For small animals, Boyden would be content with a blood spot "no larger than a matchhead," but for larger ones, "a square foot or more" of

80. Anonymous, "New and Unique" (ref. 64).

81. Alan Boyden, "Fifty Years of Systematic Serology," *Systematic Zoology* 2, no. 1 (1953): 19–30, on 30.

82. Alan A. Boyden to George Gaylord Simpson, 13 Nov 1947, APS Archives.

83. Anonymous, "New and Unique" (ref. 64).

84. Alan Boyden, "Zoological Collecting Expeditions and the Salvage of Animal Bloods for Comparative Serology," *Science* 118, no. 3054 (1953): 57–58, on 57.

blood-soaked paper was desirable. The blood samples should then be "hung up in the shade" to dry, and "carefully shielded from visitations by insects." Like the naturalist collectors before him,[85] Boyden made sure to specify that each sample should come with the scientific name of the organism from which it was collected, a date, a locality, and the name of the collector.

Convincing animal collectors on hunting expeditions to sample blood from their game was no simple task. Provided their collaboration could be secured, the requirements of serum collecting could conflict with those of (safe) hunting in the wild. In 1950, the *Serological Museum Bulletin* described in great detail how hunting practices in East Africa would be affected by the necessities of serum collecting.[86] The author began by noting that, quite obviously, it was almost always necessary to kill the animals in order to be able to bleed them.[87] Unfortunately, hunters usually aimed for the game's shoulder, leading to massive hemorrhage and blood clot in the chest cavity. Sadly, when hunters then approached their dead prey, no flowing blood could be obtained by "cutting the throat."[88] The author thus encouraged hunters to achieve "a brain shot" or shoot "in the neck across the cervical vertebrae," but recognized that this might be quite challenging, and dangerous, for big game.[89] Less dangerous, but no less challenging, was the collection of blood from marine animals, such as lobsters, fishes, mollusks, or turtles. (Fig. 5) The *Serological Museum Bulletin* published a detailed discussion about the different techniques, such as "cardiac puncture," for bleeding these animals, hoping to educate and enroll some naturalists embarked on sea expeditions in the collecting of serum for the museum.[90]

The moral economy on which Boyden's collecting enterprise rested was one of a gift relationship, by which individual collectors contributed samples to a public facility, which in turn offered services and help to collectors and institutions "in many parts of the world."[91] Even before the museum was created, Boyden often acknowledged in print his debts to individuals for "the loan or gift of specimens," and the museum simply institutionalized this practice.[92] To those

85. On the disciplining of individual collectors, see Endersby, *Imperial Nature* (ref. 5), chap. 8.

86. Bernard Weitz, "The Collection of Serum from Game in East Africa," *SMB* 4 (1950), 1–3.

87. Ibid.

88. Ibid.

89. Ibid.

90. Anonymous, "Recent Advances in the Collection of Serum from Marine Animals," *SMB* 5, (1950), 3–4 on 3.

91. Boyden, "Zoological Collecting" (ref. 84), 58.

92. Boyden, "Serology and Animal Systematics" (ref. 35), 242.

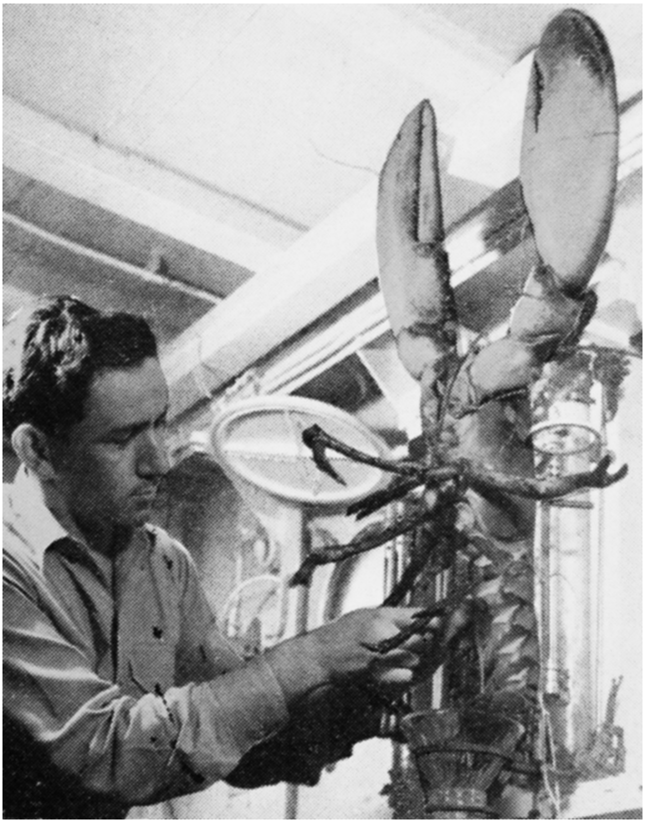

FIG. 5 Technique for bleeding a lobster. *Source*: *SMB* 5 (1950), 3.

who collaborated with the museum, he made clear that in return the *Serological Museum Bulletin*, which reported the results of the research carried out at the museum, would be distributed free of charge and that credit to the collectors would be given through acknowledgments in all the scientific reports making use of their contributions.[93] The museum's dependence on an extended network of individuals and institutions meant that it also had to work as a service institution. Boyden insisted that the museum could not "receive only and not give in return."[94] It would make "a virtue of necessity" and "serve others" so that the museum may "be served in turn."[95] The museum did ship blood to institutions and researchers; however, these shipments were mainly domestic

93. Boyden, "Zoological Collecting" (ref. 84), 58.
94. Alan A. Boyden, "The 'Flying Lemur' Comes to the Museum," *SMB* 4 (1950): 4–5, on 4.
95. Alan A. Boyden, "Major Objectives," *SMB* 4 (1950): 1.

and rather infrequent. Boyden donated, for example, a small amount of sera from two species of turtles to researchers in New York who showed by electrophoresis that it did not contain albumin like human blood does.[96] On the other hand, the museum received blood from all around the world. "Cooperating institutions" were spread throughout the five continents, and included zoological museums and universities in Europe and in North America, and colonial medical research institutions in Africa, Asia, and Oceania.[97] In the museum, a large world map hung on the wall, with radiating lines from the location of the museum in New Brunswick, New Jersey, to all the places where samples had been collected, showing the extent of Boyden's serological empire.

The cooperative and service ethos that Boyden emphasized for his museum—and for science in general—contrasted sharply with the individualist and increasingly competitive ethos prevalent in the experimental sciences at that time. A good example of the latter was the bitter priority dispute between Boyden's colleague at Rutgers University, the microbiologist Selman Waksman, and his graduate student, Albert Schatz, over the discovery of streptomycin, a rivalry that went so far as to involve a lawsuit.[98] True, the experimental life sciences had also known their cooperative moments, for example in Thomas H. Morgan's "fly group" in the 1920s and 1930s, and Max Delbrück's "phage group" in the 1940s and 1950s, but these were becoming increasingly rare in the postwar economy of science.[99] For Boyden, cooperation was not simply a practical necessity for the management of his museum, but a commitment to certain altruistic values. In 1943, for example, he complained to the university's president that he was "deeply concerned … with the character of [the] students."[100] What bothered him most was their "complete indifference to the alarming international situation" and their "supreme selfishness." They seemed "to feel no debt to society," Boyden remarked. For him, the natural biological

96. Elias Cohen and Gunnar B. Stickler, "Absence of Albuminlike Serum Proteins in Turtles," *Science* 127 (1958): 1392.

97. Alan A. Boyden, "Our Contacts Grow," *SMB* 3 (1949): 1.

98. William Kingston, "Streptomycin, *Schatz v. Waksman*, and the Balance of Credit for Discovery," *Journal of the History of Medicine and Allied Sciences* 59, no. 3 (2004): 441–62.

99. On the fly group, see Kohler, *Lords of the Fly* (ref. 6); on the phage group and more generally on the cooperative individualism fostered by the Rockefeller Foundation at Caltech, see Lily E. Kay, *The Molecular Vision of Life: Caltech, the Rockefeller Foundation and the Rise of the New Biology* (New York: Oxford University Press, 1993).

100. Alan A. Boyden to Robert C. Clothier, 21 Jul 1943, and Alan A. Boyden, "A Plan for the Training of American Youth in Responsible Citizenship," attached to Alan A. Boyden to Robert C. Clothier, 2 Oct 1943, Rutgers Archives, RG 04/A14, Box 85.

order demonstrated the perils of this attitude: "Two great factors have operated to make animal life a success, i.e., competition and cooperation [and] as we go up the animal scale the latter becomes more and more important." Boyden had thus tried to teach his students "the ideal of cooperation [and] the biological fallacy of isolationism." These views about the social and moral orders found institutional expression in the Serological Museum. It was true, as Boyden insisted, that the museum relied for both its existence and its work upon the principle of cooperation, but Boyden also saw cooperative scientific practices to have a broader moral and political meaning in the Cold War world. For Boyden, the willingness of individual scientists worldwide to donate sera to the museum was "proof that, among biologists of many lands, there can be whole-hearted and friendly cooperation" and that in "these times—in any times—it is well to have occasion to reaffirm our faith in men and their capacity for mutual aid."[101] The collective dimension of museum work, as opposed to the individual dimension of laboratory work, positioned the Serological Museum both epistemically and politically within the Cold War economy of science.

Occasionally, the museum publicized the contributions of individual collectors. In 1950 for example, the *New York Times* ran a story entitled "Woman Scientist Brings Rare Blood."[102] Boyden was interviewed while he was waiting for an Australian serologist at the airport in Newark. When she arrived, she was carrying "precious samples in a brown bag under her arm." The package contained blood samples from "twenty little-known animals" native to an island in the Pacific Ocean near the South Pole, including penguins, seals, sea lions, and sea elephants. This story offered an unusual mix of narrative tropes, describing the sanitized world of the laboratory and the adventurous world of the field, the high technology of the experimental sciences, and the paper technology of natural history. This hybrid perspective became a distinctive trait of many subsequent stories about the Serological Museum.[103] In the late 1950s, the *New York Times* reported on the "Jungle Quest" planned by Boyden to collect rare blood specimens in Central America, and the "Serum Hunt" of the museum for blood from extraordinary organisms, such as the elephant shrew or the coelacanth.[104] These stories alluded to the dangers of such expeditions,

101. Boyden, "'Flying Lemur'" (ref. 94).

102. Anonymous, "Woman Scientist Brings Rare Blood," *NYT*, 6 May 1950.

103. See for example Benjamin Adelman, "Blood Museum Traces Animal Cousins," *Science Digest* 30 (1951): 6–8.

104. Anonymous, "Jungle Quest Planned," *NYT*, 22 Jul 1957; Anonymous, "Museum at Rutgers Hunts for Serum," *NYT*, 16 Feb 1958.

including an account of how Boyden brought back blood samples from the "deadly Fer-de-lance snake" on which he "nearly stepped while walking though the jungle in Panama."[105] To be sure, these stories belonged more to the narrative genre of adventurous natural history expeditions than to that of white-coated laboratory scientists, such as Sinclair Lewis's *Arrowsmith*.[106]

The Serological Museum also emphasized its contribution to solving practical problems. For example, by using his vast collection of sera to analyze blood from an African malaria-carrying mosquito, Boyden was able to trace the origin of the disease to an antelope rather than the rodent, which was previously thought to be its source. This discovery, Boyden claimed, would open up "a complete new line of attack" against the disease.[107] The Serological Museum also contributed to helping "crime fighters," and especially the FBI, by providing a method to determine the difference between human and chimpanzee blood (even though it is unclear how often suspect lists included individuals from both species).[108] In that respect, too, the Serological Museum represented a contribution to Nuttall's enterprise, with its dual focus on taxonomy and legal medicine. At every possible juncture, Boyden emphasized his connection to Nuttall, such as when he organized a Nuttall Memorial Celebration at the Serological Museum in 1951 to mark the fiftieth anniversary of Nuttall's first use of the precipitin reaction.[109] At that occasion, he brought back a relic most precious in his eyes, dried blood samples from Nuttall's original collection.[110] Boyden retired from Rutgers University in 1962, but when his successor at the Serological Museum, Ralph J. DeFalco, died prematurely in 1971, Boyden returned from retirement to head once again the museum he had founded.[111] By that time, however, other methods to "measure species," as Nuttall had put it in 1904, were making the Serological Museum increasingly obsolete.[112] In particular, protein sequences were becoming the most widely used molecular data to establish taxonomies and especially phylogenies. Much like those who had championed serological data, the new molecular evolutionists heralded protein

105. Anonymous, "Bloody Business," *Hartford Courant*, 12 Apr 1958.

106. Charles E. Rosenberg, *No Other Gods: On Science and American Social Thought* (Baltimore: Johns Hopkins University Press, 1997), chap. 7.

107. Anonymous, "Blood Study Aids Fight on Disease," *NYT*, 27 Jul 1952.

108. Anonymous, "Tell Blood Differences," *Science News Letter*, 20 Jun 1953: 386.

109. Alan Boyden, "Nuttall Memorial Celebration," *Science* 114, no. 2954 (1951): 162.

110. Alan A. Boyden to Robert C. Clothier, 23 Oct 1950, Rutgers Archives, RG 04/A14, Box 84.

111. Anonymous, "This Museum Banks on Blood," *Home News*, 2 Mar 1971.

112. Serological systematics did not completely disappear and continued to play an important role in some fields, especially in primate evolution. See Hagen, "From Immunodiffusion" (ref. 13).

sequences as being quantitative and objective, and representing a major improvement over morphological data.[113] And like Boyden's blood samples, protein sequences from a broad range of species were being assembled into increasingly large collections, following once again this key practice of the natural historical way of knowing.[114]

CONCLUSIONS

In order to gain a deeper historical understanding of the profound transformation of the life sciences that took place in the twentieth century, including the rise of molecular biology and the current successes of genomics, we must look beyond the technological innovations of the experimental sciences. There is no doubt that genomics, for example, relies crucially on new experimental and computer technologies, but it also borrows from much older epistemic and material practices, and especially from natural history. Moreover, it does so to an extent that makes these practices perhaps more fundamentally important to historicize the broad transformation of the life sciences in the twentieth century.

If we are to understand these borrowings, it is essential to examine the places where experimentalism and natural history interacted in that period. This paper has focused on just one of these places, examining how biological diversity came to be collected and studied at the biochemical level in the laboratory. It has shown some of the ways in which blood samples of domestic, but also wild, animals made their way into laboratories in the first half of the twentieth century. The historiography of the experimental life sciences, which has emphasized the growing importance of model organisms in the twentieth century, should not overlook equally important laboratory research focusing on comparisons among much more diverse species, following a key epistemic practice of the natural history tradition. By looking at how scientific practices centered on comparison, so essential in natural history, have been put to work not only in museums but also in laboratories throughout the twentieth century, we can

113. Marianne Sommer, "History in the Gene: Negotiations between Molecular and Organismal Anthropology," *JHB* 41, no. 3 (2008): 473–528; Edna Suárez-Díaz and Victor H. Anaya-Muñoz, "History, Objectivity, and the Construction of Molecular Phylogenies," *SHPBBS* 39, no. 4 (2008): 451–68.

114. On protein collections, see Strasser, "Collecting and Experimenting" (ref. 4) and Strasser, "Collecting, Comparing and Computing" (ref. 4); and on DNA collections, Strasser, "GenBank" (ref. 4).

draw new continuities which give historical depth to some of the most recent transformations in the life sciences, such as the rise of genomics.

The researchers examined here valued experimentation and collection, model organisms and biodiversity, the laboratory and the museum. They represent some examples of a growing convergence between the methods of natural history and those of the experimental sciences, of which contemporary genomics is perhaps the most recent outcome. Understanding the rise of this hybrid culture, articulating the natural historical and the experimental ways of knowing, gives new insights into the transformation of the life sciences in the twentieth century. The narrative of a clash between molecular and organismic biologists, or between the experimental sciences and natural history—a narrative produced in many cases by scientists themselves and adopted by their historians—is an unsatisfying oversimplification.[115] The debates over the value of experimentation did not simply oppose scientists in laboratories and naturalists in museums, but took place among those interested in understanding the history and diversity of life.[116]

At first, it might be tempting to understand the stories of Nuttall, Reichert, and Boyden as epitomizing the experimentalists' invasion of the naturalists' territory. They certainly occupied natural history's intellectual territory, addressing issues of taxonomy and phylogeny with the tools of experimentation, while at the same time importing values from experimentalism, such as objectivity, measurement, and precision. However, their epistemic practices remained largely those of naturalists in that they applied the techniques of collecting and comparing, albeit to experimental data instead of bones and fossils. For that reason, these developments are better understood as the emergence of a "hybrid culture" rather than of the domination of one culture over the other. In the early 1960s, molecular biologists and biochemists bragged about their taking over the study of evolution from supposedly antiquated morphology-minded

115. In reaction to the perceived invasion of their territory by experimentalists, naturalists called for a peace treaty and the recognition of the "unity" of biology around evolutionary theory; see for example Theodosius Dobzhansky, "Taxonomy, Molecular Biology, and the Peck Order," *Evolution* 15, no. 2 (1961): 263–64. Experimentalists, on the other hand, understood "unity" as the ultimate outcome of their reductionist agenda, and insisted that every biological process was ultimately to be explained at the molecular level. On this debate, see Michael R. Dietrich, "Paradox and Persuasion: Negotiating the Place of Molecular Evolution within Evolutionary Biology," *JHB* 31 (1998): 85–111.

116. For a structurally similar argument concerning clinicians and basic scientists in the late nineteenth century, see John Harley Warner, "Ideals of Science and Their Discontents in Late Nineteenth-Century American Medicine," *Isis* 82 (1991): 454–78.

naturalists, bringing experimental methods such as protein sequencing to bear on the problems of phylogeny. Yet by the 1960s, the field had already been progressively transformed through the adoption of experimental methods and values such as Boyden's serological taxonomy.[117] There is another reason, not explored in this paper, why the simple "invasion narrative" is inadequate. The biochemists and molecular biologists who boasted of the cultural and epistemic superiority of experimentalism over natural history had often unknowingly adopted parts of the natural historical way of knowing in their experimental endeavors. For example, experimental studies on the relationships between protein structure and function, a critical component of biochemistry and molecular biology since the 1950s, relied heavily on the collection and comparison of data across species.[118] In short, natural history became more experimental, and experimentalism more natural historical in the twentieth century. This process is thus better described as the progressive creation of an epistemic and cultural hybrid—vigorous, but also exceptionally fertile.

Boyden's systematic serology fits squarely into what Joel B. Hagen has called "experimental taxonomy," or the diverse attempts of researchers, beginning in the 1920s, to bring experimental ecology, genetics, and cytology to bear on questions of taxonomy.[119] However, Boyden interacted more often with traditional systematists, such as Simpson, and serologists than with other experimental systematists. This lack of interaction is not so surprising given that experimental taxonomy never became an autonomous discipline, but grew (in the United States) essentially as an informal network around the San Francisco Bay Area, on the opposite side of the country from Boyden's New Jersey laboratory (and museum); also, most experimental taxonomists worked in botany, whereas Boyden focused on zoology.[120] More broadly, this mutual isolation reflects the fact that experimental taxonomists rarely perceived the intellectual unity of their field, and instead identified themselves with the particular

117. The episodes described by Robert E. Kohler and by Joel B. Hagen are thus part of a much broader transformation of natural history in the twentieth century, and which remains to be studied; see Kohler, *Landscapes and Labscapes* (ref. 2); Joel B. Hagen, "Experimental Taxonomy, 1920–1950: The Impact of Cytology, Ecology, and Genetics on the Ideas of Biological Classification" (PhD dissertation, Oregon State University, 1984).

118. Strasser, "Collecting, Comparing, and Computing" (ref. 4).

119. Hagen, "Experimentalists and Naturalists" (ref. 2).

120. Ibid., 260. In the first half of the twentieth century, the biological sciences were still strongly divided among botany, zoology, microbiology, and anthropology, with often little communication between these areas. I thank Joel B. Hagen for pointing out this distinction to me, and on this point, see Hagen, "Statistical Frame" (ref. 47).

experimental methods—reciprocal transplants, genetic crossings, cytological observations, serological tests—they applied to taxonomic problems.

But the point to emphasize here is that even though Boyden had been trained as an experimentalist, used experimental methods, and performed experiments in a laboratory, his professional identity was that of a natural historian. In a letter to the president of his university, he claimed unambiguously "Thank Heaven I belong to 'Natural History.'"[121] He understood his methods as part of a renewed morphology tradition, not of physiology or immunology.[122] Specifically, he felt he was continuing the "comparative morphology" enterprise, but at the biochemical level.[123] Boyden's intellectual and scientific biography suggests that it is more productive for the historian to reexamine the cognitive and material practices of these historical actors than to suppose that they fall necessarily on one side or the other of an imaginary experimentalist versus naturalist divide.

One of the aims behind Boyden's attempt to use serology for taxonomic purposes was to make classifications more objective.[124] For Boyden, objectivity could be attained through quantification, automation, and reduction of systematic comparisons to a single character (serum proteins). The drive for more objective methods that has pervaded the field of molecular evolution since the 1960s was thus already acutely felt in the interwar period.[125] The emphasis in the historical literature on the rise of molecular evolution since the 1960s should not obscure the longer historical trajectory: the reliance on experimentation (especially on molecules), quantification, and machines to achieve greater objectivity in areas considered to be part of natural history is a broad trend that runs throughout the twentieth century.[126] In addition to objectivity, experimentation promised to bring additional control over the natural world, especially by allowing the creation of "unnatural" phenomena, whereas naturalists were restricted to the juxtaposition of specimens and their dissection, and thus

121. Alan A. Boyden to Robert C. Clothier, 2 Jun 1944, Rutgers Archives, RG 04/A14, Box 85.

122. Boyden, "Serology and Animal Systematics" (ref. 35), 241: "Biochemical comparisons fall within the province of 'morphology.'"

123. Boyden, "Fifty Years" (ref. 81), 20.

124. For the same reason, other experimental taxonomists were turning to the field experiment. See for example Hagen, "Experimentalists and Naturalists" (ref. 2), 266.

125. Sommer, "History in the Gene" (ref. 113); Suárez-Díaz and Anaya-Muñoz, "History, Objectivity" (ref. 113).

126. Similarly, the historiography of molecular biology has sometimes obscured the broader trend of molecularization in the twentieth century; see Soraya de Chadarevian and Harmke Kamminga, eds., *Molecularizing Biology and Medicine: New Practices and Alliances, 1910s–1970s* (Amsterdam: Harwood Academic Publishers, 1998).

a more limited creation of phenomena. Serological taxonomists, for example, could examine the biological reaction produced when whale and rabbit serum were brought together, but naturalists could not hope to examine the results of a crossing of these two species. The cultural and epistemic authority of the laboratory, based on objectivity and control, did not lead to the exclusion of natural history, but to its transformation.

This trend toward greater objectivity is also what made naturalists such as Simpson, with their claim that instinct, intuition, and personal judgment were legitimate means for the production of knowledge, seem increasingly archaic within the life sciences. What had once been considered a reliable way to learn about the natural world was becoming suspect in the twentieth century. Indeed, as Lorraine Daston and Peter Galison have shown so clearly, by the late nineteenth century, objectivity and subjectivity had become opposite and mutually exclusive epistemic values.[127] Experimentalists sided with objectivity, but naturalists in the twentieth century had a more difficult time choosing their side. For example, as late as 1961, Simpson noted in his *Principles of Animal Taxonomy* that the identification of species depended "on the personal judgment of each practitioner of the art of classification." He added that classification could not be objective: "To insist on an absolute objective criterion would be to deny the facts of life, especially the inescapable fact of evolution."[128]

How and where, exactly, objectivity could be attained, was a matter of great difference among experimentalists, however. Some placed their hopes in molecules, statistics, and computers—or any combination of these—while others rethought the basic tenets of classification and its relationships to phylogeny.[129] Whereas Boyden began his career thinking that taxonomy should be based on phylogeny, from the late 1930s he increasingly believed that phylogeny and taxonomy should be independent intellectual endeavors, thus putting him at odds with the "new systematics," which grew out of the evolutionary synthesis.[130]

127. Daston and Galison, *Objectivity* (ref. 7).

128. Simpson, *Principles of Animal Taxonomy* (ref. 62), 152.

129. Hull, *Science as a Process* (ref. 60).

130. Joel B. Hagen, "Descended from Darwin? George Gaylord Simpson, Morris Goodman, and Primate Systematics," in *Descended from Darwin: Insights into the History of Evolutionary Studies, 1900–1970*, ed. Joe Cain and Michael Ruse (Philadelphia: American Philosophical Society, 2009), 106. The term "new systematics" came into common use after the publication of Julian Huxley's edited volume, Huxley, ed., *New Systematics* (ref. 62). On the evolutionary synthesis, see Vassiliki Betty Smocovitis, *Unifying Biology: The Evolutionary Synthesis and Evolutionary Biology* (Princeton: Princeton University Press, 1996). On the debate between the new systematists and traditional taxonomists in the 1950s, see Vernon, "Desperately Seeking Status" (ref. 59), especially

Boyden's reasons for this disjunction are not difficult to understand. Given the uncertainties surrounding specific phylogenies, especially those of primates, and the way subjective judgments influenced them, Boyden wished to prevent those debates from complicating his search for objective classifications.[131] If anything, the acute controversy that erupted over the phylogeny of primates in the 1960s, in part following Morris Goodman's classification using serological data,[132] must have confirmed Boyden's fears.

None of the researchers examined here—Nuttall, Reichert, Boyden—transformed systematics or evolutionary biology in their time. However, Boyden's tireless efforts to create a field of systematic serology finally bore its fruits in the early 1960s, when other researchers, such as Morris Goodman (who was trained by one of Boyden's students), Allan C. Wilson, Vincent M. Sarich, and Curtis A. Williams, for example, applied serological techniques with success to the study of primate phylogeny.[133] In the 1960s, serology, along with protein sequencing, protein fingerprinting, and DNA hybridization (championed by Ellis T. Bolton, another of Boyden's students), became part of the standard toolkit of the new molecular evolutionists.[134]

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224. Boyden's position is best stated in Richard E. Blackwelder and Alan Boyden, "The Nature of Systematics: Part I—The Classification of the Zoological Sciences," *Systematic Zoology* 1, no. 1 (1952): 26–29 and see the reply, George S. Myers, "The Nature of Systematic Biology and of a Species Description," *Systematic Zoology* 1, no. 3 (1952): 106–11, on 110–111.

131. Blackwelder and Boyden, "Nature of Systematics" (ref. 130), 32.

132. Sommer, "History in the Gene" (ref. 113); Hagen, "Descended from Darwin?" (ref. 130).

133. On the place of serology in physical anthropology, see Jonathan Marks, "The Legacy of Serological Studies in American Physical Anthropology," *History and Philosophy of the Life Science* 18 (1996): 345–62. For a good overview of systematic serology in the 1960s, see Charles A. Leone, ed., *Taxonomic Biochemistry and Serology* (New York: Ronald Press Co., 1964) and, with a focus on botany, Ralph E. Alston and B. L. Turner, *Biochemical Systematics* (Englewood Cliffs, NJ: Prentice-Hall, 1963), chap. 5. On serology's progressive marginalization among biochemical systematics in the 1970s and 1980s, see Christopher Amyas Wright, *Biochemical and Immunological Taxonomy of Animals* (London and New York: Academic Press, 1974); Andrew Ferguson, *Biochemical Systematics and Evolution* (New York: Wiley, 1980).

134. On DNA hybridization, see Edna Suárez, "Satellite-DNA: A Case Study for the Evolution of Experimental Techniques," *SHPBBS* 32, no. 1 (2001): 31–57, and more generally on the rise of molecular evolution, see Dietrich, "Paradox and Persuasion" (ref. 115); Hagen, "Naturalists, Molecular Biology" (ref. 2); Jay D. Aronson, "'Molecules and Monkeys': George Gaylord Simpson and the Challenge of Molecular Evolution," *History and Philosophy of the Life Sciences* 24, no. 3–4 (2002): 441–65; Gregory J. Morgan, "Emile Zuckerkandl, Linus Pauling, and the Molecular Evolutionary Clock, 1959–1965," *JHB* 31 (1998): 155–78; Sommer, "History in the Gene" (ref. 113); Edna Suárez-Díaz, "Molecular Evolution: Concepts and the Origin of Disciplines," *SHPBBS* 40, no. 1 (2009): 43–53.

Boyden's serological taxonomy based on the comparison of experimental data from a large number of species, like many other systematic endeavors based on the comparative perspective, required the constitution of a collection. Boyden's Serological Museum, like Linnaeus's herbarium, Cuvier's anatomical gallery, Simpson's paleontological collection, or the NIH's GenBank database, was the expression of the basic necessity of making various objects present in a single place, if they are to be compared by a researcher. If museums, rather than laboratories, have been a prime location of natural historical practice, it was thus not the result of some historical connection between disciplines and institutions, but because museums have been tools for collecting, and laboratories for isolating—museums assemble, laboratories disassemble. As soon as experimentalists, such as Boyden, adopted a broad comparative perspective and engaged with the diversity of life, they established collections or databases, precisely as so many naturalists before them had done. But in the twentieth century, when the values of experimentalism and the authority of the laboratory became dominant, these scientists were left with the delicate task of inventing institutions and practices that combined the naturalist's comparative perspective and the experimentalist's quest for objectivity.

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