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## Finding Patterns in Nature: Asa Gray's Plant Geography and Collecting Networks (1830s-1860s)

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*(Article begins on next page)*

Finding Patterns in Nature:  
Asa Gray's Plant Geography and Collecting Networks (1830s-1860s)

A dissertation presented

by

Kuang-Chi Hung

to

The Department of the History of Science

in partial fulfillment of the requirements

for the degree of

Doctor of Philosophy

in the subject of

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Finding Patterns in Nature:

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Abstract

It is well known that American botanist Asa Gray's 1859 paper on the floristic similarities between Japan and the United States was among the earliest applications of Charles Darwin's evolutionary theory in plant geography. Commonly known as Gray's "disjunction thesis," Gray's diagnosis of that previously inexplicable pattern not only provoked his famous debate with Louis Agassiz but also secured his role as the foremost advocate of Darwin and Darwinism in the United States. Making use of previously unknown archival materials, this dissertation examines the making of Gray's disjunction thesis and its relation to his collecting networks. I first point out that, as far back as the 1840s, Gray had identified remarkable "analogies" between the flora of East Asia and that of North America. By analyzing Gray and his contemporaries' "free and liberal exchange of specimens," I argue that Gray at the time was convinced that "a particular plan" existed in nature, and he considered that the floristic similarities between Japan and eastern North America manifested this plan. In the 1850s, when Gray applied himself to enumerating collections brought back by professional collectors supported by the subscription system and appointed in governmental surveying expeditions, his view of nature was then replaced by one that regarded the flora as merely "a catalogue of species." I argue that it was by

undertaking the manual labor of cataloging species and by charging subscription fees for catalogued species that Gray established his status as a metropolitan botanist and as the “mint” that produced species as a currency for transactions in botanical communities. Finally, I examine the Gray-Darwin correspondence in the 1850s and the expedition that brought Gray’s collector to Japan. I argue that Gray’s thesis cannot be considered Darwinian as historians of science have long understood the term, and that its conception was part of the United States’ scientific imperialism in East Asia. In light of recent studies focusing on the history of field sciences, this dissertation urges that a close examination of a biogeographical discovery like Gray’s thesis is impossible without considering the institutional, cultural, and material aspects that tie the closets of naturalists to the field destinations of collectors.

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A close, almost biographical study of any nineteenth-century American man or woman of science is never easy, particularly for an international student like me. Prof. Janet Browne's comprehensive knowledge of the history of evolutionary biology helped me situate my intellectual curiosity in a broader historiographical

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## INTRODUCTION

It is a specimen with the barcode 97751 (Figure I-1), one of five million specimens at the Harvard University Herbaria.<sup>1</sup> In many ways, this specimen is but a drop in the sea—a dried plant with a stem, leaves, and flowers; a drawing detailing the structure of the flower; several tags showing when and where the specimen was collected. It looks so normal, so typical, and so *dry* that it seems unworthy of historical attention. Nor do the metadata of the specimen 97751 faintly hint at a more interesting tale. The scientific name of this plant is *Heliotropium japonicum*, a member of the *Boraginaceae* family. It was collected in Hokkaido in 1855 by Charles Wright (1811–1885), naturalist in the North Pacific Exploring Expedition (1853–1856). The person who named this plant was Asa Gray (1810–1888), Fisher Professor of Natural History at Harvard University. In 1859, Gray first reported it in *Memoirs of the American Academy of Arts and Sciences*, which, in the world of taxonomy, credited him as “*the author*” of this new species.<sup>2</sup> Time, space, and authority: It appears that this specimen would reveal no other information.<sup>3</sup>

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<sup>1</sup> The image and the metadata of the specimen can be found at <http://asaweb.huh.harvard.edu:8080/databases/specimens?id=131688>

<sup>2</sup> Asa Gray, “Diagnostic Characters of New Species of Phaenogamous Plants, Collected in Japan by Charles Wright, Botanist of the U. S. North Pacific Exploring Expedition (Published by Request of Captain John Rodgers, Commander of the Expedition) with Observations upon the Relations of the Japanese Flora to that of North America, and of Other Parts of the Northern Temperate Zone,” *Memoirs of the American Academy of Arts and Sciences (New Series)* 6, Part II (1859), 377–452.

<sup>3</sup> On the making of the type specimen, see Lorraine Daston, “Type Specimens and Scientific Memory,” *Critical Inquiry* 31, no. 1 (2004), 153-182.

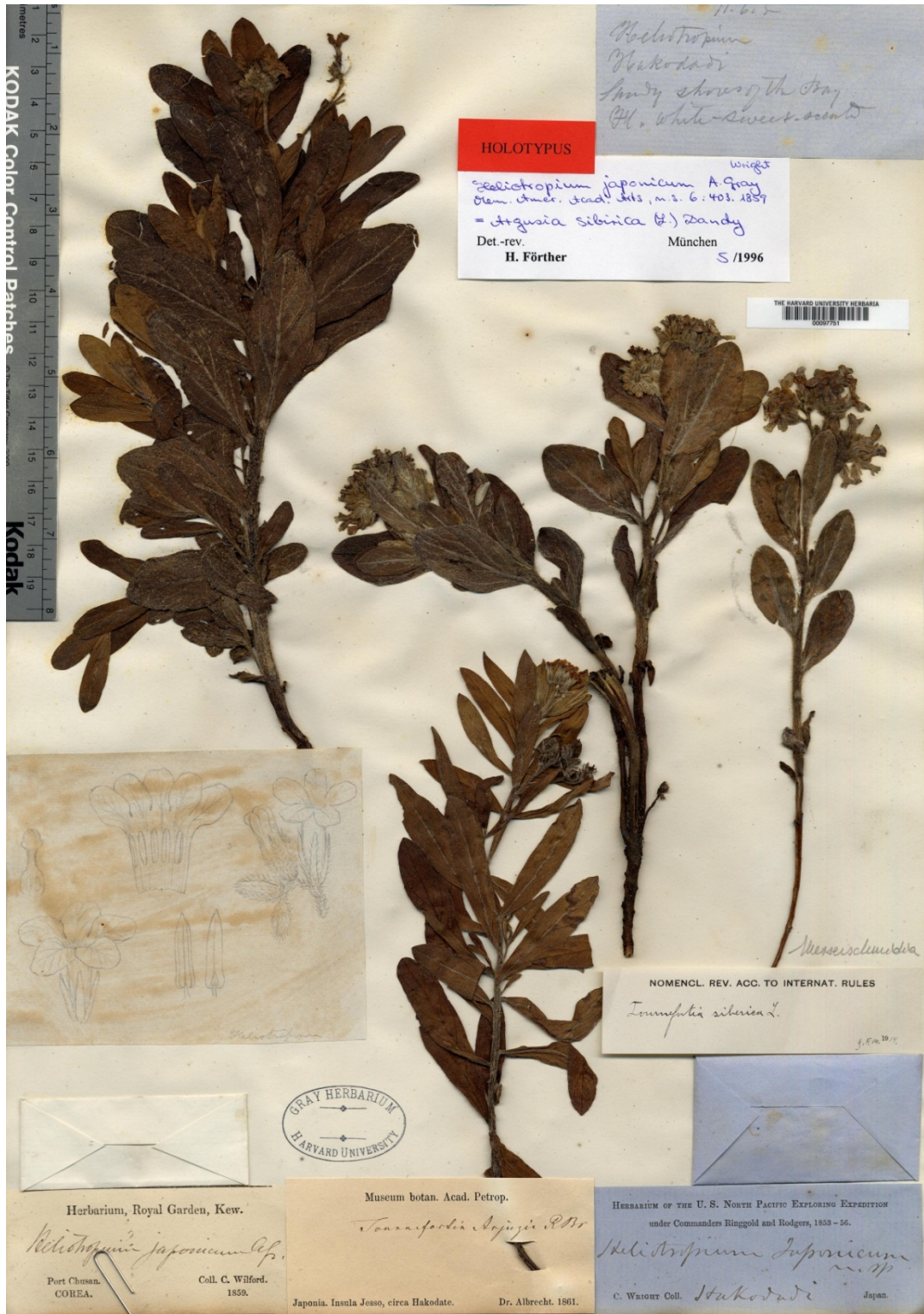


Figure I-1. The type specimen of *Heliotropium japonicum*. (Courtesy of the Gray Herbarium, Harvard University, Cambridge, Massachusetts, USA.)

But if we place specimen 97751 in its historical context—how Charles Wright gathered it, preserved it, brought it to Cambridge, Massachusetts, and placed it in the hands of Asa Gray—this dried, wrinkled thing no longer seems dead and dull. Specimen 97751 in fact ranks among the most celebrated trophies at the Harvard University Herbaria. In the late 1850s, the specimen, along with some hundreds of others collected by Charles Wright in Japan during the North Pacific Exploring Expedition, formed the basis upon which Gray would develop his “disjunction thesis” on the similarities between the flora of Japan and that of eastern North America. Gray made this thesis known to the world of science first at Harvard’s small, informal Cambridge Scientific Club in December 1858. Then, recognizing that the American Academy of Arts and Sciences had scheduled a meeting in January 1859, he rushed to submit a paper for an hour-long presentation. Gray titled his essay “Diagnostic Characters of New Species of Phaenogamous Plants, Collected in Japan by Charles Wright, Botanist of the U.S. North Pacific Exploring Expedition with Observations upon the Relations of the Japanese Flora to that of North America, and of Other Parts of the Northern Temperate Zone,” a lengthy title that indicated Gray’s intention to enumerate those new species discovered in Japan. The title was misleading. At the meeting held on January 11, Gray spoke frankly that to describe new species was not his major concern. Instead, he declared, the aim of the paper was to tackle one of the thorniest questions in botanical geography: namely, why there was a floristic similarity between Japan and eastern North America.

With the goal set out, Gray began introducing a multitude of topics. He spoke freely about the migration and interchange of species, challenged the Creator’s role

in plant distribution, and drew the audience's attention to a joint paper published in the *Journal of the Proceedings of the Linnaean Society*, in which two British naturalists, Charles Darwin and Alfred Russel Wallace, wrote about the variability and origin of species. In the startled audience sat Louis Agassiz (1807-1873), by far the most eminent naturalist in the United States. Like Gray, Agassiz was a Harvard professor, and had recently published his noted *Essay on Classification*, which established him as a knight out to ruthlessly extinguish any doubt in the divine creation of species. Agassiz felt himself being targeted by Gray. He was right. Afterward, along with the publication of *On the Origin of Species* in November 1859, these two naturalists famously engaged in what historians of science would aptly characterize as the "first Darwinian debate in the United States."<sup>4</sup> In 1872, as an established naturalist looking back to that inspiring and yet turbulent year, Gray modestly pointed out that his essay on Japan's botany had been undertaken "before the publication of Darwin's now famous volume on the *Origin of Species*... with which I attempted to deal in a moderate and feeble way."<sup>5</sup> Toward the end of his life,

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<sup>4</sup> See, among others, A. Hunter Dupree, "The First Darwinian Debate in America: Gray versus Agassiz," *Daedalus* 88, no. 3 (1959), 560-569; Dupree, *Asa Gray: American Botanist, Friend of Darwin* (Baltimore: Johns Hopkins University Press, 1988), Chapter 15; Edward Lurie, *Louis Agassiz: A Life in Science* (Baltimore: Johns Hopkins University Press, 1988), Chapter 7; Philip J. Pauly, *Biologists and the Promise of American Life: From Meriwether Lewis to Alfred Kinsey* (Princeton: Princeton University Press, 2002), Chapter 1; Janet Browne, "Asa Gray and Charles Darwin: Corresponding Naturalists," *Harvard Papers in Botany* 15, no. 2 (2010), 209-220; Christoph Irmscher, *Louis Agassiz: Creator of American Science* (Boston: Houghton Mifflin Harcourt, 2013), Chapter 4.

<sup>5</sup> Asa Gray, "Sequoia and Its History: Address before the American Association at Its Recent Meeting in Dubuque, Iowa," *The American Journal of Science and Arts (3rd Series)* 4, no. 22 (1872), 293.

he was still thrilled by his boldness in applying Darwin's theory to explain an otherwise obscure pattern in nature. In a talk given before the Harvard University Natural History Society on April 18, 1878, the sixty-eight-year-old Gray reminisced that he came to notice the floristic relationship between Japan and eastern North America when he was "a very young botanist." Then, as if to remind his listeners (most of whom were very young Harvard students) of what truly mattered in a scientific discovery, he remarked that he had "from time to time made lists of such instances" of mutually similar American and Japanese species to support his disjunction thesis.<sup>6</sup> It turned out that Gray continued making "lists of such instances" for the rest of his life. In 1886, only two years before his death, he encouraged a newly admitted Harvard student named Kingo Miyabe (1860-1951) to study the flora of the Kurile Islands, particularly focusing on its relationship to the flora of northern Japan and that of North America.<sup>7</sup> Taken altogether, as Hunter Dupree points out in his acclaimed biography of Asa Gray, Gray's 1859 essay surely represented the climax of Gray's long career as an American botanist.<sup>8</sup>

Gray's 1859 essay has become influential. To Gray's contemporaries, it was this very essay that established Gray's reputation as a "philosophical naturalist." Joseph D. Hooker (1817-1911), arguably the most influential botanist during the second

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<sup>6</sup> Asa Gray, "Forest Geography and Archaeology: A Lecture Delivered Before the Harvard University Natural History Society, April 18, 1878," *The American Journal of Science and Arts (3rd Series)* 16, no. 93 (1878), 188.

<sup>7</sup> Kingo Miyabe, "The Flora of the Kurile Islands," *Memoirs of the Boston Society of Natural History* 4, no. 7 (1870), 203-275. Miyabe mentioned Gray's advice on p. 205.

<sup>8</sup> Dupree, *Asa Gray*, Chapter 13.

half of the nineteenth century, claimed that Gray's 1859 essay was "the first entirely satisfactory essay of its kind to the science of Botanical Geography known to me."<sup>9</sup> Nowadays, botanists specializing in biogeography still consider Gray's 1859 essay one of the cornerstones of the field. With the aid of genetics, paleobotany, climatology, and plate tectonics, they investigate the evolutionary relationship between those morphologically similar East Asian and North American species from various angles.<sup>10</sup> It is worth noting that the influence of Gray's disjunction thesis is hardly confined to the field of science. Today, as tensions between Darwinism and Creationism escalate, Gray's pioneering application of Darwinian theory in

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<sup>9</sup> Charles Sargent, *Scientific Papers of Asa Gray, Vol. I: Reviews of Works on Botany and Related Subjects, 1837-1887* (Boston and New York: Houghton, Mifflin and Company, 1889), 125; Joseph Dalton Hooker, "The Distribution of the North American Flora," *Notices of the Proceedings at the Meetings of the Members of the Royal Institution of Great Britain, with Abstracts of the Discourse Delivered at the Evening Meetings* 8 (1879), 576.

<sup>10</sup> See, among others, Hui-Lin Li, "Floristic Relationships between Eastern Asia and Eastern North America," *Transactions of the American Philosophical Society* 42, no. 2 (1952), 371-429; Alan Graham, "Outline of the Origin and Historical Recognition of Floristic Affinities between Asia and Eastern North America," in *Floristics and Paleofloristics of Asia and Eastern North America*, edited by Alan Graham (Amsterdam: Elsevier, 1972), 1-18; David E. Boufford, and Stephen A. Spongberg, "Eastern Asian-Eastern North American Phytogeographical Relationships—a History from the Time of Linnaeus to the Twentieth Century," *Annals of the Missouri Botanical Garden* 70, no. 3 (1983), 423-439; Jun Wen, "Evolution of Eastern Asian and Eastern North American Disjunct Distributions in Flowering Plants," *Annual Review of Ecology and Systematics* 30, no. 1 (1999), 421-455; Hong Qian, "Relationships between Eastern Asia and North America: Test of Gray's Hypothesis," *The American Naturalist* 160, no. 3 (2002), 317-332. Regarding the place of Gray's disjunction thesis in recent developments in plant geography, see, among others, Wilfried Krutzsch, "Paleogeography and Historical Phytogeography (Paleochorology) in the Neophyticum," *Plant Systematics and Evolution* 162, no. 1-4 (1989), 5-61; Paul S. Manos and Michael J. Donoghue, "Progress in Northern Hemisphere Phytogeography: An Introduction," *International Journal of Plant Sciences* 162, no. 6 Suppl. (2001), S1-S2.

biogeography and subsequent effort to integrate Darwinism with natural theology has become a classic example of the historical relationship between science and religion.<sup>11</sup> Particularly during the worldwide celebration of the 200th anniversary of Darwin's birth and the 150th anniversary of the publication of *On the Origin of Species*, excellent studies on the alliances and debates among Darwin, Gray, and Agassiz appear with great frequency. In *Reef Madness: Alexander Agassiz, Charles Darwin, and the Meaning of Coral* (2005), for example, David Dobbs spends a whole chapter elaborating how Darwin "seduced" Gray to become a convert to his theory of evolution by natural selection. Barry Werth, on the other hand, discusses Gray's role in contributing to the "triumph of evolution in America" during the Gilded Age. In 2009, Darwin's renowned biographers Adrian Desmond and James Moore published a book titled *Darwin's Sacred Cause: How a Hatred of Slavery Shaped Darwin's Views on Human Evolution*. In it, they devote remarkable space to elaborating Gray's reception of Darwin's theory and how Darwin communicated his "shame" on and "hatred" of Agassiz's racial theory to American society through Gray. Christoph Irmscher, renowned historian specializing in early American nature-writing, also pays close attention to the Gray-Darwin alliance and the Gray-Agassiz debate in his biography of Louis Agassiz (2013). In a chapter titled

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<sup>11</sup> See, among others, Paul Jerome Croce, "Probabilistic Darwinism: Louis Agassiz vs. Asa Gray on Science, Religion, and Certainty," *Journal of Religious History* 22, no. 1 (1998), 35-58; A. Hunter Dupree, "Christianity and the Scientific Community in the Age of Darwin," in *God and Nature: Historical Essays on the Encounter between Christianity and Science*, edited by David C. Lindberg and Ronald L. Numbers (Berkeley: University of California Press, 1986), 351-368; Ronald L. Numbers, *The Creationists* (Berkeley: University of California Press, 1993).



Figure I-2. The Stamp of Asa Gray. (Courtesy of the Archives of the Gray Herbarium, Harvard University, Cambridge, Massachusetts, USA.)

“Darwin’s Barnacles, Agassiz’s Jellyfish,” Irmscher offers a succinct account of Gray’s intellectual development, analyzing how “an outsider like Gray” could “find his place at Harvard and in Boston Brahmin society” and eventually become “Darwin’s and Darwinism’s most effective spokesman in America.”<sup>12</sup> In 2011, the U.S. Postal Service issued a stamp of Asa Gray (Figure I-2; along with the stamps of three Nobel laureates: chemist Melvin Calvin, physicist Maria Goeppert Mayer, and biochemist Severo Ochoa) as part of the series of the “American Scientists (Forever®).” The

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<sup>12</sup> David Dobbs, *Reef Madness: Alexander Agassiz, Charles Darwin, and the Meaning of Coral* (New York: Pantheon, 2005); Barry Werth, *Banquet at Delmonico’s: Great Minds, the Gilded Age, and the Triumph of Evolution in America* (New York: Random House, 2009); Adrian Desmond and James Moore, *Darwin’s Sacred Cause: How a Hatred of Slavery Shaped Darwin’s Views on Human Evolution* (Boston: Houghton Mifflin Harcourt, 2009); Christoph Irmscher, *Louis Agassiz: Creator of American Science* (Boston: Houghton Mifflin Harcourt, 2013), Chapter 4 (the two quotations are on p. 130 and p. 138, respectively).



announcement of the issuance reads: “Asa Gray (1810-1888), one of the nation’s first professional botanists, advanced the specialized field of plant geography and became the principal American advocate of evolutionary theory in the mid-nineteenth century.”<sup>13</sup> Gray’s status in American science has been well established.

Thus a dissertation focusing on Gray and his disjunction thesis needs some justification. Do historians of science need more works that highlight Gray’s ultimate role in plant geography and in introducing Darwinism into the United States?

### **Basic questions**

The fact is that despite Gray’s high visibility in the history of science, historians of science for the most part remain unable to answer the following basic questions: How was Gray able to acquire specimens from Japan? Why did a Harvard professor like Gray become interested in Japan’s flora? What was this North Pacific Exploring Expedition that brought Charles Wright to Japan in 1855? Who was this Charles Wright? Such questions, trivial as they may seem, will help historians of science view the well-researched “First Darwinian Debate in the United States” with fresh eyes. The current literature on the Gray-Darwin alliance and the Gray-Agassiz debate is marked by a strong tendency to place Gray and his botany in a teleological and Whiggish narrative. That is to say, historians of science often regard Gray’s *own* intellectual development and scientific practices as a series of *stepping stones* leading to the eventual triumph of Darwinism in the United States. Here it is worth

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<sup>13</sup> The announcement can be found at: [https://store.usps.com/store/browse/uspsProductDetailMultiSKUDropDown.jsp?productId=S\\_467308&categoryId=subcatS\\_S\\_Sheets](https://store.usps.com/store/browse/uspsProductDetailMultiSKUDropDown.jsp?productId=S_467308&categoryId=subcatS_S_Sheets)

noting that I am not denying Darwin's influences on Gray's intellectual development (I devote two chapters to the subject). But if we recognize Gray's disjunction thesis as part and parcel of nineteenth-century biogeography, it seems unlikely that historians of science could reveal the thesis's historical significance by examining only those philosophical skirmishes about the origin and distribution of species.<sup>14</sup> As studies by Janet Browne, Eugene Cittadino, Jane R. Camerini, Jim Endersby, Lynn K. Nyhart, Robert E. Kohler, and Sally Gregory Kohlstedt have shown, biogeography as a discipline owes its existence to the imperial expansion and scientific expeditions of the nineteenth century, as well as the mushrooming of natural history institutions and the professionalization of natural history collecting of the twentieth century. In other words, a close examination of a biogeographical thought like Gray's thesis should not only scrutinize such scientific practices as collecting, surveying, mapping, and field-observing, but also place them against the formation of the global networks of nation-states and scientific communities.<sup>15</sup> Considering the fact

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<sup>14</sup> It should be noted that sometimes historians don't get the details of Gray's disjunction right. For example, in his admirable biography of Louis Agassiz, Irmischer briefs Gray's disjunction thesis as follows: "Wright's specimens would help him [Gray] prove that similar plant species, or even the same one, could grow in geographically distinct locations on the North American continent or in areas as different as the eastern United States and eastern Japan." Gray never claimed the similarities between the flora of *eastern* Japan and that of eastern North America; it was the flora of *northern* Japan that inspired Gray to come out with the disjunction thesis. Irmischer, *Louis Agassiz*, 136.

<sup>15</sup> See, among others, Janet Browne, *The Secular Ark: Studies in the History of Biogeography* (New Haven: Yale University Press, 1983); Browne, "A Science of Empire: British Biogeography before Darwin," *Revue d'histoire des sciences* 45, no. 4 (1992), 453-475; Browne, "Biogeography and Empire," in *Cultures of Natural History*, edited by Nicholas Jardine, James A. Secord, and Emma C. Spary (Cambridge: Cambridge University Press, 1996), 305-321; Jane R. Camerini, "Evolution, Biogeography, and Maps: An Early History of Wallace's Line," *Isis* 84, no. 4 (1993),

that current studies pay attention almost exclusively to the Gray-Darwin alliance and the Gray-Agassiz debate, thus leaving Gray's collaborations with field botanists and collectors utterly unaddressed, this dissertation aims to unveil the *geography* of Gray's biogeographical thesis, or, as David N. Livingstone famously puts it, "putting science in its place."<sup>16</sup>

A good starting point to put Gray's disjunction thesis in its place is to look closely at a portrait of Gray produced half a century after Gray's death in 1888 (Figure I-3). Originally used as an advertisement for the First National Bank of Boston in 1940, this portrait was based on a daguerreotype of Gray taken in 1842 and aimed to reconstruct an image of Gray in his early years as a botanist. In it we see Gray—well-dressed and relaxed—examining plants through a microscope alongside an open

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700-727; Eugene Cittadino, *Nature as the Laboratory: Darwinian Plant Ecology in the German Empire, 1880-1900* (Cambridge: Cambridge University Press, 1990); Jim Endersby, *Imperial Nature: Joseph Hooker and the Practices of Victorian Science* (Chicago: University of Chicago Press, 2008); Robert E. Kohler, *All Creatures: Naturalists, Collectors, and Biodiversity, 1850-1950* (Princeton: Princeton University Press, 2006); Sally Gregory Kohlstedt, "Place and Museum Space: The Smithsonian Institution, National Identity, and the American West," in *Geographies of Nineteenth-Century Science*, edited by David N. Livingstone and Charles W. J. Withers (Chicago: The University of Chicago Press, 2011), 399-438; Lynn K. Nyhart, *Modern Nature: The Rise of the Biological Perspective in Germany* (Chicago: University of Chicago Press, 2009). An intellectual historical account of biogeography can be found in Peter J. Bowler, *Life's Splendid Drama: Evolutionary Biology and the Reconstruction of Life's Ancestry, 1860-1940* (Chicago: University of Chicago Press, 1996).

<sup>16</sup> David N. Livingstone, *Putting Science in Its Place: Geographies of Scientific Knowledge* (Chicago: University of Chicago Press, 2003). On the "spatial turn" or "geographical approaches" in the history of science, see, among others, Diarmid A. Finnegan, "The Spatial Turn: Geographical Approaches in the History of Science," *Journal of the History of Biology* 41, no. 2 (2008), 369-388. I also find Suman Seth's postcolonial approach to the subject stimulating. Seth, "Putting Knowledge in Its Place: Science, Colonialism, and the Postcolonial," *Postcolonial Studies* 12, no. 4 (2009), 373-388.



Figure I-3. A closet botanist at work. (Courtesy of the Archives of the Gray Herbarium, Harvard University, Cambridge, Massachusetts, USA.)

notebook and a magnifier. Gray at work is surrounded by a variety of scientific objects, for example, specimens, potted plants, illustrations, and reference books. The message is clear: Gray's preeminence results from focused work at a well-equipped closet. Indeed, as botanist James Revel points out, the expression "closet botanist," though nowadays "a term of derision," conveyed an array of positive meanings in nineteenth-century botany. At least, he argues, it projected a botanist "armed with large libraries, significant collections, and knowledgeable colleagues—as well as having a position." In Revel's opinion, without closet botanists like Gray, "knowledge of North American botany itself would have been limited, and like the



Figure I-4. A collector at work. (Courtesy of the El Paso County Historical Society, El Paso, Texas, USA.)

period prior to the 1840s, dominated by Europeans.”<sup>17</sup>

A similar posthumous portrait of Charles Wright forms a remarkable contrast to Figure I-3. In an essay titled “Botanizer of the Boundary” (1992), Texan historian Clinton P. Hartmann and artist José Cisneros produce a compelling illustration of how Wright collected plants in the mid-nineteenth-century Texan frontier (Figure I-4).<sup>18</sup> In it we find a sturdy man, unshaved and tattered, examining a cactus in an

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<sup>17</sup> James L. Reveal, “Asa Gray and the Botanical Exploration of the American West,” *Harvard Papers in Botany* 15, no. 2 (2010), 309.

<sup>18</sup> Clinton P. Hartmann, “Charles Wright: Botanizer of the Boundary, Part I: A Connecticut Yankee in Van Horne’s Train,” *Password* 37, no. 2 (1992), 55-70;

open, rough, and barren wilderness. Whereas a well-equipped closet buttresses Gray's work, Wright's equipment is but a magnifier, a book, and a hoe. This image, together with Hartmann's vivid account of how Wright struggled to overcome a constellation of unfavorable circumstances only to satisfy Gray's scientific interests, profoundly show how heavily American botany of the later part of the nineteenth century relied on natural enthusiasts like Wright. Here it is worth noting that Hartmann's essay and Cisneros's illustration echo a research tradition that Samuel Wood Geiser shows in his widely cited *Naturalists of the Frontier* (1937). Geiser's book argues that a close examination of naturalists of the frontier can enrich Frederick Jackson Turner's famous frontier thesis. According to Geiser, if Turner's thesis manifests how settlers' experiences in the frontier helped to formulate a unique identity of American society, then it is fair to say that such fearless, unselfish, and enthusiastic frontier naturalists in many ways secured the independent status of American science.<sup>19</sup>

More historiographical implications will appear if we juxtapose Figure I-3 and Figure I-4. Scrutinizing the contrasts evident between the two figures, historians of

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Hartmann, "Charles Wright: Botanizer of the Boundary, Part II: At the Edge of the Power Struggle in the U.S. Boundary Commission," *Password* 37, no. 4 (1992), 171-187. The abovementioned illustration is on p. 177.

<sup>19</sup> Samuel Wood Geiser, *Naturalists of the Frontier* (Dallas: Southern Methodist University, 1937). For an elaboration and extension of Geiser's theme, see, among others, Elizabeth Keeney, *The Botanizers: Amateur Scientists in Nineteenth-Century America* (Chapel Hill: University of North Carolina Press, 1992) and Sally Gregory Kohlstedt, "The Nineteenth-Century Amateur Tradition: The Case of the Boston Society of Natural History," in *Science and Its Public: The Changing Relationship*, edited by Gerald James Holton and William A. Blanpied (Dordrecht: D. Reidel, 1976), 173-190.

science have emphasized the necessity of examining the means of communication or “go-betweens” by which different scientific sites become connected.<sup>20</sup> In the history of natural history in particular, a body of excellent literature has identified correspondence, patronage, gift exchange, commissioned expeditions, specimen dealership, commercial collecting, apprenticeship, and so on, as ways by which closet naturalists were able to secure specimens and information from the field and thereby establish themselves as the “center of calculation.”<sup>21</sup> This dissertation is

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<sup>20</sup> See, among others, James A. Secord, “Knowledge in Transit,” *Isis* 95, no. 4 (2004), 654-672; Simon Schaffer, Lissa Roberts, Kapil Raj, and James Delbourgo, eds., *The Brokered World: Go-Betweens and Global Intelligence, 1770-1820* (Sagamore Beach: Science History Publications, 2009); David N. Livingstone and Charles W. J. Withers, eds., *Geographies of Nineteenth-Century Science* (Chicago: The University of Chicago Press, 2011).

<sup>21</sup> David Elliston Allen, *The Naturalist in Britain: A Social History* (Princeton: Princeton University Press, 1994); Janet Browne, *Charles Darwin: The Power of Place* (New York: Alfred Knopf, 2002); Margaret Meredith, “Friendship and Knowledge: Correspondence and Communication in Northern Trans-Atlantic Natural History, 1780-1815,” in *The Brokered World: Go-Betweens and Global Intelligence, 1770-1820*, edited by Simon Schaffer, Lissa Roberts, Kapil Raj, and James Delbourgo (Sagamore Beach: Science History Publications, 2009), 151-192; Paula Findlen, *Possessing Nature: Museums, Collecting, and Scientific Culture in Early Modern Italy* (Berkeley: University of California Press, 1994); Emma C. Spary, *Utopia’s Garden: French Natural History from Old Regime to Revolution* (Chicago: University of Chicago Press, 2000); Richard Harry Drayton, *Nature’s Government: Science, Imperial Britain, and the “Improvement” of the World* (New Haven: Yale University Press, 2000); Ann Secord, “Corresponding Interests: Artisans and Gentlemen in Nineteenth-Century Natural History,” *The British Journal for the History of Science* 27, no. 4 (1994), 383-408; Secord, “Science in the Pub: Artisan Botanists in Early Nineteenth-Century Lancashire,” *History of Science* 32, no. 97 (1994), 269-315; Secord, “Artisan Botany,” in *Cultures of Natural History*, edited by Nicholas Jardine, James A. Secord, and Emma C. Spary (Cambridge: Cambridge University Press, 1996), 378-393; Jim Endersby, “‘From Having No Herbarium’: Local Knowledge vs. Metropolitan Expertise: Joseph Hooker’s Australasian Correspondence with William Colenso and Ronald Gunn,” *Pacific Science* 55, no. 4 (2001), 343-358; Endersby, *Imperial Nature*; Mark V. Barrow Jr., “The Specimen Dealer: Entrepreneurial Natural History in America’s Gilded Age,” *Journal of the History of Biology* 33, no. 3 (2000), 493-534; Robert E. Kohler, *Lords of the Fly: Drosophila Genetics and the Experimental Life*

deeply informed by this body of literature. I argue that putting Gray's disjunction thesis in its place not only leads historians of science to travel back and forth across the Atlantic along with Gray's letters to and from prominent naturalists like Darwin, Joseph D. Hooker, and George Bentham, but also requires historians of science to follow Gray's collectors and collaborators to ramble about the prairies and highlands in Texas and New Mexico, to voyage across the Pacific, to collect in Hong Kong and Canton under the threat of the Taiping Rebellion, and to explore Japan's flora when the whole nation was profoundly provoked by American Commodore Matthew C. Perry's opening-Japan expedition (1852-1854). While circumnavigating the globe with Gray's botanical collaborators, this dissertation also examines the specific means by which scientific ideas and objects traveled across geographical and social boundaries. In sum, this dissertation argues that such a seemingly *domestic* (or ivory-tower-confined) event as the first Darwinian debate in the United States stemmed from the global circulations of goods and ideas well *before* the publication of *On the Origin of Species* in 1859.

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(Chicago: University of Chicago Press, 1994); Kohler, *Landscapes & Labscapes: Exploring the Lab-Field Border in Biology* (Chicago: University of Chicago Press, 2002); Kohler, *All Creatures*; Kohler, "From Farm and Family to Career Naturalist: The Apprenticeship of Vernon Bailey," *Isis* 99, no. 1 (2008), 28-56; Matthew Laubacher, "Cultures of Collection in Late Nineteenth Century American Natural History" (PhD Dissertation, Arizona State University, 2011); Laubacher, "The 'Mandarin-Missionary' Strategy: Robert Kennicott, Spencer Fullerton Baird and Specimen Collection in the Hudson's Bay Territory," *Endeavour* 36, no. 2 (2012), 46-54; for a historiographical account of the "field science" or "collecting science," see Robert E. Kohler, "Finders/Keepers: Collecting Sciences and Collecting Practice." *History of Science* 45, no. 4 (2007), 428-454; Kohler, "History of Field Science: Trends and Prospects," in *Knowing Global Environments: New Historical Perspectives on the Field Sciences*, edited by Jeremy Vetter (New Brunswick: Rutgers University Press, 2011), 212-240.



This dissertation contains three parts, each of which deals with a specific place, and contributes to certain threads of ongoing conversations among historians of science. The sections that follow outline the central themes and historiographical concerns that each part addresses. The first place that this dissertation will visit is Harvard's Garden House, the place where Asa Gray spent most his professional life.

### **Botanist**

How did Gray get settled there? I can reframe my question according to what Robert E. Kohler tries to solve in his essay on Vernon O. Bailey (1864–1942): namely, how could “a frontier farm lad” like Bailey discover “a scientific vocation and career?”<sup>22</sup> Born in a farming family in Sauquoit, Professor Asa Gray could have become a farmer.<sup>23</sup> Trained at Fairfield Medical College, he could have become a physician. As a native of the “Yankeedom,” he could have become any member of things—a banker in New York, a planter in Texas, a gold-digger in California, a merchant doing business in the Far East.<sup>24</sup> But instead, Gray became a professor in botany, and teaching and researching botany became his profession. At some point, Gray himself might have been stunned by his boldness in choosing science as his vocation. It took him one decade to secure a professorship after receiving his doctoral degree in

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<sup>22</sup> Kohler, “From Farm and Family to Career Naturalist,” 28.

<sup>23</sup> In the words of Farlow, “In 1810 Sauquoit was a frontier settlement”; William G. Farlow, “Memoir of Asa Gray,” 163, retrieved March 25, 2013, from <http://www.nasonline.org/publications/biographical-memoirs/memoir-pdfs/gray-asa.pdf>.

<sup>24</sup> Gray's good friend Moses Ashley Curtis mentioned this term to Gray; MAC to AG, 3/29/1850, *HL*.

1831; and it took him another decade to establish himself enough to pursue his botanical studies with “satisfaction and single-mindedness.”<sup>25</sup> Part I focuses on the first two decades of Gray’s life as a botanist (1831-1851). “We need not dwell upon the details of his simple and uniformly happy life,” William Gilson Farlow (1844-1919), Gray’s favorite student and afterward close colleague, remarked, “except so far as they show the source of his influence on the progress of botany in the United States and illustrate a striking phase of American scientific education in the early part of the present century, when the young naturalist was forced to depend upon his own intelligence and enthusiasm without the aid of well-equipped laboratories, large libraries, and collections.”<sup>26</sup>

Once could characterize the first two decades of Gray’s career as a process of professionalization. To name only a few, Gray’s close affiliations with national scientific institutions, his active participation in scientific organizations, his insistence on establishing standards in botanical sciences, his eagerness to introduce what he considered the most philosophical ideas to American natural history, his astonishing publishing record, and his resolution to institutionalize botanical studies in the United States, all make Gray’s career an exemplary specimen for studying professionalization in American science.

Yet, as Adrian Desmond claims in his influential essay on the making of mid-

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<sup>25</sup> Gray described this status in a letter to senior botanist Charles W. Short. AG to CWS, 11/24/1853, William Chambers Coker, “Letters from the Collection of Dr. Charles Wilkins Short,” *Journal of the Elisha Mitchell Scientific Society* 57, no. 1 (1941), 128.

<sup>26</sup> Farlow, “Memoir of Asa Gray,” 163.

Victorian biology, “professionalization” as an analytical concept has become increasingly problematic. “Nowadays,” Desmond notes, “we want to see professionalization in its making, not as it is retrospectively stamped from a twentieth century sociological vantage point.” Hence, argues Desmond, instead of asking questions such as what constitutes a process that can be called professionalization, “the question now being asked is what these white, middle class men of science themselves thought they were doing.”<sup>27</sup> Ruth Barton, too, states in her essay on the mid-Victorian scientific communities that her “dissatisfaction with professionalizing interpretations of Victorian science stimulated this study.” In light of increasing numbers of studies that challenge this seemingly all-embracing “professionalization,” Barton suggests that historians of science turn to focus on language, particularly language that men of science applied to “identify boundaries between insiders and outsiders.” Echoing Desmond’s 2001 essay, she argues that what matters in the issue of professionalization is the ways in which men of science of the day spoke of it, rather than how scholars nowadays define it.<sup>28</sup>

Similar to their colleagues who challenge the standard historiography of professionalization of Victorian science, historians of American science have struggled to devise analytical strategies, terminologies, and categories to capture this slippery term. Aware of the increasingly independent status of American

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<sup>27</sup> Adrian Desmond, “Redefining the X Axis: ‘Professionals,’ ‘Amateurs’ and the Making of Mid-Victorian Biology—A Progress Report,” *Journal of the History of Biology* 34, no. 1 (2001), 4.

<sup>28</sup> Ruth Barton, “‘Man of Science’: Language, Identity and Professionalization in the Mid-Victorian Scientific Community,” *History of Science* 41, no. 1 (2003), 73.

scientific communities during the second half of the nineteenth century, they have examined the period from a multitude of viewpoints, including institutionalization, scientists' life histories, scientific organizations, the science-state relationship, and the like.<sup>29</sup> Paul Lucier's recent essay (2009) brings this line of inquiry into direct dialogue with studies that advocate a reconsideration of professionalization taking place in mid-nineteenth-century Victorian science. In his essay on the making of such identities as "scientists" and "professionals," Lucier claims that "there was no such person as a 'professional scientist'" in the nineteenth-century United States. In Lucier's view, during the first half of the nineteenth century, American "scientists" (i.e. those who identified themselves as botanists, zoologists, geologists, and so on) and "professionals" (i.e. physicians, clergymen, and engineers) consciously distanced themselves from each other, for they practiced their own ways of making a living. He goes on to argue that the two groups hardly converged "during the Gilded Age," but "'scientists' came to distrust commercial patrons and feared that money would corrupt American science," thus giving rise to a shared understanding that "scientists" could not be "professionals." Although he points out that this belief

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<sup>29</sup> See, for example, Robert V. Bruce, *The Launching of Modern American Science, 1846-1876* (New York: Knopf, 1987); A. Hunter Dupree, *Science in the Federal Government: A History of Policies and Activities to 1940* (Cambridge: Belknap Press of Harvard University Press, 1957); Sharon E. Kingsland, *The Evolution of American Ecology, 1890-2000* (Baltimore: The Johns Hopkins University Press, 2005); Sally Gregory Kohlstedt, *The Formation of the American Scientific Community: The American Association for the Advancement of Science, 1848-60* (Urbana: University of Illinois Press, 1976); Paul Lucier, "The Professional and the Scientist in Nineteenth-Century America," *Isis* 100, no. 4 (2009), 699-732; Charles E. Rosenberg, *No Other Gods: On Science and American Social Thought (Revised and Expanded Edition)* (Baltimore: The Johns Hopkins University Press, 1997); Hugh Richard Sloten, *Patronage, Practice, and the Culture of American Science: Alexander Dallas Bache and the U.S. Coast Survey* (Cambridge: Cambridge University Press, 1994).

on the part of late-nineteenth-century American scientists definitely challenges the legitimacy of “professionalization” as an analytical concept, he cautions that readers should not take the statement at face value. “[B]oth the professional and the scientist were species of nineteenth-century American culture,” he asserts. “Nineteenth-century American men of science took a distinctive, professional turn in their commercial relations; but the potential and real possibility that commercialization would lead to corruption spurred a few outspoken critics to envision a pure science pursued by the new American scientist.”<sup>30</sup>

It is worth noting Richard Bellon’s and Jim Endersby’s respective studies on Joseph D. Hooker, Director of the Royal Botanical Gardens at Kew from 1865 to 1885, President of the Royal Society from 1873 to 1877, Darwin’s loyal and life-long friend, and arguably the best specimen for studying professionalization of Victorian science. In his essay titled “Joseph Dalton Hooker’s Ideals for a Professional Man of Science” (2001), Bellon first recognizes Hooker’s status as a “professional botanist in mid-Victorian Britain,” but immediately comments that no historical insight can be gained from this recognition. By surveying Hooker’s voluminous correspondence, he discovers that Hooker held a rather radical view of the relationship between professional men of science and the government. In Bellon’s words, “the core aspiration of Hooker’s professionalization was to consolidate men of science into a dutiful and centralized community dedicated to national well-being.” To Hooker, Bellon elaborates, an ideal professional man of science should regard the status and funds bestowed upon him by the government as “rewards” instead of “payment.”

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<sup>30</sup> Lucier, “The Professional and the Scientist...,” 705 and 732.

Endersby, although admiring Bellon's analysis, points out that Hooker actually never called himself a "professional botanist." Unlike Bellon, who still tries to attribute "professionalization" some analytical value, Endersby advocates dismissing the concept out of hand. As an alternative, he emphasizes a wholly practice-centered approach. In his scientific biography of Hooker, for example, he produces ten chapters, each of which addresses one type of Hooker's botanical practices, for example, traveling, collecting, classifying, and charting, a fair demonstration of what Desmond has suggested in his 2001 essay.<sup>31</sup>

Shifting focus from such a universal analytical category as professionalization to scientific practices in particular time and space brings about a wide array of fresh approaches in the history of science and sociology of science alike. In the history of natural history (modern biology included) in particular, an approach intent on unveiling the system of values—what the British historian E. P. Thompson famously called the "moral economy"—that buttresses scientific communication and collaboration has generated a body of excellent research.<sup>32</sup> For instance, in his series of studies on Linnaean botany, Staffan Müller-Wille argues that the moral economy that Linnaeus and his circle practiced in their botanical research resonated with what they considered nature's political economy. Anne Secord's studies on Artisan natural history in early-nineteenth-century Britain highlight how gift exchange and

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<sup>31</sup> Richard Bellon, "Joseph Dalton Hooker's Ideals for a Professional Man of Science," *Journal of the History of Biology* 34, no. 1 (2001), 51-82 (the quotation is on p. 51); Endersby, *Imperial Nature*.

<sup>32</sup> As far as I know, Robert E. Kohler first applies the concept of moral economy to study exchanges among scientists and scientific institutions; Kohler, *Lords of the Fly*, Chapter 1.

its accompanied moral economies prompted artisan naturalists and elite naturalists to correspond across social classes, thus giving rise to a sense of community. Jim Endersby's studies on Hooker also show the importance of gift exchange in nineteenth-century British natural history. By analyzing Hooker's correspondence with colonial botanists, he argues that it was gift exchange rather than governmental surveys that helped the Royal Botanical Gardens at Kew establish unparalleled botanical collections and thereby secure its status as the world of botany's research center. Bruno J. Strasser's studies extend this line of inquiry well into twentieth-century biomedicine. In his study on experimenters' mutual collaboration to establish GenBank in 1982, he argues that the moral economy that regulated twentieth-century experimenters' exchanges of laboratory samples resembled the moral economy dominant in nineteenth-century natural history. To him the resemblance challenges one of the fundamental themes in the historiography of biology. As he puts it, "the twentieth century did not witness the replacement of natural history by experimentalism, nor even their juxtaposition, but, rather, a new articulation of these two traditions into a 'hybrid culture' reminiscent of Baconian 'experimental history'."<sup>33</sup>

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<sup>33</sup> Staffan Müller-Wille, "Nature as a Marketplace: The Political Economy of Linnaean Botany," *History of Political Economy* 35, no. 5 (2003), 154-172; Müller-Wille, "Collection and Collation: Theory and Practice of Linnaean Botany," *Studies in History and Philosophy of Science Part C* 38, no. 3 (2007), 541-562; Secord, "Corresponding Interests"; Secord, "Science in the Pub"; Endersby, "From Having No Herbarium"; Endersby, *Imperial Nature*; Endersby, "A Life More Ordinary: The Dull Life but Interesting Times of Joseph Dalton Hooker," *Journal of the History of Biology* 44, no. 4 (2011), 611-631; Bruno J. Strasser, "The Experimenter's Museum: GenBank, Natural History, and the Moral Economies of Biomedicine, 1979-1982," *Isis* 102, no. 1 (2011), 60-96 (the quotation is on p. 61); Strasser, "Collecting Nature: Practices, Styles, and Narratives," *Osiris* 27, no. 1 (2012), 303-340.

I find these sophisticated discussions surrounding professionalization, scientific practices, and moral economy of scientific practices stimulating. After surveying Gray's copious correspondence and publications, I am almost certain that Gray rarely (if ever) characterized himself as a "professional botanist," let alone a "professional scientist."<sup>34</sup> Similar to his contemporaries, Gray generally applied such terms as man of research, man of science, scientific man, cultivator of science, or savant to identify himself and the community to which he belonged. Furthermore, in the mid-nineteenth century, when his status in the world of science was established, he became aware that many of his contemporary men of science began utilizing their scientific knowledge to make a living. But could botany be that lucrative? He did not think so.<sup>35</sup> Farlow would recall what Gray had said to him about choosing botany as a profession:

When I graduated from [Harvard] college in 1866 and wished to become a botanist, Professor Gray told me that I ought to study medicine first because the possibility of gaining a living by botany was so small that one should always have a regular profession to fall back upon. In fact, at that time medicine was practically the gate through which it was necessary to pass in order to enter the field of botany.<sup>36</sup>

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<sup>34</sup> In May 1885, Gray wrote to Charles Russell Orcutt (1864-1929), horticulturalist, botanist and editor of *The West American Scientist*, noting that "scientist" was a word he never used. AG to CRO, 5/22/1885, PCRO. This statement very likely indicates Gray's *life-long* attitude toward the word "scientist"— for he passed away in 1888.

<sup>35</sup> For example, Gray seemed to consider geology a profession, largely because geologists of the age could make a living by participating in geological surveys sponsored either by governments or by companies. On geology's special status in the history of American science, see Paul Lucier, *Scientists & Swindlers: Consulting on Coal and Oil in America, 1820-1890* (Baltimore: Johns Hopkins University Press, 2008).

<sup>36</sup> William Gilson Farlow, "The Change from the Old to the New Botany in the United States," *Science* 37, no. 942 (1913), 79.



Given the fact that Gray might consider botany not a profession, or at least not a profession that allowed its practitioners to make a living, it seems ahistorical to study Gray's career as if it represents a case of professionalization of American science. More importantly, we cannot help but seek out Gray's own views on "being a botanist," if he hardly considered botany a profession. The answer to this question is not as straightforward as it may seem. Gray stepped into the world of botany when American botanists were repeatedly asking themselves what it meant to be a botanist. Should a botanist spend most of his or her time observing and collecting plants in the field? Or, in light of seemingly endless variations in nature, should a botanist devote himself or herself to studying at herbaria and examining specimens collected across the globe? Would these two distinct ways of examining nature shape a botanist's definition of species? If so, what would be a *natural* definition of species? As I show in Part I, all such questions are intertwined with Gray's self-identity as a botanist and a man of science. In fact, it seems very likely that many of Gray's contemporaries shared Gray's view of what it meant to be a botanist and a man of science. As John Carey, a British merchant and an expert in *Carex*, once confessed to Gray, "as I remember that you only value men for their Botany, you will no doubt consider me, as I often do myself, good for nothing."<sup>37</sup>

Part I focuses on the practice by which Gray and his botanical circle applied to "value men for their Botany": "free and liberal exchange of specimens." I analyze

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<sup>37</sup> JC to AG, 7/28/1843, *HL*. Here my analysis benefits greatly by Daston's application of political sociologist Benedict Anderson's concept to call naturalists' communities the "imagined communities." Lorraine Daston, "The Sciences of the Archive," *Osiris* 27, no. 1 (2012), 156-187.

how this practice united Gray with a group of young and competent botanists, including William S. Sullivant (1803-1873), Edward Tuckerman (1817-1886), George Engelmann (1809-1884), and Moses A. Curtis (1808-1872), and gave rise to a view of nature that Gray aspired to apply in studying the North American flora during the late 1840s and the early 1850s.<sup>38</sup> By closely analyzing Gray's *Genera Florae Americae Boreali-Orientalis Illustrata* (1848-1849), as well as his correspondence, book reviews, and botanical essays, I argue that Gray and his close botanical friends were convinced that a flora project ought to be liberal, egalitarian even, and that it was meant to demonstrate the Creator's plan in nature.<sup>39</sup> I also identify a methodology pervasive in Gray's circle: namely, the flora of North America could be represented by its genera, which could then be represented by one or two species. Here it is worth noting that Gray's view of nature and its association with the flora project were not as idiosyncratic as they might have been. In her essay

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<sup>38</sup> In researching Gray's relationships with his botanical collaborators, I have benefited from the following biographies and studies: Andrew Denny Rodgers III, *"Noble Fellow," William Starling Sullivant* (New York: G.P. Putnam's Sons, 1940); Rodgers III, *John Torrey: A Story of North American Botany* (Princeton: Princeton University Press; London: H. Milford, Oxford University Press, 1942); Rodgers III, *American Botany, 1873-1892: Decades of Transition* (Princeton: Princeton University Press, 1944); Edmund Berkeley and Dorothy Smith Berkeley, *A Yankee Botanist in the Carolinas: The Reverend Moses Ashley Curtis, D.D., 1808-1872* (Berlin: J. Cramer, 1986); Oscar H. Soule, "Dr. George Engelmann: The First Man of Cacti and a Complete Scientist," *Annals of the Missouri Botanical Garden* 57, no. 2 (1970), 135-144; Norton G. Miller, "Asa Gray and the Development of Bryology in the United States," *Harvard Papers in Botany* 15, no. 2 (2010), 287-304.

<sup>39</sup> Asa Gray, *Genera Florae Americae Boreali-Orientalis Illustrata: The Genera of the Plants of the United States Illustrated by Figures and Analyses from Nature, Vol. 1* (Boston: James Munroe and Company, 1848); Gray, *Genera Florae Americae Boreali-Orientalis Illustrata: The Genera of the Plants of the United States Illustrated by Figures and Analyses from Nature, Vol. 2* (Boston: James Munroe and Company; New York: George P. Putnam; London: Putnam's American Agency, 1849).

titled “On Scientific Observation,” Lorraine Daston used Gray’s *Genera* as an example to show nineteenth-century naturalists’ “concerted attempts” to come up with “a universal, not a particular,” “laconic, even schematic” descriptions to “capture the essence of a species or an entire genus.” Concurring with Daston’s analysis, I regard Part I as a study focusing on how this mode of scientific observation related to the way in which nineteenth-century men of science acquired their objects of scientific observation. And insofar as Gray and his botanical circle were concerned, I point out that their view of nature gradually lost its prominence as a group of “professional collectors” came onto the scene.<sup>40</sup>

### **Collector**

Notably, despite Gray’s reluctance to characterize botany as a profession and himself as a professional botanist, he definitely regarded botanical *collecting* as a profession. In the late 1830s, after his research trip to distinguished botanical establishments in Europe, he became convinced that a collector—if well trained—could not only make a living by collecting plants but also make money “quite largely.”<sup>41</sup> In the early 1840s, almost immediately after his appointment as Professor in Natural History at Harvard, Gray initiated a decade-long project (in collaboration with physician-botanist George Engelmann of St. Louis) on the flora of Texas and New Mexico. The collectors whom Gray and Engelmann secured and

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<sup>40</sup> Lorraine Daston, “On Scientific Observation,” *Isis* 99, no. 1 (2008), 102-103.

<sup>41</sup> Gray told Torrey in December 1838 that “All the collectors make money.” AG to JT, 12/12/1838, *LAG*, 90.

placed under their auspices were Ferdinand Lindheimer (1801-1879) and Augustus Fendler (1813-1883), both of whom were German immigrants charged with unabashed enthusiasm for nature. Under the support of some twenty subscribers, the two collectors amassed a vast number of specimens, and Engelmann and Gray took responsibility for enumerating species, producing catalogues, grouping specimens into sets, distributing them to subscribers, and finally collecting the subscription fee. In the history of American botany, what these four figures achieved is probably the first systematic and biogeographical study of the American frontier.<sup>42</sup>

Interestingly, this first biogeographical survey of the country's frontier took place when governmental surveys were rare and scientific societies were for the most part unable to launch collecting expeditions on any significant scale. Against this backdrop, historians of science have tended to consider the natural-history collecting practiced in the 1840s and early 1850s as sporadic and random—an

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<sup>42</sup> In his biography of Gray, Dupree devotes some five pages to this Gray-Engelmann-Lindheimer-Fendler alliance. He does not differentiate the subscription system from other types of collecting: for example, commercial collecting and governmental surveys. Dupree, *Asa Gray*, 160-164. My analysis of the subscription system benefited from the following works: Joseph William Blankinship, *Plantae Lindheimerianae, Part III* (St. Louis: Missouri Botanical Garden, 1907); Minetta Altgelt Goynes, *A Life among the Texas Flora: Ferdinand Lindheimer's Letters to George Engelmann* (College Station: Texas A&M University Press, 1991); Michael T. Stieber and Carla Lange, "Augustus Fendler (1813-1883), Professional Plant Collector: Selected Correspondence with George Engelmann," *Annals of the Missouri Botanical Garden* 73, no. 3 (1986), 520-531; Elizabeth A. Shaw, *Augustus Fendler's Collection List. New Mexico, 1846-1847* (Cambridge: Gray Herbarium of Harvard University, 1982); Shaw, *Charles Wright on the Boundary, 1849-1852, or Plantae Wrightianae Revisited* (Westport: Meckler Pub. Corp.; Cambridge, England: Chadwyck-Healey, 1987); Cornelius Herman Muller, ed. *Journey to Mexico during the Years 1826 to 1834* (Austin: Texas State Historical Association in cooperation with the Center for Studies in Texas History, University of Texas at Austin, 1980).

interesting but insignificant predecessor of the systematic and exacting “surveying collecting” dominant in American natural history after the late 1850s. In his dissertation focusing on the “culture of collection” in American natural history, for example, Matthew Laubacher states that American naturalists before the late 1850s could only “supplement their own collection efforts with those of amateur nature enthusiasts, actively soliciting for specimen donation.”<sup>43</sup>

I argue that American naturalists before the late 1850s did devise a system to acquire specimens *in addition to* “actively soliciting for specimen donation.” I call this system the “subscription system.” Here I should say that I am not rescuing something from oblivion—instead, those familiar with botanical magazines published throughout the nineteenth century can hardly fail to notice those advertisements and correspondence intending to enlist individuals and institutions to support certain collecting expeditions by subscribing to sets of incoming collections. Thus far, however, historians of science have not taken this subscription system seriously. In historiographical terms, this indifference may stem from the assumption that the subscription system was but a variation on gift exchange and commercial collecting. I disagree with this assumption. First, the functioning of the subscription system relied on money, which differed markedly from the spirit of reciprocity and generosity highlighted in gift exchanges of specimens. Second, despite the central role of money in the subscription system, it would be wrong to regard the subscription system as a derivative form of commercial collecting. According to *The Oxford English Dictionary*, the term “subscribe” or *subscribere*

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<sup>43</sup> Laubacher, “Cultures of Collection...,” 2-3.

implies “to underwrite” and “to sign at the bottom of a document.”<sup>44</sup> That is to say, to subscribe means to make a commitment and to maintain a regular and lasting relationship with individuals or institutions who supply objects or services. The subscription system presumably embraced its own type of moral economy that distinguished it from market transactions. (Imagine the difference between two scenes: In one scene you purchase a magazine in the bookstore, and in the other you subscribe with the publisher to receive the magazine.) In fact, recent studies have suggested that historians of science examine money’s role in connecting different realms in the world of science instead of using money as a sole criterion to solidify such distinctions as specimen dealership and gift exchange. As Casper Andersen, Jakob Bek-Thomsen, and Peter C. Kjærgaard point out, historians of science should pay close attention to the “money trail” and analyze what kind of social relationship takes shape as money travels. In her essay on the dealer-buyer relationship as shown in the market of “giant Irish deer remains,” for instance, Juliana Adelman argues that the relationship in many ways “resembled one of patronage despite the fact that they were engaging in a commercial transaction.”<sup>45</sup>

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<sup>44</sup> *The Oxford English Dictionary* (online version) (Oxford: Oxford University, 2013), <http://www.oed.com.ezp-prod1.hul.harvard.edu/view/Entry/192952>, accessed July 14, 2014.

<sup>45</sup> Casper Andersen, Jakob Bek-Thomsen, and Peter C. Kjærgaard, “The Money Trail: A New Historiography for Networks, Patronage, and Scientific Careers,” *Isis* 103, no. 2 (2012), 310-315; Juliana Adelman, “An Insight into Commercial Natural History: Richard Glennon, William Hinchy and the Nineteenth-Century Trade in Giant Irish Deer Remains,” *Archives of Natural History* 39, no. 1 (2012), 16–26. On the role of commercial trade in the history of natural history, see, among others, Christine Brandon-Jones, “Edward Blyth, Charles Darwin, and the Animal Trade in Nineteenth-Century India and Britain,” *Journal of the History of Biology* 30, no. 2

Besides specimens, a conspicuous “product” of the subscription system was the catalogues authored by established botanists aiming to enumerate, name, describe, and sometimes illustrate collectors’ specimens. Take the subscription system run and maintained by Gray, for example. Between 1843 and 1853, Gray authored five catalogues for subscribers’ reference, all of which were published in scientific journals, all of which presented to the scientific world a remarkable number of new species and genera, and all of which were made on the basis of hundreds of pages of manuscripts, field notes, letters, and thousands of specimens.<sup>46</sup> How can historians of science make sense of the enormous labors botanists devoted to compiling catalogues?

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(1997), 145-178; Barrow Jr., “The Specimen Dealer”; Jane Camerini, “Wallace in the Field,” *Osiris (2nd Series)* 11 (1996), 44-65.

<sup>46</sup> George Engelmann and Asa Gray, “Plantae Lindheimerianae: An Enumeration of the Plants Collected in Texas, and Distributed to Subscribers, by F. Lindheimer, with Remarks, and Descriptions of New Species, &c.,” *Boston Journal of Natural History, Containing Papers and Communications* 5, no. 2 (1845), 210-264; Asa Gray, “Plantae Fendlerianae Novi-Mexicanae: An Account of a Collection of Plants Made Chiefly in the Vicinity of Santa Fé, New Mexico, by Augustus Fendler; with Descriptions of the New Species, Critical Remarks, and Characters of Other Undescribed or Little Known Plants from Surrounding Regions,” *Memoirs of the American Academy of Arts and Sciences (New Series)* 4, Part 1 (1849), 1-116; Gray, “Plantae Lindheimerianae, Part II: An Account of a Collection of Plants Made by F. Lindheimer in the Western Part of Texas, in the Year 1845-6, and 1847-8, with Critical Remarks, Descriptions of New Species, &c.,” *Boston Journal of Natural History* 6, no 2 (1850), 141-240; Gray, *Plantae Wrightianae Texano-Neo-Mexicano: An Account of a Collection of Plants made by Charles Wright, A. M., in an Expedition from Texas to New Mexico, in the Summer and Autumn of 1849, with Critical Notices and Characters of Other New or Interesting Plants from Adjacent Regions, &c. Part I* (Washington, DC: Smithsonian institution, 1852); Gray, *Plantae Wrightianae Texano-Neo-Mexicano: An Account of a Collection of Plants Made by Charles Wright, A.M., in Western Texas, New Mexico, and Sonora, in the Years 1851 and 1852. Part II* (Washington, DC: Smithsonian institution, 1853).

Here it is worthwhile to note Gordon McOuat's studies on "cataloging power" deployed by established naturalists and natural-history institutions in mid-nineteenth-century Britain. McOuat, after researching copious notes left by John Edward Gray (1800-1875), zoological curator of the British Museum, argues that J. E. Gray developed a rather "cynical" strategy to tackle the increasingly vexing "species problem" in the 1840s. In the face of the proliferation of natural systems (each of which claimed it reflected the order of nature), McOuat argues, J. E. Gray quietly and steadily revolutionized the way in which the British Museum cataloged its collections. In McOuat's words, J. E. Gray's revolution embraced two principles: first, "Species, not specimen, would form the fundamental currency of intercourse between naturalists"; second, "the British Museum had to be so constructed as to be the mint, the storehouse, for that currency." McOuat argues that these two principles worked hand in hand, gradually ruling out any debate surrounding the term "species." Such "cataloging power," coupled with the increasingly important role played by *The Rules of Zoological Nomenclature* in taxonomy and nomenclature (1842; advocated and promoted by the British Association for the Advancement of Science), gave rise to a widely accepted definition of species among naturalists throughout the second half of the nineteenth century: "species are merely what competent naturalists say they are."<sup>47</sup>

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<sup>47</sup> Gordon McOuat, "Species, Rules and Meaning: The Politics of Language and the Ends of Definitions in 19th Century Natural History," *Studies in History and Philosophy of Science Part A* 27, no. 4 (1996), 473-519; McOuat, "Cataloging Power: Delineating 'Competent Naturalists' and the Meaning of Species in the British Museum," *The British Journal for the History of Science* 34, no. 1 (2001), 1-28; McOuat, "From Cutting Nature at Its Joints to Measuring It: New Kinds and New Kinds of People in Biology," *Studies in History and Philosophy of Science Part A* 32, no.



McOuat's account of "cataloging power" recently finds resonance in various fields in the history of science and sociology of science. For instance, in their *Focus* section in the *Isis* (2012), James Delbourgo and Staffan Müller-Wille discuss what they call "Listmania" in early modern science. Contributors to the section discuss various roles played by lists in early modern savants' careers and daily lives, from a device for administrating one's personal life to a research tool for developing a natural classification.<sup>48</sup> Alex Csiszar's essay on the rise of scientific journals in nineteenth-century Britain and France also shows how such manual practices as list-making and catalogue-compiling offer a window onto the working of a scientific mind. According to Csiszar, toward the turn of the nineteenth century and the twentieth, when naturalists devoted themselves to publishing their findings in scientific journals instead of writing books, their views of nature underwent remarkable changes. Csiszar then quotes renowned French naturalist Henri Poincaré's famous view of nature to support his point. That is to say, instead of an "open book," nature in the age of scientific journals became "a vast expanse of print

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4 (2001), 613–645; McOuat, "The Origins of 'Natural Kinds': Keeping 'Essentialism' at Bay in the Age of Reform," *Intellectual History Review* 19, no. 2 (2009), 211-230. The quotation is on McOuat, "Cataloguing Power," 23. I also found my inspirations in Mary P. Winsor's and Richard A. Richards's works: Winsor, "Non-Essentialist Methods in Pre-Darwinian Taxonomy," *Biology and Philosophy* 18, no. 3 (2003), 387-400; Richards, *The Species Problem: A Philosophical Analysis* (Cambridge: Cambridge University Press, 2010).

<sup>48</sup> James Delbourgo and Staffan Müller-Wille, "Introduction," *Isis* 103, no. 4 (2012), 710-715; Valentina Pugliano, "Specimen Lists: Artisanal Writing or Natural Historical Paperwork?" *Isis* 103, 4 (2012), 716-726; Staffan Müller-Wille and Isabelle Charmantier, "Lists as Research Technologies," *Isis* 103, no. 4 (2012), 743-752.

matter, a body the scientist did not so much read through, as search, select from, and catalog.”<sup>49</sup>

Inspired by such thoughtful discussions on “cataloging power” and the “power of the catalogue,” Part II not only examines how Gray acquired specimens by running his subscription system, but also shows how Gray compiled catalogues for subscribers. I argue that Gray’s use of the subscription system in exploring the American West secured his “cataloging power,” thus establishing him as the “mint” or the “storehouse” that produced “fundamental currency” in the world of botany—even though by then Gray was but a professor with limited income and virtually with no assistance from the state.

But running the subscription system did bring about a multitude of changes in Gray’s career. Of importance was his encounter and subsequent collaborations with Charles Wright, a Yale graduate (class of 1835) who aspired to become a professional collector instead of a college professor (preceding such eminent Yale-educated explorers as Josiah D. Whitney, William Henry Brewer, and Clarence King). Wright began corresponding with Gray early in the 1840s. Between 1849 and 1851, Gray and Wright collaborated intensively in exploring the flora of Texas and New Mexico. Taking advantage of the country’s rising interest in these regions after the U.S.-Mexican War (1846-1848), they managed to place their botanical endeavors under the government’s patronage. It worked. At some point, Wright collected as a

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<sup>49</sup> Nick Hopwood, Simon Schaffer, and Jim Secord, “Seriality and Scientific Objects in the Nineteenth Century,” *History of Science* 48, Parts 3/4, no. 161 (2010), 251–285; Alex Csiszar, “Seriality and the Search for Order: Scientific Print and its Problems during the Late Nineteenth Century,” *History of Science* 48, Parts 3/4, no. 161 (2010), 399–434.

member of the U.S. Mexican Boundary Survey, while Gray published the catalogue of Wright's collections as part of the *Smithsonian Contributions to Knowledge* series. In 1852, in part because of Wright's close relationship with Gray, and in part because of Wright's rising reputation as a professional collector, the Smithsonian Institution recommended Wright to the Navy Department to serve as the botanist of the North Pacific Exploring Expedition. Wright's active participation in governmental expeditions made his name. For the rest of his life, he remained active in the field. He corresponded with prominent naturalists, including Joseph D. Hooker of Kew Gardens and Spencer F. Baird of the Smithsonian, and occasionally contributed essays to *The American Naturalist*.<sup>50</sup> In 1881, when Thomas A. Thacher (1815-1886), a Yale professor and administrator, aspired to compile biographical accounts of distinguished alumni of Yale, he contacted Wright's botanical friends for information. In reply, Gray wrote that Wright had been "not only a capital and indefatigable explorer and collector, but also an acute observer." By then a world-renowned botanist, he claimed that he himself had "profited not a little by his [Wright's] observation and remarks." Daniel C. Eaton (1834-1895), a distinguished botanist at Yale, admired Wright's achievement even more highly than Gray did. Wright's experience in botanizing made Wright stand "almost without an equal" in

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<sup>50</sup> See, for example, Charles Wright, "A Comical Owl," *The American Naturalist* 2, no. 8 (1868), 420-422; Wright, "Deer and Deer-Hunting in Texas," *The American Naturalist* 2, no. 9 (1868), 466-476. For studies on Wright's botanical undertakings, see, among others, Hartmann, "Charles Wright"; Geiser, *Naturalists of the Frontier*; Shaw, *Charles Wright on the Boundary*; Richard Alden Howard, *Charles Wright in Cuba, 1856-1867* (Westport: Meckler Corp.; Cambridge, England: Chadwyck-Healey, 1987); Donald H. Pfister, ed, *Cryptogams of the United States North Pacific Exploring Expedition, 1853-1856: Unpublished Manuscripts* (Cambridge: Farlow Reference Library and Herbarium of Cryptogamic Botany, Harvard University, 1978).

the world of botany, he told Thacher: “Sir Joseph Hooker being the only equal I can think of.”<sup>51</sup>

Besides detailing Gray’s collaborations with Lindheimer and Fendler in running the subscription system, Part II contains a thorough analysis of Gray’s encounter with Wright and their collaborations with the government to advance their collecting enterprise. My analysis shows that the emergence of government participation in natural-history collecting disturbed the moral economies that had played critical roles in shaping botanists’ and collectors’ respective practices and identities. In fact, in the late 1840s, when Gray and Wright endeavored to place themselves under the government’s patronage, they scarcely expected that the endeavor would cause a multitude of troubles, including a lawsuit filed by the U.S. Boundary Commission against Wright regarding “ownership” of Wright’s collections. The early 1850s therefore witnessed surging anxiety on Gray and Wright’s parts as they familiarized themselves with a new type of moral economy (in which the state was a dominant actor). But thanks to such “go-betweens” as Baird of the Smithsonian, men of science’s moral economy and the state’s political economy rapidly became articulated. Particularly during the 1850s, when multiple surveying teams unearthed an astonishing number of unknown biological forms from the American West, and when American naturalists at large aspired to secure priority of naming and describing new species, to delineate geographical

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<sup>51</sup> AG to TAT, 8/19/1881; DCE to TAT, 6/2/1881, Thomas A. Thacher, *Biographical and Historical Record of the Class of 1835 in Yale College, for the Fifty Years from the Admission of the Class to College* (New Haven: Tuttle, Morehouse & Taylor, 1881), 179-180.

distribution of species, to estimate variability of species, and to explore the distinction between species and variety, Gray's view of nature changed accordingly. He became increasingly inclined to regard the flora as "a catalogue of species," species as "coins or banknotes," and competent naturalists as legitimate "producers" of such coins-and-banknotes-like species. I argue that it was against such shifts in views of nature that the "type-specimen" concept emerged. I conclude Part III by analyzing how Gray and other like-minded naturalists endeavored to apply this concept to turn the biota of North America into a series of catalogues.<sup>52</sup> Following Lynn K. Nyhart's excellent study on the emergence of "biological perspective" in late-nineteenth-century Germany, I call Gray and his close collaborators "practical naturalists." Although unlike Nyhart's practical naturalists, American practical naturalists in the 1850s did not get involved with disseminating certain views of nature through lectures and museum exhibits, I argue that their painstaking grouping of individual specimens into species, arranging species in a natural order, and publishing the arrangement in a series of journal articles gave rise to a keen appreciation of the patterns in nature—an appreciation akin to what Nyhart has identified in the case of German animal geography during the late nineteenth

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<sup>52</sup> On the making of the type-specimen concept, see Daston's "Type Specimens..."; Paul Lawrence Farber, "The Type-Concept in Zoology during the First Half of the Nineteenth Century," *Journal of the History of Biology* 9, no. 1 (1976), 93-119; Samuel J. M. M. Alberti, "The Status of Museums: Authority, Identity, and Material Culture," in *Geographies of Nineteenth-Century Science*, edited by David N. Livingstone and Charles W. J. Withers (Chicago: The University of Chicago Press, 2011), 51-72.

century.<sup>53</sup> In comparison with current accounts of Gray's reception of Darwin's evolutionary theory, I argue that it was such hands-on practices as cataloging species that made Gray aware of how useful Darwin's species theory could be in solving practical questions in natural history and in elevating his and other practical naturalists' scientific status.

## **Two Charleses**

The final part of the dissertation deals with Gray's collaborations with the two "Charleses" who made Gray's study of Japan's flora possible: Charles Darwin and Charles Wright. First I focus on the period between the 1830s and the early 1850s, and show how Gray accumulated and arranged those cases suggestive of a likely relationship between the flora of North America and that of East Asia. I argue that these earliest engagements proved influential, for they altogether offered a basis upon which Gray could subsequently issue his theory on species and geographical distribution. Then I detail the ways in which the Gray of the late 1850s integrated those thoughts—suggested to him by Darwin, Bentham, Hooker, and James D. Dana—in order to explain the cases he had amassed during the previous decades. I argue that, though it is clear that Gray deliberately intended to deploy Darwin's species theory against Agassiz's, his 1859 essay as a whole should not be regarded as or reduced to a mere *application* of Darwin's theory to botany: Gray did use

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<sup>53</sup> Nyhart, *Modern Nature*; Nyhart's definition of the "practical naturalist" is on pp. 35-36.

Darwin's evolutionary theory to develop his disjunction thesis, but did not use it in a way Darwin expected.

But Gray's reception of Darwin's theory of evolution by natural selection only reveals one side of the story. Darwin might have furnished Gray with the theory by which to work; still, Gray needed material to work with. Part III includes an analysis of the "materiality" of Gray's disjunction thesis: that is to say, I investigate the course of the North Pacific Exploring Expedition (hereafter NPEE), the expedition that brought Charles Wright to collect in Japan and furnished the material and information by which Gray could test Darwin's theory of natural selection. Notably, despite the general understanding that Gray's 1859 essay on Japan's flora triggered the "first Darwinian debate" and then the Darwinian Revolution in the United States, historians of science rarely note that this very essay is in essence a report for a surveying project named NPEE. Part III aims at filling this historiographical gap.<sup>54</sup>

Here it would be useful to provide some background information about the NPEE. Although not as well-known as the U.S. South Seas Exploring Expedition (1838-1842) and the U.S.-Japan Exploring Expedition (1852-1854), the NPEE represents the most systematic scientific survey of the North Pacific before the Civil War. In August 1852, Congress placed \$125,000 in the hands of the Navy Department for "prosecuting a survey and reconnaissance for naval and commercial

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<sup>54</sup> The role of the scientific expedition report in disseminating science knowledge is a new field worthy of further investigation; for a pioneering analysis, see Lynn K. Nyhart, "Voyaging and the Scientific Expedition Report, 1800-1940," in *Science in Print: Essays on the History of Science and the Culture of Print*, edited by Rima D. Apple, Gregory J. Downey, and Stephen L. Vaughn (Madison: The University of Wisconsin Press, 2012), 65-86.

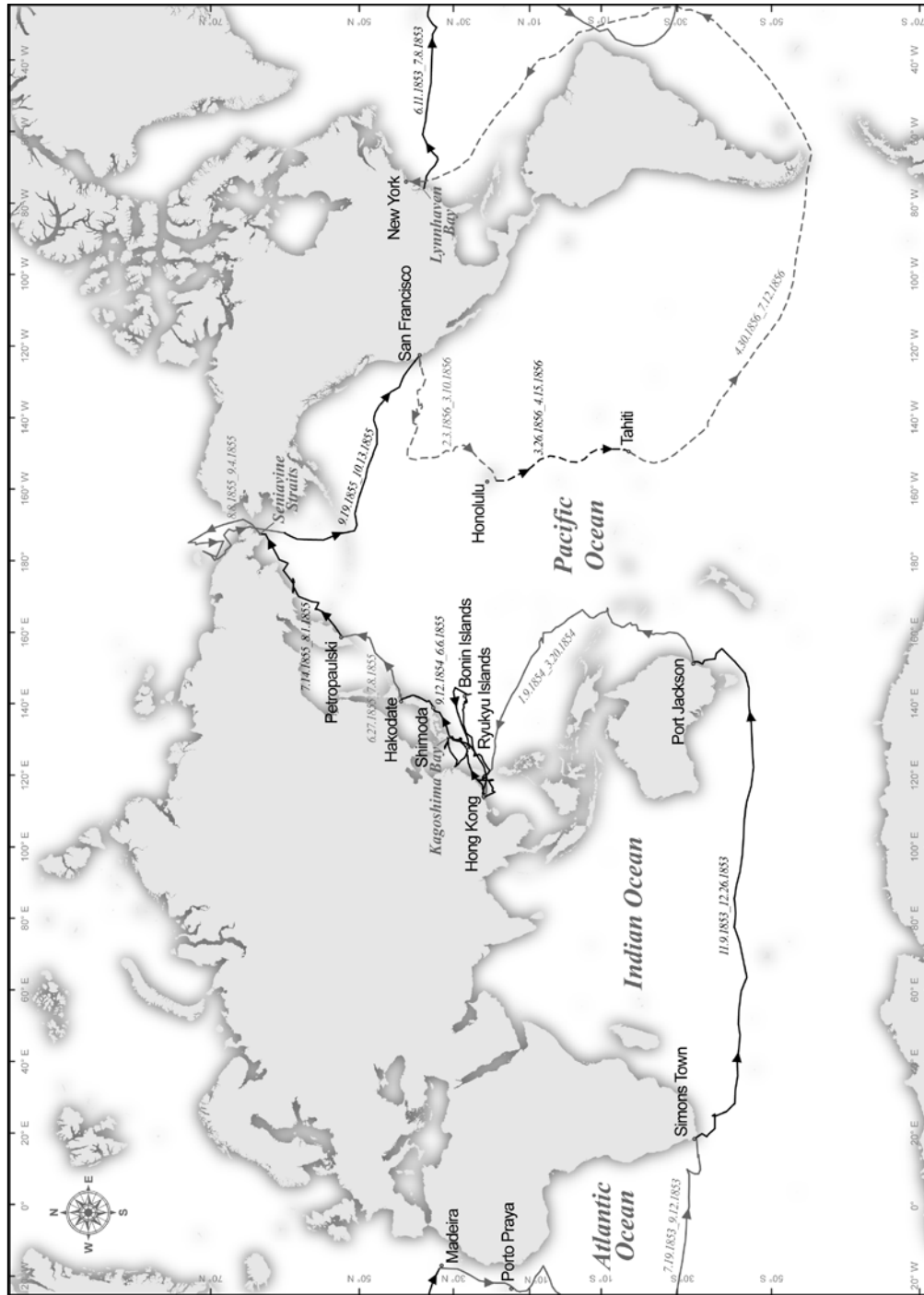


Figure I-5. The track of cruise of the *Vincennes*, the flagship of the NPEE and the carrier that brought Wright to Japan. The line between San Francisco and New York is dashed, because the NPEE officially concluded in San Francisco. (The latitude and longitude coordinates of the track is based on Vasile *et al.*, *William Stimpson's Journal.*)



purposes, of such parts of Behring's Straits, of the North Pacific Ocean and of the China Seas, as are frequented by American whaleships and by trading vessels in their routes between the United States and China." Then, between 1853 and 1855, under the commandship of Cadwalader Ringgold (1802–1867) and then John Rodgers (1812–1882), a squadron of five vessels coursed through and surveyed such locations as Madeira, the Cape Verde Islands, the Cape of Good Hope, Sydney, Hong Kong, the Bonin Islands, the Okinawa Islands, Japan, Okhotsk, and the Bering Straits, finally ending up in San Francisco (Figure I-5). Early in 1857, under the influence of prominent scientific organizations like the Smithsonian, Congress approved a \$15,000 appropriation for the subsequent analysis and publication of the NPEE's scientific contributions. For the next two years or so, the Navy Department recruited dozens of men of science to study the NPEE's zoological and botanical collections. Early in the 1860s, when these men of science completed or were about to complete their manuscripts, the Civil War broke out. As a result, a great portion of this scientific labor was put aside and has remained unpublished even until today (Figure I-6). The NPEE's publishing project reached its unfortunate nadir when the Great Chicago Fire of 1871 destroyed most of the NPEE's zoological collections and part of the manuscripts (which were housed at the Chicago Academy at the time). William Stimpson (1832-1872), who had once served as the NPEE's zoologist and as the superintendent overseeing the NPEE's natural-history reports, was profoundly shocked by the incident. In 1872, he died.<sup>55</sup>

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<sup>55</sup> On relevant studies, see, among others, Allan Burnett Cole, ed., *Yankee Surveyors in the Shogun's Seas: Records of the United States Surveying Expedition to the North Pacific Ocean, 1853–1856* (Princeton: Princeton University Press, 1947);



Figure I-6. An unpublished painting of *Rubus wrightii*, a new species discovered by Wright in Hakodate in 1855 and named by Gray. (Courtesy of the Archives of the Gray Herbarium, Harvard University, Cambridge, Massachusetts, USA.)

There are two ways to consider the NPEE's role in the making of Gray's disjunction thesis. First, for those who have some knowledge about the diplomatic history between the United States and Japan, the NPEE's success in exploring the

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Pfister, ed., *Cryptogams of the United States North Pacific Exploring Expedition*; John Dryden Kazar, "The United States Navy and Scientific Exploration, 1837-1860" (PhD dissertation, University of Massachusetts, 1973); George M. Brooke, *John M. Brooke, Naval Scientist and Educator* (Charlottesville: University Press of Virginia, 1980); Marc Rothenberg, "In Behalf of the Science of the Country': The Smithsonian and the U.S. Navy in the North Pacific in the 1850s," *Pacific Science* 52: 4 (1998), 301-307; Ronald S. Vasile, Raymond B. Manning, and Rafael Lemaitre, eds., *William Stimpson's Journal from the North Pacific Exploring Expedition, 1853-1856* (Tokyo: Carcinological Society of Japan, 2005); Derek Hayes, *Historical Atlas of the North Pacific Ocean: Maps of Discovery and Scientific Exploration 1500-2000* (Seattle: Sasquatch Books, 2001), 146-151.

natural history of Japan seems self-evident. At least, it seems natural to assume that because the famous U.S.-Japan Expedition (or Perry's Japan Expedition) had *opened* Japan to the United States (by signing the Treaty of Kanagawa with the Tokugawa bakufu in 1854), Gray could readily dispatch Wright to fully explore Japan's flora with the NPEE and thereby provide further evidence in support of his good friend Charles Darwin's theory of evolution.<sup>56</sup> This assumption, however, is to overstate the diplomatic power that a Western expedition project could possibly deploy upon an Asian governing body. As Michael R. Auslin points out, the Treaty of Kanagawa was "nonrevolutionary" and "limited in nature" in terms of the bakufu's handling of Japan's foreign relations. According to Auslin, before Townsend Harris's signing of the Treaty of Amity and Commerce with Japan in 1858, the Treaty of Kanagawa did not "herald a revolution in Tokugawa diplomacy": that is to say, any foreigner who visited Japan at the time would be subject to all the regulations that a rather xenophobic government could impose upon visitors. How could Charles Wright collect in a country that was at that time notorious for its hostility toward foreigners? How did Wright-Gray's collecting enterprise get involved with the United States' scientific, diplomatic, and commercial interests in East Asia in the form of a scientific expedition?<sup>57</sup>

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<sup>56</sup> See, for example, A. Hunter Dupree, "Science vs. the military: Dr. James Morrow and the Perry Expedition," *Pacific Historical Review* 22, no. 1 (1953), 29–37; Jeffrey A. Keith, "Civilization, Race, and the Japan Expedition's Cultural Diplomacy, 1853–1854." *Diplomatic History* 35, no. 2 (2011), 179–202.

<sup>57</sup> Michael R. Auslin, *Negotiating with Imperialism: the Unequal Treaties and the Culture of Japanese Diplomacy* (Cambridge: Harvard University Press, 2004); the quotation is on p. 17; also noteworthy is Robert I. Hellyer's innovative study on Japan's foreign relations during the so-called *sakoku* period; Hellyer, *Defining*

Second, if we reexamine Gray's study of Japan's flora and the related subsequent Darwinian debates as an intellectual engagement associated with a governmental exploratory project, it seems that a whole new historiographical territory opens itself to our exploration. First and foremost are the studies on the relationships between Darwin's breakthroughs in biology and the famous *Beagle* voyage. Thanks to Darwin biographers such as Adrian Desmond, James Moore, Janet Browne, and Sandra Herbert, it becomes clear that the overarching goal of the *Beagle* voyage was by no means to carry Darwin around the globe to discover the principle of natural selection but to carry out a surveying project for the British Admiralty—in other words, Darwin's intellectual breakthroughs coincided tightly with the ever-expanding infrastructure of the British Empire.<sup>58</sup> Equally important, as studies by Jim Endersby, Iain McCalman, Adrian Desmond, and others have shown, the course of Darwin's career can easily exhibit resonances in the lives and careers of Victorian men of science like Joseph D. Hooker, Thomas Henry Huxley, and Alfred Russel Wallace.<sup>59</sup> In reviewing recent studies that have advocated a reconsideration of the

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*Engagement: Japan and Global Contexts, 1640-1868* (Cambridge: Harvard University Asia Center, 2009). In researching the intricate interrelationships among science, military matters, and diplomacy in the NPEE, I found my chief inspirations in the following works: Janet Browne, *Charles Darwin: Voyaging* (New York: Alfred Knopf Inc., 1995); Nathaniel Philbrick, *Sea of Glory: America's Voyage of Discovery: The U.S. Exploring Expedition, 1838-1842* (New York: Viking, 2003); Ken Alder, *The Measure of All Things: The Seven-Year Odyssey and Hidden Error That Transformed the World* (New York: Free Press, 2002).

<sup>58</sup> Adrian Desmond and James Moore, *Darwin* (London: Michael Joseph; New York: Viking Penguin, 1991); Browne, *Voyaging*; Sandra Herbert, *Charles Darwin, Geologist* (Ithaca: Cornell University Press, 2005).

<sup>59</sup> Endersby, *Imperial Nature*; Iain McCalman, *Darwin's Armada: Four Voyages and the Battle for the Theory of Evolution* (New York: Norton, 2009); Adrian

Darwinian Revolution, Vassiliki Betty Smocovitis claims that such biographical studies of Victorian men of science altogether manifest just how necessary it is to treat “practices of collecting and transporting specimens” as “vital objects of empire.” “How institutions like gardens, zoos, and private collections selected these specimens, and whether or not they were displayed, and in what manner they were displayed, are all part of understanding the 19th century context which fostered evolution, as is the interplay of local or ‘native’ knowledge with ‘elite’ scientific knowledge.”<sup>60</sup>

Unsurprisingly, an equally rich body of literature on the interrelationships among science, exploration, and collecting can be found in the history of American sciences. As early as the 1960s, esteemed historian William H. Goetzmann demonstrated that Americans’ “winning of the West” closely related to a group of topographical engineers and surveyors who had taken charge of a wide array of surveying projects such as the U.S.-Mexican Boundary Survey, the Pacific Railroad Surveys, and the California Geological Survey.<sup>61</sup> For Goetzmann, focusing on this group of specialists yielded fruitful historical insights. As he points out, by

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Desmond, *Huxley: From Devil's Disciple to Evolution's High Priest* (Reading: Addison-Wesley, 1997).

<sup>60</sup> Vassiliki Betty Smocovitis, “It Ain’t Over ‘Til It’s Over”: Rethinking the Darwinian Revolution,” *Journal of the History of Biology* 38, no. 1 (2005), 39. For a relevant discussion, see Sally Gregory Kohlstedt, “‘Thoughts in Things’: Modernity, History, and North American Museums,” *Isis* 96, no. 4 (2005), 586-601.

<sup>61</sup> William H. Goetzmann, *Army Exploration in the American West, 1803–1863* (New Haven: Yale University Press, 1959); Goetzmann, *When the Eagle Screamed: The Romantic Horizon in American Diplomacy, 1800–1860* (New York: Wiley, 1966); Goetzmann, *Exploration and Empire: the Explorer and the Scientist in the Winning of the American West* (New York: Knopf, 1966).

investigating American surveyors' pursuit of their projects, his research shows that exploration should be considered a culturally and socially "programmed" activity instead of a sequence of discoveries.<sup>62</sup> A similar appreciation of surveyors' and surveying projects' roles in science can be found in Robert E. Kohler's *All Creatures: Naturalists, Collectors, and Biodiversity, 1850–1950* (2006). Although there Kohler's concern is less about the winning of the American West than about the discovery of the world's biodiversity, his research regarding the U.S. Biological Survey reveals how the seemingly "random and individualistic" efforts of several dozen limited-scale surveying projects of the American West were "synchronized by larger cultural, economic, and social trends." Furthermore, according to Kohler, it is necessary for historians of science to make a distinction between Humboldtian collecting (1830s–1850s) and survey collecting (1880s–1920s). The former, Kohler argues, despite its conspicuity in the public's mind, achieved little in terms of scientific understanding of biodiversity. For Kohler, it is the surveying collecting—for its ultimate emphasis on the systematic, comprehensive, and intensive manner of compiling field data—that generated what can be called "exacting sciences" and for the first time unveiled the richness of American biota to the world of science.<sup>63</sup>

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<sup>62</sup> Goetzmann, *Exploration and Empire*, xi.

<sup>63</sup> Kohler, *All Creatures*, Chapter 1; the quotation is on pp. 5-6. On studies relevant to Kohler's "Humboldtian collecting," see, among others, Aaron Jacob Sachs, *The Humboldt Current: Nineteenth-Century Exploration and the Roots of American Environmentalism* (New York: Viking, 2006); Laura Dassow Walls, *The Passage to Cosmos: Alexander von Humboldt and the Shaping of America* (Chicago: University of Chicago Press, 2009).

Another body of relevant literature rests on studies of scientific imperialism with a particular geographical focus on East Asia.<sup>64</sup> A short essay written by Dupree and published in 1953 shows how Gray's interest in Japan's flora benefited from the specimens of Commodore Perry's Japan Expedition and yet simultaneously encountered obstacles in the form of Perry's reluctance to collaborate with civilian men of science like Gray. Then, in his biography of Gray, Dupree summarizes the goals and trajectories of the NPEE and emphasizes Gray's close association with the expedition. In his opinion, because of the NPEE's role in establishing the United States' position in the world of science, as well as the United States' niche in the world of commerce, it seems appropriate to treat Gray's 1859 essay on Japan's flora as a "peculiar American document"—even though Gray acquired his theoretical framework mostly from Charles Darwin and Joseph D. Hooker.<sup>65</sup>

Yet, in light of recent studies regarding scientific imperialism in East Asia, it seems worthwhile to reconsider Dupree's above acknowledgment. Here a particularly relevant body of literature is Fa-Ti Fan's series of studies on Victorian naturalists in Qing China. Fan's major argument is that the framework that

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<sup>64</sup> Regarding the relationship between botany and imperialism, see, among others, Londa Schiebinger and Claudia Swan, eds. *Colonial Botany: Science, Commerce, and Politics in the Early Modern World* (Philadelphia: University of Pennsylvania Press, 2005); Daniela Bleichmar, *Visible Empire: Botanical Expeditions & Visual Culture in the Hispanic Enlightenment* (Chicago: University of Chicago Press, 2012); Drayton, *Nature's Government*. On the United States' scientific imperialism, see, for example, Camilo Quintero, "Trading in Birds: Imperial Power, National Pride, and the Place of Nature in U.S.-Colombia Relations," *Isis* 102, no. 3 (2011), 421-445; Warwick Anderson, "Hybridity, Race, and Science: The Voyage of the *Zaca*, 1934–1935," *Isis* 103, no. 2 (2012), 229-253.

<sup>65</sup> Dupree, "Science vs. the military" and *Asa Gray*; the quotation is on *Asa Gray*, 263.

historians of science habitually apply to study science and colonialism does not work well in the case of mid- and late-nineteenth-century China. In his opinion, the Chinese Qing Empire's multiple unequal treaties with the British Empire did not prevent the former from maintaining a remarkable degree of autonomy—in other words, Victorian naturalists could hardly study China as freely as they might have studied Britain's colonies. So Fan introduced a modified concept of “informal empire.” What he meant by the “informality” of British scientific imperialism in China reflects the wide array of China-based scientific projects undertaken by British missionaries, medical doctors, soldiers, diplomats, and officers working in customs, in collaboration with local Chinese people. Only when historians of Chinese science appreciate networks that crisscross such conventional categories as East vs. West, metropolis vs. periphery, and the colonizer vs. the colonized, Fan argues, can historians become more capable of capturing the dynamic contexts in which “factual” or “useful” knowledge about China's natural history took shape. In this regard, another noteworthy body of research is Erik Mueggler's work on botanical explorations in southwestern China during the first half of the twentieth century. Mueggler, a distinguished anthropologist specializing in the cultural history of minority communities in Yunnan and Sichuan, vividly shows how local people's knowledge about the origins and distributions of living beings was integrated into the botanical discoveries made by George Forrest and Joseph Francis Charles Rock.<sup>66</sup> In historiographical terms, both Fan's and Mueggler's studies belong to a

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<sup>66</sup> Fa-Ti Fan, *British Naturalists in Qing China: Science, Empire, and Cultural Encounter* (Cambridge, MA: Harvard University Press, 2004); Fan, “Science in Cultural Borderlands: Methodological Reflections on the Study of Science, European



growing body of research on lay people's approaches to carrying out scientific observation, the relationships between metropolitan naturalists and their local collaborators, the arguable distinctions between professionals and amateurs in natural historical studies, and so on.<sup>67</sup> In light of this remarkable scholarship, if we come back to Dupree's statement that Gray's 1859 essay should be considered a "peculiar American document," we are led to ask how peculiar this document might appear if we were to account for Wright's collecting practices in Japan. Did Wright rely on local Japanese people to collect plants for him? To what extent did Japanese people's local knowledge about their flora give shape to Wright's collecting practices?

Regarding these historiographical inquiries, historians of science are fortunate because Wright was an ardent letter-writer and many of his letters survive (though scattered across more than ten archives). As far as Wright's botanical collections are concerned, a valuable collection of Wright's letters can be found at the Gray Herbarium (which also includes Gray's letters to Wright). This collection allows historians of science and botanists alike to answer such questions as when and where Wright made his collections. Yet, if we follow Kohler's suggestion and pay more attention to Wright's collecting practices (instead of Wright's collections), then

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Imperialism, and Cultural Encounter," *East Asian Science, Technology, and Society: An International Journal* 1, no. 2 (2008), 213-231; Erik Mueggler, *The Paper Road: Archive and Experience in the Botanical Exploration of West China and Tibet* (Berkeley: University of California Press, 2011).

<sup>67</sup> See, among others, Jeremy Vetter, "Lay Participation in the History of Scientific Observation," *Science in Context* 24, no. 2 (2011), 127-141; Endersby, "From Having No Herbarium"; Bleichmar, *Visible Empire*.

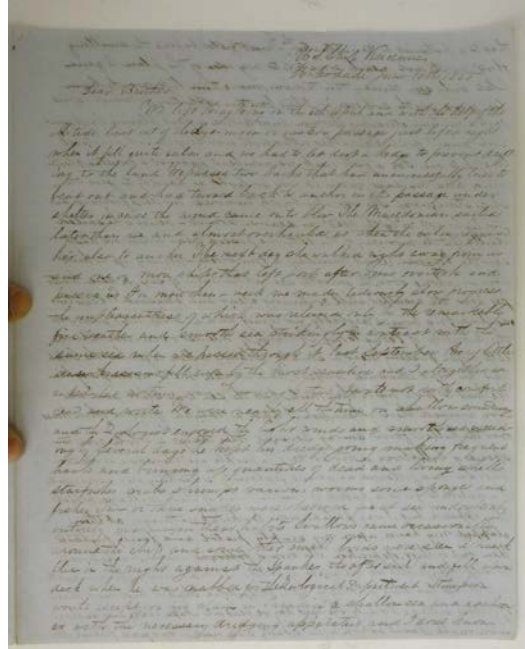


Figure I-7. The first page of a letter Wright sent home from Hakodate in 1855. (Courtesy of the Connecticut State Library, Hartford, Connecticut, USA.)

we find another invaluable archival source in a collection of Wright's letters to his family, now preserved at the Connecticut State Library.<sup>68</sup> Wright produced nearly one hundred thousand words in some thirty-eight letters to his family during his three-year service in the NPEE (Figure I-7). Chiefly addressing his brother John, Wright expected that his letters would be read aloud by John in the company of their widowed mother and two unmarried sisters. Hence, he packed these letters with vivid details about what he had experienced in the foreign land of Japan. (After all, that was a period when travelers could neither purchase postcards nor take pictures.) Here it is worth noting that so far no researcher has made use of Wright's family letters to study the NPEE or Gray's disjunction thesis. For that matter, Part III relies heavily on and quotes intensively Wright's words in these family letters to

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<sup>68</sup> Kohler, "Finders/Keepers."

reveal how Wright discharged his duty as a botanist in the NPEE. I argue that Wright's letters suggest that the NPEE should be considered one of the first governmental projects that highlighted the spirit of exacting science. Also, although Wright mentioned only part of such field details to Gray, I argue that Wright's experience in the field and his collecting practices framed the ways in which Gray developed his disjunction thesis, and thereby framed the course that the so-called first Darwinian debate would take in the years to come.

Furthermore, it is worth noting that nearly all studies about the NPEE have failed to use sources in Japanese. In fact, if we examined such printed archival sources as *Bakumatsu gaikoku kankei monjo* 幕末外國關係文書 (*Documents on Diplomacy during the Late Bakufu Period*), we would be surprised by the great excitement that the NPEE provoked at various levels of the Japanese government of the day (i.e., the Tokugawa bakufu).<sup>69</sup> As Yoshida Kensuke 吉田賢輔 (1838–1893) and Tanabe Taichi 田邊太一 (1831–1915) remark in their respective memoirs, the appearance of the NPEE squadron on Japan's shore surely was one of the most significant diplomatic affairs triggering the transformation of Japan into a modern state. According to these two commentators, the NPEE's importance was comparable to Commodore Perry's opening of Japan and Townsend Harris's signing of the Treaty of Amity and Commerce with Japan (1858).<sup>70</sup> Likewise, printed

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<sup>69</sup> Tōkyō Daigaku Shiryō Hensanjo 東京大学史料編纂所, ed., *Bakumatsu gaikoku kankei monjo* 幕末外國關係文書 (Tokyo: Tōkyō Daigaku Shiryō Hensanjo, 1972–).

<sup>70</sup> Tanabe Taichi 田邊太一, *Bakumatsu gaikō dan* 幕末外交談 (Tokyo: Fuzanbō, 1898), 21–31. A brief but useful discussion about the NPEE's significance in Japan's diplomatic history can be found in Makabe Jin 眞壁仁, *Tokugawa kōki no gakumon*

primary sources like *Ryūkyū Ōkoku Hyōjōsho monjo* 琉球王国評定所文書 (*Documents of the Ryukyu Royal Government*) also include fascinating—and sometimes disturbing—details about how Ryukyu authorities and common people conceived of the visits paid by the NPEE squadron.<sup>71</sup> These types of sources, in conjunction with sources concerning the NPEE in the National Archives, the Smithsonian Institution Archives, and the Library of Congress, have enabled me to construct a less heroic and less linear narrative than has been customary regarding the contexts in which scientific discoveries were made during the NPEE. By the same token, Part III uses local records to show that scientific discoveries achieved by the NPEE (Gray's examination of Japan's flora included) can hardly be considered a natural result of U.S. gunboat diplomacy or scientific imperialism. As Auslin points out, Japanese diplomatic officers of the late Tokugawa period, despite their far inferior bargaining power, deployed multiple strategies to gain the upper hand during their negotiations with foreign powers over Japan's international status.<sup>72</sup> Indeed, as we will see, despite Commander Rodgers' preference for displaying arms as “a passport to observation,” the Japanese authorities had their own method of dealing with this American Commander—a method that set aside Japanese swords in favor of oral arguments and persuasion.

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*to seiji: Shōheizaka Gakumonjo jusha to bakumatsu gaikō hen'yō* 徳川後期の学問と政治: 昌平坂学問所儒者と幕末外交変容 (Nagoya: Nagoya Daigaku Shuppankai, 2007).

<sup>71</sup> Urasoe-shi Kyōiku Iinkai 浦添市教育委員会, ed., *Ryūkyū Ōkoku Hyōjōsho monjo* 琉球王国評定所文書 (Urasoe-shi: Urasoe-shi Kyōiku Iinkai, 1988–2003).

<sup>72</sup> Auslin, *Negotiating with Imperialism*.

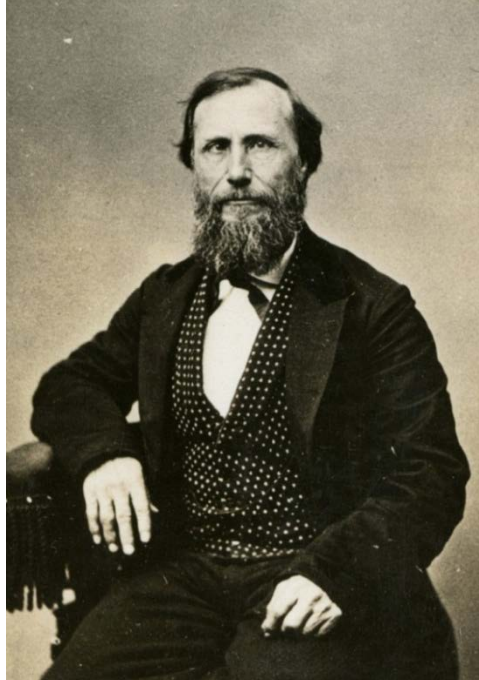


Figure I-8. Charles Wright at rest. (Courtesy of the Archives, Harvard University, Cambridge, MA, USA.)

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Beginning this introduction by juxtaposing the portraits of Gray and Wright, I want to conclude it with a pair of photos (Figures I-8 and I-9).

This photo of Wright was taken when Wright was on a collecting expedition to Cuba. For my part, when I examine the photo and conjecture about what kind of person Wright might have been, I cannot help noticing Wright's formidable arms and smiling eyes (which are not properly aligned with each other). I also notice Wright's effort to squeeze a smile. I feel sympathetic about it. In fact, according to one of Wright's letters to his family, a portion of Wright's tooth had decayed and then "crumbled to pieces" during his participation in the NPEE. (The process, in



Figure I-9. Charles Darwin “in motion.” (The photo comes from Down House, English Heritage; courtesy of Cambridge University Library.)

Wright’s words, did not give him “any pain or uneasiness,” except for “the queer sensation of a great hole apparently large enough to accommodate half a dozen teeth.”<sup>73</sup>) I also recall what Gray had said to Baird of the Smithsonian: Wright was rough, but was “a kind, considerate, truly benevolent man, probably unselfish.”<sup>74</sup> This photo of Wright is made more revealing when we compare it with the other “Charles” who figured into the Gray-Agassiz’s debate of 1859 and 1860. Figure I-7 shows a photo of Charles Darwin on his horse “Tommy” outside Down House. (Darwin wrote underneath a copy of this photo “Hurrah—no letters today!”<sup>75</sup>) Well known among Darwinian scholars, the photo has been discussed in different

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<sup>73</sup> CW to “Mine good Sister Molly,” 2/5/1855, *CWC*.

<sup>74</sup> AG to SFB, 4/15/1865, Folder 18, Box 22, *CM*.

<sup>75</sup> Browne, *The Power of Place*.

historiographical contexts, but here I want to highlight Jim Secord's remark in his lecture on "Global Darwin" (delivered as part of the Darwin College Lecture Series on January 30, 2009). In Secord's opinion, this photo shows a Darwin "potentially" in motion. "This is a Darwin that actually gives us a sense Darwin himself and Darwinism might actually move around," he comments. The point Secord aims to clarify is how inappropriate it is for historians of science to study the dissemination of Darwinism according to national boundaries. He suggests that historians of science closely examine how people communicate by networks of letters, prints, talks, and so on. Indeed, as he puts it in his widely cited "Knowledge in Transit," historians of science "need to come to terms with diversity by understanding science as a form of communication."<sup>76</sup>

My purpose in juxtaposing these two photographs is to illustrate what I consider a major shift in focus in the history of natural history and biology alike. As I indicated previously, there are two Charleses who helped Gray initiate the debates surrounding the origin and distribution of species in the United States. Recognizing the first Charles's role in initiating "the first Darwinian debate in the United States," this dissertation seeks to foreground another much less known Charles. By closely investigating Charles Wright's collecting practices, I want to make the following point clear: that a close examination of a collector who, after a demanding field trip, sits quietly, writes down his or her field observations, groups specimens, checks out reference books, writes letters, and the like, could yield historical insights as fruitful

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<sup>76</sup> The video of Secord's talk can be found at <http://sms.cam.ac.uk/media/526546> (the part on Darwin's "Hurrah" photo begins roughly in 28min 23sec); Secord, "Knowledge in Transit," 654.

as a study of a gentleman naturalist who for various reasons cannot embark on an expedition, but eagerly pushes his or her thoughts to travel across national and geographical boundaries through multiple means of communication. This dissertation belongs to a collective and increasingly conspicuous effort among historians of science to equate collecting practices with laboratory practices, to break up the boundary that has been taken for granted between the collector in the field and the naturalist in the closet, and to consider the history of collecting sciences a field worthy of analysis in its own right. As Kohler puts it in the beginning of *All Creatures*, natural-history collecting “is not one activity but a diverse family of practice,” and this family of practice constitutes “a subject ripe for historical survey.”<sup>77</sup>

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<sup>77</sup> Kohler, *All Creatures*, 15.



**PART I**  
**BOTANIST**

And what is Genius but finer love, a love impersonal, a love of the flower and perfection of things, and a desire to draw a new picture or copy of the same?

— Ralph Waldo Emerson

## CHAPTER ONE

### To Be a Botanist

If there was a watershed moment in his life, Professor Asa Gray believed that it took place in the spring of 1828. One day he rambled about a bare wood and glimpsed a delicate flower with five pale petals “peeping through dead leaves.” He was intrigued. He brought this fragile piece of vegetation home and studied it with Amos Eaton’s *Manual of Botany*. He recognized its name as *Claytonia virginica*.<sup>1</sup> This sequence of discovering, collecting, and classifying proved inspiring. As Gray put it afterward, he then went on “collecting and examining all the flowers I could lay hands on,” and began a herbarium of “shockingly bad specimens” (Figure 1-1).<sup>2</sup>

The desire to improve his botanical knowledge and skills eventually led him to contact John Torrey, a professor teaching various subjects (chiefly chemistry) in New York, a botanist reputed for his generosity toward budding naturalists, the ambitious author of *A Flora of the Northern and Middle Sections of the United States* (1824), and above all a radical who introduced the so-called natural system to the United States, thus challenging the seemingly unquestionable authority of Carl Linnaeus’s artificial system in the country (Figure 1-2). Around two years after he

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<sup>1</sup> Afterward Gray corrected that the plant actually belonged to *C. caroliniana*. “[T] he two [*C. virginica* and *C. caroliniana*] were not distinguished in that [Eaton’s] book,” he explained. *LAG*, 14-15. Also see Emily W. Wood, “Gray’s Herbarium to the Gray Herbarium to the Harvard University Herbaria,” *Harvard Papers in Botany* 15, no 2 (2010), 339-340.

<sup>2</sup> *LAG*, 14-15.



Figure 1-1. The experience of making this specimen of *Claytonia caroliniana* might have provoked Gray to become a botanist. (Courtesy of the Gray Herbarium, Harvard University, Cambridge, MA, USA.)



Figure 1-2: Gray (left) and Torrey (right) in the early 1840s. (Courtesy of the Library [left] and the Archives [right] of the Gray Herbarium, Harvard University, Cambridge, MA, USA.)

encountered *Claytonia virginica*, Gray carried his crates of specimens, procured a letter of introduction from his professor, drove some ninety miles from Bridgewater to Albany, took the steamer to New York, knocked on the door of Torrey's house in Charlton Street, and expressed his wish to meet Torrey in person. Then, learning that Torrey was out of town, he left the specimens and the letter, returned home and waited. Torrey's reply soon came. In it Torrey reviewed Gray's specimens and admired the medical student's specimen-making skills. At the end of the letter, he made a proposal that would change his and Gray's careers alike.

If you continue to prosecute Botany & collect specimens, may I ask the favour of another supply of the following species, as I desire them for my foreign correspondents... Please inform me in your next whether you are desirous of obtaining the plants of this region, & whether you take any interest in exotic botany for my duplicate Herbm is now tolerably rich, & might perhaps afford some species not at present in your herbarium. It will always give me pleasure to supply you with the peculiar production of this vicinity & New Jersey, or with good specimens of European plants if you desire them.<sup>3</sup>

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<sup>3</sup> Quoted from Rodgers III, *John Torrey*, 91.

Gray was encouraged. "If you should be desirous of additional specimens," he replied, "I will supply you in the course of next summer."<sup>4</sup> In 1831, upon his graduation, he made an excursion as far west as Niagara Falls, Buffalo, Aurora, and Ithaca. Freeing himself from the heavy atmosphere of the medical school, he breathed the fresh air, marveled at all forms of living beings, collected botanical specimens, compiled field information, made friends, and eagerly communicated his observations to Torrey.<sup>5</sup> Torrey was delighted. As a professor with a meager salary, he did whatever he could to furnish this budding botanist's outfit, including sending Gray a microscope. "I hope it will enable you to make a great number of useful discoveries," he wrote indulgently as he asked Gray to accept the gift. "Little can be done in Philosophical Botany without such an instrument for the knowledge that is acquired by a mere superficial examn is scarcely worth possessing."<sup>6</sup>

Returning from the graduation trip, Gray found his professional life dreary. To him collecting botanical specimens for botanists became more interesting than selecting botanicals for patients. "The practice of medicine," he wrote to Torrey in April 1832, had ultimately excluded him from "paying any further attention to natural history." Pondering how he could balance the two, the young physician wrote that he had found himself being intrigued by the climate and "botanical and mineralogical riches" of Mexico. "Has the country [Mexico] been explored by any botanist since Humboldt in 1803?" he wrote enthusiastically. "I am young (twenty-

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<sup>4</sup> AG to JT, 1/1/1831, *LAG*, 33-34.

<sup>5</sup> Rodgers III, *John Torrey*, 94.

<sup>6</sup> JT to AG, 4/29/1831, *HL*.

one), without any engagements to confine me to this section of country, and prefer the study of botany to anything else.”<sup>7</sup>

The twenty-one-year-old Gray’s ambition to follow Humboldt’s step to Mexico did not last, however. It was New York—instead of Mexico—that offered opportunities for the retiring physician to devote himself entirely to botany. So Gray remained in New York. Having abandoned his profession, he earned his daily bread by serving as a natural-history teacher and as a curator at various institutions (the Utica Gymnasium, the Hamilton College, the New York Lyceum of Natural History, to name a few). Torrey was touched. He decided to appoint Gray to work with him on a volume titled *Synopsis of North American Plants*. He told Gray that he was unable to pay even a modest salary, but Gray could live with his family, use his herbarium and library, and from there Gray could get his foothold. Gray accepted the offer with reluctance. He wanted to be independent, but understood that Torrey’s offer was the chance to establish himself in the world of botany. “I am writing two scientific articles on a difficult branch of botany for a scientific journal or magazine, which will give me a little notoriety,” he soon wrote home. He hoped that he could acquire “a reputation that must sooner or later secure me a good place.”<sup>8</sup>

Gray made his first attempt to make a living as a botanist early in 1834. He selected the best specimens from his collections of *Gramineae* and *Cyperaceae*, mounted them on good sheets of paper, gave each specimen a label indicating its distribution, scientific name, habitat, and other relevant information, bound them

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<sup>7</sup> AG to JT, 4/6/1832, *LAG*, 36.

<sup>8</sup> AG to “his father,” 11/21/1834, *LAG*, 48.

together, and looked for persons or institutions who were interested in subscribing to such sets of specimens (the “grass book,” as Gray called it).<sup>9</sup> Torrey supported Gray’s publishing project wholeheartedly. In February, when Gray was busily selecting, mounting, and labeling specimens, he wrote to his friend Constantine Samuel Rafinesque (1783-1840) about Gray’s project. “Did I tell you my friend and assistant, Dr. Gray was about publishing 1st No. of his North American Gramineae and Cyperaceae?” he wrote. “Each number is to contain 100 good specimens fastened on handsome white paper bound in neat covers, with printed labels, title page, index etc. the price is 5 dollars per number... I think his prices are low.”<sup>10</sup>

Torrey also communicated Gray’s publishing project to Sir William Jackson Hooker, Regius Professor of Botany at Glasgow University. Sir William, then the most renowned author of the North American flora, maintained his intellectual status chiefly by corresponding and collaborating with American botanists. He promptly composed a notice about Gray’s project and circulated it through his *Companion to the Botanical Magazine*. For it was the first time that Gray’s name was made known to scientific communities at large, he carefully set a stage to introduce Gray to the scene. “If ever there was a period, when, more than at any other, a JOURNAL was required which might give an account of the progress of Botanical Science, it is surely the present,” he wrote passionately. “[W]e are very happy to be

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<sup>9</sup> On Gray’s “grass-book,” see Harold William Rickett and Charles Louis Gilly, “Asa Gray’s Earliest Botanical Publications (1833-1836),” *Bulletin of the Torrey Botanical Club* 69, no. 6 (1942), 461-470.

<sup>10</sup> JT to CSR, 2/5/1834, Nathan Reingold, *Science in Nineteenth-Century America: A Documentary History* (Chicago and London: University of Chicago Press, 1964), 44.

able to announce that," he went on, Torrey's "valuable assistant" Dr. Gray had just published "Specimens illustrative of the Grasses and Cyperaceae of North America." Gray's volume might "fairly be classed among the most beautiful and useful works of the kind that we are acquainted with," and was "now on sale here, as well as in America."<sup>11</sup>

But it seemed that selling sets of specimens at the rate of \$5 per hundred specimens could not afford Gray a decent life. Torrey was concerned and tried to secure a position for Gray at Princeton College. He failed. Gray was undiscouraged. "It is unpleasant to be embarrassed in such matters," he wrote home in February 1835, "for I should like much to be independent, and this with my moderate wishes would require no very large sum, and I have no great desire to be rich."<sup>12</sup>

Five years after he began corresponding with Torrey, Gray made a decisive step in his botanical career. He decided to publish a botanical textbook. "As to my book," he wrote home in September 1835, "I am trying to make a bargain with two publishers; the prospects seem pretty fair, and I shall probably get \$300..." He had been "determined to persevere for a little while yet," before abandoning "all hopes from science as a pursuit for life," the son declared. "I have now, and expect to have,

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<sup>11</sup> William J. Hooker, *Companion to the Botanical Magazine; Being a Journal Containing such Interesting Botanical Information, as Does Not Come within the Prescribed Limits of the Magazine; with Occasional Figures, Vol. 1* (London: Edward Couchman, 1835-1836), 14.

<sup>12</sup> AG to "his mother," 2/7/1835, LAG, 48-49.



a great many discouragements, but I shall meet them as well as I can, until it shall seem to be my duty to adopt some other profession for my daily bread.”<sup>13</sup>

### **Elements of Botany**

Gray’s textbook was published in 1836, under the ambitious title *Elements of Botany*.<sup>14</sup> To contemporary readers, the volume not only presented Gray’s resolution to make a living as a botanist, but also demonstrated his ambition to take on some thorny issues that had intrigued and plagued the U.S. botanical community. To be sure, Gray’s struggles to make a living as a botanist did not mean that botany was not a subject relevant to people’s daily lives. The economy of the United States of the early nineteenth century relied heavily on the country’s botanical wealth, and society at large believed that botany held the key to the country’s prosperity. Early in the 1830s, a message regarding “how to preserve vegetables from degenerating” was made widely known through popular agricultural magazines, including *The New England Farmer*, and *Horticultural Journal*:

Never plant gourds near squashes, lest you make the squashes bitter. Never plant squashes near pumpkins, lest you cause both sorts to degenerate. Never place different sorts of seed cabbages near each other, lest create a mixture, nameless and not worth nameing [*sic*]. Never plant seed turnips near seed cabbages, lest both should be spoiled. Never plant sorts of seed beets, seed radishes, &c, near other; and as a general rule, never plant near one another vegetables which resemble each other in some respects, but having qualities which should be kept distinct...

*The New England Farmer* went on to quote botanist and horticulturist John Claudius

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<sup>13</sup> AG to “his father,” 9/28/1835, *LAG*, 54-55.

<sup>14</sup> Asa Gray, *Elements of Botany* (New York: G. & C. Carvill & Co, 1836).

Loudon (1783-1843)'s opinions, claiming that "Loudon says, the great art of the seed gardener is to grow seeds true to their kind, for which purpose, one grower must not attempt too many varieties of the same species; but he may grow a number of different species, provided they do not come into flower at the same time." These methods to correctly distinguish species and "grow seeds true to their kind" were not based on groundless speculation, but were fundamental issues with which every enterprising farmer was supposed to familiarize themselves.<sup>15</sup>

Similar interest in botanical matters was not confined to farms or gardens but could also be found in the country's seemingly infinite wilderness. An individual who played an important role in fostering such interests was Daniel Drake (1804-1859), editor of *The Western Journal of the Medical and Physical Sciences* and Professor of Medical Medicine in the Medical College of Ohio. Early in the 1830s, Drake contacted John L. Riddell (1807-1865), a botanist who had been studying the Ohioan flora for years, inviting him to contribute an essay on plant collecting to *The Western Journal*. In 1836, when publishing Riddell's essay, Drake added a note to clarify his purpose: "Every day diminishes the number of individual specimens, and, perhaps, every year annihilates several species of the plants indigenous to the Valley of the Mississippi." Finding such remarkable diminishment and annihilation of

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<sup>15</sup> Anonymous, "To Preserve Vegetables from Degenerating," *The New England Farmer, and Horticultural Journal* 10, no. 44 (1832), 350.

species “a matter of the deepest interest,” Drake noted, he hoped that Riddell’s essay would encourage medical students to further the subject.<sup>16</sup>

One botanist who made good use of (and, in some degree, capitalized on) the public’s interest in botany was Amos Eaton (1776–1842), senior professor of botany at the Rensselaer School, New York. A popular teacher in natural history, Eaton cultivated a whole generation of American botanists. Eaton also extended his influence by publishing textbooks. His *Manual of Botany for North America*, first published in 1817, was a standard text widely used in high schools. Eaton was an enthusiastic advocate of Linnaeus’s artificial system. To him, its simplicity (grouping plants into twenty-four classes by examining their reproductive organs) ensured botany’s popularity, thus elevating the nation’s scientific level.<sup>17</sup>

But no later than the 1820s, Eaton had become deeply worried about the increasing popularity of the “natural system” among botanists. Developed and refined by Antoine Laurent de Jussieu and Augustin Pyramus de Candolle, the natural system highlighted a more physiologically or “naturally” based classification in contrast with Linnaeus’s artificial system.<sup>18</sup> On the American side of the Atlantic, it was Constantine Samuel Rafinesque who eagerly promoted the superiority of the

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<sup>16</sup> See John L. Riddell, “Particular Directions for Collecting and Preserving Specimens of Plants, Extracted from an Unpublished Treatise on Practical Botany,” *The Western Journal of the Medical and Physical Sciences* 8, no. 1 (1835), 18.

<sup>17</sup> Ethel M. McAllister, *Amos Eaton, Scientist and Educator, 1776-1842* (Philadelphia: University of Pennsylvania Press, 1941).

<sup>18</sup> Regarding the history of the natural system, see Peter F. Stevens’s influential *The Development of Biological Systematics: Antoine-Laurent de Jussieu, Nature, and the Natural System* (New York: Columbia University Press, 1994).

natural system. In his 1820 lecture on botany at Transylvania University, he argued that the Linnaean system produced “many anomalies and irregularities,” thus prompting the “best botanists” to familiarize themselves with the “natural method or classification.”<sup>19</sup> Eaton was annoyed by Rafinesque’s attack on the Linnaean system. Why didn’t Rafinesque “give up that foolish European foolery,” he exclaimed to Torrey. This so-called natural method or classification would not lead Rafinesque anywhere but to “treat Americans like half-taught school boys.”<sup>20</sup>

So Eaton was vexed to learn that Torrey was turning his back on the Linnaean system and would become an ardent follower and advocate of the natural system. Early in the 1830s, when preparing the sixth edition of *Manual of Botany for North America* (1833), he exclaimed that “Since Dr. Faustus first exhibited his printed bibles in the year 1463, no book has probably excited such consternation and dismay as Dr. Torrey’s edition of Lindley’s Introduction to the Natural System of Botany.” Moreover, he added, “to make the horrors of the students as well as the teacher, still more appalling, Dr. Torrey’s Catalogue of American Plants at the end of his Lindley was so singularly presented that it would seem to indicate an awful catastrophe to all previous learning.” Eaton went on to claim that he had made himself “thoroughly acquainted with Lindley, Hooker, Loudon, and the four

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<sup>19</sup> Charles Boewe, ed., *A C. S. Rafinesque Anthology* (Jefferson: McFarland, 2005), 125.

<sup>20</sup> Quoted from Leonard Warren, *Constantine Samuel Rafinesque: A Voice in the American Wilderness* (Lexington: University Press of Kentucky, 2004), 60. A notable analysis of Rafinesque’s classification can be found in Jim Endersby, “The Vagaries of a Rafinesque’: Imagining and Classifying American Nature,” *Studies in History and Philosophy of Science Part C* 40, no. 3 (2009), 168-178.

published volumes of De Candolle.” Based on his intensive and extensive study, he claimed, “I do not believe that either of these authors has published any improvement useful to the student in North American Botany, which he will not find in this book, excepting the physiological part.” Eaton explained why he had refused to “*servilely*” follow de Jussieu’s system:

... he imposes on his readers the labor of learning a multitude of new names, without even a shadow of pretence [*sic*]. Then to sustain him in his unnecessary novelties, he flatters his adherents by adopting all their ridiculous names and whimsies, and by holding them up to the world as eminent botanists. It is certainly the duty of every one, who is desirous to see the state of society improved by the introduction of the study of Nature among common citizens, to oppose the introduction of an overwhelming host of new and unutterable names, and to load our book-shelves with a still more frightful terminology. Those who feel a jealousy at the encroachment of common school-teachers upon the sanctity of the learned professor’s chair, are more excusable for following innovations which are calculated to embarrass the student. As far as I have any influence I pledge it here, that the embarrassing innovations of De Candolle and others, are of no *possible use* to the science of Botany.<sup>21</sup>

Eaton also grew worried that growing numbers of botanists now preferred to cage themselves at herbaria and believed that the so-called natural affinities among species would reveal themselves through the study of dried specimens. He considered that this fancy was one of the gravest consequences that the natural system had caused. “Great numbers of errors are yearly added to our original stock, by closet-authors,” Eaton remarked, “who rely upon the dried specimens of the herbarium for their new names and new descriptions.” In fact, he continued, “The business of collecting and preserving plants is the only way to make a good botanist,” and the herbarium, therefore, “after being made, serves to revive our

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<sup>21</sup> Amos Eaton, *Manual of Botany for North America: Containing Generic and Specific Descriptions of the Indigenous Plants and Common Cultivated Exotics, Growing North of the Gulf of Mexico (6th Edition)* (Albany: Oliver Steele, 1833), iii-v.

recollections of growing plants; but is far from being a faithful guide in drawing descriptions and making nice distinctions.” Finally, from his “twenty-five years of experience,” Eaton put forth a criterion for what being a botanist meant: “no one can be a botanist, until he collects, in fields and woods, at least 600 species of plants.”<sup>22</sup>

But Eaton did not offer in his *Manual* a manual for botanical collecting and preservation (“I have no room to discuss the subject”). Charles W. Short, Professor of Materia Medica and Medical Botany at Transylvania University, was among Eaton’s botanical colleagues who aspired to fill what Eaton had not addressed. In 1833, probably in response to requests made by fellow botanists about how to make specimens of good quality (Short was well known for his specimen-making skills), he published an essay titled “Instructions for the Gathering and Preservation of Plants for Herbaria,” in *The Transylvania Journal of Medicine and the Associate Sciences*. Composing his essay in a form of a letter to “a young botanist,” Short made it clear that “no one can be a botanist without collecting plants and making up an herbarium with his own hands.”

...no pleasure can exceed that of an enthusiastic botanist, when thrown into the midst of the wilds of nature; and whilst his senses revel on the beauties of “herb and fruit and flower,” his bodily powers participate in the invigorating enjoyment; for by this healthful exercise

O'er hills, through vallies and by mountain tops,  
Is life both sweetened and prolonged.<sup>23</sup>

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<sup>22</sup> Eaton, *Manual...*, viii-ix.

<sup>23</sup> Charles W. Short, “Instructions for the Gathering and Preservation of Plants for Herbaria,” *The Transylvania Journal of Medicine and the Associate Sciences* 6, no. 1 (1833), 60-61.

As for how to arrange plants in the herbarium, he noted two approaches: “One party—(for there are parties in Botany as well as in every thing else,) direct the species to be arranged according to natural order of Jussieu, and another prefers the artificial system of Linnaeus.” For his part, Short claimed that he “decidedly” preferred the Linnaean system, and highly recommended any beginner in botany to do so. “After having become more deeply initiated into the general principles and philosophy of science, by enlarging your knowledge of plants, you will be the better enabled to appreciate the advantages, if it really has any, of the natural method, as it is called; and may then, if you prefer it, very easily modify your collection according to its rules.”<sup>24</sup>

Short’s advocacy of the Linnaean system provoked John L. Riddell’s immediate response. In his essay published in *The Western Journal*, Riddell claimed that he could hardly “concur with Dr. Short in advising beginners to marshal their plants according to the system of Linnaeus.” In his opinion, arranging specimens according to the Linnaean system was akin to putting them in the “alphabetical order.” After all, he pointed out, “The Linnaean system is a contrivance for the sole purpose of conducting us easily to the name of a plant; and that once found, we have no farther use for it.” By contrast, he continued, “The professed object of the *natural method*, is to follow nature; to recognise those great orders or tribes making up the vegetable creation, which are distinguished by differences of general organization.” By applying the natural system to arrange his herbarium, concluded Riddell, “the student will then acquire correct notions of philosophical botany: he will learn to

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<sup>24</sup> Short, “Instructions...,” 69, 71.

appreciate those indications of relationship, which will often enable him upon first sight, to assign a plant he has never before seen its true place in the system, and to form a correct idea in regard to its hidden qualities.”<sup>25</sup>

What made the contrasts between the “closet botanist” and the “field botanist” as well as the artificial system and the natural system more complex was the publication of the first part of Constantine Samuel Rafinesque’s *New Flora of North America* (1836).<sup>26</sup> In Rafinesque’s words, his *New Flora* could be claimed new because it was arranged according to the natural system. But unlike most authors who practiced the natural system, Rafinesque was by no means a closet botanist, and *New Flora* indeed embraced some of the most fascinating accounts of field collecting that had ever appeared in the genre of flora.

Let the practical Botanist, who wishes like myself to be a pioneer of science, and to increase the knowledge of plants, be fully prepared to meet dangers of all sorts in the wild groves and mountains of America. The mere fatigue of pedestrian journey is nothing compared to the gloom of solitary forests, when not a human being is met for many miles, and if met he may be mistrusted; when the food and collections must be carried in your pocket or knapsack from day to day; when the fare is not only scanty but sometimes worse; when you must live on corn bread and salt pork, be burned and steamed by a hot sun at noon, or drenched by rain, even with an umbrella in hand, as I always had.

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<sup>25</sup> Riddell, “Particular Directions...,” 38-39.

<sup>26</sup> Constantine Samuel Rafinesque, *New Flora and Botany of North America, Being a Supplemental Flora, to the Various Floras and Botanical Works of Michaux, Muhlenberg, Pursh, Nuttall, Elliot, Torrey, Beck, Eaton, Bigelow, Barton, Robin, Hooker, Riddell, Darlington, Schweinitz, Gibbs, &c. Besides the General Works of Linnaeus, Willdenow, Vahl, Vitman, Persoon, Lamark, Decandole, Sprengel, Jussieu, Adanson, Necker, Lindley, &c. Containing Nearly 500 Additional or Revised New Genera, and 1500 Additional or Corrected New Species, Illustrated by Figures in Autikon Botanikon (First Part)* (Philadelphia: Printed for the Author and Publisher, 1836).



Just when readers pictured a botanist with an umbrella against the background of the magnificent or even surreal American landscape, Rafinesque went on to list an array of circumstances that would “vex,” “perplex,” “lame,” “overtake,” “annoy,” “surround,” or even kill a traveling botanist. But no “pure botanist” would be deterred, Rafinesque claimed. “You feel an exultation, you are a conqueror, you have made a conquest over Nature, you are going to add a new object or a page to science. This peaceful conquest has cost no tears, but fills your mind with a proud sensation of not being useless on earth, of having detected another link of the creative power of GOD.”<sup>27</sup>

But what astonished Rafinesque’s readers were not only such vivid accounts of field experiences but also the botanist’s ability to discover new species and genera in the field. In *Flora Telluriana*, for example, Rafinesque claimed that he would establish about 1,000 “totally new genera,” including some of those he had previously published. Then, in 1836, when Rafinesque published the first part of the *New Flora*, botanists were shocked to learn that Rafinesque had not only achieved the goal but also gone far beyond it. Besides 1,000 “totally new genera,” Rafinesque declared, the *New Flora* included “nearly 500 additional or revised new genera, and 1500 additional or corrected new species”!<sup>28</sup> No wonder even Rafinesque’s close colleagues would complain that he had been “too fond of novelty,” and the term

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<sup>27</sup> Rafinesque, *New Flora... (First Part)*, 12-15.

<sup>28</sup> Constantine Samuel Rafinesque, *New Flora and Botany of North America, or a Supplemental Flora, Additional to All the Botanical Works on North America and the United States. Containing 1000 New or Revised Species* (Philadelphia: [Printed for the Author and Publisher,] 1836-1838).

“Rafinesquism” would be coined to feature certain naturalists’ unsavory fondness for novelty, “the most foolsome and disgusting manner of speaking in one’s own praise.”<sup>29</sup>

Gray was aware of such flourishing interests in plant collecting, preservation and classification. He agreed that botanists should practice and perfect their specimen-making skills, because, “An imperfect and badly-dried specimen not only fails to exhibit the character of the species, but cannot be preserved for any length of time.” He disagreed with Eaton’s words that specimens served no purpose except for reviving “recollections of growing plants.” As he put it in *Elements of Botany*, “When it is considered that about 80,000 species are already known to botanists, it will be scarcely necessary to add, that no one can pretend to trust to his unassisted recollection, even in respect of plants which have frequently passed, under his observation.” Only by the “actual inspection of the plants in the Herbaria” were botanists able to clear up “many errors as to the identity of particular species.”<sup>30</sup>

Gray condensed his eight-year experience both in the field and at the herbarium into an essay titled “Of Collecting and Preserving Plants for An Herbarium” and included it in *Elements of Botany*. In the beginning of the essay, he explained why he had deemed it necessary to offer a guide for those who embarked on a botanical expedition: “we hear from our friends returned from a jaunt into perhaps a new and interesting part of the country remarks respecting the beautiful plants they casually met with, mingled with regrets that they had no possible means of securing

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<sup>29</sup> Quoted from Warren, *Constantine Samuel Rafinesque*, 60-61.

<sup>30</sup> Gray, *Elements...*, ix, 396.

specimens for their botanical acquaintance.”<sup>31</sup> This regret was predictable, he noted, and his essay was to offer a manual and a “checklist,” so that those “well-disposed” travelers and their “botanical acquaintances” could better contribute to advancing botanical sciences.

As the essay’s title implied, here Gray managed to prove that it was the herbarium—instead of the field—that made collecting a critical element in the making of a botanist. He hardly agreed that collecting was merely a “healthful exercise,” emphasizing that its successful undertaking demanded sufficient preparation and strict self-discipline. Before embarking on a collecting trip, he urged botanists to check meteorological data and maps, estimate the duration of the trip, familiarize themselves with collecting facilities, communicate with local botanists, and so on.<sup>32</sup> In the field, he suggested that botanists “gather whatever struck them upon their journey as new and unusual” and never forget to dry up the papers, sort out duplicate specimens, and compile field notes, so that “specimens of great interest to their botanical friends at home might be obtained at small cost of labor to themselves.”<sup>33</sup> There was no other way to make valuable specimens, he assured his readers. “From the want of proper attention in this respect, the collections of several of the older Botanists, made with great labor and expense, are now nearly valueless.”<sup>34</sup>

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<sup>31</sup> Gray, *Elements...*, 402.

<sup>32</sup> Gray, *Elements...*, 397-398.

<sup>33</sup> Gray, *Elements...*, 402.

<sup>34</sup> Gray, *Elements...*, ix.

In Gray's view, the true climax of any botanical expedition took place *after* the expedition concluded—that is to say, when a botanist settled at his or her herbarium, unpacked luggage, took out plants, mounted them on sheets of paper, examined them with botanical books, decided plants' names, labeled them, grouped them according to each plant's genus, and finally combined this new set of specimens with specimens that had been classified. Here he emphasized the importance of the natural system: "The natural method of arrangement proposes to express the mutual affinities of plants as far as possible; and to effect this object, its classes and other divisions are founded, not upon a single character or set of characters, but upon several; not part of the vegetable being neglected."<sup>35</sup> Gray was anxious to wrest the authority of Linnaeus from the likes of Eaton. Linnaeus himself had claimed that "the artificial method is but a substitute for the natural," and had made the following statement, Gray noted:

*... I have labored to establish it: I have made many discoveries, but have not been able to perfect it; yet while I live I shall continue to labor for its completion.—In the mean time I have published what I have been able to discover; and whosoever will resolve the few plants which still remain shall be my magnus Apollo.—those are the greatest Botanists who are able to correct, augment, and perfect this method: which those who are unqualified should not attempt.*<sup>36</sup>

As for the increasingly troublesome term "species," Gray offered what he considered to be the most orthodox, straightforward, and in some measure foolproof (and bulletproof) definition: namely, "A *species*... is defined to be the assemblage of all the individuals, which resemble each other in all important

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<sup>35</sup> Gray, *Elements...*, 310-312.

<sup>36</sup> Italics original; Gray, *Elements...*, 310.

particulars, and reproduce similar individuals by seed.” As for an equally troubling “variety,” he defined, “Any considerable change in the ordinary state or appearance of a species is termed a *variety*. These arise for the most part from two causes, viz: the influence of external circumstances, and the crossing of races.”

The fact that botanists often differ in opinion as to what are species, is no evidence against the existence or non-permanence of species, or the correctness of our definition. We are often under the necessity of judging from probable evidence, founding opinions upon characters which may occasionally deceive us; where one botanist recognizes a species, another sees a variety merely, and vice versa. But should what we are accustomed to regard as a single species be proved to comprise two or more species; or, on the other hand, should nearly all the North American species, as they are considered, of *Sagittaria*, prove to be a single species modified by local circumstances (as is no means unlikely), the idea of species would not be at all affected by our failures in practically distinguishing them.<sup>37</sup>

### **“In case I shall be compelled to work alone”**

With *Elements of Botany* completed, Gray thrust his energy into preparing a review essay of John Lindley’s *Natural System of Botany* (1837). He claimed that the value of Lindley’s book was greatly enhanced by an extensive list of 7,840 known genera. Compared to, for example, Linnaeus’s *Systema Naturae*, which listed only 1,228 genera, he argued, “This great and rapid increase is perhaps chiefly owing to the discovery of new plants: but it is also attributable in a good degree to the more accurate knowledge of those already known.” In either case, he continued, “it is the natural result of the progress of discovery... it may be safely said that at no previous period could a really useful knowledge of the vegetable kingdom be acquired with so little labor.” Botanists were now endowed with an unprecedented chance to

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<sup>37</sup> Gray, *Elements...*, 288-290.

complete and perfect the natural system, he cheered. "It is of course taken for granted that the student will avail himself of all the advantages of modern physiological botany and of the natural system: for so rapid has been the discovery of new and strange forms of structure, for which the artificial arrangement of Linnaeus makes no provision, that the student who takes that system as his guide has indeed a hopeless task before him."<sup>38</sup>

The enthusiasm about exploration and discovery in Gray's review of Lindley's volume bespoke the young botanist's rising self-confidence as an American botanist. In the summer of 1836, Congress decided to appoint him as "Botanist to the U.S. Exploring Expedition," an unparalleled opportunity that would bring Gray to explore the world beyond Humboldt's Mexico and reach all the way to the Antarctic and those island kingdoms of the Pacific. The U.S. Exploring Expedition represented the young nation's most ambitious attempt to catch up with what Captain James Cook had contributed to the geographical expansion of the British Empire. And if the young American botanist Asa Gray could make good use of the chance, he might be another Joseph Banks (who joined Captain Cook's first voyage [1768-1771] as a naturalist).

Delighted, Gray threw himself into a flurry of preparation. A letter he sent to the naval officer Charles Goodwin Ridgley to specialize "the number & kinds of instruments &c. &c. procured for the Scientific corps" indicated how much he had

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<sup>38</sup> Asa Gray, "A Natural System of Botany; or a Systematic View of the Organization, Natural Affinities, and Geographical Distribution of the Most Important Species in Medicine, the Arts, and Rural or Domestic Economy; by John Lindley, Ph.D. &c.," *The American Journal of Science and Arts* 32, no. 2 (1837), 301-303.

been occupied for this long-awaited expedition. He clearly managed to procure all necessary instruments to explore every known province of the botanical kingdom, including strong medium paper, ordinary wrapping paper, medium printing paper, blotting paper, botanical presses, portable presses, collecting portfolios, collecting tin boxes, binder's board, stoppered glass bottles, stone jars, packing boxes, and so on. At the end of this list, he signed his title "Botanist to the Exploring Expedition" beneath his name.<sup>39</sup> He was now not only a botanist but also a botanist appointed in an expedition under the aegis of the United States of America.

But Gray soon decided to give up this prestigious title. For the past two years or so, he had received no salary from Congress despite his relentless work as "Botanist to the Exploring Expedition." He realized that he could no longer indulge himself in an occupation of this sort. So when the newly chartered University of Michigan offered him a professorship with the annual salary of \$1,500, he promptly made his way to Detroit with the ambition not only to secure his own professorship but also to secure two additional professorships for Torrey and for the senior naturalist Joseph Henry (then Professor at Princeton College).<sup>40</sup>

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<sup>39</sup> AG to CGR, 2/7/1838, *AGPP*.

<sup>40</sup> AG to "his father," 8/6/1838, *LAG*, 67. Torrey kept Henry closely updated about how Gray's negotiation with the University of Michigan went:

Dr. Gray has not returned from Michigan. We received a letter from him to-day, & he speaks in the strongest language of the beauty of the country & the enterprise of the managers of the University. I regret to learn, however, that they have employed Davis as the architect—for I don't entertain a very high opinion of his science. Dr. Gray thinks as I do about him. The Dr. says, that the plans which they expected from Mr. D. had not all arrived, & that the architect thought it but to go on to Michigan & see the grounds himself. Dr. G. will probably wait there for him, & then return to New York. He says that the Council are

Then, between November 1838 and November 1839, when the scientific corps of the U.S. Exploring Expedition were exploring the most unknown corners of the globe, Gray immersed himself in the stimulating atmosphere of the scientific centers of Europe. With the University's generous support, he visited major herbaria, conversed with established botanists, and purchased natural-history books for the University's library. Returning home with charged confidence and an optimistic view of the future, he found his employer was in a financial crisis. He could retain his position, he learned, but he would receive no salary for a while. He was not troubled, however. He had a long list of things to do, and wanted to do them all with Torrey in New York. "I am much distressed at the thought of your anticipated engagements with Princeton," he wrote to Torrey from Kinross. "While there is the slightest hope remaining I do not like to relinquish the thought that we may hereafter work together and live near each other. The fear that this may not be the case has of late rendered me much more anxious to obtain books and specimens, in order that I may get on by myself in case I shall be compelled to work alone."<sup>41</sup>

***"So many nobis"***

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determined to make one or two prospective appointments in the autumn—and that you will be one of them—keep this close. I will give you more particulars when the Dr. returns. Mr. Whipple, an old friend and pupil of mine, who was at West Point when I was stationed there, is one of the chief men in the University and can carry any reasonable plan. He will be in New York in a week or two, & stop at my house, both on his way to Philadelphia and on his return. It may be well for you to happen this way when I give you a hint. (JT to JH, 8/[undated/]1838, *HL*)

<sup>41</sup> AG to JT, 1/2/1839, *LAG*, 91.



Gray summarized his research experiences in Europe in an essay entitled “Notices of European Herbaria” and published it in *The American Journal of Science and Arts*.<sup>42</sup> William Darlington, physician, politician, and botanist in Philadelphia, would later comment on Gray’s essay in a volume outlining the great moments in the history of American botany: “This acceptable service he was enabled to perform, in consequence of having devoted a year to the inspect of those *herbaria*, examining the American specimens, and ascertaining exactly what plants were intended, by the published names. It was a glorious privilege for an accomplished botanist; and was the only mode by which a long-existing uncertainty, in regard to many species, could be satisfactorily cleared up. The task, moreover, could not have been entrusted to more competent hands; and the *Flora of North America*, whenever completed will no doubt receive the full benefit of the knowledge thus obtained.”<sup>43</sup>

The immediate fruit that ripened from Gray’s research trip to Europe was the newest volume of *A Flora of North America* (Volume II, Part I; 1841).<sup>44</sup> For botanists of the day, the publication of this particular volume was a landmark—not only because it exhibited the two botanists’ resolution to carry on this formidable project to the end, but also because it included sections on such troublesome botanical

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<sup>42</sup> Asa Gray, “Notices of European Herbaria,” *The American Journal of Science and Arts* 40, no. 1 (1841), 1-18.

<sup>43</sup> William Darlington, *Memorials of John Bartram and Humphrey Marshall, with Notices of Their Botanical Contemporaries* (Philadelphia: Lindsay & Blakiston, 1849), 30.

<sup>44</sup> John Torrey and Asa Gray, *A Flora of North America: Containing Abridged Descriptions of All the Known Indigenous and Naturalized Plants Growing North of Mexico; Arranged According to the Natural System, Vol. II, Part I* (New York and London: Wiley and Putnam, 1841).

tribes as *Asteroideae*.<sup>45</sup> When preparing the section on *Asteroideae*, Torrey and Gray clearly knew that they were entering territory few American botanists had ventured to traverse. They set forth a principle to clarify their ground.

Those botanists who are most familiar with our *Asters*, in their native situations, and with the changes produced by difference of soil, exposure, season, &c., will not be greatly surprised at numerous reductions of species, which others may think unwarranted. We have only to say, that we have seldom ventured upon such reductions, except on the authority of a full suite of specimens which appeared to present absolute transitions. An obvious difference between two or three specimens is often entirely inappreciable in a fuller series, and this loses its value as a means of distinction; but the claims of a genuine species are generally confirmed by a large number of specimens. It must, however, be admitted, that in this as in all large and natural genera, several species which we cannot but consider as distinct, (such for instance as *A. cordifolius* and *A. sagittifolius*,) do frequently present very puzzling intermediate forms; and that an apparent transition is not always real. Yet it is better, perhaps, to hazard the occasional reduction of even true species to varieties, than to multiply species which we are confessedly unable to define.<sup>46</sup>

Torrey and Gray audaciously put the principle in practice. Of particular note was their diagnosis of *Aster laevis* Linn (now known as *Symphyotrichum laeve*), which reduced some 13 species into synonyms (11) and varieties (2) and placed them altogether under the name *A. laevis*, an extraordinary reduction of the number of species even by European standards.<sup>47</sup>

In many ways, Torrey and Gray's note could be read a *manifesto* that affiliated them with the boldest "species lumpers" of the day. More specifically, the note reflected what George Bentham had maintained in *Labiatarum genera et species*

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<sup>45</sup> "[T]he terror and stumbling-block of American botanists," John Carey, Torrey and Gray's close colleague, would comment in a review essay. Carey, "Notice of a Flora of North America," *The American Journal of Science and Arts* 41, no. 2 (1841), 279.

<sup>46</sup> Torrey and Gray, *A Flora of North America, Vol. II, Part I*, 104.

<sup>47</sup> Torrey and Gray, *A Flora of North America, Vol. II, Part I*, 116.

(1832-1836):

... there seems still every reason, in the case of species, to consider that it has a really distinct existence in nature as a group of individuals, varying from each other only within the limits of individuals descending from one common stock; and the question is therefore here, *are* two plants of the same species or not? Of this it is seldom we can obtain positive evidence; we must reason by analogy—we must take into account all the variations likely to be caused by soil, climate, and other external agents—by cultivation, by hybridity, by disease, &c., as well as by the age and period of development of the specimen before us—we must consider whatever circumstantial evidence we can deduce from station, from abundant of individuals resembling each of the two specimens before us, or from any other source we can get at; and from these data we must then form our judgment.

Bentham then claimed that he had been “led into these observations as a sort of answer to those who may consider me presumptuous in setting up my opinions, formed, in a great measure, from dried specimens, against the observations of local botanists and directors of botanical gardens, often of great merit, who have studied the plants living.” But, he asked, if an experienced botanist like him, who had devoted most of his time to studying in distinguished botanical establishments across Europe, was still considered “presumptuous,” “how much more must it be with those who, with all his national pride in the number of species comprised in his Flora, have none of his talent, and carry to so high a degree the sometimes mercenary and always foolish vanity of tacking so many *nobis* to botanical names?”<sup>48</sup>

John Carey took responsibility for reviewing this particular volume of *A Flora of North America*. In a review essay published in *The American Journal of Science and*

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<sup>48</sup> George Bentham, *Labiatarum Genera et Species; or, a Description of the Genera and Species of Plants of the Order Labiatae; with Their General History, Characters, Affinities, and Geographical Distribution* (London: James Ridgway and Sons, 1832-1836), xlviii-xlix.

*Arts*, he first congratulated Torrey and Gray for making a decisive move toward the eventual completion of a “truly national work.” “It may seem extraordinary,” he then groaned, “that this undertaking, which has attracted so much attention amongst European naturalists, should have excited so little comparative interest among those of our own country, for whom it is more especially designed.” In Carey’s opinion, such silence indicated the underdeveloped status of botany in the United States. Although botany had occupied a corner of colloquiums of schools across the country for many years, he noted, the “*low standard* adopted by our professors” had induced the “prevalent opinion” that botany was “of very subordinate importance,” a sort of science that offered “little information beyond *the mere names of things*.” In Carey’s view, this truly deplorable circumstance stemmed from the unusual popularity of the artificial system and utter negligence of “vegetable physiology” in American botany. But thankfully, he asserted, *A Flora* was “a sound physiological work,” a testimony that American botanists were capable of engaging in “the philosophy of the science” as much as their European counterparts.<sup>49</sup>

Carey also admired Torrey and Gray’s approach to lumping those similar species together instead of splitting them apart. “We feel convinced,” he wrote, “that the onward progress of science will have a tendency rather to diminish than increase the number of orders, which have been so largely multiplied since the days of Jussieu.”

... with our increased, and daily increasing, knowledge of vegetable forms, we become acquainted with genera so exactly intermediate in character between what have been regarded as separate groups, or orders, existing in nature, that

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<sup>49</sup> Carey, “Notice...,” 275-276.

we are compelled to view the dividing lines as of secondary, or subordinate value; and hence, we conceive, will arise the necessity of reuniting some, perhaps ultimately many, orders now considered as distinct. It is in this way only, that families of plants can be preserved as *natural* alliances; and any approach towards *artificial* arrangement will be deprecated by every true botanist, who feels that his province is limited to the observation of kindred tribes, and the elucidation of such characteristics as they are severally found to present. No doubt the types of new and very distinct orders are yet to be discovered, but we repeat our conviction, that for the present, at least, the number will, upon the whole, be rather reduced than augmented.<sup>50</sup>

Carey published this review in the volume 41 of *The American Journal of Science and Arts*. The journal's readers of the day could not have failed to notice another essay published in the same volume under the title "Des Moulins's General Considerations on Restricting the Number of Species of the Genera Unio and Anodonta" (1841). The essay was an English translation of a written comment made by French naturalist Charles des Moulins (1798-1875) on American naturalist Issac Lea's diagnosis of the genus *Anodonta*. Philip H. Nicklin (1876-1842), a retired bookseller and natural-history lover, when coming across Moulins's comment, felt obliged to introduce it to American readers. "The paper of Mr. Des Moulins," he commented, "though critical, is highly honorable to the labors of our countrymen, shewing that they are casting their mites into the general treasury of the world's knowledge." Indeed, he continued, he had been troubled by his "countrymen's" eagerness to name new species. He found that such eagerness had prevented American naturalists from thinking closely about a truly philosophical question: if naturalists could accept the claim that *Natura non facit saltum* ("Nature makes no

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<sup>50</sup> Carey, "Notice...", 278.

leaps”), what did the term “species” mean against this claim?<sup>51</sup>

During the late 1830s and the early 1840s, increasing numbers of American naturalists became worried about how “low” their science appeared in the eyes of their European colleagues. “I hope that the account of Sherwood’s [Hall Sherwood’s] discovery did not reach the British Association,” Torrey told Joseph Henry, “& thus expose us to ridicule before that noble body.” The moment was coming, declared the botanist, when, “We must do all in our power to put an end to quackery in our land—& every man must feel so jealous of his own particular science as to refuse all pretended discoveries.”<sup>52</sup>

Gray soon joined Torrey’s effort to “put an end to quackery.” The late 1830s witnessed his rising interests in publishing “Botanical Notice and Book Review” in *The American Journal of Sciences and Arts* (with the initials of “A. G.”). The purpose of publishing such notes and reviews, he informed Sir William, was “merely to awaken and deepen the interest of our scattered botanists and lovers of plants, most of whom see that journal, and few of whom have any other means of knowing what is going on in the botanical world.”<sup>53</sup> Readers of *The American Journal of Sciences and Arts* were soon overwhelmed by various topics cascading from Gray’s pen. The young botanist’s interests seemed wide and omnivorous, ranging from W. Griffith’s “Report on the Tea Plant of Upper Assam” to J. F. W. Johnston’s “Lectures on the

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<sup>51</sup> Philip H. Nicklin, “Des Moulins’ General Considerations in Restricting the Number of Species of the Genera Unio and Anodonta,” *The American Journal of Science and Arts* 41, no. 1 (1841), 105, 107.

<sup>52</sup> JT to JH, 8/[undated/]1838, *HL*.

<sup>53</sup> AG to WJH, 1/15/1841, *LAG*, 277.

Applications of Chemistry and Geology to Agriculture.” “The rapidity with which you turn out so much reading matter, as well unprinted as printed, is quite a mystery to my duller & slow moving brain,” Moses A. Curtis told Gray.<sup>54</sup>

In October 1840, he wrote to Benjamin Silliman, Professor in Geology at Yale College and founder and chief editor of *The American Journal of Science and Arts*: “Do you know that Rafinesque is dead? If I cannot prevail upon Dr. Torrey to prepare a notice of his life & writings I shall do it myself.” Then, in November, he informed Silliman that he had decided to write a review article on Rafinesque’s life and work.

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<sup>54</sup> MAC to AG, 1/6/1845, *HL*. Notably, as though he was conscious of his relatively junior status in the world of botany, Gray was always careful about his wording when composing his reviews or botanical notices. For example, when reviewing *A Manual of Botany, Adapted to the Productions of the Southern States*, written by John Darby, Professor in Chemistry and Natural Philosophy at Georgia Female College, Gray made the following comments: “To write a flora of any extensive district, or even to prepare a creditable compilation, involves an amount of labor of which few who have not made the attempt can have any conception; and there are, moreover, several circumstances which just at present render the production of a Southern Flora a difficult undertaking.” Carefully choosing his terms, Gray pointed out that readers “should more highly estimate” Darby’s account of plant physiology. Gray’s words seemed to hurt Darby deeply however. “What on earth have you been doing to him [Darby]?” Curtis asked Gray. “He seems woefully hurt at some passes you have made at him about that Book of his.” After all, remarked Curtis, “He [Darby] is an amiable & well disposed man, & although his Botany may be defective, yet is it not worth consideration that at the time of his publication there was need of something, & that imperfect work will be of some service as a pioneer? ... Be as charitable as you can towards his labors.” Asa Gray, “A Manual of Botany, Adapted to the Productions of the Southern States; in Two Parts: Part I. Vegetable Anatomy and Physiology; Part II. Descriptive Botany, Arranged on the Natural System, Preceded by an Analysis; by John Darby, A.M., Prof. Chem. And Nat. Phil. in the Georgia Female College,” *The American Journal of Science and Arts* 41, no. 2 (1841), 375-376; MAC to AG, 8/1/1842, *HL*.

Gray's "Notice of the Botanical writings of the Late C. S. Rafinesque" soon appeared in the journal early in 1841.<sup>55</sup>

In this, one of his earliest review essays, Gray acknowledged Rafinesque as one of the pioneers in American botany who was "acquainted with natural affinities" and in fact as the "only person in this country, who had any pretensions to that kind of knowledge." Unfortunately as it was, Gray noted, Rafinesque's research was "by no means well grounded in structural and systematic botany," and a careful reader could easily detect a "gradual deterioration" in Rafinesque's botanical writings "from 1819 to about 1830," "when the passion for establishing new genera and species, appears to have become a complete *monomania*." Gray then argued that Rafinesque's unparalleled ability to produce new genera and species did not derive from his insistence on the natural system but from his lamentable view on species and variety.

According to his [Rafinesque's] principles, this business of establishing new genera and species will be endless; for he insists, in his later works particularly, that both new species and new genera are continually produced by the deviation of existing forms, which at length give rise to new species, if the foliage only is changed, and new genera when the floral organs are affected. He assumes thirty to one hundred years as the average time required for the production of a new species, and five hundred to one thousand years for a new genus; and on a preceding page he remarks, that "new varieties and species were often met with by me at long intervals, in wild places well explored before, grown from seeds of akin species."<sup>56</sup>

Surprisingly, besides producing articles and reviews, Gray still found time to

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<sup>55</sup> AG to BS, 10/5/1840, *AGPP*; AG to BS, 11/15/1840, *AGPP*; AG to BS, 12/31/1840, *AGPP*.

<sup>56</sup> Asa Gray, "Notice of the Botanical Writings of the Late C. S. Rafinesque," *The American Journal of Science and Arts* 40: 2 (1841), 231, 237-239.



undertake a botanical expedition. Early in 1841, he sent a letter to Curtis of Wilmington, North Carolina, a clergymen-botanist who successfully undertook a botanical expedition in 1839 to the southern Appalachians.<sup>57</sup> Phrasing his letter in the position of a junior botanist who ventured to initiate correspondence with a senior colleague, he wrote that he would appreciate it if Curtis could furnish him with field information and “introductory letters” by which he could contact local naturalists. If Curtis could do him this favor and if his expedition turned out to be successful, he noted that he would “tribute” part of his collections to Curtis.

Curtis was impressed by this rising botanist’s enthusiasm. He immediately replied, confirming that he would do whatever he could to smooth Gray’s expedition. He warned Gray not to underestimate the obstacles that he might encounter in mountains of North Carolina. “If you visit our Mts. you will be obliged to travel on foot or horseback along intricate cattle paths, & put up with accommodations on the way, such as you never dreamed of. You will need vigorous legs & a strong stomach.” As for the “introductory letters,” he noted in another letter, claiming that it was the least issue Gray needed to be worried about. He doubted if people living close to Gray’s interested botanical grounds would know how to read.<sup>58</sup>

It turned out that Gray had such “vigorous legs” and “a strong stomach.” From late June to late July, he embarked on a botanical expedition to the southern

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<sup>57</sup> Regarding Curtis’s 1839 expedition to the southern Appalachians, see Ronald H. Petersen, “Moses Ashley Curtis's 1839 Expedition into the North Carolina Mountains,” *Castanea* 53, no. 2 (1988), 110-121.

<sup>58</sup> MAC to AG, 5/21/1841, *HL*; MAC to AG, 6/21/1841, *HL*.



Figure 1-3. Gray's notes taken during his expedition to the Carolinas. (Courtesy of the Archives of the Gray Herbarium, Harvard University, Cambridge, MA, USA.)

Appalachians with John Carey and James Constable (Figures 1-3 and 1-4). Taking Jefferson, North Carolina, as their headquarters, the three botanists explored mountains, valleys, and highlands, including an ascent of the Grandfather Mountain and the Roan Mountain. It was for the first time that Gray met the alpine flora of eastern North America. He was struck by the fact that a number of arctic plants actually “reappeared” on this roof of the American South (an understanding that would play a critical role when Gray developed his thesis on disjunct distribution). Upon his return, Gray immediately published his traveling account in *The American Journal of Science and Arts*. In it he enumerated plant species that had called his attention en route, described and named new species, and offered vivid accounts of the American frontier life from a Yankee's point of view. Gray's readers might find the botanist's following journal entry (dated July 2) intriguing:



Figure 1-4. *Lilium grayi*, a species first gathered by Gray in his expedition to the Carolinas in 1840. (Courtesy of the Library of the Gray Herbarium, Harvard University, Cambridge, MA, USA.)

We may remark indeed, that during our residence amongst the mountains we were uniformly received with courtesy by the inhabitants; who for the most part lacked the general intelligence of our obliging host at Jefferson, and could scarcely be made to comprehend the object of our visit, or why we should come from a distance of seven hundred miles to toil over the mountains in quest of their common and disregarded herbs. Curiosities as we were to these good folks, their endless queries had no air of impertinence, and they entertained us to the best of their ability, never attempting to make unreasonable charges. A very fastidious palate might occasionally be at a loss; but good cornbread and milk are everywhere abundant; the latter being used from preference quite sour, or even curdled. Sweet milk appears to be very generally disliked, being thought less wholesome, and more likely to produce the “milk sickness,” which is prevalent in some very circumscribed districts; so that our dislike of sour and fondness for sweet milk was regarded by the simple people as one of our very many oddities.<sup>59</sup>

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<sup>59</sup> Asa Gray, “Notes of a Botanical Excursion to the Mountains of North Carolina, &c; with Some Remarks on the Botany of the Higher Alleghany Mountains,” *The American Journal of Science and Arts* 42, no. 1 (1842), 24.



Figure 1-5. Gray in his early thirties. (Courtesy of the Archives of the Gray Herbarium, Harvard University, Cambridge, MA, USA.)

As promised, Gray sent duplicates of his North Carolina collections to Curtis along with the published traveling account. Curtis was delighted, and so were the intellectual circle of North Carolina. “Your Carol: Excursion pleases the folks at our University [University of North Carolina, Chapel Hill] very much,” he told Gray. “Prest. Swain [David Lowry Swain] has requested me to copy it for our Newspaper here, believing that it will be copied thence into our other papers & read with interest.”<sup>60</sup> Sir William also took an active role in making Gray’s botanical expedition known to European botanists. Gray soon received a letter from Henry Botton Fielding of Lancaster asking for exchanging specimens. In the decade to come, Fielding would be of great help to the collecting expeditions under Gray’s charge.<sup>61</sup>

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<sup>60</sup> MAC to AG, 4/5/1842, *HL*.

<sup>61</sup> HBF to AG, 7/14/1842, *HL*.

On November 10, 1841, Gray was elected into the American Academy of Arts and Sciences (at the age of 31; Figure 1-5), thus beginning his life-long association with this distinguished scientific organization in North America.<sup>62</sup> His financial status worsened, however. In March 1842, Gray could not help expressing his despair to Sir William about how difficult it was to be a botanist in the United States: “Having no income for the last two years, I have been greatly embarrassed, and have struggled through great difficulties, I scarcely know how.”<sup>63</sup>

### **“Harvard is the place”**

The Fisher Professorship in Natural History at Harvard College rescued Gray from such uncertainty and embarrassment. The position was based on the endowment of Joshua Fisher, a Dedham-born physician in Beverley, and its duties included giving lectures *both* on botany and zoology and superintending the Harvard Botanical Garden (seven acres) with the annual salary of \$1,500. The search committee first contacted Francis Boott (1792–1863), a Bostonian practicing medicine and studying botany in London. Boott declined the offer, largely because he was unwilling to teach zoology. Indeed, the workload of the Fisher Professorship was daunting (particularly considering its relatively low salary). In a talk before the Franklin Institute, a naturalist named Samuel S. Haldeman portrayed a dim future for Harvard’s yet-to-be-appointed Fisher Professor: “In grasping at too much, he [the

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<sup>62</sup> Corresponding Secretary from 1844 to 1850, and again from 1852 to 1863; Chairman of the Committee of Publication from 1846 to 1850; President from 1863 to 1873.

<sup>63</sup> AG to WJH, 3/30/1842, *LAG*, 282-283.

professor] may not have the time to make a single discovery, or original observation—to add a single chapter to the gross amount of knowledge upon the subjects he is expected to understand; and he consequently degenerates into the mere compiler.”<sup>64</sup>

So it may have been. But by the mid-nineteenth-century standard, a position in natural history with a reasonable salary did call a number of naturalists’ attention, including such competent Boston-based naturalists as Thaddeus William Harris (1795-1856) and Jeffries Wyman (1814-1874).<sup>65</sup> But likely because of Boott’s recommendation, the search committee became interested in hiring Asa Gray. Late in 1841, when Gray learned of the committee’s interest, he contacted Benjamin D. Greene, a wealthy Bostonian and a plant-lover. Greene responded to the inquiry by inviting Gray to visit Boston. In January 1842, Gray set foot in Boston for the first time in his life. He met Harvard President Josiah Quincy (who happened to be Greene’s father-in-law) and a circle of local botanists at a dinner party arranged by Greene. Three months later, in April 1842, the Harvard Corporation offered the position to Gray with annual salary of \$1,000. (The salary had been reduced,

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<sup>64</sup> Quoted from Clark A. Elliott, *Thaddeus William Harris (1795-1856): Nature, Science, and Society in the Life of an American Naturalist* (Bethlehem: Lehigh University Press, 2008), 64.

<sup>65</sup> Regarding Harris’s and Wyman’s Harvard connections, see Elliott, *Thaddeus William Harris*; Toby A. Appel, “A Scientific Career in the Age of Character: Jeffries Wyman and Natural History at Harvard,” in *Science at Harvard: Historical Perspectives*, edited by Clark A. Elliott and Margaret W. Rossiter (Bethlehem: Lehigh University Press, 1992), 96-120. Wyman would soon join Harvard’s faculty and become Gray’s close colleague. Regarding Wyman’s life and career at Harvard, especially his responses to Darwinism, see Appel, “Jeffries Wyman, Philosophical Anatomy, and the Scientific Reception of Darwin in America,” *Journal of the History of Biology* 21, no. 1 (1988), 69-94.

because Gray's duties *only* included lecturing on botany and superintending the Harvard Botanical Garden.)

Gray's close friends sent their warm regards to the new appointed Fisher Professor Asa Gray. They were glad that the intelligent and hard-working Gray eventually got a *paid* job. "I am sure that you will have both the power & the will to render essential service to American Botany there," Sir William told Gray.<sup>66</sup> Curtis, too, congratulated Gray, saying that he was delighted that Gray had been "provided for in one of the Atlantic States," and he wished Harvard would guarantee "a good salary."<sup>67</sup> Sullivant of Columbus, Ohio, though disappointed that Gray was unable to join him to explore the American West, assured Gray that "Harvard is the place." "If I judge aright Botany receives rather feeble attention in that quarter—in this respect there is room for action—considering all things—you have made a lucky hit—go ahead—take care of your health & I will underwrite results will be as they should be."<sup>68</sup>

For the newly appointed Harvard professor Asa Gray, giving lectures and recitations belonged to his "duties" or "professional occupations." But how both overwhelming and stimulating such professional occupations were! On Friday, March 3, 1843, Gray for the first time talked to a crowd as a botany professor, not merely a botanist. He reflected on this extraordinary experience in a letter to Mrs. Torrey:

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<sup>66</sup> WJH to AG, 11/10/1842, *HL*.

<sup>67</sup> MAC to AG, 8/1/1842, *HL*.

<sup>68</sup> WSS to AG, 4/19/1842, *HL*.

Yesterday afternoon I met the first two sections of my class of Freshmen for recitation. It went off very well. I am pretty good at asking questions. The lads were well prepared. Next Tuesday I meet the third and fourth sections; and on Thursday, the ides of March, I give my first lecture on Botany. If I succeed well, I am sure no one will be more pleased and gratified than yourself, and that of itself is enough to incite me to effort. If I don't altogether succeed, neither satisfying myself nor others, I shall not be discouraged, but try again, as I am determined to succeed in the long run. Nil desperandum. I shall have the president to hear me; but he is said always to fall asleep on such occasions, and to be very commendatory when he awakes....<sup>69</sup>

"I am now occupied with my college duties which occupy me, either with recitations or lectures, every afternoon albeit Saturday," he told a close correspondent.<sup>70</sup>

Becoming a professor in botany gave Gray a great motivation to finish his "botanical handbook," a project that he had initiated some two years before. This "handbook" would be a completely rewritten edition of *Elements of Botany* (1836), Gray told Curtis hopefully. Curtis was delighted that Gray had once again decided to explore and exploit the increasingly important market of scientific textbooks. "I am glad you have a "Handbook" in embryo," he replied. "I hope though that you do not intend to give it that name, when we already have an English word meaning the same thing, & which is much more euphonic."

Curtis went on to note the importance for a man of science like Gray to produce scientific textbooks:

Botany is but little studied in any of the Schools of this State. As far as I can, I will help the spread of your Books. It is high time that Eaton, Lincoln, Comstock &c. should meet their deaths. I think it shameful, that the latter has been permitted to realize a fortune from the sale of scientific text books in the whole cycle of the sciences, about none of which did he know even the elements. I have never read

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<sup>69</sup> AG to "Mrs. Torrey," 3/2/1843, *LAG*, 301.

<sup>70</sup> AG to ECM, 4/14/1845, Box 1, Folder 3, *AGPL*; Gray's use of the term "professional occupations" appears in AG to ECM, 6/16/1843, Box 1, Folder 3, *AGPL*.



one of his books, but I acquire the above opinion from a curious tale told me by Bachman. He told me that while examining the flowers in Hogg's establishment, he was accosted by a stranger with—"you appear to be acquainted with Botany, Sir,—how long will it take me to write a book upon the subject?"—"Why, that will depend upon how much you know of it."

"Oh, I know nothing at all. But I have published books on Chemistry, Philosophy, &c. &c. & knew nothing more about them." Dr. Bachman found the stranger to be Comstock.—Well sure enough, the very year Bachman told me this tale I saw Comstock's Botany advertised. I never heard of a more impudent piece of quackery, & the man that can be guilty of it ought to be published from Maine to Florida.<sup>71</sup>

Gray accepted Curtis's suggestion. The final title read *The Botanical Text-Book: For Colleges, Schools, and Private Students* (1842).<sup>72</sup>

In the course of turning *Elements of Botany* to *The Botanical Text-Book*, how to apply the natural system or method to study botany remained Gray's chief concern. But unlike the 1830s, when botanists at large were unsure how to choose between Linnaeus's artificial system and de Jussieu and de Candolle's natural system, botanists now were practicing *so many* natural systems. Gray's good friend, Edward Tuckerman, for example, was a faithful follower of the natural system developed by the Swedish botanist Elias Magnus Fries (1794-1878), with inclination to and appreciation of German vitalism and romantic natural history. Tuckerman once told Gray that he had decided to send all his "Lichens & everything else whatever to Fries." "[T]his great man has already grasped the whole system of Vegetable Nature—and everything comes to him eloquent as type or deviation."<sup>73</sup> How could a

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<sup>71</sup> MAC to AG, 3/14/1842, *HL*

<sup>72</sup> Asa Gray, *The Botanical Text-Book: For Colleges, Schools, and Private Students* (New York: Wiley and Putnam, 1842).

<sup>73</sup> ET to AG, 1/28/1844, *HL*.

student of botany tackle the plethora of natural systems, each of which had its followers and advocates, and each of which was said to be “correspondent to nature”?

In the face of the increasingly vexing natural systems, Gray was greatly inspired by William Herbert’s *Amaryllidaceae* (1837). With a pencil in hand, he marked the following paragraphs:

For the purpose of assisting our view of nature, we arrange them in groups, to which however no distinct limits were assigned by the Creator; and, though we are trying to find out the ways of nature, our classifications, by whatever name we may call them, are artificial, and if we proceed beyond one step at a time, we must be liable to find ourselves baffled by the reality.<sup>74</sup>

Partly because of Herbert’s strong opinion about the “unnaturalness” of every natural system that was said to be natural, Gray made a clear distinction between the natural system and the natural method in *The Botanical Text-Book* (1842). The natural system must “of course be natural,” he argued, because of its “methodical distribution of plants according to their organization.”<sup>75</sup> He then explained why the natural method must be “to a considerable extent artificial,” although it aimed to reveal “the natural relationship of plants, as far as practicable”:

1st, Because the affinities of a particular group cannot be fully estimated until all its members are known; thus the progress of discovery leads to changes, or modifies our views, as in every other department of knowledge.

2nd, Because the boundaries of groups are not so arbitrarily circumscribed in nature, as they necessarily are in our classification; but individuals depart from

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<sup>74</sup> William Herbert, *Amaryllidaceae; Preceded by an Attempt to Arrange the Monocotyledonous Orders, and Followed by a Treatise on Cross-Bred Vegetables, and Supplement* (London: James Ridgway, 1837), 18. Gray’s own copy of *Amaryllidaceae* can be found at the Gray Herbarium.

<sup>75</sup> Gray, *The Botanical Text-Book*, 183-184.

the assigned limits in various directions (like rays from a centre); the “edge of difference being as it were softened down by an easy transition.”<sup>76</sup>

3rd, Because that, even supposing the groups to be perfectly natural, and their affinities completely understood, it is impossible to arrange them in a single continuous series, in such a manner that each shall be preceded and followed by its nearest allies; since the same family, for instance, may be about equally related to three or four others, only two of which points, at best, can be indicated in the lineal series which must be adopted in books.<sup>77</sup>

Unsurprisingly, what concerned Gray foremost was how to define the species group. Here he chiefly retained what he had said in *Elements of Botany* and defined species as the “produce of a common parent.”<sup>78</sup> Plain as it was, the definition indicated two pillars of Gray’s view of species: the single origin of each and every species and the immutability of species. As he would argue in a review essay of *Explanations: A Sequel to Vestiges of the Natural History of Creation* (1846),

... the regular development of the animal or vegetable frame from the embryo to the mature state, the regular performance of the offices and functions which we clearly perceive it was specially designed to perform, and the unvaried production by each species of an offspring like itself,—the oak producing oaks, and never pines, animalcules giving rise to animalcules, and not to fish or quadrupeds, monkeys giving birth to monkeys, and not (as the writer maintains) to men,—this general fact, confirmed by the most extended observation, that each species remains true to its character,—all show, to be sure, that the organic creation is under law in the same sense that the heavenly bodies are, but all speak of a very different law from that of transmutation. If any thing has been settled by human observation, it would seem to be this,—the actual uniform production by each species of seed specifically after its kind...<sup>79</sup>

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<sup>76</sup> Here Gray’s quotation is from Herbert, *Amaryllidaceae*, 24.

<sup>77</sup> Gray, *The Botanical Text-Book*, 191-192.

<sup>78</sup> Gray, *The Botanical Text-Book*, 179-180.

<sup>79</sup> Asa Gray, “Explanations:—A Sequel to Vestiges of the Natural History of Creation.” *The North American Review* 62, no. 131 (1846), 470-471.

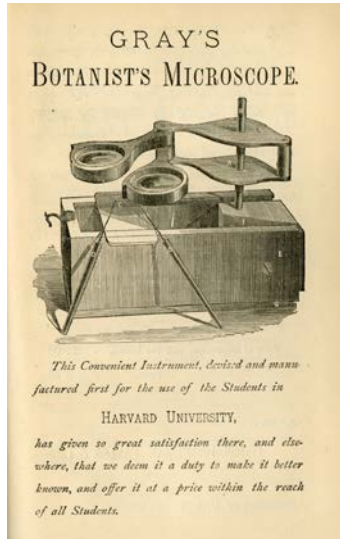


Figure 1-6. The description of this particular microscope reads: “This convenient Instrument, devised and manufactured for the use of the Students in HARVARD UNIVERSITY, has given so great satisfaction there, and elsewhere, that we deem it a duty to make it better known, and offer it at a price within the reach of all Students.” (Courtesy of the Archives of the Gray Herbarium, Harvard University, Cambridge, MA, USA.)

He would argue that the “general fact” that “each species remains true to its character” implicated the existence of not only the Creator but also a governor in nature.

To be sure, however, what Gray wanted to impress upon his readers was neither the existence of the Creator in nature nor a categorical definition of species. Instead, he wanted his readers to gain hands-on experiences in botany with the aid of his book (Figure 1-4). As he put it in a letter to Elizabeth Carrington Morris, a plant enthusiast in Philadelphia,

As to dissecting seeds, you will find—with a little practice—that you can cut in two very small ones with a sharp pen-knife. I cut them in this way on the stage of my microscope:—(for a mounted microscope is quite necessary for such examinations); or soak them in warm water, and take the skin off, or tear them in pieces—and extricate the embryo—with a pair of sharp needles set in handles. Little wooden handles—made of sets of pine or any other soft wood will do very well. You work under the lens—with which you view the object able to

dissect most seeds with ease. I wish I could give you a few lessons on the subject. I am apt to forget, that many of these to me simple manipulations have in fact cost me much time and trouble to learn at first.<sup>80</sup>

“A little practice enables one to perform wonders,” he wrote encouragingly in another letter.<sup>81</sup> After exchanging some letters with Morris regarding how to undertake botanical observation, he became aware of the lady’s interest in establishing her own herbarium. “With your perseverance, I don’t despair of your becoming really a proficient in the Natural System,” he told Morris, and promptly sent her a set of selected specimens. To Gray, only such a series of painstakingly conducted observations could lead to accurate classification of plants.<sup>82</sup>

Beyond giving lectures and instructions to the country’s plant-lovers, Gray himself became an ardent lecture-goer and participant. (“When your letter arrived I was on my way to hear a lecture on the Comet,” he told Morris.<sup>83</sup>) He was active in a number of local scientific societies, including the Boston Society for Natural History, the Massachusetts Horticultural Society, and the American Academy of Arts and Sciences. Notably, as if all such lecture-giving and lecture-going activities had not been enough, on November 14, 1842, with his colleagues Joseph Lovering, Benjamin Peirce, Daniel Treadwell, James Walker, and Morrill Wyman, he founded the Cambridge Scientific Club, “a small informal dining club where members exchanged

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<sup>80</sup> AG to ECM, 4/9/1843, Folder 3, Box 1, *AGPL*.

<sup>81</sup> AG to ECM, 6/16/1843, Folder 3, Box 1, *AGPL*.

<sup>82</sup> AG to ECM, 11/4/1843, Box 1, Folder 4, *AGPL*.

<sup>83</sup> AG to ECM, 4/9/1843, Box 1, Folder 3, *AGPL*.



Figure 1-7. The Garden House. The painting was drawn by Isaac Sprague in 1854. (Courtesy of the Archives of the Gray Herbarium, Harvard University, Cambridge, MA, USA.)

ideas on a wide variety of topics including, but not limited to, religion, music, the natural sciences, political economy, the law, ancient history, and the fine arts.” To the Club’s members, every meeting was a feast—literally and figuratively. In September 1846, prominent educator Edward Everett recorded that one of the Club’s talks was accompanied by “a hearty meal of chicken, tongue, ham, pastry, Scotch Ale, and two or three kinds of wine.” Although Everett eventually decided to resign his Club membership—for he was worried that “his partaking of such extravagant meals would not set a positive model for students,” he confided in his diary that it had been difficult to make the decision, for the Club offered “the only relaxation I have in Cambridge from the monotony of my duties.”<sup>84</sup>

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<sup>84</sup> All quotations are from the “Historical Essay” in the inventory of the Records of the Cambridge Scientific Club, Harvard University Archives. See <http://oasis.lib.harvard.edu/oasis/deliver/~hua19008>.

Gray was ready to settle in this particular corner of the country—both intellectually and physically. As the superintendent of the Harvard Botanical Garden, he was qualified to reside in the so-called Garden House (just inside the Garden; Figure 1-7). “I am at length comfortably established in my final residence at the Botanical Garden—with—for the first time in my life—plenty of elbow-room—a great thing for a Botanist,” he told Miss Morris with satisfaction.<sup>85</sup> For a botanist who had been wandering from one herbarium to another for more than ten years, Harvard was indeed the place.

### **“Use up the quacks!”**

Gray also felt compelled to communicate sciences to the general public. He recognized himself as a “man of research,” and to him being a *sound* man of research meant that he had to help “unprofessional readers” make the right judgments in a world full of “quacks.” In 1844, when the world of science was agitated by the transmutationist and materialist views of nature advocated by the anonymously published *Vestige*, Gray threw the following question to readers of *The North American Review*: “why, we ask, should not the unprofessional reader rely upon their independent testimony, in respect to facts which they are the most competent witnesses of, and inferences of which they have the best means of judging?” His answer to this question was that the reader should not. “Who but the men of research have ever established sound and comprehensive views of nature or have made stable generalizations in any branch of science? Did Newton, Herschel,

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<sup>85</sup> AG to ECM, 11/9/1844, Box 1, Folder 3, *AGPL*.

Laplace, Cuvier, Davy, De Candolle, or Humboldt, give to the world mere naked facts, the germs of great views that had to fall into other minds ere they were developed or grew fecund?"<sup>86</sup>

Gray found *The North American Review* an ideal medium to fulfill his role as a man of research. He soon decided to engage in a theme that had occupied this celebrated literary magazine's editorials, essays, poems and reviews for the past decade: the longevity of trees. Several concerns prompted Gray to make the decision. First and foremost was Americans' surging consciousness of how young their nation was. Unlike most of the European nations, they realized, the United States of America did not have such things as magnificent architecture that had survived to embody the nation's past. But *could* a nation's past be embodied in ancient architectures? Many Americans did not think so. The fact was that the young United States of America had creatures that arguably outlived the oldest civilization in the world: ancient forests. The intellectual circle in New England was particularly fond of this interpretation. *The North American Review* then served as a forum in which they could define, refine, and disseminate the intertwined relationships among nation, nature, and history. In 1832, for example, an essay titled "American Forest Trees" eloquently wrote, "The tower, as soon as it is completed, begins to

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<sup>86</sup> Gray, "Explanations...", 506.



decay; the tree, from the moment when it is planted, grows firmer and stronger for many an age to come.”<sup>87</sup>

Gray thought that such a tower-tree contrast needed to be examined scientifically. In his “Longevity of Trees,” he echoed what “American Forest Trees” had manifested some ten years earlier: “we must still regard some of these trees, not only as the oldest inhabitants of the globe, but as more ancient than any human monument,—as exhibiting a *living* antiquity, compared with which the mouldering relics of the earliest Egyptian civilization, the pyramids themselves, are but structures of yesterday.” But how should Americans make sense of such a living antiquity? To this question most of his contemporaries would have preferred a theologically oriented answer, but he took a rather materialist stand. He assured his readers that French botanist Augustin Pyramus de Candolle had so “masterly” discussed the subject in *Physiologie végétale* (1832). The point was that a tree was *not* an individual, he wrote. “To establish a proper analogy, we must not compare the tree with man, but with the coral formations, in which numberless individuals, engrafted and blended on a common base, though capable of living when detached from the mass, conspire to build up those arborescent structures, so puzzling to the older naturalists, that they were not inappropriately named ‘zoophytes,’ or *animal-plants*.” In this view, he went on,

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<sup>87</sup> D. J. Browne, “American Forest Trees,” *The North American Review* 35, no. 77 (1832), 404. It was Richard W. Judd’s excellent essay that drew my attention to Browne’s essay in *The North American Review*. Richard W. Judd, “A ‘Wonderfull Order and Balance’: Natural History and the Beginnings of Forest Conservation in America, 1730–1830,” *Environmental History* 11, no. 1 (2006), 8–36.

Though the wood in the centre of the trunk and large branches—the produce of buds and leaves that have long ago disappeared—may die and decay; yet whole new individuals are formed upon the surface with each successive crop of fresh buds, and placed in as favorable communication with the soil and the air as their predecessors, the aggregate tree would appear to have no necessary, no inherent, limit to its existence.<sup>88</sup>

To Gray's readers, the above statement reflected doubt in some deep-seated beliefs in Christian theology. "I receive your essay on the 'Longevity of trees,'" Curtis wrote to Gray, "which was particularly interesting from the fact that I have been for some time wanting the facts in the case, & had no books to refer to."

I am not disposed to carry the age of trees beyond the era of the flood, though I know not why Cypresses may not have endured a few months submersion without destruction. Taking the Septuagint chronology which I deem the true one, we have 500 yrs: between the Deluge & the present time, & I have reckoned that trees will hardly be found older than that.<sup>89</sup>

Gray continued to explore the issue of the longevity of trees in a review essay titled "Chemistry of Vegetation." In it he applied even more neutral and rationalist language to describe what in many Americans' eyes belonged to an indescribable monument in nature:

In its growth, this tree withdrew from the air 1,305,333 pounds of carbonic acid gas, and replaced it with 949,333 pounds of oxygen gas; a quantity sufficient to maintain the respiration of a single man for 1,100 years; so that year by year, it has supplied oxygen to the air just at the rate at which a single man would consume it... An immense operation, therefore, on a scale commensurate with the grandeur of nature, is in progress, and has been going on ever since the world began. *What is its object?*<sup>90</sup>

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<sup>88</sup> Asa Gray, "The Longevity of Trees," *The North American Review* 59, no. 124 (1844), 199, 210, 238.

<sup>89</sup> MAC to AG, 7/10/1844, *HL*.

<sup>90</sup> Asa Gray, "Chemistry of Vegetation," *The North American Review* 60, no. 126 (1845), 176.

It was in another review essay titled “The Scientific Result of the Exploring Expedition” that Gray made more explicit his answer to this question. This time he approached the issue from a zoological point of view. “How would you define an animal?” he asked his readers. “For the genuine credentials of an animal are, not the faculty of locomotion, which is an incidental convenience rather than a necessity of animal life, nor the possession of a head or heart, one or both of which are frequently wanting, but (let the *gourmand* be thankful to science for the distinction) the possession of a mouth and a stomach.” Then he introduced Abraham Trembley’s famous experiments on the Hydra: “Some polyps, such as the Hydra, it is well known, may be turned inside out, like the finger of a glove,—or as the pliant office-holder turns his coat when the *ins* and the *outs* change places,—and still feed and digest unconcernedly, and thrive and batten in all respects just as well as before.” Notably, Gray assured his readers, it was such an indifferent creature that built up one of the most spectacular pieces of architecture in nature: coral reefs. Similar to an ancient tree, he wrote, a coral reef was by no means an individual.

The coral is... a body-corporate, or community,—not by any means constituted, however, on the democratic principle of the association of originally independent individuals for the promotion of common objects, but really formed on the patriarchal system,—an analogy which we commend to the notice of writers on the theory of government.<sup>91</sup>

From ancient forests in New England to coral reefs in the Pacific, from the longevity of trees to the chemistry of vegetation, Gray’s series of essays in *The North American Review* revealed his fascination with harmony, continuity, and mutual

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<sup>91</sup> Asa Gray, “Scientific Results of the Exploring Expedition,” *The North American Review* 63, no. 132 (1846), 215-216, 219-220.

dependency that prevailed in various levels of nature—from those building blocks that constituted an individual, to those individuals that constituted a species, to those species that constituted the system of nature. No wonder he soon asked his readers to consider “the general economy of organic nature, in its relations to man.” Man was “an interpolation” upon nature, he urged his readers to bear in mind. “The whole system [of nature] is perfect without him. It is independent of man, and might have gone on—as geologists assure us it did go on—for ages before his introduction upon the stage. But when man was at length introduced into this his temporary residence, and to him was ‘given dominion over’ the organic world, he found a system in operation, every part of which he could turn to good account.” Thus, he continued, men’s place in this “general economy of organic nature” could be summarized as below:

Every other portion of creation, every species of plant or animal, stands in some particular part of the series, in more or less explicit relation to certain tribes upon which it feeds, and to certain others by which it consumed. But omnivorous man steps in at each and every stage through which organic matter, or food, passes. Sometimes, interrupting the series at the initial step, he consumes the green and tender herb, or the foliage,—the very instrument for creating food. To subsist in this way, however, requires almost uninterrupted feeding. Possibly, men could thus subsist in the warmer climates; but a small number of them would require a large extent of territory, and even the lowest form of civilization would be unknown. Man, therefore, waits for the next step in vegetation; he allows the leaves to perform their office; and when they have accumulated a store of more concentrated nourishment in the root, as in the turnip, or in subterranean stems, as in the potato, or even in the leaves themselves, as in the cabbage, he seizes the deposit as his own.<sup>92</sup>

So how could man, as an intruder of nature and a creature gifted with the intelligence to utilize nature, come to know nature? Gray gave a direct answer: “The

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<sup>92</sup> Gray, “Chemistry of Vegetation,” 181-183.

unity which we perceive in nature,—the striking adaptation of one part to another, and of each to the great whole, the mingling of beauty with usefulness,—to these sound science has ever delighted to point, as the proof that all is the direct handiwork of a single omniscient Creator.” Even more so, he added, he had reasons to believe that there *was* a “Governor” overseeing the workings of nature.

... that living beings have been created upon a foreordained, harmonious plan; that the animals of each great division or class have been formed according to a common type, which may be traced as a basis through the whole; and that the affinities or resemblances we behold, and which so mislead our author and his school, spring naturally and necessarily from this fundamental unity of plan; that the various kinds or genera of each class or order are modifications (not of any animal of this or a lower class, but) of the type or ideal plan which it has pleased the Almighty Creator to follow,—modifications, by express and specific design, adapting each species to the conditions in which it is intended to live. Thus would we account for that unity in the midst of diversity which the naturalist observes and admires, when with enlarged vision he sees that all the apparent complexity around him is pervaded by, and based upon, a real simplicity and harmony...

“It is this view which well grounded naturalists have ever delighted to contemplate, as affording the highest evidence of design,—of design at once the most comprehensive and the most special,” he concluded.<sup>93</sup>

Gray defended his view of nature relentlessly. He was not reluctant to apply the harshest terms to assault what he considered hideous views of nature. He called *Vestiges* “a sort of Hindoo tansmigration run mad” and “a heathenish incantation.” Regarding a volume by American chemist John William Draper in favor of explaining “every thing in the animal and vegetable economy upon chemical principles,” he wrote that “an ill-natured critic might compare [it] to an *omelette soufflée*, of which

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<sup>93</sup> Gray, “Explanations...,” 470, 499-500.

Dumas and Cuvier may have furnished the egg, Humboldt, Lyell, and others, the condiments, and Dr. Draper the intumescence.”<sup>94</sup>

“I shall look for your splash at the Vestiges with much interest,” Curtis wrote to Gray. “I read that work with much pleasure on account of its interesting subject & ingenious handling, but I abominate its philosophy.... I feel that his system is wrong, though it would not be easy for me to give it a thorough logical refutation, because I have not science enough.”<sup>95</sup> In another letter, he claimed that Gray had done a superb job in demolishing Draper’s authority. “Poor Draper,” he exclaimed, “Nobody will want to read a book that he knows beforehand contains such intolerable bombast as the two last extracts in your article.” “[U]se up the quacks!” urged Curtis, “they had reigned long enough in this country.”<sup>96</sup>

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Gray found that he had been granted ample opportunity to “use up the quacks.” In 1844, the prestigious Lowell Institute invited him to give a series of lectures on botany (for \$1,000). Gray considered the lecture a big deal (both intellectually and financially). In March 1844, after giving the first lecture, he wrote to Morris that “I am deeply engaged with my public lectures in Boston—before the Lowell Institution—(which I shall finish in two weeks more—) and which go off with

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<sup>94</sup> Gray, “Explanations...,” 473, 499-500; Gray, “Chemistry of Vegetation,” 192-194.

<sup>95</sup> MAC to AG, 3/21/1846, *HL*.

<sup>96</sup> MAC to AG, 1/6/1845, *HL*.

considerable éclat. The lively interest which my large and most intelligent audiences seem to take in the subject quite repays me for my labor.”<sup>97</sup>

Gray’s lecture was so successful that the Lowell Institute decided not to let the professor go. The Institute’s insistence (as well as his financial status) compelled him to give a second in 1845, and he agreed to give a third in 1846. On Thanksgiving evening, 1845, when he just finished the 1845 lecture series and was preparing the lectures of the next year, he pulled out a sheet of paper, settled himself, and began a letter to Miss Morris. “Here, my dear friend,” he wrote, “you have annexed what, with the aid of a lively imagination, will give you some idea of the ground I wish to occupy in my ensuing course.” “The materials I wish to use are very widely scattered—it takes much time to gather them, and they have to be strung upon the thread spun out of my own brain,—for I can do nothing till I can work out my own order of conception.”

Step by step, he outlined his “order of conception” for Morris’s reference. In his view, he would begin the series by offering the audiences a “biographical sk. [sketch] and historical development” of botany down to Linnaeus, particularly focusing on Linnaeus’s development of the artificial system. Then he would bring about de Jussieu and de Candolle’s natural system and use *Rosaceae* as an example to show how to apply the system to botanical study. With such background knowledge offered, he would use eight lectures (from Lecture 3 to Lecture 10) to show “Illustrations of striking or important natural families” (including *Ranunculaceae*, *Coniferae*, *Leguminosae*, *Umbelliferae*, and *Compositae*), with focus

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<sup>97</sup> AG to ECM, 3/25/1844, Box 1, Folder 3, *AGPL*.

on the “actual distribution of plants over the earth’s surface” and the “natural botanical regions or provinces of the world.” He would then use the last two lectures to discuss “man’s place in the general economy of nature,” and would elaborate such issues as “the influence of civilized man in interfering with the distinctions of these regions,” “the influence of man over the character properties & products of the plants he domesticates,” and so on. Once the lecture series concluded, Gray told Morris, he expected that the audience would think critically of issues like “effects of culture,” “origination of races,” “liability of artificial forms to run out,” “limit of flexibility & change in a species,—effect of the withdrawal of man’s influence in restoring the original state of things,” “permanence of species,” and the like.<sup>98</sup>

Almost seventeen years had passed since that day he ran into a *Claytonia virginica* “peeping through dead leaves.” During these years, he had ignited in others his spiral of delight in nature by publishing, lecturing, traveling, and corresponding. His view of nature had been well-grounded and established. He was a *bricoleur* who critically assembled elements taken from vitalism, transcendentalism, and natural theology, into a methodology advocated by renowned natural philosophers like Humboldt, Herschel, and Whewell. He felt ready to move on. “Should a kind Providence spare my life and health I shall hope to accomplish in a year or two—those unfinished labors which weigh so heavily upon me,” Gray told Morris in late

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<sup>98</sup> AG to ECM, [dated “Thursday (Thanksgiving) evening”; postmarked 11/26/1845,] Box 1, Folder 3, *AGPL*.



1844.<sup>99</sup> Now filling his mind was “a complete United States Flora” arranged by the natural system.

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<sup>99</sup> AG to ECM, 11/9/1844, Folder 3, Box 1, *AGPL*.

## CHAPTER TWO

### Being a Botanist

How could an American botanist author a *Flora of North America*? Professor Asa Gray often asked himself this question. Although he had spent the past decade pursuing this *Flora* project, he remained uncertain about the answer. He was only sure of one thing: to make his *Flora* project as complete and scientific as possible, he needed more specimens and information from the field; he needed an army of collectors who could survey the seemingly infinite American West; he needed to undertake the *Flora* project besides his daily teaching duties with the salary of \$1,000; and he needed colleagues and friends who could collaborate with him and accompany him through thick and thin.

Harvard was indeed “the place,” he thought. Now he had status, steady income, memberships to prominent scientific societies, and, above all, a botanical garden where he could cultivate, observe, and experiment on various plants characteristic of the North American flora. As he told Engelmann of St. Louis,

... for our Botanical Garden here; which I wish to renovate, to make creditable to the country and subservient to the advancement of our favorite science. I wish to see growing here all the hardy and half-hardy plants of the United States (as well as many exotics, etc.), and shall exert myself strenuously for their introduction. The Garden contains seven acres; the trees and shrubs are well grown up; we are free from debt, and have a small fund.

With the Garden serving as his experimental ground, he continued, he would have ample opportunities to examine living plants, which he thought should “vastly increase the correctness of our ‘Flora’.”<sup>1</sup>

Mid-nineteenth-century Boston was perhaps the best place in the country for researchers like Gray who pursued the possible marriage between botany and horticulture. Boston’s uniqueness in this respect was largely due to the active role of the Massachusetts Horticultural Society (hereafter MHS). Since its founding in 1829, the MHS had taken as its goal to “encourage and improve the science and practice of Horticulture, promoting the amelioration of the various species of trees, fruits, plants, and vegetables, and the introduction of new species and varieties.”<sup>2</sup> Inspired by the success of the Garden at Chiswick, the MHS acknowledged that the most effective way to achieve this goal was to establish an experimental garden.<sup>3</sup> The MHS then allied itself with the city movement, which advocated to change Bostonians’ custom of burying their dead in public space. In 1831, the MHS purchased a tract of land in Cambridge, Massachusetts, in part for setting up a “Mount Auburn Cemetery” and in part for establishing the experimental garden. But the MHS was soon forced to relinquish its right to the land at the insistence of those who had purchased the cemetery lots. However impressed they were by the MHS’s effort to honor the dead with flowers and shrubs, most lot-purchasers found it

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<sup>1</sup> AG to GE, 7/26/1842, *LAG*, 291-292.

<sup>2</sup> The Massachusetts Horticultural Society, *Transactions* (Boston: Isaac R. Butts, 1829), 14.

<sup>3</sup> The Massachusetts Horticultural Society, *Transactions*, 65

difficult to accept the juxtaposition of the experimental garden and the rural cemetery.<sup>4</sup>

But this apparent defeat proved beneficial for the MHS's long-term development. The MHS was then promised an annual compensation of some \$2,500 for the next two decades. In this light, John A. Lowell, a wealthy and well-respected businessman and an active member in the MHS, advocated a new agenda for the MHS's future development. In an essay published in *The New England Farmer* in 1835, he remarked that the MHS's failure to establish the experimental garden had undermined the "attempt to unite the private efforts of individuals with a vast scheme of carrying on a great garden on a joint account." As an alternative, he suggested that the MHS select "the able practical men and place them at the head of the society." "Let us remember," he wrote passionately, that "the limits of the society are the whole State, and that every cultivator is our brother and friend."<sup>5</sup>

For the next decade, the continuous influxes of cash and new blood made the MHS a permanent institution, in sharp contrast to other horticultural institutions of

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<sup>4</sup> This paragraph is based on Philip J. Pauly's notable *Fruits and Plains: The Horticultural Transformation of America* (Cambridge: Harvard University Press, 2007), 55-57, and I have checked the primary material that Pauly has used in his analysis. It should be noted that in his recent *Arcadian America: The Death and Life of an Environmental Tradition* (New Haven: Yale University Press, 2013), Aaron Sachs offers an excellent analysis of the role of the Mount Auburn Cemetery in American environmental thinking.

<sup>5</sup> John A. Lowell, "The Massachusetts Horticultural Society," *The New England Farmer, and Gardener's Journal, Containing Essays, Original and Selected, Relating to Agriculture and Domestic Economy, with the Prices of Country Produce* 13, no. 50 (1835), 393.

the day.<sup>6</sup> A network that entwined “practical men” with “latent talents” across New England emerged. In addition to wealthy Bostonians like Lowell, who enthused to set up their own gardens along with herbaria and libraries, an increasing number of nurserymen, pomologists, and rosarians earnestly devoted themselves to study how botany could contribute to horticulture and vice versa.<sup>7</sup> By the time Gray took charge of the Harvard Botanical Garden, James E. Teschemacher (1790-1853), corresponding secretary of the MHS, had celebrated the elevation of horticulture into a true branch of science. In his annual address given in 1842, he declared that horticulture was no longer “a mere crude mass of gardeners’ secrets for propagating or growing certain plants, of nostrums and recipes for destroying insects and cleansing trees.” All such matters, he pronounced, had “become the constantly improving art of applying scientific, rational and well-digested principles.” Horticulture was now “capable of becoming to the agriculturist what the chemical laboratory is to the dyer and the manufacturer,” he asserted.<sup>8</sup>

Gray’s ambition to rebuild the Harvard Botanical Garden and Bostonians’ rising interest in scientific horticulture all gave shape to his thinking as he prepared *The Botanical Text-Book* (1842).<sup>9</sup> Besides instructions on plant-collecting and

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<sup>6</sup> For an exemplary case in this regard in the history of the New York Horticultural Society, see Pauly, *Fruits and Plains*, 55-57.

<sup>7</sup> Pauly, *Fruits and Plains*, 58-79.

<sup>8</sup> James Englebort Teschemacher, *An Address Delivered before the Massachusetts Horticultural Society, at Their Fourteenth Anniversary, September 16, 1842* (Boston: Dutton and Wentworth’s Print, 1842), 7-8.

<sup>9</sup> In fact, a year before he came to Harvard, he had helped Andrew J. Downing (who would soon become the country’s foremost landscape gardener) publish the first

preservation for herbaria, he decided to add a section on how to collect living plants for the garden, with a remark on the recently invented Wardian case, a greenhouse-like device that enabled collectors to deliver living plants across long distances.<sup>10</sup>

Then, between August 11 and November 3, 1843, he and Sullivant embarked on an expedition to the Alleghany Mountains with the goal of obtaining living plants and seeds for the Harvard Botanical Garden.<sup>11</sup> Before his departure, he invited his old collecting partner John Carey to join them. Carey declined. “I wish you much happiness in your projected tour,” he wrote. “[B]ut if Mr. S be, as you report, so very energetic a man, I shall expect to hear that like the famed Kilkenny Cats—[you] eat each other all up—But if he comes in any degree short of your measure of restlessness, you will doubtless ‘use him up’ or kill him.”<sup>12</sup> Gray also sent his itinerary to Curtis of Hillsborough, gauging the southern botanist’s reaction. “Your contemplated southern tour is not well arranged by any means,” Curtis replied.

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American edition of John Lindley’s *The Theory of Horticulture* (1841; the original edition was published in London in 1840).

<sup>10</sup> Gray, *The Botanical-Text Book*, 396. Gray met the device’s inventor, Nathaniel Bagshaw Ward, in 1839, when he was doing research and purchasing books in London. He described the encounter in a letter to Torrey: “I called last evening, for the first time on Mr. Ward. His new fashioned green house is in capital condition and I have promised to breakfast with him to-morrow that I may see it by day-light...” AG to JT, 9/24/1839, *AGPP*. In 1853, he would quote Sir William’s words, assuring his readers that “not a single portion of the civilized world” had not “benefited” from the Wardian cases. Asa Gray, “N. B. Ward, F. R. S., &c., On the Growth of Plants in Closely Glazed Cases. Second Edition. London: Van Voorst. pp. 143, 12mo.,” *The American Journal of Science and Arts (Second Series)* 66, no. 46 (1853), 133.

<sup>11</sup> AG to WJH, 8/11/1843, *LAG*, 306.

<sup>12</sup> JC to AG, JC to AG, 7/28/1843, *HL*.

September is the most sickly month at Wilmington & Fayetteville. And although you might escape disease, you would run much risk of incurring it at that season... You would be in the swamps & savannas breathing malaria continually, & exposed to the hot sun of midday, both of which are unsafe experiments. You might do all this, it is true, & escape disease; but you would run some risk. In a small way, I have done such things without harm; but such things have brought people to beds of sickness & to the grave.<sup>13</sup>

Gray's plan would drive himself and Sullivant to step on "the most dangerous course," noted Curtis.

Gray and Sullivant prevailed. "[H]ow glad I was with all our Botanists to know that you had returned in safety from the perilous South," Edward Tuckerman wrote to Gray in December 1843. "Dr. Torrey mentioned in a letter that your absence was protracted longer than he expected—& he feared that you might have been seized with sickness." "What a Garden we shall have!" he exclaimed in anticipation.<sup>14</sup>

Indeed, as Tuckerman expected, Gray and Sullivant brought back great numbers of roots, seeds, and bulbs that characterized the flora of the southern Appalachians. Gray soon opened up a section in his garden for these fragile immigrants. Among those botanical novelties that made the expedition worthwhile was *Schweinitzia odorata*. It was at the base of Table Mountain that Gray and Sullivant found this plant "growing in a cluster from the roots of Galax." Intrigued, they knelt down and removed the whole mass. Their anxiety to preserve the fragile *S. odorata* in "a living

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<sup>13</sup> MAC to AG, 5/31/1843, *HL*.

<sup>14</sup> ET to AG, 12/16/1843, *HL*.

state” ran so high that they did not even examine the species’s “mode of attachment.”<sup>15</sup>

Gray packed the living specimen of *S. odorata* in a box full of Sphagnum and brought it back to Cambridge. He was glad that this fragile creature not only survived but also thrived and bore well-formed buds. With the living specimen of *S. odorata* in hand, he felt ready to prepare a thorough reexamination of the characters of *S. odorata*. His target was Thomas Nuttall’s description of *S. odorata* in *Genera of North America Plants*. He found that Nuttall’s authority over the North American flora had obscured even the most competent botanist’s mind. Take, for example, the acclaimed Augustin Pyramus de Candolle. Although de Candolle possessed a specimen of *S. odorata*, Gray realized, the French botanist had failed to offer a correct diagnosis of the species for his trust on Nuttall’s classification.<sup>16</sup>

Such “scientific-horticultural operations” were as fruitful as Gray had expected.<sup>17</sup> In January 1846, Gray communicated an essay titled *Chloris Boreali-Americana* to the American Academy of Arts and Sciences. The chief aim of the essay, he noted, was to illustrate “new, rare, or otherwise interesting North American plants, selected chiefly from those recently brought into cultivation at the

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<sup>15</sup> Asa Gray, *Chloris Boreali-Americana: Illustrations of New, Rare, or Otherwise Interesting North American Plants: Selected Chiefly from Those Recently Brought into Cultivation at the Botanic Garden of Harvard University* (Cambridge: Metcalf, 1846), 16.

<sup>16</sup> Gray, *Chloris Boreali-Americana*, 16, 18-19.

<sup>17</sup> AG to ECM, 12/5/1842, Folder 3, Box 1, ASPL.



Botanical Garden of Harvard University, Cambridge.”<sup>18</sup> The essay was well-received. It was soon published in *Memoirs of the American Academy of Arts and Sciences*, the MHS’s *Transaction*, and many other horticultural and natural-history magazines. Gray was satisfied. He thought that he could direct Bostonians’ unabated enthusiasm for botanical novelties to the cause of science. “I have a plan to publish, from time to time, figures of rare or interesting North American plants, chiefly those cultivated in our Garden and those upon which I may throw some light,” he told Sir William. “I think there are persons enough here [in Boston] interested in the matter, including gentlemen of public spirit here who would encourage it for the Garden’s sake, to nearly defray the expense, which is all I desire or expect.”<sup>19</sup>

But the 1843 expedition to the Alleghany Mountains turned out to be the *last* expedition that Gray embarked on for three decades. Constrained by his teaching schedule, obligations to scientific societies, and taxonomic work at the herbarium, he hardly stepped out of the space confined to and defined by Garden Street and Harvard College. He longed for a long-distance expedition. Two years after his arrival at Harvard, his physical health weakened (“my overtaken frame,” he groaned to Elizabeth Carrington Morris). He felt confined, isolated even. He was away from home and friends who had supported him through thick and thin. His father passed away in September 1845, and he did not even have a chance to see his father for the

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<sup>18</sup> Gray, *Chloris Boreali-Americana*, 1.

<sup>19</sup> AG to WJH, 2/28/1843, LAG, 299.

last time.<sup>20</sup> “Shall you ever come down East, and look in upon a botanist, surrounded with herbs living and dead?” he wrote solemnly to Morris.<sup>21</sup>

Carey was concerned about Gray’s increasingly delicate health. (Gray, the Kilkenny Cat, was being domesticated, he might have thought.) “Exercise,” he suggested, “of a strong kind, appears necessary for your well-being, & I should think your proposal to ride a horse-back, before breakfast, might answer an excellent purpose.”<sup>22</sup>

Gray marked this paragraph.

### **“My Maximus Apollo”**

Fortunately for Gray’s part, although he stayed put in Boston, the city had rapidly become the nexus of transatlantic and transcontinental communications. (By the time Gray had arrived at Harvard, a railroad had connected Boston with St. Louis and made what Gray had long regarded as the Far West accessible.)<sup>23</sup> Gray took full advantage of Boston’s increasingly complete transportation system. Through his hands, numerous boxes of specimens were unpacked, grouped, labeled, repacked, and addressed to individuals and institutions across Europe and North America. He aspired to make himself a bridge that connected the botanical communities of both

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<sup>20</sup> “AG to ECM, [dated “Thursday (Thanksgiving) evening”; postmarked 11/26/1845,] Box 1, Folder 3, *AGPL*.

<sup>21</sup> AG to ECM, 4/14/1845, Folder 3, Box 1, *AGPL*.

<sup>22</sup> JC to AG, 10/29/1845, *HL*.

<sup>23</sup> See AG to GE, 11/4/1843, *LAG*, 312.

sides of the Atlantic. Gray's close botanical friends were delighted that the Harvard professor offered such public services. Curtis, when asking Gray to secure a present copy of Sullivant's *Musci Alleghanienses* for him, wrote, "You should then be my Magnus—no, you are Magnus already—my Maximus Apollo." "I note what you say in particular with regard to your Botanic Garden & an interchange of plants with ours," Sir William wrote to Gray from Kew. "Nothing you may be sure will be more gratifying to me."<sup>24</sup>

But a Maximus Apollo cornered in Cambridge, Massachusetts, needed correspondents to extend his botanical territory. In 1843, when just settling down in Cambridge, he informed Engelmann that he was now "running about the country after plants for it, and imploring correspondents in every part of the country to send me all they can." Gray's correspondents responded to the call with enthusiasm. Sullivant replied that, receiving Gray's request, he "immediately went out to the Darby plains & brought home roots & seeds of what I could find." Curtis, too, replied that he would soon have "a box of roots" prepared to send to Gray. And Gray soon got in contact with a large group of plant enthusiasts who studied botany in kitchen gardens. In December 1842, through the introduction of William Darlington of Philadelphia, Elizabeth Morris sent a parcel of seeds to Gray. Gray was delighted. "Perhaps our garden, though not very rich in showy plants, may contain many-hardly species especially which you might like to cultivate," he replied. "Seeds also, especially from abroad begin to come in; any of which I shall most gladly share with

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<sup>24</sup> MAC to AG, 1/13/1846, *HL*; WJH to AG, 11/10, 1842, *HL*.

you... Trusting that I may have the pleasure of your correspondence, as circumstances permit.”<sup>25</sup>

Gray’s corresponding network expanded and flourished. By the time he published his well-received *A Manual of the Botany of the Northern United States* (1848), he had recruited a group of colleagues, all at around the same age, and all charged with the fiery passion for studying the North American flora. Four pillars in particular supported Gray’s botanical circle: William S. Sullivant, Edward Tuckerman, Moses A. Curtis, and George Engelmann.

Early in the 1840s, William S. Sullivant decided to retire to his estate in Columbus, Ohio, after accumulating wealth by investing in the Ohio Stage Company, the Clinton Bank, the City Bank of Columbus, flour milling, and so on. Filling this successful merchant’s mind were now plants, in particular *Musci* and *Hepaticae*, or mosses and liverworts. He soon proved himself perfectly capable of handling such miniature creatures. Gray was impressed. When preparing for *A Manual*, he invited Sullivant to take charge of the section on *Musci* and *Hepaticae*. The two botanists then collaborated on various projects, including a series of botanical reports for the U.S. Exploring Expedition. Sullivant was independently wealthy and capable of dedicating his full time to botany. “[Y]ou understand,” he once told Gray, “I have never received & do not expect anything for my own services.” Sullivant was by virtue a gentleman naturalist, or “a noble fellow,” as Gray once told Sir William.<sup>26</sup>

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<sup>25</sup> AG to GE, 9/26/1842, *AGPG*; WSS to AG, 11/21/1842, *HL*; MAC to AG, 11/7/1843, *HL*; AG to ECM, 12/5/1842, Box 1, Folder 3, *AGPL*.

<sup>26</sup> WSS to AG, 11/8/1856, *HL*. See Rodgers III’s remarkable “*Noble Fellow*.”

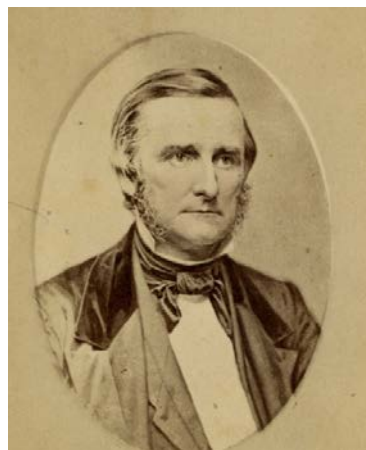
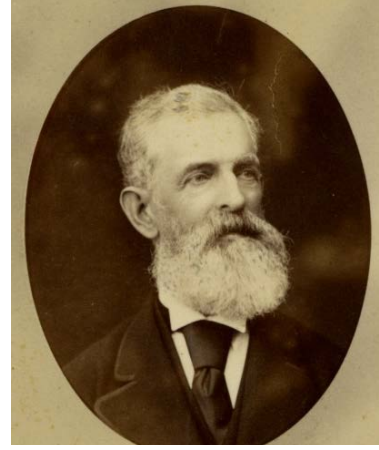
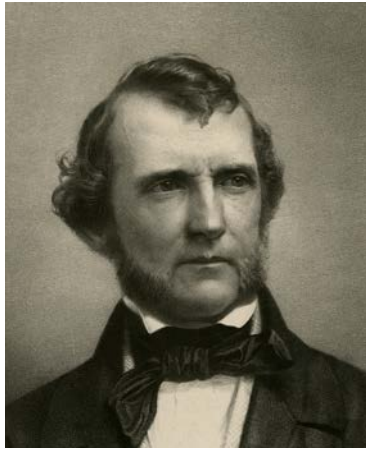


Figure 2-1. Gray and his botanical circle (clockwise from top left): Gray, Torrey, Tuckerman, Engelmann, Curtis, and Sullivant. Courtesy of the Archives of the Gray Herbarium and the Archives of the Farlow Herbarium of Cryptogamic Botany, Harvard University, Cambridge, MA, USA.)

Another figure central to Gray's botanical network was Edward Tuckerman. Born to a merchant family in Boston, Tuckerman studied at the Union College in Schenectady and then at Harvard Law School. Between 1841 and 1842, when Gray had just begun his career at Harvard, Tuckerman traveled about Europe and studied with Elias Fries, renowned mycologist at Uppsala. He then returned to Boston, and launched several collecting trips to the vicinity of New England, including a trip to the White Mountain with Gray. For the next ten years or so, he taught and served as a museum curator at Union College in Schenectady and became a lecturer in history at Amherst College in 1854. "This is a very pretty place & has fine collections in mineralogy, geology, & zoology—& such a genial natural philosopher is Dr. Hitchcock whom every body knows," he wrote to Gray upon arriving in Amherst. Tuckerman would spend the rest of his life there, first as a professor in history, then a professor in botany. He enjoyed the quiet—if not tranquil—environment of Amherst, although sometimes he did miss the bustling and vibrant academic atmosphere of Boston. Gray became the depot from which Tuckerman could keep in touch with the scientific circle at large. "[P]erhaps it is my fault) I do not know anything about the publication of the Academy, now I am so far away nor have I seen any proceedings for years," he told Gray.<sup>27</sup>

Moses A. Curtis was a clergyman chiefly residing in Hillsborough, North Carolina. Raised in a religious family, he took the Holy Orders in his early years. He graduated from the Williams College, and before becoming the rector of the Protestant Episcopal Church in Hillsborough, he served as a tutor in a governor's family in

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<sup>27</sup> ET to AG, 5/31/1858, *HL*; ET to AG, 12/28, 1854, *HL*.

Wilmington, North Carolina. Curtis found the flora of Carolinas dear to his heart. He botanized intensively, distributed his collections widely, and packed his letters to friends with vivid experiences in the field. “I had intended leaving here to day,” he wrote to Gray, “but have a bad cold, & am moreover half distracted with the poison of *Rhus radicans* [poison ivy].” “[H]ow they itch!” he cried. “Oh, Sir, your northern fellows know nothing of what we southerners endure in the collection of objects of Nat. Hist[ory].”<sup>28</sup>

In the mid-1840s, Curtis decided to devote his spare time to studying fungi. “I ascend now to more primitive organisms—to darker mysteries—to the life that lives upon death, & which finishes & begins the circle of vitalism,” he wrote to Gray half-jokingly. Then, for the next decade or so, he painstakingly composed the instruction for collecting fungi, circulated it among his correspondents, and begged his correspondents not to send specimens abroad but to him instead. During the 1840s and the 1850s, by collaborating with the renowned British mycologist Miles J. Berkeley, Curtis emerged as the country’s foremost mycologist. “I can safely say now, that I have the largest Cryptogamic collection in America,” he told Gray in 1852.<sup>29</sup>

Finally, a correspondent without whom Gray’s network could hardly reach the vast American West was George Engelmann of St. Louis. A German immigrant and physician, Engelmann received the most prestigious scientific training in Europe before moving to the U.S. in 1832. He proved himself capable of dealing with plants

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<sup>28</sup> MAC to AG, 11/23/1846, *HL*; MAC to AG, 3/9/1848, *HL*.

<sup>29</sup> MAC to AG, 4/16/1849, Folder 2, Box 1, *CMAC*; MAC to AG, 8/9/1852, *HL*.

and patients simultaneously. Taking full advantage of St. Louis's geographical location, he devoted all his leisure time to studying the peculiar flora of the American prairies on the one hand and to smoothing specimen exchange on a transcontinental scale on the other. "I have a few days ago finished an immense labour," he once told Gray, "distributing about 8000 or 9000 duplicates amongst some 30 correspondents!" Gray greatly admired Engelmann's devotion to botany. In 1842, he assured Engelmann he definitely needed Engelmann's "invaluable service" to furnish the Harvard Botanical Garden with "almost every thing Western." In the years to come, the two botanists would dispatch a group of talented men to secure "every thing Western." The *Annual Report of the Board of Regents of the Smithsonian Institution* would soon note the two botanists' joint contribution to extending the intellectual frontier in tandem with the ever-expanding American frontier.<sup>30</sup>

### **"Free and liberal interchange of specimens"**

Notably, none of the four figures in Gray's close botanical circle made their livings by studying botany or regarded themselves as "professional botanists" (Sullivant was a merchant, Engelmann a physician, Curtis a clergyman, and Tuckerman—although he spent most of his professional life at Amherst College—was appointed as a professor in history). "Botany is only recreation to me," Curtis told Engelmann in

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<sup>30</sup> GE to AG, 6/25/1846, *HL*; AG to GE, 9/26/1842, *AGPG*; The Board of Regents of the Smithsonian Institution, *Annual Report of the Board of Regents of the Smithsonian Institution to the Senate and House of Representatives, Showing the Operations, Expenditures, and Condition of the Institution during the Year 1849* (Washington: the Printers to the Senate, 1850), 16.



1841. "I am afraid to open a larger play ground than is embraced by N. America... You will see and appreciate the ground of my action, when I inform you that I am Clergyman, & not an 'Esqr.' I have no leisure to become a general Botanist."<sup>31</sup> To Gray, he wrote,

So now for the chief matter which induces me to write—the European plants on the way to you. I would not snap my finger for the whole of them! I have no ambition to be a Botanist, properly so called. I should like it well enough, if I had nothing else to do; but as I meddle with Flora, only for recreation, I find quite play enough in her N. American products. To those I resolved from the first to confine myself, & have always given away whatever foreign plants have come to hand.<sup>32</sup>

Even the well-trained botanist Tuckerman developed the same opinion as Curtis. He did not care much about those rules that defined botany as a discipline. He studied botany for his enjoyment or sense of achievement. He published his studies at his own expense and circulated them among his close correspondents. As he put it in a letter to Gray, "My being alone here must be my excuse for venturing to publish conclusions on doubtful plants without laying them before those whose opinion might save me from many errors and make the results infinitely more valuable but I assure you I have never studied my plants before as I have there; and I hope and feel confident that the article will not meet your disapprobation." In another letter, he exclaimed to Gray that he had developed an entirely new classification of some 300 species of *Carex* (a notoriously difficult genus) and "thereby a view or sketch of the whole genus." Tuckerman was excessively proud of what he had achieved as a botanical enthusiast. "I do not think a professional man,

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<sup>31</sup> MAC to GE, 3/4/1841, Folder 2, Box 1, *CMAC*.

<sup>32</sup> MAC to AG, 6/18/1846, *HL*.

however patient could have given the labour that this has cost. I believe now I could name any European Carex.”<sup>33</sup>

A critical element that gave shape to mid-nineteen-century botanists’ self-identity was the so-called free and liberal exchange of specimens. In his 1833 essay on plant collecting, senior botanist Charles Short elaborated how such a seemingly trivial manner mattered in American botany in general: “It should be the constant aim of every botanist not only to increase his own knowledge, by every possible opportunity, but to add something to the general stock; and this is most readily and effectually done, by a free and liberal interchange of specimens with other botanists.” “In this way,” he continued, “you will contribute essentially towards the formation of that great desideratum in our Botany—AN ACCURATE AND COMPLETE FLORA OF THE AMERICAN UNION.”<sup>34</sup>

Gray admired Short’s words greatly. As he remarked in *Elements of Botany* (1836),

In collecting plants, a botanist should always bear in mind the probable wants of his friends and laborers in the same field of science. Thus not only will he have the high gratification of imparting what he knows will be joyfully received, and of contributing to the enlargement and diffusion of correct knowledge, in which all *true* naturalists have a common interest; but he will also by such means be certain of receiving, in exchange for his duplicates, the plants of those districts and countries which he might be unable to obtain by any other means; thus advancing his own attainments whilst promoting those of others.<sup>35</sup>

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<sup>33</sup> ET to AG, 1/2/1843, *HL*; ET to AG, 2/17/1843, *HL*.

<sup>34</sup> Short, “Instructions...,” 73-74. A letter Short sent to Rafinesque well demonstrates how he put the principle into practice. CWS to CSR, 9/7/1834, Reingold, *Science in Nineteenth-Century America*, 47-48.

<sup>35</sup> Gray, *Elements...*, 403.

Gray would retain this view for the rest of his life. In 1844, when he received a letter of thanks from Morris about botanical specimens he had sent previously, he replied, “Surely I am glad the dried plants please you, and that the duplicates will allow you in part to share the pleasure I enjoyed;—in giving them away to your correspondents. This is really one of the pleasures of Botanists, who while glad to receive, are always ‘ready to distribute’.”<sup>36</sup> Some forty years later, in November 1885, touched by a silver vase presented to him by his students and colleagues for his 75th birthday, he prepared a thank-you note: “Among fellow—botanists—more pleasantly connected than in any other pursuit by mutual giving and receiving,—some recognition of a rather uncommon anniversary might naturally be expected.”<sup>37</sup>

“Being a Botanist” turned out to be a regular phrase when one botanist proposed for specimen exchange with his or her fellow botanists. For example, in 1837, after sending a letter to John Carey about his plan in rearranging his herbarium, Gray received a letter from Carey asking for specimen exchange. “Now, if you are as good a fellow as I think you are,” Carey noted, “you will not put this letter into your pocket & forget all about it, but keep it handy for reference, as you are examining your plants, that I may be the better for what you have to spare.”<sup>38</sup> Curtis, too, when trying to initiate correspondence with Engelmann, wrote, “I take the liberty of

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<sup>36</sup> AG to ECM, 4/24/1844, *AGPL*.

<sup>37</sup> The text of Gray’s thank-you letter can be found in the website created by the Gray Herbarium for Gray’s Bicentennial Celebration ([http://www.huh.harvard.edu/libraries/Gray\\_Bicent/gray\\_birthday.htm](http://www.huh.harvard.edu/libraries/Gray_Bicent/gray_birthday.htm)). For my part, it was during my research at the Huntington Library that this particular letter called my attention. The letter is part of *Papers of Charles Russell Orcutt, 1882-1899*.

<sup>38</sup> JC to AG, 4/24/1837, *HL*.

addressing you, on the supposition that, being a Botanist, you will be disposed to make exchange of specimens.”<sup>39</sup>

But if two corresponding botanists were separated not only by geographical space but also by social standing, the tone and style would not be as egalitarian and reciprocal as Curtis’s letter. Normally those who were hoping to initiate the correspondence would have to secure “introduction letters” or “referees,” and those who received such introduction letters would at times send letters to check the applicant’s credibility. “I got a letter lately from one Mead in Illinois, who says he was advised by yourself to make acquaintance with me. I never heard of him before. Who is he? & what sort of a Botanist is he?” Curtis asked Gray.<sup>40</sup> Likewise, when Gray asked him if there were good florists residing in the Carolinas, Curtis suggested a Mr. McRee and offered some hints by which Gray could successfully initiate the correspondence.

I have for the last five years begged him to get a few rare plants for me, but have got nothing. This year I offered a large bribe of living & dried plants if he would collect a few things, which he accepted, only adding that I must get him a parcel of Tulips from this place which is rather rich in that plant. I have not heard from him yet, & know not what he has done. He is a generous man, & is perfectly willing to assist anybody; only his heavy practice & a good deal of our Southern vis inertiae affords an excuse for not drawing upon his moments of rest & occasional leisure.<sup>41</sup>

Sometimes a correspondent with relatively junior scientific standing had to “present” or “tribute” specimens to senior colleagues without asking for any in

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<sup>39</sup> MAC to GE, 11/9/1840, Folder 2, Box 1, *CMAC*.

<sup>40</sup> MAC to AG, 3/14/1842, *HL*.

<sup>41</sup> MAC to AG, 9/29/1842, *HL*.

return. In February 1844, Sullivant wrote to Gray about his correspondence with the leading British bryologist William Wilson (1799-1871). Though surprised by how much Wilson had asked from him, wrote Sullivant, he would supply whatever Wilson wanted until he proved himself as a “profitable correspondent.”<sup>42</sup> Gray, too, when he was but a nominal professor, told Curtis that he would “tribute” whatever he might be able to collect in the Carolinas so long as Curtis could furnish him with essential traveling information.<sup>43</sup> Curtis, likewise, when Sullivant put him in contact with the celebrated British mycologist Miles J. Berkeley, used the following words to seduce Berkeley to accept him as a correspondent:

Relying upon the kindly feelings which I have ever found to characterize the student of Natural Science, I will offer no apology for addressing you. I have been making enquiries for some person skillful in Mycology, with whom I might have the benefit of sympathy, correspondence, & exchange of specimens, & have been recommended to apply to you. I make the application with more confidence & wish more pleasure since I learn that you are a Clergyman of the Church of England; bring myself a Clergyman of the Prot: Episc: Church in this country.

He then grumbled about how difficult it was to study mycology in his side of the Atlantic. “[W]ithout suitable books, & with no man in this country to give me the least assistance,” he wrote, “I find the work to be slow & tedious.” He looked forward to “the encouragement of sympathy, & the assistance of an active, able, hand.” Surely enough, he assured Berkeley, “If the acquisition of American Fungi be an object, I can make you a full return for all the aid you can extend to me.”<sup>44</sup>

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<sup>42</sup> WSS to AG, 2/20/1844, *HL*.

<sup>43</sup> MAC to AG, 6/21/1841, *HL*.

<sup>44</sup> MAC to MJB, 4/14/1846, Folder 1, Box 1, *CMAC*.

When two botanists eventually came to agree to begin regular correspondence, the corresponding relationship between them remained subject to change. With letters and specimens in hand, botanists carefully evaluated if it was worthwhile to maintain correspondence according to quality and quantity of specimens in exchange, etiquettes as shown in the other party's letters (for example, how many days it took for the correspondent to reply), epistolary forms, authenticity of information contained, and other criteria. In a letter to Gray, Curtis declared that he had decided to cease his correspondence with senior botanist William Oakes (1799-1848):

I do not think I shall bring myself to write to Oakes, if he does not approach me so far as to acknowledge the plants I have sent. I am not fond of the cold distance of royalty: A man so far up in the air—down in the mud—or over the sea—that he cannot be spoken with, will not do for me. I have already made about as much approach—all under your direction—as I can well compass. A mere nod of recognition would induce me to take a few more steps,—but probably not a step without it.<sup>45</sup>

In 1848, learning from Berkeley that August Carl Joseph Corda, a Czech mycologist, would come to New Braunfels to superintend the estate of Prince Colloredi and might be happy to exchange specimens with him, Curtis promptly wrote to Engelmann asking for an introduction letter. "I desire an exchange of Fungi with him," he explained. But it turned out that Corda was not a valuable correspondent for Curtis. Curtis soon discontinued his correspondence with Corda. "The high favor of his patronage I do not regard as sufficient equivalent for the specimens I would have sent him," he told Engelmann. "There is some fiction in his letter, to which, as

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<sup>45</sup> MAC to AG, 1/15/1847, *HL*.

you are aware, he is somewhat addicted. He said to a correspondent of mine, that he had traced spiral vessels for 40 feet!"<sup>46</sup>

A botanist who failed to properly practice the free and liberal exchange of specimens would discover that his or her correspondents could be nasty. The once intimate and supportive network of correspondence could be transformed into the spider's web—as it were—that entangled his or her botanical endeavors. Thomas Nuttall, arguably the most celebrated botanical explorer in the United States during the first half of the nineteenth century, was among those who once got entangled in the web. The one who nailed him down was a young botanist named Asa Gray. Now known as the Nuttall-Gray controversy, the incident took place late in 1840, when Nuttall just finished his collecting trip across the North America. In November, Gray came to the Academy of Natural Sciences in Philadelphia for herbarium research and happened to meet Nuttall there. He was impressed by Nuttall's new collections. But upon hearing Nuttall's public talk concerning the botanical discoveries made during the collecting trip, he grew disappointed. He found that Nuttall utterly neglected his and Torrey's *A Flora*, thus announcing the discovery of a stunning number of new species. Worse, he found, Nuttall was selling these so-called new species for \$10 per hundred specimens. In January 1841, Gray complained to Sir William about the matter. Considering Hooker's generous support for advancing

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<sup>46</sup> MAC to GE, 11/25/1848, Folder 2, Box 1, *CMAC*; MAC to GE, 6/13/1850, Folder 2, Box 1, *CMAC*.

knowledge of the North American flora, he wrote, Nuttall “ought to send all these to you, but his *amor pecuniae* [love of money] is rather strong.”<sup>47</sup>

Gray must have made his dissatisfaction felt to Nuttall. In response, Nuttall packed up 118 specimens of *Compositae* and addressed them to Gray. He demanded Gray to examine the collection and then forward them to Sir William. Aware of and annoyed by Gray’s criticism, he exclaimed that his collection had “cost a good deal of *money*, much *time*, and considerable *risk* in procuring, for which, what I set for specimens is not like to remunerate me.” Regarding Gray’s accusation of his making up too many new species (thus making money by selling these “new species”), he fought back.

I was, I had thought, in consideration of what I had done, not *in the closet* but in the *field*, entitled to expect, the same privilege of consulting Dr. Torrey’s herbarium that you have of consulting the Herbarium of the Academy. It is now determined, I find, that I shall be obliged to work in the *dark*, and somebody will then come after and hold after my unavoidable errors and mistakes as a beacon on which to establish something de novo.<sup>48</sup>

Nuttall’s words hit the bull’s-eye. “[N]either Dr. Torrey nor myself have ever refused you the privilege of consulting freely our collections,” Gray replied in exasperation. “The insinuation that *somebody* wishes to oblige you to work in the dark, and then form a reputation for science by holding up your unavoidable errors... is as untrue as it is unworthy of you.” “No botanist, living or dead, has reason to complain of me

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<sup>47</sup> Part of this letter of Gray can be found in *LAG*, 278-279. But the sentence about Nuttall’s *amor pecuniae* is missing there. Fortunately a typed copy of this particular letter of Gray still remains at Gray Herbarium. Dupree (1952) has made use of this typed copy in his remarkable essay on the Nuttall-Gray controversy. A. Hunter Dupree, “Thomas Nuttall’s Controversy with Asa Gray,” *Rhodora* 54, no. 648 (1952), 293-303.

<sup>48</sup> Quoted from Dupree, “Thomas Nuttall’s Controversy...,” 298.



in this matter,” Gray declared.<sup>49</sup>

Among Gray’s close correspondents, Curtis practiced the free and liberal exchange of specimens and information most consistently. No matter how much time and energy he had invested to put up sets of specimens for his correspondents, he declined any offer of money and insisted that the return should be made “in kind.” Early in the 1850s, however, when his health failed and his daily duties as a clergyman no longer allowed him to embark on any long-distance expeditions, the thought that he might be unable to reciprocate his correspondents’ favors pained him. He soon found an alternative in his sons. As he told Gray in April 1852, “I should hardly be able to do any more exchange, if it were not for my Boys. One has taken to Mosses & Liverworts, another to Lichens, & a third to Phaenogams, & so among us all the work still goes on.” In October the same year, he proudly reported to Gray how fruitful his little army’s harvest in mountains and fields had been: “My boys have now completed their season’s collection—over 600 species (about 4000 specimens) of Phanego, & 100 species of Musci, Hepaticae, & Lichenes.” Later on, when his correspondence network grew wider, and his little army could no longer meet his correspondents’ demand, he dispatched slaves to collect specimens. “Since receiving yours of the 10th ult: I have a bold dash after Fraxinus, both by myself, & by Niggers,” he once told Gray. “By the latter, I got only leaves. By myself, I got good specimens of one species—all I have found—in fruit.”<sup>50</sup>

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<sup>49</sup> TN to AG, 4/7/1841; AG to TN, 4/14/1841, quoted from Dupree, “Thomas Nuttall’s Controversy ...,” 298-299.

<sup>50</sup> MAC to AG, 4/13/1852, *HL*; MAC to AG, 10/25/1852, *HL*; MAC to AG, 7/3/1852, *HL*

Curtis's insistence on enforcing the free and liberal exchanges of specimens in a strict and narrow sense sometimes strained his relationships with other botanists. For example, in 1845, Tuckerman asked if Curtis could collect lichens for him. Curtis replied that he would do Tuckerman this favor and would not ask for any monetary return, wrote the clergyman, except for Tuckerman's sharing of information about botany in general and lichenology in particular.<sup>51</sup>

So Curtis was surprised when Tuckerman sent him the bill for specimens ("I am sorry you enclosed payment for express on the parcel," he replied). Although "obliged to you for the consideration," he replied, he felt compelled to return Tuckerman's payment. Meanwhile, however, Curtis was disappointed to find that Tuckerman seemed parsimonious about sharing botanical information. As he told Henry W. Ravenel, a South Carolina planter and plant-lover, he would be very "uncertain" if Ravenel asked Tuckerman to share specimens or introduce correspondents. In a letter to Gray, he poured out his "vexation" toward Tuckerman's way of managing correspondence. "I have desired to serve him, & to furnish specimens of Lichens, because I felt enough of national pride to wish to have our species plantarum first published at home." "I should have done vastly better, if I had had nothing to do with him," he added.<sup>52</sup>

Curtis's harsh criticism of Tuckerman is relatively gentle compared to his complaint in a letter to Ravenel in December 1852. Curtis and Ravenel had become

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<sup>51</sup> MAC to ET, 11/7/1845, Folder 1, Box 1, *CMAC*.

<sup>52</sup> MAC to ET, 8/4/1848, Folder 1, Box 1, *CMAC*; MAC to HWR, 9/17/1846, Folder 3, Box 1, *CMAC*; MAC to AG, 4/16/1849, Folder 2, Box 1, *CMAC*.

correspondents in 1846. Curtis was glad to find that Ravenel was fascinated with fungi as much as he had been. He shared specimens and information with Ravenel and enjoyed Ravenel's returns. Yet, in 1852, when Curtis discovered that Ravenel had begun delaying his returns, he grew annoyed. He composed a letter detailing those unfair exchanges and enclosing a curse that would certainly terrify a person who studied fungi. "If I could only visit you with a night-mare in that innocent dream of yours, it should be in the shape of a Polyporus, of a ton's weight, growing out of your stomach, sucking out all the stagnant juice of your self complacent forgetfulness." "Now listen to the revenge I can do for you," he declared. "I do not think I shall find time to send you another parcel, until you have found time to furnish what I want of you."<sup>53</sup>

### **"My herbarium"**

Circulating along botanists' networks were not just letters but such objects as dried plants or fragments of plants. Such objects in circulation could be complete specimens mounted on sheets of good paper, or several petals or a stem with some leaves attached (Figure 2-2). But no matter how complete or how fragmented these objects were, they were supposed either to capture specific or generic characteristics of the plant in question or to convey the correspondent's view of the species's relation to other species and to the rest of the natural system. Failing to do so would be regarded by its recipient as an insult. It could even be said that when a botanist selected specimens from his or her herbarium, labeled them, made a list of

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<sup>53</sup> WAC to HWR, 12/16/1852, Folder 3, Box 1, *CMAC*.



Figure 2-2. A specimen enclosed in a letter from Charles Wright to his botanical friends for identification. (Courtesy of the Courtesy of the Connecticut State Library, Hartford, Connecticut, USA,)

them, packed them, and consigned them to his or her correspondent, he or she expected that the package's recipient would be able to decode his or her view of nature by examining the specimens. In a letter to Gray in 1844, for example, Tuckerman outlined his plan for preparing a set of plants in return for those favors he had received during his travel in Europe:

I mean however not only that the specimens shall be clear handsome picked ones—like some maples of Oaks's such are handsome than any I have ever seen of Nature's exhibitory—though this is very well.—but that the plants shall shew their metamorphoses—their colours their varieties &c. Some of the commonest plants will shine lustrously with this treatment. In this way I am confident of being able to be useful in some degree to Sir Wm. H.—& look forward & have done so with the greatest pleasure—to that parcel—such shall be made up if I am able to ramble again in woodland fields. My desire however is only to satisfy the Europn Botanists who gave me plants...<sup>54</sup>

The herbarium—the place where botanists stored, cataloged, and studied specimens—turned out to be a critical element in shaping botanists' self-identities. Being a botanist meant maintaining a herbarium. Indeed, when a plant-lover

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<sup>54</sup> ET to AG, 1/28/1844, *HL*.

declared to correspondents that he or she was prepared to establish a herbarium, it was a message confirming that he or she was now ready to practice the free and liberal exchange of specimens. (Normally the correspondents would reply to the message with packages of specimens.) And when this plant-lover did establish a herbarium, its conditions and arrangements would become a recurrent subject in correspondence with botanical friends. Botanists exchanged opinions about how to keep their herbaria neat, claimed to each other that “it always gives me special pleasure to add anything new to your herbarium,” paid visits to each other’s herbaria, and derided those botanists who messed up their herbaria. The herbarium, as it turned out, became not only the microcosm that (presumably) represented the order in nature but also the monument that embodied botanists’ mutual kindness, friendship, and generosity—in other words, botanists’ social relationships. Understandably, botanists defended their herbaria’s integrity as if defending their identities. In a letter to Gray, for example, Curtis cried out that his herbarium had been invaded. “I found last Summer that a miserable depredator had got into my Herbarium in the shape of a black worm 3/4 in. long, jointed, hard, & wiry. Whew, what havoc he made! He must have been introduced in some parcels from abroad, & I suspect from Louisiana. Do you know the rascal & how to keep clear of him?”<sup>55</sup>

A remarkable example of the herbarium’s role in shaping a botanist’s identity and social life was the herbarium John Carey reestablished after his first one had been destroyed by fire. Carey, a Londoner who came to the United States in 1830

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<sup>55</sup> MAC to AG, 1/30/1844, *HL*.

with three motherless children, started a fresh life in New York. Besides taking part in his brother's business, he devoted his leisure time to studying botany, and befriended botanists across North America. He soon established his reputation as one of the ablest botanists specializing in the notoriously difficult genus *Carex* (or sedges). But an accident in 1845 almost knocked him down to the nadir of his productive life. On July 18, 1845, when Carey was overwhelmed with grief at the death of one of his sons, he found his herbarium was on fire. "I lost every thing!" he cried to Gray some two months after the accident:

My Herbarium—Books—Mss. &c. all are gone, & in my present desolation of heart, you can scarcely imagine how I miss them—I go no where, & see no one; & having (as I believe I intimated the intention, when I had the pleasure of seeing you) resigned my engagement, in Wall Street, I am now thrown upon my own resources at a time of mental & corporal prostration such as I never knew, or expected to experience—Indeed, Sam has urged me to endeavour to take to the woods & wilds again, but (had I the heart) I have no longer the strength, as my Nervous System is so impaired by the excitement & trials of the past 2 years, that my Right Side is, at times, seriously affected.<sup>56</sup>

Gray was astonished. He replied that he could send Carey his duplicates of specimens if Carey made his mind to begin a herbarium from scratch. Carey replied that Gray's proposal was "very tempting" and he had been asking himself the same question since the incident. He doubted himself if he could even initiate such a formidable work. "To a person collecting, for the first time, every step is an advance, & gives courage; whereas to one who with infinite difficulty, & years of toil, has climbed a very high hill, to the bottom of which he suddenly tumbles, the efforts to climb again, gradually, & as high as inferior advantages permit, have not the same

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<sup>56</sup> JC to AG, 8/25/1845, *HL*.

exciting influence.” Notably, he then packed the remaining space of the sheet with a series of inquiries.

I noticed in Sill. Jour. that 2 or 3 Bots. (amongst them Geyer) were collecting in the West (& amongst the Ry. Mts.?) Are they still doing so, & at what rate do they sell their plants? Is Dr. Chapman still in S. Caro.? If not, are there any other good collectors there? What of Engelmann? Does he sell his plants? I want you to weight these queries, for an hour or two, & to give me your opinion as to prospective chances of building up enough of an herbarium to interest me—not such as I had before, of course, but types of most of our N. Am. Genera—If I begin again, I shall take a smaller sizzle paper than before—what is your size? Do you like, for working purpose, that proposed by Lindley as the best—10¾ x 16 ½ in? I shall not again, adopt the plan of fastening the specms by strips of paper, tho’ I like it best on some accounts, as it would be far more tedious than sling—As I suppose that you have recently done a great deal of this, in arranging your Herbm tell me what you find the best & shortest plan. Do you put each paper separately under a weight until the glue is dry? Or how else ensure cohesion, the specms being for the most part unequal in surface? Again, I beg you to tell me what is the English model of arrangement which you are adopting? This better will seem to you frightful in its requirements, & the more so, since I must beg you to write me your views (leisure permitting) without any delay, as I shall guide myself, in good measure, by your opinion, as to recommencing a collectn, & am desirous of answering those friends who have spontaneously tendered me help.<sup>57</sup>

Carey’s tremendous loss soon spread through American botanical circles. “There is not a Botanist in the Country who will not unite with me,” Curtis wrote to Carey, and “if you will give me permission I will confer with most of them on the subject & procure their contributions towards restoring, as far as can be, your Herbarium.” He soon wrote to Engelmann, “Have you heard of the destruction of John Carey’s splendid Herbarium in the disastrous fire in New York? ...I know that several of our Botanists have done & are doing what they can to replace his loss, & I shall do all I can. Can you lend a hand to the work of charity?”<sup>58</sup>

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<sup>57</sup> JC to AG, 9/22/1845, *HL*.

<sup>58</sup> See JC to AG, 9/22/1845, *HL*; MAC to GE, 11/3/1845, Folder 2, Box 1, *CMAC*.

Carey was touched that so many American botanists aspired to join Curtis's "work of charity." Still, he could be depressed if he found that his correspondents did not take his suffering seriously. In February 1846, he complained to Gray that he had just received from a botanical correspondent "a set of his—Salix labels!!!" "How could this fellow call himself a botanist!" he cried. "I really regret that I yielded to your suggestion in writing to him—It makes me 'feel cheap'—May this man of 16,000 duplicates die in the full & undisturbed possession of his cherished treasures—I can not deprive him of any of them, assuredly—" "Truly, my dear Gray," he exclaimed in another letter, "I am sick to death of these petty jealousies; & do not desire as a friend or correspondent any one who has not confidence enough in me to feel assured of my good will & frank intentions towards him."<sup>59</sup>

Gray was involved in this "work of charity" from the very start. Besides sending Carey duplicates, introducing Carey to his correspondents, and so on, he invited Carey to join his project of *A Manual of the Botany of the Northern United States* (1848), especially the section on *Carex*. Gray made such a proposition not only because Carey was a *Carex* expert, but also because as the coauthor of *A Manual*, Carey could *oblige* other botanists to send him specimens. Carey agreed and dispatched letters to botanists across the North America to acquire specimens. With Carey's effective assistance, *A Manual* progressed, and Carey's new herbarium took shape rapidly. Toward the end of 1848, Gray told Carey that he wanted to dedicate *A Manual* to Carey. Flattered, Carey replied that he would rather decline Gray's

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<sup>59</sup> JC to AG, 2/12/1846, *HL*; JC to AG, 11/17/1847, *HL*.



dedication. Gray should dedicate *A Manual* to Torrey, he wrote. "Do you not think he will expect it?"<sup>60</sup> Gray followed Carey's suggestion.

Carey regained his identity and self-respect as a botanist and remained active in Gray's botanical circle. Decades later, Gray would reminisce about American botanists' selfless dedication to help a suffering fellow. Right after Carey's accident, he would write, "American botanists vied with each other in the endeavor to repair this serious loss, and another large collection of United States plants was formed, critically studied, and carefully annotated."<sup>61</sup>

### **"If we only keep united"**

Aspiring to complete such a "national work" as the "complete flora of North America," Gray and his botanical circle compelled anyone who applied to join their circle to exchange and share specimens and information on a liberal and reciprocal basis. Even the federal government was no exception. In the late 1840s, what agitated Gray's circle was the way in which to properly deal with the botanical collections made by the U.S. Exploring Expedition (or the Ex. Ex. as it was called by contemporaries and still today).

In 1842, the Ex. Ex. concluded, bringing back a stunning number of living and dried plants from the far-flung corners of the world, ranging from the southern Pacific to Oregon. Charles Wilkes, commander of the Ex. Ex., soon summoned a

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<sup>60</sup> JC to AG, 12/4/1847, *HL*.

<sup>61</sup> Asa Gray, "John Carey," *The American Journal of Science (3rd Series)* 19, no. 113 (1880), 422.

group of botanists to work with the project's Scientific Corps to produce the botanical report. According to Wilkes's scheme, the botanist in charge would be William Rich, a plant-lover residing in Washington, DC, who had joined the Scientific Corps as botanist (after Gray's resignation in 1838).<sup>62</sup>

Gray kept a close eye on the progress of the Ex. Ex. botanical report. Given the expedition's wide geographical scale, he had little doubt that its botanical report could only be completed with assistance from European botanists. But he decided not to intervene. Since Captain Wilkes did not even bother to consult him, he was content to steer clear of this formidable work.

The arrival of the Ex. Ex. botanical collections in Washington, DC, soon inspired keen interest among European naturalists. Sir William was particularly electrified. His son, Joseph D. Hooker, had just made a remarkable collection in the south Pacific during his service in the Ross Expedition (1839-1843), and was now working on the expedition's botanical report. As a loving father and as a responsible botanist superintending Britain's most distinguished botanical establishment, Sir William did whatever he could to pave the way for Joseph's career. Soon he asked Gray if Joseph could be granted the chance to study the Ex. Ex. botanical collections and thus to "form a joint work or to form a separate memoir done by us as an appendix to the American work." "[I]t is quite certain that we have in this country better means of elaborating such collections than can possibly be found in America. It would be a

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<sup>62</sup> Regarding the history of the Ex. Ex. and its role in American science, see William Ragan Stanton, *The Great United States Exploring Expedition of 1838-1842* (Berkeley: University of California Press, 1975); Philbrick, *Sea of Glory*.

pity that Plants, in many respects, so similar, should be worked up at nearly one & the same time, by different Botanists.”<sup>63</sup>

Then, upon learning of Gray’s unwillingness to get involved in the Ex. Ex. business, he turned to Charles Pickering, the Ex. Ex.’s chief naturalist, expressing his opinion about the necessity of allowing British botanists (his son in particular) to study the Ex. Ex. collections. Pickering felt offended, however. He replied that Sir William’s words had implied “disrespect” to the Ex. Ex. naturalists in particular, if not American naturalists in general.<sup>64</sup> Sir William was taken aback. He decided not to risk his friendship with Pickering because of the Ex. Ex. business. In August 1843, he wrote to Gray detailing his correspondence with Pickering. He felt terribly sorry, wrote the distinguished British botanist. “This is the first time I have ever given offense to an American Botanist & I hope it will be the last.”<sup>65</sup>

Gray did his best to turn the research and publication of Ex. Ex. botanical collections into a transnational project. Early in 1844, when Sullivant was invited to visit Washington, DC, to examine the Ex. Ex. bryological collections, he asked Sullivant to bring the issue of specimen exchange to the Ex. Ex. people’s attention. Sullivant replied afterward.

I mentioned an exchange of mosses with the British explo. exped. as you suggested—they appear quite indifferent on the subject—say they have nothing to do with such matters in fact they don’t know enough to see the advantage of it—Tell young Hooker to send them nothing of the kind at present—wait

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<sup>63</sup> WJH to AG, 11/10/1842, *HL*.

<sup>64</sup> See WJH to AG, 12/1/1843, *HL*.

<sup>65</sup> WJH to AG, 8/30/1843, *HL*.

awhile—things there will get into better hands before long—then something can be done—<sup>66</sup>

As time passed, however, both Sullivant and Gray were disappointed to learn that “things in Washington” got into no better hands. In December 1845, Gray wrote to Joseph with annoyance. “The botanist who accompanied the expedition [the Ex. Ex.] is no doubt perfectly incompetent to the task, so greatly so that probably he has but a remote idea how incompetent he is.” Even more so, he continued, those who had power to decide how to distribute the Ex. Ex. specimens (the “big folks”) refused to share them with the wider botanical community. “The whole business,” proclaimed Gray, had fallen “in the hands till now of Senator [Benjamin Tappan], the most obstinate, wrong headed, narrow minded, impracticable ignoramus that could well be found.” What he could tell from Tappan’s attitude respecting the issue of specimen exchange was “an utter ignorance of those principles of comity and the spirit of interchange that prevail among naturalists.” “You will see that nothing is to be expected from such sources.”<sup>67</sup>

Gray’s presumption was correct. No later than the summer of 1847, Captain Wilkes had become excessively worried about the progress of the Ex. Ex. botanical report. “On examination it has been found too incomplete to admit of its being put to press,” he reported to James Alfred Pearce, Chair of the Library Committee, “besides there are many of the Intertropical plants of which he [William Rich] has taken no notice, and there is in my opinions an absolute necessity of the whole being gone

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<sup>66</sup> WSS to AG, 4/21/1844, *HL*.

<sup>67</sup> AG to JDH, 12/31/1845, *LAG*, 337.

over, by some one who is competent to the task, for it cannot be printed in its present state.”<sup>68</sup>

So sometime in August 1847, Wilkes wrote to Torrey asking for an interview (“something of importance to communicate”). Torrey felt curious. Before meeting Wilkes, he contacted his friend and the Ex. Ex. horticulturalist William Brackenridge for internal information. Would Captain Wilkes appoint him as the botanist supervising the Ex. Ex. botanical report? If yes, wrote Torrey, given his pressing duty as a chemistry professor, he could hardly “spare the time necessary for studying efficiently exotic plants.” “Should there be a good place opened in Washington so that I could make a competent support for my family,” he wrote wishfully, he would quit his Princeton professorship and “devote most of my time to Botany—but I don't see any immediate prospect of this.”<sup>69</sup>

About two months later, in October 1847, Torrey again wrote to Brackenridge updating him on his negotiation with Captain Wilkes. The captain had wished him to “take all the plants of the Expedn. to work up,” he wrote. Interested as he was, Torrey noted that he could “give no definite answer at the time.” “No one botanist [in the United States] could do justice to such an immense collection” without “making a voyage to Europe,” wrote Torrey. Considering the fact that Wilkes had “hitherto been altogether unwilling that any but Americans, or those who have been

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<sup>68</sup> CW to JP, 12/30/1847, F000290, R5, *CWPL*.

<sup>69</sup> JT to WDB, 8/18/1847, Folder 5, Box 1, *WDBP*.

settled in our country, should have a share in the work," he concluded, his suggestion was the only way to get the Ex. Ex. botanical report done.<sup>70</sup>

Brackenridge replied that Wilkes's decision to enlist Torrey to take charge of the Ex. Ex. botanical report was thus far "the most judicious move he has yet made towards bringing the botany of the Expn. before the public." He could hardly see "wherein the difficulty of your undertaking the work exists, particularly if assisted by such a distinguished botanist as Dr. Gray." Still, he warned Torrey not to underestimate the difficulties of turning the Ex. Ex. botanical collections into a scientific report. "I would also remind you my good Sir, that it is a big job, and I hope to see Uncle Sam paying liberally for it, and it would be well for the Nation, that more of its money was expended as a reward for the kind of knowledge that is required in this case."<sup>71</sup>

To Torrey's surprise, Captain Wilkes disappeared after the meeting of August 1847. When he grew concerned, a letter from Gray arrived. In it Gray announced his incoming trip to Washington, DC, and Captain Wilkes's eagerness to have Gray serve as the superintendent and the major author of the Ex. Ex. botanical report. Torrey was taken aback. He replied that he was "not surprised at Wilkes's late movement," and he hoped that Gray would "make a good bargain with him." He detailed his first meeting with Wilkes.

From the first I told him that the only way to get the Exped. plants properly done was to have them put out in monographs, but he laughed at the suggestion, & said the work must be done at home. I offered to undertake the distribution, & to

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<sup>70</sup> JT to WDB, 10/8/1847, Folder 5, Box 1, *WDBP*.

<sup>71</sup> WDB to JT, 10/22/1847, Folder 3, Box 1, *WDBP*.

visit Europe for the purpose—but he said it was out of the question. On no account would the expenses of a life to Europe be paid by Government. I finally told him that I had no desire to do any more of the plants, besides those which I have in hand,—except, perhaps, the Sandwich Island Collections, which can best be made a special study than any other of the extra-American plants. I also assured him that unless you or Engelmann undertook the work it would never be done at all.

“As to remuneration,” added Torrey, Gray should ask for an annual payment of an amount between 1,250 and 1,500 dollars for four or five years. “According to Pickering’s statement,” he explained, there were about 10,000 species in the Ex. Ex. botanical collections, and “Many of the specimens have no duplicates—& no material is left to work on.” “Tell Wilkes plainly what you will do—& what you won’t do.—& get the contract written out so that there will be no misunderstanding as to the terms. He will kick & flounder—but finally accede to your demands.”<sup>72</sup>

After sending off the letter to Gray, Torrey promptly asked Brackenridge if he had known anything about Wilkes’s “late movement.” “Dr. G will do the work well (better than I could do it) if his terms are agreed to—& let me tell you he will drive a harder bargain than I could have accomplished. He will not only be well paid of the job,—but require much more—The committee must be at the expense of his voyage to England & France!” Brackenridge replied that he had “not the most distant idea that Capt. Wilkes was about to negotiate with Dr. G.” Torrey replied that such reluctance to share information was “characteristic” of all that Wilkes had done in the “whole matter” of the Ex. Ex. “Capt. W. ought to have informed me of his doings,” he complained, “for it appears that he had made up his mind to pay the expenses of a voyage to Europe, & to allow whoever took the work, to employ such aid as was

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<sup>72</sup> JT to AG, 5/30/1848, *HL*.

necessary.” But aside from his personal embarrassment, Torrey assured Brackenridge that “Gray, however, is a better botanist than I am, & will do the work in excellent style. This is perfect harmony between us.”<sup>73</sup>

Then Gray’s letter came. In it Gray happily announced that he had completed his visit to DC, and had settled the terms with Captain Wilkes about preparing and publishing the Ex. Ex. botanical report. “The chairman of the committee and Wilkes behaved very well, and told me they were very desirous I should take it up,” wrote Gray. “My terms were based on the supposition that there is five years’ work in preparing for the press the collections left on hand, and in superintending the printing.”<sup>74</sup>

To American botanical communities, the appointment of Asa Gray as an author and a supervisor of the Ex. Ex. botanical report was a triumph of science. With Gray serving as a node that connected men of science with “big folks” in DC, they expected that the Ex. Ex. botanical collections would become an important addition not only to American science but also the common stock of human knowledge. More importantly, botanists considered Gray’s appointment a signal and symbol that the big folks had familiarized themselves with the “principles of comity and the spirit of interchange that prevail among naturalists.” Hearing of Gray’s appointment, Brackenridge told Torrey that he expected that “a few small stumbling blocks” would still “roll in the way” but could “easily be turned aside if we only keep united.”

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<sup>73</sup> JT to WDB, 6/5/1848, Folder 5, Box 1, *WDBP*; WDB to JT, 6/9/1848, Folder 3, Box 1, *WDBP*; JT to WDB, 6/21/1848, Folder 5, Box 1, *WDBP*.

<sup>74</sup> AG to JT, 6/[undated/]1848, *LAG*, 359.



Torrey replied that Gray was definitely “in good spirit about the Exploring Expedition plants,” and meant to do the work “in his best style.” “If his life is preserved you will not be sorry that the task has devolved on him. Glad indeed I am that he consented to undertake it he shall work with perfect cordiality—& he will throw into my hands any portion of the collections that I may wish to elaborate.”<sup>75</sup>

### **“The same particular plan”**

Emerging from Gray’s “free and liberal” exchange of specimens and information with his fellow botanists were two fat and lavishly illustrated volumes under the title *Genera Florae Americae Boreali-Orientalis Illustrata*, or *The Genera of the Plants of the United States* (1848-1849). As Gray put it in the introduction,

The ample and rapidly accumulating materials at my disposal, both of specimens in the Herbarium, and of living North American plants in the Botanical Garden under my charge, and the prompt assistance offered by a large number of zealous correspondents, while they afford unusual advantages for the purpose, rendering an undertaking which may serve to facilitate the more thorough study of Botany in this country, and perhaps contribute in some degree to the general advancement of the science.

The *Genera*’s overall goal was to illustrate “the Botany of the United States by figures, with full analyses, of one or more species of each genus, accompanied by descriptive generic characters and critical observations,” he announced (Figure 2-3).<sup>76</sup>

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<sup>75</sup> WDB to JT, 6/26/1848, Folder 3, Box 1, *WDBP*; JT to WDB, 9/26/1848, Folder 5, Box 1, *WDBP*.

<sup>76</sup> Gray, *Genera...*, Vol. 1, 5-6.

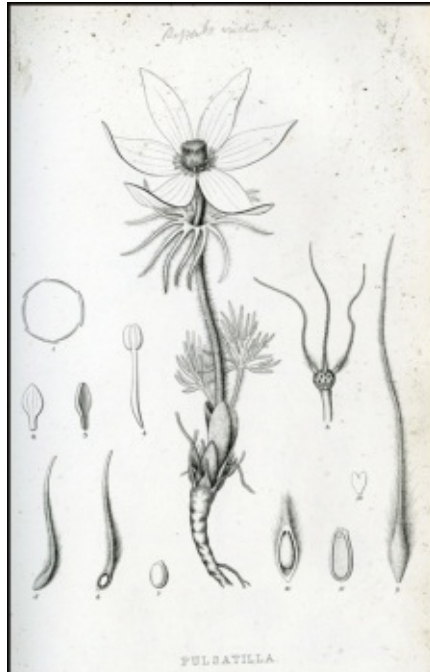


Figure 2-3. In the *Genera*, Gray used the species *Pulsatilla patens* to illustrate the genus *Pulsatilla*. (Courtesy of the Library of the Gray Herbarium, Harvard University, Cambridge, MA, USA.)

Indeed, with numerous dried and living plants pouring into his herbarium and garden, and with information rioting on his worktable and in proof sheets, Gray felt compelled to identify a taxon with which he would both satisfactorily arrange all this data and quickly gain an accurate glimpse of those specimens. The “genus” emerged as his preferred taxon. But what was a botanical genus? Did this category truly exist in nature? Or was it an artificial category established by botanists in order to tackle nature’s infinitive diversity and seriality? Such were questions that concerned botanists of the day. Bentham, for example, asked his readers to reconsider the following questions in his *Labiatarum Genera et Species* (1832-1836): “But if, in regard to genera, I have laid down as a principle that the question is not whether such a group *is* a genus or section? but whether it would be *most convenient to rank* such a group as a genus or as a section?” Bentham assured his readers that

he preferred the second view.<sup>77</sup> Herbert, too, discussed similar issues in his *Amaryllidaceae*. He noted that the distinction of genera hinged on botany's "fundamental office," but the difficulties involved in distinguishing one genus from another inevitably made botany "a science of conjecture."<sup>78</sup>

Gray struggled relentlessly to reconcile the view that considered the genus a reality in nature and the view that considered the genus a "most convenient" construction. As he put it in *Elements of Botany* (1836), "The genus may be taken as the type, from which, by slight modifications of structure, form, &c., the species it comprises may be conceived to arise: but it is more philosophical to deduce the idea of a genus synthetically, by the assemblage of nearly-related species." Genera were "certainly founded in nature," he asserted, and "not unfrequently" genera were "even more obvious than species."<sup>79</sup>

Gray put this principle into practice in his and Torrey's 1841 *A Flora of North America*. Together with Torrey, he established a great number of new genera. Readers would encounter no great difficulties in distinguishing these new genera, the two botanists explained, so long as they familiarized themselves with the natural system. Carey commented on this significant multiplication of genera in his review of *A Flora*:

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<sup>77</sup> Bentham, *Labiatarum Genera et Species*, xlviii.

<sup>78</sup> Herbert, *Amaryllidaceae*, 17. Regarding Bentham's, Herbert's, and many other nineteenth-century botanists' views on the genus, see Peter F. Stevens, "Why Do We Name Organisms? Some Reminders from the Past," *Taxon* 51, no. 1 (2002), 11-26.

<sup>79</sup> Gray, *Elements...*, 291.

In some sort [Torrey and Gray's new genera], these do no doubt exist, but the differences are scarcely more than matters of *degree*, and hence, where extensive means of comparison are not within reach of the student, the appreciation of the terms necessarily employed to describe these varieties, becomes mere matter of opinion, and therefore open to misapprehension. These difficulties, inseparable from the nature of the case, are not within the control of any systematic writers, and we believe that the present arrangement will not disappoint the hopes of the authors...<sup>80</sup>

Carey was apparently overoptimistic about readers' reception of Torrey and Gray's new generic distinctions. In February 1847, Sullivant sent Gray a paragraph transcribed from a letter written by their common botanical friend Charles Short:

Can you [Sullivant] tell me what progress Dr. Gray is making in his work on the elucidation of American Genera Plantarum?—I fear he will find it difficult to exhibit graphically on paper some of the hair-splitting distinctions which have recently been perpetrated in the establishment of American genera. The difference for instance between his *Brachychaeta* and the old fashioned *Solidago* will be found I fear to be a tweedledum & tweedledee... I am truly sorry to see one deservedly so high in the estimation of all botanists as Dr. Gray following in the footsteps of poor old Rafinesque to whom the commonest thing could not be shown that he did not exclaim—my new species.

"See what mischief you are doing!" Sullivant exclaimed after the paragraph.<sup>81</sup>

Gray loved Short's expression. In a letter sent to "J. L. L.," he wrote half-jokingly:

I have been addling my brain and straining my eyes over a set of ignorable Pondweeds (alias *Potamogeton*) trying to find the

"difference there should be Twixt tweedle-dum and tweedle-dee,"

and wasting about as much brain in the operation as your dear paternal would expend in an intricate law case, for all of which I suppose nobody will thank me and I shall get no fee. Indeed, few would see the least sense in devoting so much time to a set of vile little weeds. But I could not slight them. The Creator seems to

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<sup>80</sup> Carey, "Notice...", 282-283.

<sup>81</sup> WSS to AG, 2/3/1847, *HL*.

have bestowed as much pains on them, if we may use such a word, as upon more conspicuous things, so I do not see why I should not try to study them out.<sup>82</sup>

To be sure, Gray directed his attention to *Potamogeton* not entirely to figure out the Creator's intricate work, but to revise his good friend Tuckerman's classification. For the past five years or so, Tuckerman had been disappointed by Gray's refusal to accept his new species in *Potamogeton*. To convince Gray that he had made the correct judgment, he sent his collections to Gray for reference.<sup>83</sup> Afterward, when Gray still did not include Tuckerman's new species in the new edition of *A Manual*, he grew annoyed. "I believe *P. Claytonii* as good a species," he exclaimed to Gray. "[T]his part of the Manual is enough to discourage a botanist."<sup>84</sup> Gray remained adamant, however. As he explained to Darwin afterward, Tuckerman thought that *Potamogeton* "of which the species are close, & difficult to distinguish by reason of their similarity;—but the species themselves not remarkably variable," whereas he preferred to consider *Potamogeton* the "species of which are themselves so variable, & approximating, that it becomes difficult to say where one species ends & the next begin."<sup>85</sup>

Gray hardly yielded to his readers' and colleagues' challenges to his generic distinctions. Instead, he became increasingly entrenched in the view that botanists made generic distinctions on the basis of almost intuitive and transcendental

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<sup>82</sup> AG to JLL, [undated/] 1847, *LAG*, 350.

<sup>83</sup> ET to AG, 4/2/1856, *HL*.

<sup>84</sup> ET to AG, 11/1/1856, *HL*.

<sup>85</sup> AG to CRD, 5/24/1857, *CCD*, Vol. 6, 398.

experience. To Gray, the genus was both ideal and real. As he noted in *A Manual* (1848), “genera, or kinds, are ideal assemblages of nearly related species, viz. of those which, notwithstanding specific differences, agree with each other closely in structure and appearance.” In effect, he added, “In respect to genera and species all classifications in botany agree.” After all, he claimed elsewhere, “a genus is a group of species which present the same particular plan.”<sup>86</sup>

What did Gray mean by “the same particular plan?” Though, as a rule, Gray avoided introducing such terms as God or Creator in his botanical writings, his review essays written at the time altogether suggested his belief that the genus embodied Creator’s plan in nature (Gray would write explicitly about the Creator in 1857; see Chapter 9). But to Gray the genus was not ideal at all, nor was it a category in the Creator’s mind that evaded naturalists’ investigation (as renowned naturalists such as Richard Owen or Louis Agassiz suggested). Instead, as he put it in a letter to Spencer F. Baird, “I am always glad to see a genus that is marked geographically confirmed”—that is to say, Gray was “glad to see” that those congeneric plants were distributed in regions characterized by similar climatic conditions.<sup>87</sup> Afterward, when discussing the issue with Joseph D. Hooker, he explained what he meant by the geographically confirmed genus: “If it be true that congeneric forms are often found in two widely separated areas of similar climate (as in the U.S. & Japan, Arctic

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<sup>86</sup> Asa Gray, *A Manual of the Botany of the Northern United States: From New England to Wisconsin and South to Ohio and Pennsylvania Inclusive* (Boston: James Munroe and Company, 1848), xxxvii-xxxviii; Gray, *The Botanical Text-Book: for Colleges, Schools, and Private Students (2nd Edition)* (New York: Wiley and Putnam, 1845), 305-306.

<sup>87</sup> AG to SFB, 6/1/1846, Folder 4, Box 23, *SFBP*.

& Antarctic regions &c. &c.), then the occurrence of two closely resembling, yet originally distinct species in widely separated places of similar climate is just what I should *a priori* expect.”<sup>88</sup>

With the definition of the genus set out, Gray made *Genera Florae Americae Boreali-Orientalis Illustrata* a work that not only revealed the Creator’s plan in nature, but also unveiled some of the most interesting patterns of nature. For that matter, although he insisted that his studies of the genera only covered plants native to the United States of America (that is, “the States of the Federal Union as now constituted,” including Texas, “but not the country west of the organized States of Arkansas and Missouri”), he did not confine his analytical scope to the political boundaries of the United States of America. Instead, he paid close attention to ensure a genus was “geographically confirmed”—whether the distribution of the genus’s species (or congeneric species) was in North America, China, Africa, or elsewhere. For instance, when describing the genus *Loeflingia*, he wrote, “There are three recognized species of the genus; two of them natives of the southwestern borders of Europe, while the third belongs to the analogous region of the New World, namely, to Texas and California.”<sup>89</sup> Likewise, when delineating the geographical distribution of *Hermannia Tourn*, he noted, “This genus belongs to the Cape of Good Hope (where it is numerous in species); with the exception of two plants recently detected in Mexico and Texas, which appear to be truly congeneric

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<sup>88</sup> AG to JDH, 2/21/1854, Duncan M. Porter, “On the Road to the *Origin* with Darwin, Hooker, and Gray,” *Journal of the History of Biology* 26, no. 1 (1993), 16.

<sup>89</sup> Gray, *Genera...*, Vol. 2, 24.

with South African *Hermannia*.”<sup>90</sup> In fact, it was in *Genera* that he began discussing the pattern in nature that would eventually make his name: the similarities between the flora of Japan and that of eastern North America. For example, he remarked on the geographical distribution of the genus *Magnolia* L. with the following statement: “Native of Eastern North America, and of Eastern Asia, namely of Japan, China, and Nepaul [*sic*].”<sup>91</sup> As for the genus *Trautvetteria* Fisch. & Mey, he wrote, “The original species is found along shaded streams, throughout the Alleghany Mountains from Virginia southward, and along their western confines: it also occurs sparingly in Illinois, and apparently reappears in Northern Oregon. A second, but imperfectly known species has been recently indicated by Siebold and Zuccarini in Japan.”<sup>92</sup>

Gray considered the *Genera* a milestone in his career both as an American botanist and a Harvard professor. What he showed therein should meet the highest standard in natural philosophy: namely, demonstrating the plan in nature by intensive and extensive observation of nature, a line of inquiry eagerly prompted and pursued by Humboldt, Herschel, and Whewell. He believed that the *Genera* would do him “more credit” than anything he had ever published, he told Joseph Henry of the Smithsonian wishfully.<sup>93</sup>

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<sup>90</sup> Gray, *Genera...*, Vol. 2, 88.

<sup>91</sup> Gray, *Genera...*, Vol. 2, 61.

<sup>92</sup> Gray, *Genera...*, Vol. 2, 26.

<sup>93</sup> “Now, I am proud of the production, and think it does more credit to my scientific reputation than any thing I have done. The plates for the 2d vol. are preparing and engraving,—the drawing &c. of higher order than in this first vol. I am very anxious to carry it on: but feel discouraged. The State of Europe cuts off entirely all hope of



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The year of 1848 proved to be an important year for Gray, his renowned biographer Hunter Dupree would claim.<sup>94</sup> The man once thought married only to the scientific work entrusted to him by the U.S. government and the botanical community married Jane Lathrop Loring (1821-1909; Figure 2-4), the daughter of Charles Greely Loring and Anna Pierce (Brace) Loring. On May 6, 1847, when he had just received Jane's positive answer to his proposal, he jovially reported the news to his newly widowed mother. "The news is just this, I am engaged to be married to a lady who I think is every way calculated to make me happy," the son cheered. "She moves in the best, though seldom the most brilliant circles of Boston.—possesses all the usual accomplishments of persons in her station, but is most remarkable for a well-cultivated mind, and for her excellent practical powers."<sup>95</sup> "I'll see if I can do any

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foreign (at least Continental) sale, on which I had a good deal relied, to help pay the bills. And some of our rich men, who entered into the scheme as subscribers with spirit, have behaved very shabbily, (the poorer ones, however, all fulfil their engagements with alacrity).—I must not continue to sink money, & time, and Sprague's invaluable talents in it, unless it will remunerate him, & free me from actual loss,——which I fear it will not do.—I know no more national work; my friends assure me it does great credit to our country abroad. I will soon give you their statements on that point from abroad." AG to JH, [Late June 1848,] *PJH*, Vol. 7, 348.

<sup>94</sup> Dupree, *Asa Gray*, Chapter 9.

<sup>95</sup> Quoted from [http://www.huh.harvard.edu/libraries/Gray\\_Bicent/gray\\_jane.htm](http://www.huh.harvard.edu/libraries/Gray_Bicent/gray_jane.htm).



Figure 2-4. Jane Gray in 1877. The photo was taken when Jane was with Gray and Joseph Hooker on an expedition to Colorado (Courtesy of the Archives of the Gray Herbarium, Harvard University, Cambridge, MA, USA.)

better hereafter, when I have a wife to write letters for me,” he told his new correspondent Charles Wright.<sup>96</sup>

The wedding of the young Harvard professor and the daughter of the “eminent lawyer” was scheduled on May 4, 1848 in the Harvard Botanical Garden.

Unfortunately, none of Gray’s close botanical friends was able to attend this “very particular incident in a young gentleman’s life,” as Sullivant put it.<sup>97</sup> Torrey wrote to Gray afterward, apologizing for his absence.

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<sup>96</sup> AG to CW, 1/17/1848, *LAG*, 354. Wright promptly reported this good news to his sister Mary Ann: “[F]or your information I can inform you that he [Gray] is to be married the present month, as he tells me, and promises me then that his wife will assist him in his correspondence so I and you too perhaps may expect to receive some letters from Mrs. Gray if we continue our correspondence as heretofore.” CW to MAR, 5/12/1848, *CWC*.

<sup>97</sup> WSS to AG, 4/29/1848, *HL*.

I counted the days, one by one, for some time before the important event,—& would gladly have been present at the ceremony, had it taken place under other circumstances. I am a comparative stranger to nearly all the persons who were present & then you were all to scatter in every direction immediately & after the wedding—so that I should have been left alone with no one to cheer me. Do you know that a wedding generally makes me feel sad—& you would have made me so in a special reason. I fully believe that you have received from the Lord one of the best of women; & that you will live happily together, but I have had difficulty in keeping down an undefined jealous sort of feeling, as if she had taken from me one of my best friends.<sup>98</sup>

On the day of the ceremony, the thirty-eight-year-old Harvard professor Asa Gray stood before the plots tapestried with plants that he had painstakingly gathered and transplanted from places across the country, receiving greetings from his eminent guests. A special moment, indeed: the frontier farm lad had successfully transplanted himself to the “best circles” of Boston.<sup>99</sup>

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<sup>98</sup> JT to AG, 6/12/1848, *HL*.

<sup>99</sup> Regarding Gray’s marriage and Jane’s critical role in Gray’s botanical career, see Lisa Ann DeCesare’s pioneering study, DeCesare, “Jane Lathrop Loring Gray (1821–1909) and the Archives of the Gray Herbarium,” *Harvard Papers in Botany* 15, no. 2 (2010), 221-230.

**PART II**  
**COLLECTOR**

What does the West stand for?

—Henry David Thoreau

## CHAPTER THREE

### “All the Collectors Make Money”

In 1837, Sir William Hooker, Regius Professor of Botany at Glasgow University, published a short notice in *Companion to the Botanical Magazine*:

It gives us pleasure to be able to say that a botanical collector is about to proceed to Santa Fé, in North Mexico, under the auspices of Dr. Torrey of New York. His outfit is calculated at three hundred dollars currency. Those, who contribute to this, will receive plants at the rate of one hundred specimens for every five dollars. To those who purchase specimens on the return of the collector, without having contributed to his outfit, the price will be seven dollars per hundred. Considering the highly interesting character of the country to be explored, and the difficulty of getting access to it (the whole journey to and from St. Louis having to be made on horseback), the terms are certainly extremely moderate.<sup>1</sup>

“You know how deeply I feel interested in the Botany of all N. America,” Sir William also wrote to Torrey. “Nothing could give me more pleasure than to hear that *you* will send a collector to the southern extremity of the Rocky Mountains & I rejoice particularly that *you* have fixed upon the person who is to be the collector.”<sup>2</sup>

The person whom Torrey “fixed upon” was Peter D. Knieskern (1798-1871), a medical doctor and plant enthusiast.<sup>3</sup> Gray, by then studying at Sir William’s herbarium in Glasgow, was glad that Torrey’s Santa Fe expedition seemed on the right track. “Can’t Knieskern safely make the excursion to Sante [*sic*] Fé in the

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<sup>1</sup> William J. Hooker, *Companion to the Botanical Magazine; Being a Journal Containing such Interesting Botanical Information, as Does Not Come within the Prescribed Limits of the Magazine; with Occasional Figures, Vol. II* (London: Edward Couchman, 1836-1837), 185-186.

<sup>2</sup> Quoted from Rodgers III, *John Torrey*, 132-133.

<sup>3</sup> LAG, 90.

coming spring?" he wrote to Torrey. "If he can, and will work hard, he will make \$1000 clear of expense! All the collectors make money."<sup>4</sup> The fact was that Gray had been pondering the subject for a while. In a letter to Torrey in February 1838, he had written,

I am most clearly of the opinion that any person who will make extensive collections of North American plants, both Northern and Southern, and include also a good collection from Santa Fé, the Platte country, etc., have his sets named according to our work, and who would devote four or five years to the business, could, if he were really industrious and prudent, realize \$1000 per annum (clear). He should continue my grass-book for one thing, giving loose sets only for the present price, and while from time to time he sells off collections as he can, should retain some fifty sets in all the most interesting genera or small families, get all the species, and publish them in monographic sets. Knieskern could make, with the aid we would gladly furnish, at least ten times as much money, as long as he lives, and he ever will at physic, besides being engaged in a much pleasanter way. I know how all this should be managed now.<sup>5</sup>

It turned out that Knieskern was unsuitable for an expedition to the edge of American civilization. Torrey's planned expedition to the American West never got off the ground. Gray did not abandon the thought that American botanists should organize expeditions to explore the American West. In July 1842, when he had just accepted the appointment as Fisher Professor at Harvard, he revealed his plan for "Botany in this country" to Engelmann.

Allow me therefore to say that yourself and your friend Lindheimer in Texas would render me, and also the cause of Botany in this country, the greatest aid (which I will take every opportunity of publicly acknowledging), if you will send me roots or seeds of any western plants, especially the rarer, and those not yet figured or cultivated abroad...

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<sup>4</sup> AG to JT, 12/12/1838, *LAG*, 90.

<sup>5</sup> *LAG*, 135.

“I shall not be idle myself,” he claimed. “I will defray all expenses of collection and transportation (boxes may be sent via New Orleans, directly to me at Boston). If you wish to cultivate anything that I have or can procure, it shall be forthcoming.”<sup>6</sup>

The fact was that at the moment no one in Gray’s botanical circle could have stayed idle. They were aware that their country was expanding, and they had to adjust their research scope accordingly. Here Curtis’s changing attitudes toward the Texan flora were illustrative. For a while, Curtis had refrained from touching upon any material not pertaining to what he had regarded as the flora of the United States. As he told Engelmann in 1841, he had decided to confine “my most careful attention only to plants of this State.” Five years later, however, when Texas officially became part of the United States, he anxiously dispatched off a note to Engelmann. “The S. Western part of the U.S. is almost terra incognita to me,” he wrote. “You are now the only one west of the Alleghanies, with whom I make exchange.” Would Engelmann be able to spare some Texan specimens?<sup>7</sup>

Against this backdrop, American botanists were dismayed when they learned that their European colleagues were sending collectors to bring back unknown American species, publishing them, and thereby snatching away the credit that should be American botanists’ due. In September 1842, Curtis dashed off a letter to Gray about his conversation with a plant enthusiast and collector named Samuel B. Buckley. In it he worried that, according to Buckley, “a mean man” named Ferdinand

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<sup>6</sup> AG to GE, 7/26/1842, *AGPG*.

<sup>7</sup> MAC to GE, 3/4/1841, Folder 2, Box 1, *CMAC*; MAC to GE, 7/31/1846, Folder 2, Box 1, *CMAC*.

Rugel had collected many plants in the mountains of North Carolina and had shipped them back to Europe. Worse, he added, “The European Botanists have written him [Rugel] not to let Americans have his plants.” He could hardly allow such ungentlemanly behaviors to go on, Curtis declared. “I am determined therefore to work up the few plants I have into a paper for the Jan: No. of Silliman’s Journal, so as to anticipate, as far as I can, any European Botanists. I wish you would help me out in this, if you have time.” In November, Curtis told Gray that he had submitted the essay to *The American Journal of Science and Arts*, and had “begged” chief editor Benjamin Silliman to publish it as soon as possible. “I should have trusted my Mss. to you instead of publishing,” he admitted, and “thereby avoided multiplying some synonymes perhaps.” But he had been driven to do so, he emphasized, for he wanted to secure “an early chance so as to get ahead of Rugel’s European correspondents.”<sup>8</sup> Tuckerman, on the other hand, exclaimed to Gray that “For the first time does an American have appeared in the running title of the pages!” when his diagnosis of new American species was about to be published in European botanical journals. “Further than this we cannot go—but may this not stand alone—may others gain the same position! We depend no longer on Europe; unless as Europe depends upon us, in natural his[tory].”<sup>9</sup>

An issue that stoked the rivalry between American botanists and their European colleagues was the increasingly important role played by *The Rules of Zoological Nomenclature*, a framework eagerly promoted by the prestigious British Association

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<sup>8</sup> MAC to AG, 9/29/1842, *HL*; MAC to AG, 11/18/1842, *HL*.

<sup>9</sup> ET to AG, 4/8/1844, *HL*.



for the Advancement of Science (BAAS). In 1842, in light of the heated debates revolving around the term “species” (for example, did the category “species” truly exist in nature? If yes, did species have any “essence”? If no, how could naturalists draw the line between species and varieties?), a committee led by renowned British zoologist Hugh Edwin Strickland drafted *The Rules* and submitted them to the BAAS for deliberation. They were resolved at the BAAS’s meeting a year later and in some measure became guidelines for other natural-history establishments in Britain. The spirit of *The Rules*, as historian of science Gordon R. McOuat puts it, was to advocate that, “Species are what *those* elite naturalists say they are, and nothing more.” That is to say, so long as naturalists at large could stop talking about the essence of species and trusted judgments made by elite naturalists (i.e. naturalists backed on a strong institutional basis, by large herbaria and libraries, and so on), the so-called species problem could be largely ruled out.<sup>10</sup>

The BAAS’s radical move to solve the “species problem” impressed European and American naturalists alike. Louis Agassiz, for example, discussed the issue in his *Nomenclator Zoologicus* (1842), optimistically expecting that *The Rules* would bring about a wave of revolution in nomenclature. American naturalists, on the other hand, paid particular attention to *The Rules*’s “twelfth proposition”: “a name which has never been clearly defined in some published work should be changed for the

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<sup>10</sup> Gordon R. McOuat, “The Origins of ‘Natural Kinds’: Keeping ‘Essentialism’ at Bay in the Age of Reform,” *Intellectual History Review* 19, no. 2 (2009), 226. For a detailed analysis of the BAAS’s *The Rules*, see McOuat’s insightful and influential analysis, “Species, Rules and Meaning: The Politics of Language and the Ends of Definitions in 19th Century Natural History,” *Studies in History and Philosophy of Science Part A* 27, no. 4 (1996), 473-519.

earliest by which the object shall have been so defined.” In his review essay of *The Rules and Agassiz’s Nomenclator Zoologicus*, celebrated American zoologist Augustus A. Gould noted that “The twelfth proposition seems to us the most important of all, after the fundamental one.”

Until lately, the right of priority has been claimed where a man could not show, that, at some anterior period, he had given a name to a specimen in his cabinet, or had read a paper upon the object, and perhaps circulated specimens among his friends. The consequence has been, a superficial acquaintance with the works of naturalists, and an indifference to publication. It was much easier for a man to sit down and attach a ticket to every object in his cabinet which his ignorance suggested *might* be new, and await his chance of claiming his names for such of them as some patient and thorough student should prove to be actually new, than to undertake the task of conning all the published works in which it were likely to find such objects noticed.<sup>11</sup>

In 1845, James D. Dana, a rising naturalist at Yale, also submitted a report to the Association of American Geologists and Naturalists (which, in 1848, would be renamed the American Association for the Advancement of Science), with particular focus on *The Rules’* possible impacts upon American naturalists. He was concerned by the twelfth rule, as well. “It has been customary with some naturalists to give names to species in their cabinets, or in a published catalogue, and on this ground, to claim authority for such names. This should not be allowed. Neither is it sufficient that the description appear in a public newspaper, or in a journal not generally

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<sup>11</sup> Augustus A. Gould, “Notice of Some Works, Recently Published, on the Nomenclature of Zoology,” *The American Journal of Science and Arts* 45, no. 1 (1843), 5.

known for its scientific character, or in language so brief and indefinite that the object cannot be recognized by it.”<sup>12</sup>

To American naturalists, though willing to accept the twelfth proposition as a standard in nomenclature, found themselves in a quandary. They were worried that *The Rules* would eventually deprive them of any “right of priority” in naming new *American* species. As Gould put it,

In this country, we have indeed, from the destitution of books on natural history, been compelled, per force, to risk something, or do nothing. But this should have rendered [us] doubly cautious in imposing names, and ever ready to retract them when they are proved to be synonyms, rather than be tempted by the idea, that no one is so likely to be acquainted with the objects at our door as we ourselves, especially when we happen to live in a district which has sustained no naturalist before us. The actual fact is, that as a general thing, the natural objects peculiar to this country have been better known and better described abroad than at home.... There are collectors constantly employed in this country by foreign naturalists, who, in a quiet way, send across the water immense stores of all kinds of natural objects; and one is surprised when he sees the flood of such objects, collected at our doors and without our knowledge, in the public and private collections abroad.<sup>13</sup>

Gray heartily embraced *The Rules* in general and the twentieth proposition in particular. He had no doubt that such regulations captured what Linnaeus had eloquently stipulated in botanical nomenclature.<sup>14</sup> He would soon publish a review essay of Agassiz’s *Nomenclator Zoologicus* (1842-1846) in *The American Journal of*

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<sup>12</sup> James Dwight Dana, *Report on Scientific Nomenclature, Made to the Association of American Geologists and Naturalists, New Haven, May, 1845* (New Haven: B. L. Hamlen, 1846), 4.

<sup>13</sup> Gould, “Notice...,” 6.

<sup>14</sup> Gray had written in *Elements of Botany* (1836): “The names first published, whether of genera or species, are always to be adopted, even in preference to much better ones, unless the former are absolutely in contradiction to universally received rules.” Gray, *Elements...*, 358.

*Science and Arts* and suggest that American botanists follow the regulations that British zoologists had established in nomenclature.<sup>15</sup> As for his colleagues' shared worries about how to keep "the right of priority" in the hands of American naturalists, his solution was simple: He would apply the subscription system to send a collector to explore unknown botanical territories in North America, a dream that Torrey had failed to realize in 1837, and a way of acquiring specimens that Gray became familiar with during his research trip to Europe.

### ***"Famulus botanicus"***

The Lindheimer mentioned in Gray's 1842 letter to Engelmann was Ferdinand Lindheimer, a German immigrant residing in Houston (Figure 3-1). Receiving the best scientific training before he set foot on American soil in 1834, Lindheimer however shifted from one job to another, at various points working as a planter of bananas and pineapples in Mexico, a soldier in General Sam Houston's army, and a truck farmer in Houston. "My love of living out of doors in Nature has been constant," he wrote to Engelmann in 1841, as though he were embarrassed by his inability to control his wanderlust.<sup>16</sup>

Engelmann had known Lindheimer since they were both junior members of a botanical society in Frankfurt (one of his "earliest and best friends," Engelmann told

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<sup>15</sup> Asa Gray, "Nomenclator Zoologicus, continens Nomina Systematica Generum Animalium, tam viventium quam fossilium, etc.; Auctore L. Agassiz. Fasc. 1-10, (Soleure, 1842-46,) 4to," *The American Journal of Science and Arts (2nd Series)* 3, no. 8 (1847), 302-309.

<sup>16</sup> FL to GE, 8/[illegible/]1841, Goynes, *A Life...*, 34.

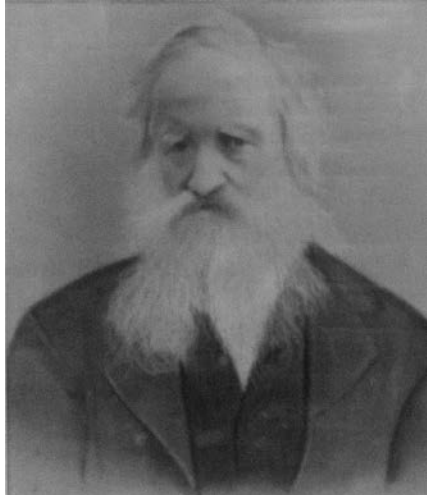


Figure 3-1. Ferdinand Lindheimer (Courtesy of the New Braunfels Conservation Society, New Braunfels, TX, USA.

Gray).<sup>17</sup> Touched by his old friend's unabated enthusiasm for nature, he suggested that Lindheimer equip himself with the necessary skills for plant collecting and prepare himself for a botanical expedition. If Lindheimer agreed to do so, he wrote, he would do his best to subscribe to Lindheimer's collecting enterprise (for example, providing Lindheimer with necessary equipment, selling collections to interested botanists, and so on), and Lindheimer could then turn his "love of living outdoor in Nature" into a means to make a living. Lindheimer was interested but unsure if he was ready to thrust himself into the wildness as a full-time collector. After all, his truck-farming business in Houston demanded a lot of attention. "[I have been] like an embarrassed boy who is kneading his hat with his hands and cannot take leave," he told Engelmann in January 1841. "The poor boy cannot tear himself away, but it is really time he left."<sup>18</sup> Months later, he made up his mind ("My mother

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<sup>17</sup> Goyne, *A Life...*, 30; GE to AG, 3/20/1842, *HL*.

<sup>18</sup> FL to GE, 1/5/1841, Goyne, *A Life...*, 33.

is dead. My partner has gone to Yucatan to join the Federalists," he explained). If Engelmann outfitted him properly, he wrote, he would then devote the next summer (1842) to collecting plants. "I am your loyal *famulus botanicus* and friend Lindheimer."<sup>19</sup>

In March 1842, Lindheimer informed Engelmann that he had received \$50 for his "botanical outfitting," including "a horse for \$15, a keg of flour for \$9, 5 bushels of corn, a pair of shoes, a bit of paper, etc., and the remainder in cash." He also expressed his gratitude for Engelmann's gifts: Amos Eaton's *Manual of Botany* and magnifying glasses. These two gifts, insignificant as they might seem, exponentially expanded Lindheimer's botanical outfit. As Lindheimer admitted, though he had been interested in botany for a while, what buttressed his study were but Nuremberg's microscope glass and three reference books (some fragments of Torrey and Gray's *Flora of North America*, a textbook he brought from Dresden, and *Description of the Plants of the United States and Exotica* by Almira Hart Lincoln).<sup>20</sup>

Lindheimer then set up a camp four miles east of Houston. The chance that he could make a living by plant-collecting electrified this forty-year-old German. "I find new plants every day that I had not observed during the six preceding years," he exclaimed to Engelmann (Figure 3-2).<sup>21</sup> Still, he understood that he had a long way to go before he could turn those "new plants" into a steady income. First, he needed

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<sup>19</sup> FL to GE, [date unknown/]1841, Goyne, *A Life...*, 35.

<sup>20</sup> FL to GE, 1/13/1842, Goyne, *A Life...*, 39, 44.

<sup>21</sup> FL to GE, 5/16/1842, Goyne, *A Life...*, 46-47.



Figure 3-2. One of the earliest specimens Lindheimer collected in Texas as a fulltime collector showing the species *Cirsium filipendulum* Engelm. (Courtesy of the Gray Herbarium, Harvard University, Cambridge, MA, USA.)

proper language to communicate with Engelmann about what he had collected and still had to collect. “Send me the names soon,” he demanded Engelmann after sending off part of his collection, “so that I don't have to keep creating nicknames such as I have been using as an aid, especially for the grasses, for instance narrow ear, panicle ear, long ear, twin ear, ληπτόν, ληπτώτατον, blossom ear, the slender one, the dusky one, star grass, flat fruit, etc.”<sup>22</sup>

<sup>22</sup> FL to GE, 1/19/1842, Goyne, *A Life...*, 43.

Second, although he had Engelmann's word that his collections would be "cashed" sooner or later, he needed cash *now* to prevent himself from starving. Late in 1841, the want of cash drove Lindheimer to make a deal with a British naturalist named William P. Smith, who at the time was collecting natural-history specimens in Houston for Edward Smith Stanley, thirteenth earl of Derby.<sup>23</sup> In a letter to Engelmann, Lindheimer groaned about how reluctant he had been to make the deal. He said that Smith would pay him \$5 per hundred specimens and would not require him to relinquish any novelty. Thus, he demanded, "in order not to have to give Smith new plants, I must know which are the known ones." "If I sent plants to Saint Louis and he [Smith] to Liverpool simultaneously and there were new ones among them, it would be sure to cause a confusing double naming to occur."<sup>24</sup>

Engelmann was concerned about the deal. After examining Lindheimer's collections made in Houston, he was impressed by his friend's ability to hunt botanical novelties. He told Gray in March 1842 that Lindheimer probably had found a new species in *Compositae* (Gray's favorite family). He asked Gray to examine the specimen, and if the species proved to be new, he wanted to honor Lindheimer by naming it "*Lindheimeria Texana*."<sup>25</sup>

Engelmann also informed Lindheimer that a new species (and a new genus as well) might be named in Lindheimer's honor. Lindheimer was pleased. "Did you write my name among the stars with this little Asteroid?" he replied. "Did I serve

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<sup>23</sup> Goyne, *A Life...*, 38.

<sup>24</sup> FL to GE, 5/23/1842, Goyne, *A Life...*, 48.

<sup>25</sup> GE to AG, 3/20/1842, *HL*.



botany in that way? Not by knowledge of it but rather by love of this sleeping, dreaming daughter of Flora... So, if I die childless, then I shall nevertheless leave a little immortal daughter, the *Lindheimeria texensis!*"<sup>26</sup> Interestingly, he then confessed that he actually had no clue of what this "little immortal daughter" looked like. He guessed that he collected it on the banks of Buffalo bayou in October 1841. At that time, he recalled, he only gathered one specimen of every plant that appeared to belong to *Compositae*. This was not his fault, he explained. "When I put it in [the package], I still recalled clearly the indifference with which you put aside the Asteroids in San Louis, and I hardly had the heart to put in more than one."<sup>27</sup>

In January 1843, Lindheimer came to stay with Engelmann in St. Louis. Engelmann, feeding the exhausted collector with wine, bread, gossip, and botany, concluded that he alone could hardly support Lindheimer for another year. He recalled Gray's desire to procure plants from Texas and thought that Gray might be interested in subscribing to Lindheimer's collecting enterprise. So he wrote to Gray suggesting that they share the costs of sending Lindheimer to the field. He told Gray that Lindheimer had decided to "devote a few years entirely to the exploration of Texas and the collection of plants there, and intend to make at least his living by it." To achieve this, Engelmann wrote, Lindheimer had agreed to "offer their collections for sale, by Centuriae." He reminded Gray of Lindheimer's extraordinary specimen-making skills. While Lindheimer devoted himself to collecting and preserving plants, Engelmann wrote that he and Gray should serve as gatekeepers or quality-control

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<sup>26</sup> FL to GE, 5/16/1842, Goyne, *A Life...*, 47.

<sup>27</sup> FL to GE, 5/16/1842, Goyne, *A Life...*, 47.

for Lindheimer's collections. "We would guarantee... the good preservation and careful selection of the specimens and that only the rarest plants of the western and southwestern county would be contained in their collection. That therefore only two to four Centuriae in one year could be furnished... [and] the price could not be under 8 to 10 dollars." "What do you say to that plan?" Engelmann asked.

If you approve of it, and think it feasible, you will do me a favour to publish an advertisement in Silliman's Journal to that effect, and botanists who wish to obtain a collection, may apply to you or to me; it would be well to say in the Journal also a few words, referring to the advancement and to publish the same also in England—I will take care to put it in French and German journals. In that way we would encourage adventurous and ardent botanists, would get a better knowledge of distant and little known countries, and obtain full sets of good specimens therefore."<sup>28</sup>

Gray replied, "I note with interest what you propose in regard to Lindheimer's collections for sale in Centuriae, fall into your plans, and will advertise in 'Silliman's Journal' (and in 'London Journal of Botany') when all is arranged. Pray let him get roots and seeds for me. I will do all I can for him." In fact, delighted by Engelmann's "discovery" of Lindheimer and having been impressed by Lindheimer's specimen-making skills, he thought perhaps Lindheimer could contribute to North American botany in general instead of Texan botany in particular. "[I]f the Oregon bill passes, a party under Lieutenant Frémont, or some one else, will go through the Rocky Mountains to Oregon; and parties of emigrants or explorers will go also. So why not send Lindheimer in some of these? Probably the government party would afford him protection, and probably he might be formally attached to the party." Compared to the Texan flora, Gray pointed out, "The interesting region (the most so in the

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<sup>28</sup> GE to AG, 1/18/1843, *HL*.

world) is the high Rocky Mountains about the sources of the Platte, and thence South!!” Accordingly, he clarified the deal that he could offer for Lindheimer’s expedition. If Lindheimer decided to go to the Rocky Mountains, he would “warrant ten dollars per hundred for every decent specimen”; but if Lindheimer preferred to remain in Texas, he thought “eight dollars per hundred [specimens] is enough.”<sup>29</sup>

Engelmann replied that Lindheimer’s “predilections, his health and the very proper wish” would cause him to “prefer Texas to any other field of operations.” Then he again invited Gray to join him to serve as the gatekeeper, quality-controller, and catalogue-compiler for Lindheimer’s incoming collections.<sup>30</sup> Gray was inspired. “When you get sufficient collections of any of these botanists for distribution,” he replied, “you will please forward me a set, with your own critical remarks.” Although he had “excessively” disliked studying special collections far ahead of his systematic, family-by-family survey of the North American flora, noted Gray, he agreed that by supporting Lindheimer’s collecting enterprise, they would encourage “adventurous and ardent botanists” to “get a better knowledge of distant and little known countries, and obtain full sets of good specimens therefore.”<sup>31</sup> The first botanical expedition under the aegis of American botanists thus began.

As promised, Gray published a short note regarding Lindheimer’s and other two collectors’ expeditions in *The American Journal of Science and Arts* and *The London Journal of Botany*, respectively. “We take much pleasure,” he began, “in announcing

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<sup>29</sup> AG to GE, 2/13/1843, *AGPG*.

<sup>30</sup> GE to AG, 3/9/1843, *HL*.

<sup>31</sup> GE to AG, 1/18/1843, *HL*.

that three enterprising botanists are now engaged in exploring the most interesting portions of the *far West*, and that their collections of dried plants will be offered to subscribers, in sets, as they come to hand." For those who were interested in Texan botany, wrote Gray, he wanted to introduce "Dr. Lindheimer," an "assiduous botanist," who had decided to "devote a few years to the exploration of Texas." Lindheimer "pledges himself to exclude from his sets all the common plants of the south-western United States," he emphasized. The rate for Lindheimer's incoming collection was fixed "at eight dollars (or £1,13s. 6d. sterling) per hundred [specimens]."

At the end of the note, as though Gray was aware of possible suspicion about the subscription system that was for the first time in American botanists' charge, he added a passage:

These several collections will be assorted and distributed, and for the most part ticketed, by Dr. Engelmann of St. Louis; assisted, as far as need be, by the authors of the *Flora of North America*, who promise to determine the plants, so far at least as they belong to families published in that work; and for the information of subscribers, particular notices of the *centuria* offered for sale, will probably appear in this Journal, as they come to hand.<sup>32</sup>

Gray's message evidently electrified botanical communities on both sides of the Atlantic. *The Magazine of Horticulture, Botany, and All Useful Discoveries and Improvements in Rural Affairs*, a widely circulated journal in New England, helped Gray and Engelmann spread the message. Torrey and Sullivant expressed their interests in joining Lindheimer's subscription list, so did Sir William of Kew Gardens and Alexander Braun of the Polytechnic School of Karlsruhe. Curtis was interested,

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<sup>32</sup> Asa Gray, "Notice of Botanical Collections," *The American Journal of Science and Arts* 45, no. 1 (1843), 226.

but was afraid that he would not be able to afford the subscription fee. “Is there any way by which I can get some of the plants from the Collectors Geyer, Lindheimer, &c. without money?” he asked Engelmann. “Money is an article that I am able to use only for my bread and meat;—but if I can get some of their plants by paying in kind, I should very much like to do it.”<sup>33</sup>

Curtis’s reaction was not exceptional. In fact, the rate Gray and Engelmann settled for Lindheimer’s collections was higher than the rate that Sir William usually set for the expedition under his charge. Moreover, Lindheimer’s subscribers even needed to pay the freight *in addition* to the subscription fee.<sup>34</sup> In June 1843, Engelmann told Gray that he had heard from Sir William and Braun about likely difficulties in selling Lindheimer’s collections under current arrangements.

Braun suggests to sell the largest and most splendid specimens in America and in England, and send the smaller ones for a lower price to Germany, which might be a good plan.—... but I think we might put the price as agreed at 8 and 10 dollars in St. Louis or New York. Do you think it should be proper to send a number of collections to Europe? I think not at first—let us try so!—... I wish you would publish them [Lindheimer’s collections] without delay in all the proper journals here and in Europe.—For the present we will say nothing of smaller collections for lower prices, but write first.<sup>35</sup>

### **“Like the children of God of old”**

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<sup>33</sup> MAC to GE, 8/24/1843, Folder 2, Box 1, *CMAC*.

<sup>34</sup> “...payable on delivery of the sets at St. Louis, Missouri, by Dr. George Engelmann; at New York by Wiley & Putnam, 161, Broadway, and Stationers’ Hall Court, London; and Prof. A. Gray, of Harvard University, Cambridge, Massachusetts, to either of whom subscribers may address themselves (*post paid*) by mail. The additional expense of transportation, doubtless trifling in amount, will be charged upon the sets deliverable in London.” Gray, “Notice of Botanical Collection,” 226.

<sup>35</sup> GE to AG, 6/4/1843, *HL*.

While botanists gravitated to support Gray and Engelmann's joint effort to explore the American southwestern flora, Lindheimer found himself being "moored along the Gulf Coast."<sup>36</sup> He hardly anticipated this. He had visited Engelmann in St. Louis, had enjoyed "wine and the conversation of friends," had acquired Gray's approval of taking care of his collecting enterprise, and had received a multitude of gifts from Engelmann (including a dog named Juno). He had envisaged that "this botanical detour" would be the last one that his heart had longed for.

For almost forty years, like the children of God of old, I have now been running around on this earth looking for my promised land, and still I have been unable to find anything. I hope this botanical detour will be, not my last trip but the last one that my heart longs for... Even if I do not find the Cardamine, I shall come to Canaan, I mean Saint Louis, just the same, and even if milk and honey, which I am happy to leave for the *children* of God, do not flow, there will be wine and the conversation of friends instead, things better suited to a *man* of God.<sup>37</sup>

Engelmann became worried when he knew Lindheimer's whereabouts. After all, the Gulf Coast in general, and Galveston and Houston in particular, had been a well-surveyed botanical ground. He thought that Lindheimer would not have secured sufficient novelties for subscribers, had such a stationary collecting style persisted. Vigorously, he urged Lindheimer to get out of the Gulf Coast, reach San Antonio, and push forward "north to the Sierra Madre and west to the Rio Grande."<sup>38</sup>

Lindheimer, depressed by his confinement, wrote lengthy letters to explain his "idleness." In August 1843, he wrote from Houston that he had collected some 2,000 specimens but had no money to pay "freight from here to New Orleans." Besides, he

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<sup>36</sup> Goyne, *A Life...*, 52.

<sup>37</sup> FL to GE, 1/19/1842, Goyne, *A Life...*, 43-44.

<sup>38</sup> See FL to GE, 9/14/1843, Goyne, *A Life...*, 75.

added, traveling in Texas demanded cash, which he had little. "I could only take trips on which I did not need any cash, so I could not cross any larger river, could not go eastward beyond the Trinity, westward beyond the Brazos." "I could have collected three times more beyond what I actually did, or four times, and much better material, had I not lacked even a few dollars," he cried.<sup>39</sup>

Engelmann advanced Lindheimer money from his own pocket to help Lindheimer set off. In September 1843, Lindheimer wrote to Engelmann that he was about to undertake a twelve-day excursion with "a little corn for my horse and a little cornmeal, 2lbs, of bacon, 1lb. of coffee, 1 lb. of sugar, and 1 pt. of whiskey for me." But in the following letter regarding his whereabouts from October 1 to November 3, the collector lamented that he had hardly moved one mile for the past one month. He got stuck in a "swamp hole" after leaving Houston, he explained.

So the old story that I have gone through so often in this wet year started again. The horse had to be unhitched, and a long, strong lever hewn, a long dam built, etc. the north wind was blowing quite briskly. Only after I had gotten my clothes thoroughly wet and muddy did I decide to take off the uncomfortable clothing and let my featherless biped (as Plato designates the human being) in its natural state get to work.<sup>40</sup>

Then, in November 1843, Lindheimer reported that he had encountered "an accident that almost cost my life in the Brazos Bottom." The accident took place when he tried to get himself and his collections over the Brazos River. He got wounded on the left thumb, and the wound soon swelled and developed into the so-called worm. Had he been St. Louis, for example, his swelling thumb could be readily

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<sup>39</sup> FL to GE, 8/10/1843, Goyne, *A Life...*, 68; FL to GE, 9/14/1843, Goyne, *A Life...*, 70; FL to GE, 8/10/1843, Goyne, *A Life...*, 67.

<sup>40</sup> FL to GE, 9/14/1843, Goyne, *A Life...*, 70; FL to GE, 10/1/1843, Goyne, *A Life...*, 80.

treated by surgery. But he was now in Katspring, where no such things as penknives and lancets existed. He had suffered unspeakably, Lindheimer reported to Engelmann. "I could sleep very little, and that little only by sitting up in bed with my head supported by my knees." What deepened his misfortune was that he had lost most of his collection made during the year 1843.<sup>41</sup>

Depressed and disheartened as he was, the poor collector's agony was partly lifted as he read Engelmann's reply. "Oh, yes, you dear people," he wrote back.

I would very much like to live in your vicinity again for a few months, but I must man my post for a while yet and accomplish something so that nobody will get ahead of me and I be found dilatory. I am, to be sure, only a minor helper in the secret temple, not that of Solomon but that of science; not one of the higher officers, but nevertheless a fighter in the front ranks of the great phalanx with which intellect conquers the world.<sup>42</sup>

He then bought a horse (\$35) and told Engelmann that he had decided to leave Wild Cat Spring for the German settlement of Industry, Austin County.<sup>43</sup> In October 1844, he wrote to Engelmann from Industry that he would soon make his way to San Saba, the "middle of the best hunting grounds of the mighty Comanches." He had been thrilled by his own boldness, he admitted.<sup>44</sup> "Now I think it is better that I go, even though I have no money, no shirts, and no blankets. I have a sound horse, good weapons, and health, and six or seven reams of paper. Concerning the number of specimens to collect, I shall have to limit myself for the time being to you and friends

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<sup>41</sup> FL to GE, 2/26/1844, Goyne, *A Life...*, 90; See GE to AG, 2/17/1844, *HL*.

<sup>42</sup> FL to GE, 2/26/1844, Goyne, *A Life...*, 88.

<sup>43</sup> See FL to GE, 2/26/1844, Goyne, *A Life...*, 88 and FL to GE, 5/21/1844 on p. 93.

<sup>44</sup> See FL to GE, 4/18/1845, Goyne, *A Life...*, 113.



and subscribers.” “Stay in your huts, your tents, but let me rest on my saddle!” he exclaimed.<sup>45</sup>

Still he found another equally compelling option. He had learned from the newspaper *Schnellpost* that later in the year, a group of German immigrants would arrive in Port Lavaca, march inland, locate a piece of fertile land, and establish it as their new homeland. The whole project was initiated by a company comprising 25 German princes and nobles. Naming itself the “Verein zum Schutze deutscher Einwanderer in Texas,” or “Society for the Protection of German Immigrants in Texas,” the Society had helped immigrants secure a tract of land northeast of San Antonio known as *las fontanas* or Comal Spring (later known as New Braunfels).<sup>46</sup>

Lindheimer decided to combine his botanizing enterprise with his fellow Germans’ colonizing endeavor. In April 1845, he traveled with some 500 German immigrants to Comal Spring. He promptly wrote to Engelmann upon his arrival, expressing his delight at being enveloped by nature’s pure beauty and fecundity. “It is sufficient that we are at least here, where the streams flow crystal clear over the rocky beds. The fluid element gleams emerald green, and in its greater depths the fish rush back and forth visibly. Powerful springs cascade down from the rocky hills.” What did such magnificent surroundings matter to him? The collector then elaborated.

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<sup>45</sup> FL to GE, 10/19/1844, Goyne, *A Life...*, 102.

<sup>46</sup> Regarding Lindheimer’s coming across the news of German immigrants, see FL to GE, 10/19/1844, Goyne, *A Life...*, 102; regarding the history of Society for the Protection of German Immigrants in Texas, see Goyne’s introduction for the chapter “Life on the High Ground,” Goyne, *A Life...*, 105-109.

Palmate yuccas, cactus, and mimosas and the fragrance and blossoms of them all, that's for me. Here I have seen for the first time the total splendor of the prairies. Flower upon flower, richer than the richest Persian carpet. Fragrances that sometimes remind one of violets, often of vanilla, flow around the wanderer. The sea wind breathes with a living freshness over the prairies and streams through the treetops of the forest like the murmur of a distant surf.—Yes, Nature is beautiful, beautiful everywhere where mankind does not make it ugly; subjectively by means of its mood, objectively by means of its destruction and waste.<sup>47</sup>

### **“We have broken the ice!”**

While Lindheimer gradually pushed himself to the interior of western Texas, the intensity of the correspondence between Engelmann and Gray reached new heights. The two botanists discussed various issues respecting Lindheimer's expedition, from whether or not Lindheimer had discovered a new species, to the best timing to distribute specimens to subscribers, to the manners of mounting specimens and printing labels. Neither Engelmann nor Gray had run a subscription system before. But the increasingly complete post system in the country enabled them to learn the running of the subscription system as though they were not separated by more than one thousand miles but residing in the same neighborhood. (It took less than a week for a letter to be delivered between Boston and St. Louis.) Both Gray and Engelmann were ardent letter-writers and treasured each other's letters. For the next five years or so, the two botanists would run an international business that covered New Braunfels; St. Louis; Boston; Washington, DC; London; Lancaster; Heidelberg; Berlin; Paris; and beyond, simply by correspondence.<sup>48</sup>

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<sup>47</sup> FL to GE, 4/18/1845, Goyne, *A Life...*, 112.

<sup>48</sup> Regarding how a multitude of social, cultural and technological changes conspired to initiate a communication revolution in the United States during the 1840s, see

In January 1844, Engelmann informed Gray that he had addressed “a full set of Lindheimer’s collection for sale” to Cambridge and had numbered each specimen so that they could make sure which specimen they were speaking of. About a month later, he reported to Gray that Lindheimer had “lost his full collection, all his seeds, and nearly his life” and suggested that they immediately enumerate Lindheimer’s 1844 collection, publish the catalogue, and then collect the subscription fee. They had to do something to rescue Lindheimer from such miseries, Engelmann declared. Gray disagreed with Engelmann’s proposed course of action, however. Instead of hurrying on to push Lindheimer’s still-limited collection to market, he replied, they should wait until they had more specimens in hand. Engelmann agreed. In April, he wrote with relief that Lindheimer had made a new collection in Brazos and Colorado. According to Lindheimer, he went on, this newly acquired collection embraced some 2,500 specimens and would make its way to St. Louis soon. “This coincides well with your wish to postpone the first publication till we can distribute more at a time.”<sup>49</sup>

In addition to numbering and sorting specimens and serving as a messenger between Lindheimer and Gray, Engelmann also took responsibility for printing labels (to be attached on the sheets of specimens). “How [to] put our names?” he asked Gray. “I thought those new plants, especially of the lower orders, which I must

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David M. Henkin, *The Postal Age: The Emergence of Modern Communications in Nineteenth-Century America* (Chicago: The University of Chicago Press, 2006); Daniel Walker Howe, *What Hath God Wrought: The Transformation of America, 1815-1848* (Oxford: Oxford University Press, 2007).

<sup>49</sup> GE to AG, 1/8/1844, *HL*; GE to AG, 2/17/1844, *HL*; GE to AG, 4/8/1844, *HL*.

leave you to describe we put Gray & Engelm. and to those which I describe Engelm & Gray—However as you think proper and right—perhaps only Gray to the one part & Engelm to the other.” Gray disagreed with the format. Instead of listing every piece of information for subscribers, he suggested that the label have the number only. But Engelmann had to ensure that the labels were “decent and large,” he stipulated, so that subscribers could put their own remarks. Again, Engelmann consented.<sup>50</sup>

Engelmann also agreed to Gray’s superior status in plant classification. In March 1844, he sent Gray a set of manuscripts based on his studies on Lindheimer’s plants. “I have no doubt that a number of those, described by me as new will be [verified] by you as already described; my literary means are so limited, so that I can not do better than describe every plant not found there.” In May, after sending Gray another set of manuscripts, he wrote, “I have to leave it entirely to you, of course, how to alter or abridge my account of the Texas plants. I have described a number, which no doubt, are known already; where therefore a mere mention of names, locality and date will be sufficient. But you will also find a number of remarks, partly derived from Lindheimer letters, partly from the investigations of a large numbers of specimens which I think would be valuable to retain, if correct.”<sup>51</sup>

What Engelmann could hardly agree with was Gray’s continued postponement of printing out the catalogue, of distributing Lindheimer’s plants to subscribers, and of collecting the subscription fee. “[Y]ou must then not defer it any longer,” he wrote to Gray in December 1844. “I owe it to Lindheimer and owe it to the subscribers to

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<sup>50</sup> GE to AG, 2/17/1844, *HL*; GE to AG, 10/5/1844, *HL*.

<sup>51</sup> GE to AG, 3/11/1844, *HL*; GE to AG, 5/13/1845, *HL*.

distribute the plants.” In January 1845, realizing that Gray had hardly made a move to bring out the catalogue, he grew concerned. “[W]e must not wait any longer; for Lindheimer must have money; he has already refused offers, to sell his collection to others, but he will be obliged to do it, if we do not now haste the distribution of his plants... Lindheimer writes that it shall not be his fault if the nomenclature of Texan plants gets mixed up and entangled. He will send nothing elsewhere but here, but we must not let him lack the necessary means; I have therefore sent him 200 dollars on his collection.”<sup>52</sup>

“I really think that Lindheimer’s interest is best subserved by our keeping back the sets until the number becomes considerable,” Gray replied. “[T]here are so many plants on sale, that only good, very good collections take well.” He suggested that they began considering the issue when Lindheimer got “more and more in the interior” and when the species collected became “like those of the southern United States generally.”<sup>53</sup>

“I shall like to retain Lindh. Collection till we have more,” Engelmann replied. “[B]ut some subscribers become impatient and what is still more important, Lindheimer must have money, though he wants very little; and I can not pay him any longer for his plants, as I have done heretofore before, if I do not get any returns from the subscribers.”<sup>54</sup>

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<sup>52</sup> GE to AG, 12/6/1844, *HL*; GE to AG, 1/11/1845, *HL*.

<sup>53</sup> AG to GE, 2/3/1845, *AGPG*.

<sup>54</sup> GE to AG, 2/20/1845, *HL*.



Figure 3-3. *Gaura lindheimeri* (Courtesy of the Missouri Botanical Garden, Saint Louis, MO, USA [left] and the Gray Herbarium, Harvard University, Cambridge, MA, USA [right].)

Gray was indeed amazed by the botanical wealth that Lindheimer’s collections had revealed and hoped to introduce it to New England’s horticultural circle. “*Gaura Lindheimeri* is a very fine plant,” he wrote admirably to Engelmann. “I have saved *Gaura Lindheimeri* by cuttings put in pots last autumn,” he wrote in another letter. “We shall have it in flower early in the spring, and then shall exhibit it at the Horticultural Society’s rooms in Boston.” In private, he sent seeds of *Gaura lindheimeri* to Miss Morris in return for the lady’s “Forget-Me-Not” (Figure 3-3).<sup>55</sup>

Another species that impressed Gray with its horticultural potential was *Brazoria truncata*, a new species in the mint family (*Lamiaceae*). In 1846, when Gray came to characterize the species, he struggled to come out with appropriate terms to describe the species’s beauty (Its corollas were “an inch long, dull purplish rose-color, slightly striped and conspicuously dotted with deep purple; the lower lip is

<sup>55</sup> AG to GE, 11/4/1843, *LAG*, 313; AG to GE, 2/2/1844, *LAG*, 313; AG to ECM, 9/24/1844, *AGPL*; AG to ECM, 11/9/1844, *AGPL*.

paler, and tinged with yellowish inside.”) Equally important, he found that the species actually had been discovered in the 1830s. As he elaborated in *Chloris Boreali-Americana* (1846), the species was first collected by French naturalist Jean Louis Berlandier (1805-1851) but misclassified by George Bentham as *Physostegia truncata* (because Berlandier’s specimen lacked fruits). In 1833 and 1834, British collector Thomas Drummond (1780-1835) again encountered the very species, collected it, and sent its seeds back to Sir William at Kew. Even so, Sir William still mistook it for *Physostegia truncata*, and even “altered the specific character to make it accord with the plant before him.” Now, thanks to Lindheimer’s thorough and systematic survey of Berlandier’s and Drummond’s collecting sites, Gray and Engelmann now rescued the species from sinking into oblivion.<sup>56</sup>

Gray began immersing himself in Lindheimer’s 1843-1844 collections in May 1845. “I wish that the account should do us credit as botanists,” he told Engelmann, “and you know how many points I shall have to investigate—how many books &c. to consult.” Needless to say, Gray emphasized, he would detect and describe any new species that might appear in Lindheimer’s collections and ensure that there was not “the slightest danger of any one taking them up before us.”<sup>57</sup>

While checking references in the library, examining dried specimens at the herbarium, and observing living specimens in the garden, Gray tried to recruit more subscribers for Lindheimer’s expedition. Tentatively, he wrote to his close

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<sup>56</sup> Gray, *Chloris Boreali-Americana*, 35-37.

<sup>57</sup> AG to GE, 2/3/1845, *AGPG*.

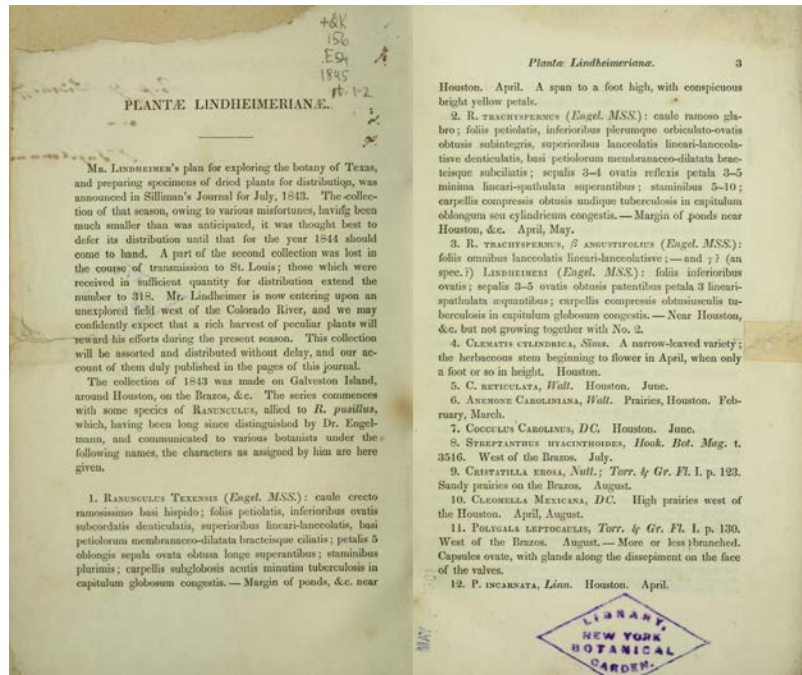


Figure 3-4. "Plantae Lindheimerianae, Part 1" (1845), the first fruit ripened from Gray and Engelmann's long-term collaboration to explore the American West. (Courtesy of the Peter H. Raven Library, Missouri Botanical Garden, Saint Louis, MO, USA.)

correspondent Elizabeth Morris (who had recently expressed an interest in setting up her own herbarium).

The Texan collection,—which still occupies my spare time, is made by Mr. Lindheimer under Dr. Engelmann's superintendence. The numbers run up to 315, but there will be further and I hope still more interesting collections this summer—from the Colorado, &c.—I am daily expecting the sets from Engelmann, which except a few subscribed for—or to be subscribed here—will go aboard. The price is \$8.00 per hundred, which is counted for. Dr. Darlington is not a subscriber, I believe. Should you want them I will stop a set when they reach me on the way to England. I should of course send you our account of them, as soon as printed.<sup>58</sup>

On September 3, 1845, Gray communicated his enumeration to the Boston Society of Natural History. He titled the essay "Plantae Lindheimerianae: An Enumeration of the Plants Collected in Texas, and Distributed to Subscribers, by F.

<sup>58</sup> AG to ECM, 6/9/1845, Box 1, Folder 3, AGPL.



Lindheimer; with Remarks, and Descriptions of New Species, &c.” and ensured it to be published in the society’s *Boston Journal of Natural History* (Figure 3-4).

Coauthored with Engelmann, *Plantae Lindheimerianae* contained 318 species and varieties, including 39 new species (six of which were named after Lindheimer) and two new genera (*Thysanella* and *Brazoria*).<sup>59</sup> Engelmann was satisfied with the fruit ripened from his and Gray’s joint labor. “I am much pleased with the whole but think you do me too much honor in putting my name first; my labor was certainly great, but probably not so efficient as yours.”<sup>60</sup>

Early in 1846, Gray began sending sets Lindheimer’s 1843-1844 specimens to subscribers along with copies of *Plantae Lindheimerianae*. Henry Botton Fielding was impressed.

In the autumn I received your paper on Lindheimer’s collections shortly after my share had been forwarded to me. Thus it was rendered doubly welcome, as enabling me at once to affix the name to the specimens, before laying them into my Herbarium. I thought them very interesting, and beautifully dried, which was far from being the case with those of Drummond, with which collection many of the species are identical, I should be highly gratified to hear, if you have an hour to spare that he continues his collecting, and that it has met its reward in the discovery of many new and rare plants.<sup>61</sup>

Although the publication of *Plantae Lindheimerianae* definitely gave Gray and Engelmann credit as American botanists, Engelmann grew worried about whether Lindheimer could be sufficiently remunerated for his botanical labor. On that matter, he wrote, he had sent seeds and bulbs in Lindheimer’s name to Gray,

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<sup>59</sup> Engelmann and Gray, “*Plantae Lindheimerianae*.”

<sup>60</sup> GE to AG, 10/6/1845, *HL*.

<sup>61</sup> HBF to AG, 8/8/1846, *HL*.

entreating Gray to send Lindheimer some cash as remuneration. If Gray could do so, Engelmann went on, Lindheimer's zeal would be "instigated," because he would see the perspective of "earn[ing] with his collection so much that he could extend his investigation further."<sup>62</sup>

Gray replied, "If we succeed in raising some more for our Garden this winter—as I have some hopes—I shall be very glad to offer a small pension to Lindheimer for seeds. If not, though I should like to do something, I must probably do it from my own pocket." And after measuring the depth of his pocket, he turned to raise money from those "gentlemen of public spirit."<sup>63</sup>

A "gentleman of public spirit" who would play a decisive role in Gray's subscription system was John A. Lowell (1798-1881). Twelve years older than Gray, Lowell was a successful banker, a member of the Corporation of Harvard College, and an active figure in such public trusts as the Athenaeum, the Massachusetts General Hospital, and the Lowell Institute. Besides, Lowell was an agricultural innovator, an ardent horticulturalist, and an enthusiastic plant lover. (Lowell mostly acquired his botanical knowledge from Francis Boott, a Bostonian, an expert in *Carex* and a physician in London who once had a great chance to become the first

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<sup>62</sup> GE to AG, 1/11/1845, *HL*.

<sup>63</sup> AG to GE, 2/3/1845, *AGPG*; AG to WJH, 2/28/1843, *LAG*, 299. Charles Mason Hovey, a rising star in New England's horticultural circle, helped Gray and Engelmann make Lindheimer's botanical enterprise known to wealthy plant-enthusiasts in the United States (by far no American had joined Lindheimer's subscription list). Charles Mason Hovey, "Boston Journal of Natural History, Containing Papers and Communications Read before the Boston Society of Natural History, and Published by Their Direction," *Magazine of Horticulture, Botany and All Useful Discoveries and Improvements in Rural Affairs* 12 (1846), 63-66.

Fisher Professor in Natural History at Harvard.) In November 1841, right before Gray came to settle down at Harvard, Lowell was elected as the Fellow in the Botanical Section in American Academy of Arts and Sciences. Taken altogether, it was not surprising that Gray turned to Lowell for help.

In November 1845, Gray exclaimed to Engelmann that “We have broken the ice!”

As Mr. Lowell expressed a desire to aid in this matter and as he is a cultivator, keeper a gardener, conservatory etc. I asked him to eke out our poverty here at the Garden, by joining me in a remittance, and if successful—in offering an annual stipend to Lindheimer for sending us seeds, bulbs, cacti (especially) tubers, and indeed roots of live herbs and shrubs (Don't forget *Alganabia glandulosa*!) which with the present & prospective communication between any Texan host and New York or Boston, can be safely and promptly sent. He will do it and take part of the plants. As soon as Lindheimer will send us something, you may draw on me for \$75. If all goes well, I hope hereafter it may be made \$100 per annum, and, if we even get our Garden funds replenished even more. So, pray set Lindheimer to work... You point out to him the things [that] most likely please me.<sup>64</sup>

### **“Hope a little more”**

In May 1846, when *Plantae Lindheimerianae* was ready, and both Engelmann and he were busy distributing Lindheimer's sets to subscribers and collecting the fee to support Lindheimer's Texas expedition, Gray decided to initiate a new project. He learned that Colonel Stephen Kearny would lead the “Army of the West” to conquer Santa Fe and then proceed to occupy California. He was enthralled. As Gray later put it, when he heard of Colonel Kearny's plan, he became “Desirous to render the occupation of New Mexico by the United States troops subservient to the advancement of science, and to make known the vegetation of a region which had

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<sup>64</sup> AG to GE, 11/20/1845, *AGPG*.

scarcely been visited by a naturalist.”<sup>65</sup> “We must have a collector for plants living and dry to go to Santa Fe, with the Government Expedition,” he declared to Engelmann. “If I were not so tied up, I would go myself. Have you not some good fellow you can send? We could probably get him attached somehow so as to have the protection of the army, and if need be I could raise here two hundred dollars as an outfit. He could make it worth the while. He could collect sixty sets of five hundred plants (besides seeds and Cacti) very soon, which, named by us, would go off at once at ten dollars per hundred.”<sup>66</sup>

The fact was that both Gray and Engelmann had been disappointed by their inability to gather firsthand information about the flora of the Rocky Mountains. In 1843, Gray had advertised that two collectors (“Mr. Lüders and Mr. Charles A. Geyer”) would set their foot toward the “far West,” and their incoming Rocky Mountains collections would be sold at the rate of \$10 per hundred specimens. Since then, both Gray and Engelmann had longed to receive a complete set of Lüders’s and Geyer’s respective collections. (Engelmann even had advanced a \$150 to Geyer.) But it turned out that neither Lüders nor Geyer would meet Gray and Engelmann’s expectations. Lüders lost his collections as he pulled himself across the Columbia River, and Geyer decided to take all his collections (which could hardly have been made without Gray and Engelmann’s assistance) to Britain and place them in the hands of Sir William. Even the great “pathfinder” John Charles Frémont exhibited little interest in collaborating with Gray and Engelmann during his historical

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<sup>65</sup> Gray, “Plantae Fendlerianae Novi-Mexicanae,” 1.

<sup>66</sup> AG to GE, 5/30/1846, *LAG*, 341.

expeditions across the North America. In a letter to Gray in December 1844, Engelmann expressed his despair about the misfortunes and misconduct of these three explorers.

I believe I have written you that I had a letter from Geyer from Oregon; he will take his plants directly to England (and not pay his debts here in St. Louis, I expect!) Fremont has seen Lüders on the Columbia, who had lost every thing he had in the river. Fremont himself writes me that most if his plants were destroyed—It appeared somewhat regular to me, that during a stay of 8 or 10 days here in St. Louis he would not allow me to open and dry his moulding packages. Did he distrust me? He appears to me rather selfish—I speak confidently—and declined to let any body share in his discoveries, anxious to reap all the honour, as well as undertake all the labour himself. He objected to take any botanist or geologist along with him, though the expense would hardly have been!<sup>67</sup>

The collector who eventually substantiated Engelmann and Gray's knowledge about the flora of the Rocky Mountains was Augustus Fendler, a German immigrant from East Prussia, and the second collector who joined the subscription system maintained by Engelmann and Gray. Unlike Lindheimer, Fendler endeavored to make a living by collecting plants from the very start. As he later recalled, his interest in this particular enterprise could be dated back to 1844, when he briefly returned to Germany and paid a visit to Königsberg. There he met a botany professor named Ernst Meyer and learned that "a certain number of sets of dried specimens of plants for the herbarium might be disposed of at a reasonable price."<sup>68</sup> Fendler was impressed. It seemed to him that no other way of making a living could have better cultivated his budding interests in natural history.

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<sup>67</sup> GE to AG, 12/6/1844, *HL*.

<sup>68</sup> William M. Canby, ed. "An Autobiography and Some Reminiscences of the Late August Fendler. I," *Botanical Gazette* 10, no. 6 (1885), 288.

Returning to the United States, Fendler chose the flora of St. Louis as his starting point. While working there as a lamp-maker, he familiarized himself with the essential skills of making botanical specimens. Soon he contacted Engelmann for instructions. He wondered if Engelmann could introduce interested persons so that he could sell his St. Louis collection. Engelmann replied that Fendler's proposal was improbable. He suggested that Fendler study more especially the flora of St. Louis's neighborhoods and afterward collect in some distant regions. Fendler accepted Engelmann's suggestion. He soon picked Santa Fe as his destination and planned to botanize there in 1847. Then Gray's letter about Colonel Kearny's expedition came. Expecting that the Santa Fe expedition would constitute a watershed moment in his career, Fendler sold his property for \$75, outfitted himself, recruited his young brother as an assistant, and told Engelmann that he was ready to go.<sup>69</sup>

Engelmann was proud of his discovery of Fendler. It seemed to him that Fendler possessed great potential to become a collector as capable as Lindheimer. ("Fendler appears to be one of those men who will never be sedentary—rambling has become another nature to them... Besides that he is, sober, active, and entirely trustworthy," he told Gray.<sup>70</sup>) Still Engelmann knew that Fendler was but a novice in the world of collectors, and a novice collector might cause great losses to the botanists in charge and subscribers alike. In a letter to Gray in June 1846, he pondered how to dispatch Fendler in the most economic manner.

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<sup>69</sup> See GE to AG, 7/3/1846, *HL*.

<sup>70</sup> GE to AG, 9/26/1849, *HL*.

The question is, how to send him, how he should be fitted out? Must he rely entirely on the government expedition or ought he to have a light waggon with one or two mules, and his outfit all for himself, and only be under the protection of government.—I would prefer this last [option]; think the first even impossible. I think government would not trouble itself with more, and least of all the commander of the Santa Fe expedition, Col. Kearney, after all I have heard of him.—If he must have a waggon [and] a mule himself, he must have about \$250 loaned to him, if not, he wants very little. Perhaps he could have the paper and baggage taken to Santa Fe by traders (high freight) or government, and go himself on a mule, and begin his work only there. That would be the cheapest way; if he did well, he could afterwards procure a more commodious and efficient outfit.—On forth consideration I think I will propose and try to effect this: send him as cheaply as possible and do more for him, when he proves himself worthy... If he is willing to go, I think I will not wait for your answer, I will lend him a hundred dollars, relying on your wealthy friends in Boston etc. to refund the money; I have lost too much with Geyer and am always in considerable advance with Lindheimer so that I can not undertake more now for my own pocket.<sup>71</sup>

A week after sending out the letter, Engelmann again wrote to Gray updating him on Fendler's Santa Fe expedition. To stir up Fendler's enthusiasm, he wrote, he had promised Fendler that he (Engelmann) could probably sell 30 sets of Fendler's Santa Fe collection at the rate of \$10 per hundred specimens and that Fendler could probably gain \$8 per hundred specimens sold. Regarding the outfit, Engelmann wrote that Fendler had asked for \$150 and had decided to rely on the trading Mexicans to deliver his luggage and collections. Understanding that in so doing he would have to pay "a pretty heavy freight," Engelmann wrote, Fendler had decided to minimize the outfit—he would not even purchase a horse but travel on foot. Engelmann was touched by the young man's perseverance. He wondered if Gray could contact the authorities in DC to see if Fendler could get "free transportation for him with such a baggage train." As for Fendler's required \$150, Engelmann

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<sup>71</sup> GE to AG, 6/25/1846, *HL*.

wrote that he wished Gray could collect it from Gray's "rich friends in Boston" ("if not, I will have to learn a part of it," he added). "I think you and I should be [exempt] of paying money [for Fendler's collections], as we have to perform a great deal of labour in the matter, and as we both, I believe, have it not very superfluously."<sup>72</sup>

Engelmann's note kindled Gray's enthusiasm. About a week after receiving Engelmann's letter, he replied that he had gotten money and free transportation for Fendler's expedition. First, he wrote, he had dashed off letters to the Secretary of War and to the head of the Corps of Topographical Engineers, making clear Engelmann's plan for Fendler. Now it should be possible for Fendler to attach himself to the government expedition and rely on the Army's wagons to deliver his luggage and collections. Second, regarding Fendler's outfit, Gray said that he had asked Sullivant to forward \$50 to Fendler to "take his pay in Mosses and Hepaticae, and to give instructions about collecting these, his greatest favorites." Most importantly, he cheered, he had persuaded Lowell to advance \$150 to Fendler. "Is it not handsome?" he wrote. "Now Fendler has money enough to begin with" and he had little doubt that the Santa Fe expedition would bring Fendler a fortune. "As soon as he is in the field, and shown by his first collections that he is deserving, I can get much more money advanced for him, from other parties. If he only makes as good and handsome specimens as Lindheimer, all will be well."<sup>73</sup>

"Fendler left here yesterday for the Fort [Leavenworth]," Engelmann notified Gray in August. "I paid him 50 dollars from Mr. Sullivant and \$120 from Mr. Lowell,

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<sup>72</sup> GE to AG, 7/3/1846, *HL*.

<sup>73</sup> AG to GE, 7/15/1846, *AGPG*.



together \$170, all he wanted for the present.” Also, he added, he had prepared “full written instructions” for Fendler about plant-collecting and note-taking, and believed Fendler would do well. Engelmann concluded the letter from the perspective of a German immigrant who had possessed the most erudite scientific training and yet had been living in a frontier city of the United States: “I was delighted to see with what prompters all acted, you yourself, Mr. Sullivant, Mr. Lowell, the Secretary and our Quarter master here, and begin to hope a little more from this country for science!!”<sup>74</sup>

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In 1846, while Fendler was on his way to Santa Fe, Gray and Engelmann’s effort to turn Lindheimer’s 1843-1844 collections into cash began. *Plantae Lindheimerianae* had been ready, and their remaining task was to distribute Lindheimer’s sets of plants to subscribers and collect the fee (Figure 3-5). Unexpected obstacles soon surfaced. In 1843, when Gray and Engelmann contemplated Lindheimer’s Texas expedition, they had targeted Britain as the major market. Sir William had consented to take responsibility for receiving Lindheimer’s sets of specimens and distributing them to subscribers. Gray, conscious of how much additional labor Sir William would have to invest in the process, had insisted that Engelmann should afford the freight from St. Louis to Boston, and he himself would take care of the freight from Boston to London. Engelmann had consented then, but now felt the

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<sup>74</sup> GE to AG, 8/2/1846, *HL*.

*Lindheimer plants 1843 & 1844*

1. Hooker	211	9.17.44	4.1.46	27.00
2. Kelly	215	27.44		27.00
3. Dr. Mearns	219	27.44		27.00
4. Deak	217	27.36		27.00
5. B. Smith	194	27.12	Dec. 46	27.00
6. Deak	211	24.18	Jan. 46	24.00
7. Jones	202	24.16	Jan. 46	24.00
8. S. Mearns	206	23.64	25. 46	23.00
9. Mearns	200	23.20	Dec. 46	23.00
10. Knapp	221	22.88		22.00
11. W. Mearns	207	21.26		21.00
12. Deak	210	20.00		20.00
13. Weger	227	18.96		18.00
14. Lewis Museum	237	18.16		18.00
15. J. Mearns	254			
16. Deak	214	17.12	21. 46	17.00
17. B. Mearns	207	16.76	Jan. 46	16.00
18. B. Mearns	200	16.00	Jan. 46	16.00
19. Deak	200	15.20	Jan. 46	15.00
20. Deak	186	14.88	1. 46	14.00
21. Mearns	169	12.72		12.00
22. Deak	161	12.08		12.00
23. Deak	153	12.24		12.00
24. Deak	189	11.92		11.00
25. Deak	146	11.68		11.00
26. Deak	128	11.04	2. 46	11.00
27. Deak	132	10.56		10.00
28. Deak	126	10.08		10.00
29. Deak	115	10.00	1. 46	10.00
30. J. T. Gray	118	10.00	2. 46	10.00

*Balance and the balance of the 1843 & 1844 collections*

Figure 3-5. The “distribution list” for Lindheimer’s collections made in 1843-1844 (Courtesy of the Peter H. Raven Library, Missouri Botanical Garden, Saint Louis, MO, USA.)

whole arrangement unfair. In September 1846, he declared to Gray that he had to charge Lindheimer two dollars for every parcel that he sent to Sir William. After advancing so much money to Lindheimer, he told Gray, he could no longer afford the freight rate of \$4.37 per parcel. Lindheimer’s “subscribers should pay for their part of the freight,” he complained in another letter. “[I]t will be very little to them but a great deal to Lindheimer who lives in a very expensive country.”<sup>75</sup>

Nor did the scheme that they could sell most of Lindheimer’s sets in Britain work well. “Hooker writes me that 2 of Lindheimer’s parcels are still undisposed of,” Engelmann informed Gray in January 1846. According to Sir William, he wrote, Lindheimer’s specimens were “difficult for sale in England,” as “containing mostly

<sup>75</sup> GE to AG, 9/1/1846, *HL*; GE to AG, 3/23/1847, *HL*.

the same plants as Drummond's." "Now would it not be better to send them back to you, as Hooker can do that free of expense, and give them to Messrs Oakes & Lowell [so that ] they would get more and probably better specimens than in the last sets which I have here." Gray agreed. Slightly embarrassed, he asked Hooker to return the two undisposed sets. "[T]here is here a demand for additional sets," he explained.<sup>76</sup>

Hooker felt obliged to help the two junior fellows as much as he could. So he published a notice in *London Journal of Botany* under the title "Lindheimer's Plants of Texas." In it he admired the quality of Lindheimer's 1843-1844 collections and Engelmann and Gray's taxonomic skills as exhibited in *Plantae Lindheimerianae*. Unsurprisingly, he noted, Lindheimer's sets had sold well, and now only two sets (one contained 186 species and the other 181 species) remained "undisposed of." To Hooker's satisfaction, the notice worked. The remaining two sets were soon sold.<sup>77</sup>

On September 1, 1846, nearly a year after *Plantae Lindheimerianae* was published, Engelmann wrote to Gray with relief: "All the sets of Lindheimer are now sold, at least all in this county and England; there may be some 2 or 3 unsold in Germany."<sup>78</sup> Looking back, he estimated that Lindheimer's 1843-1844 expedition

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<sup>76</sup> GE to AG, 1/3/1846, *HL*; AG to WJH, 2/28/1846, Letter 255, Reel 2, *CAB*.

<sup>77</sup> William J. Hooker, *The London Journal of Botany; Containing Figures and Descriptions of Such Plants as Recommend Themselves by Their Novelty, Rarity, History, or Uses; Together with Botanical Notices and Information, and Occasional Memoirs of Eminent Botanists, Vol. V* (London: Hippolyte Baillière; Paris: J. B. Baillière; Leipzig: T. O. Weigel, 1846), 12; GE to AG, 3/20/1846, *HL*.

<sup>78</sup> GE to AG, 9/1/1846, *HL*.

yielded some 30 sets of specimens, and earned the collector some \$350, *before* deducting expenses incurred in transporting specimens, printing labels, purchasing sheets, offering Lindheimer's stipend, and so on. "I am afraid his last two years work will bring him hardly more than 300 dollars," Engelmann told Gray worryingly.<sup>79</sup>

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<sup>79</sup> GE to AG, 1/11/1845, *HL*.

## CHAPTER FOUR

### “A Bad Business”

“What is Lindheimer about? Why is not his last year’s collection yet with you?

Professor Asa Gray wrote to his collaborator George Engelmann with profound annoyance. “We have just got things going, and we can sell fifty sets right off of his further collections, and he can go on and realize a handsome sum of money, if he will only work now! And he will connect his name forever with the Texas Flora!”<sup>1</sup>

“We have to wait till the collection of this season comes in,” Engelmann replied. He assured Gray that Lindheimer would “do better this season.” As far as he knew, he noted, “he is better prepared, has an assistant, a good house etc., when part of last year he was camping out, travelling, etc.—I will do what I can to spur him up... when he got my letter informing him of your wishes and Mr. Lowell’s offer, he will do better next year.”<sup>2</sup>

Lindheimer got *pretty* spurred up. “Gray writes to you about me as follows: ‘and he can go on and realize a handsome sum of money, if he will only *work* now!’ Indeed this sounds quite good. I mean to work too. I would have worked more last year too, if it had been possible for me to do so. But I don’t want to hear about ‘a handsome sum of money’ at all, as long as I can get no cash. I cannot think of

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<sup>1</sup> AG to GE, 4/8/1846, *LAG*, 144.

<sup>2</sup> GE to AG, 4/27/1846, *HL*.

anything I prefer to wandering around and gathering plants and collecting and preserving them.”<sup>3</sup>

Lindheimer thought that he had every reason to make his dissatisfaction felt to Engelmann and Gray. He had been married to a German woman named Eleonore Reinartz of Aachen (in 1846), and plant collecting was the family’s only source of income. But circumstances suggested to him that collecting was no longer a promising enterprise in Texas.

When I have been able to work without disturbances, I have been able to preserve almost 200 plants a day. In the meantime the nice domestic relationship and the health and good humor of myself and my wife leave nothing to be desired. But what causes us a lot of discontent is the constant [castration] or the levying of 200% to 500% one must put up with for indulging in even the least little pleasure, pleasures one is not always able to deny oneself.—What sort of pleasures are these anyway? you will ask. Answer: hardly anything but the most essential requirements of life that a person can still have from a little often bitter cornmeal and a little beef.

Lindheimer could not help wondering if he had chosen the wrong profession. “The farmers here are in better shape than we because they have milk, butter, eggs, pumpkins, vegetables, corn, and, in addition to that, often a daily wage that is to be paid in cash (mine).” Now he could do nothing else. He would then collect “as many and as good plants” as he could.<sup>4</sup>

Engelmann, sandwiched between Gray and Lindheimer, made every effort to satisfy both parties. He advanced money to Lindheimer and relied on Gray to have the expenses reimbursed.<sup>5</sup> In May 1846, he informed Gray that Lindheimer “had

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<sup>3</sup> FL to GE, 7/8/1846, Goyne, *A Life...*, 157-158.

<sup>4</sup> FL to GE, 7/8/1846, Goyne, *A Life...*, 158.

<sup>5</sup> GE to AG, 3/23/1847, *HL*.

sent about the end of March a box," containing living and grown plants. He then asked Gray if he could draw upon Lowell for \$75.<sup>6</sup> Gray replied, apparently still annoyed by Lindheimer's "idleness."

I am concerned about the box from Lindheimer, which you say was sent about the end of March. It has therefore two months on the way. He has not notified me of it, nor of the way it was sent, which is often quite necessary. I fear the contents will be spoiled. I think it inexpedient to draw upon Mr. Lowell till something comes, when I will do it. Lindheimer must do better, very much better, or I fear we shall have to look out for another collector to send into Texas. We ought now to be forming large collections of Texan plants, and, now that the preliminary difficulties are overcome, we can sell promptly all the sets he can make. He must work this summer, in drying plants, and have a large collection in your hands (to eke out last year) in August,—and then make an autumnal collection besides.<sup>7</sup>

One and a half months later, he exclaimed to Engelmann that he could hardly believe that Lindheimer was "industrious." As for Lindheimer's box, he commented, "I have not got or heard of the box,—hope I shall not now,—for all must be dead."<sup>8</sup>

Lindheimer's boxes arrived in January 1847. Hardly thinking that any living plant would have survived, Gray still examined the box. "Strange to say," he wrote curiously to Engelmann, "one or two of the Cacti were alive,—but will not survive, I think."<sup>9</sup>

In June Lindheimer's second box arrived. Gray opened it with anticipation and closed it with apprehension.

They reached me with freight and charges amounting \$15.—But every thing except the Yucca and Yucca-like dioecious plant (palm-like, with prickly leaves)

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<sup>6</sup> GE to AS, 5/19/1846, *HL*.

<sup>7</sup> AG to GE, 5/30/1846, *AGPG*.

<sup>8</sup> AG to GE, 7/15/1846, *AGPG*.

<sup>9</sup> AG to GE, 1/16/1847, *AGPG*.

and the other with narrow leaves, were dead and rotten and worm-eaten,—dry as dust—the boxes being bored full of holes,—through which also the mice got access and eat bulbs &c.—and all were swarming with grubs.—A bad business, the more so as it so disheartens me from applying to Mr. Lowell, since there is almost nothing for him.<sup>10</sup>

Gray decided to cancel the arrangement with Lindheimer about shipping living plants to Harvard Botanical Garden. Now Lindheimer had to avail himself of collecting specimens—had this German still ventured to make a living by collecting plants.

### **“Ravish it”**

With Gray’s \$170, Engelmann’s instructions, the War Department’s free transportation, and his own \$200, Fendler left Fort Leavenworth, Kansas, on August 10, 1846, with Colonel Sterling Price’s troop consisting of 1,200 mounted volunteers. In October, Engelmann told Gray that Fendler was supposed to arrive in Santa Fe by the middle or the end of September, but he had not heard of anything yet. Two months later, Gray asked if Engelmann knew Fendler’s whereabouts. “[William] Hooker says that region, the mountains especially, is the best ground to explore in North America! There is a high mountain right back of Santa Fe, Fendler must ravish it.”<sup>11</sup>

Neither Gray nor Engelmann had any doubt that Fendler would safely arrive in Santa Fe and readily “ravish” the flora of Santa Fe. The route from St. Louis to Santa Fe through Fort Leavenworth was a highway, literally and figuratively, being made

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<sup>10</sup> AG to GE, 6/1/1847, *AGPG*.

<sup>11</sup> GE to AG, 10/18/1846, *HL*; AG to GE, 1/5/1847, *AGPG*.



up by streams of Mexican traders coming back and forth along the way. And Colonel Stephen Kearny's conquest of Santa Fe also went well. On August 18, 1846, when Kearney's Army of the West had not achieved Santa Fe (some 29 miles away), the Mexican Governor Manuel Armijo had abandoned the town and fled. In the afternoon, the troop entered Santa Fe with ease. When the evening came, the flag of the United States was hoisted over the palace, and a salute of thirteen guns fired from the artillery.<sup>12</sup> Colonel Kearny then appointed a fur trader named Charles Bent as the first U.S. Governor of New Mexico Territory. It seemed a wise choice. J. J. Abert, Chief of the Corps of Topographical Engineers, after a visit to Santa Fe late in September 1846, reported to Congress that Governor Bent enjoyed high popularity among Santa Fe's diverse population. "In the morning I called upon Governor Bent, who, to all the qualification necessary to his office, possesses those of a long residence in this country, a constant intercourse with the people, and an intimate knowledge of their language and character."<sup>13</sup>

Abert apparently overestimated the United States' control over this new territory. Those who were discontent with the U.S. takeover of Santa Fe soon united under the leadership of a local politician named Pablo Montoya and a Taos Indian named Tomasito. Early on the morning of January 19, 1847, they launched their attack. Breaking into Governor's Bent's house, they scalped the governor and hacked

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<sup>12</sup> William H. Emory, *Notes of a Military Reconnaissance, from Fort Leavenworth, in Missouri, to San Diego, in California, Including Part of the Arkansas, Del Norte, and Gila Rivers (Ex, Doc. No. 41. 13th Congress, 1st Session)* (Washington: Wendell and Van Benthuisen, 1848), 30-32.

<sup>13</sup> Abert's words can be found in Emory, *Notes...*, 447-448.

his head off. Then they ravaged the town. Some twenty Americans reportedly lost their lives during the attack.<sup>14</sup>

When the news of the massacre reached St. Louis, Engelmann grew worried. “What in that case will become of Fendler?” he wrote to Gray. “He [Fendler] writes he has a barrel full of Cactus many species of pines, and perhaps 100 species of other plants. He asks for more money, and more drying papers. Shall I send him 100 dollars more for Mr. Oakes or somebody else?”<sup>15</sup>

“Fendler must have some more money—and if alive, be kept at work the present season,” Gray replied. “The Americans will certainly put down the insurrection and Fendler must make large collections, and climb mountains—this summer.” “Fendler must be kept in the field,” he wrote to Engelmann again some three weeks later, and “make a large collection this season—in mountains, if possible—let us [be] sure [to] have the collection, and we can turn them to account at once.”<sup>16</sup>

“Fendler,” Engelmann replied, “I understand indirectly, is sick with Survey and sick of matters in Santa Fe and wants to return. His boxes sent off in April have not arrived yet. I am afraid Uncle Sam is a bad forwarder,—but there is a misfortune & ill luck about this Santa Fe—.” In September, he reported to Gray that Fendler’ box still had not arrived, and Fendler had not received the \$100 either. Fendler had been

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<sup>14</sup> Accounts about this so-called Taos Revolt are numerous. See, among others, Stephen Garrison Hyslop, *Bound for Santa Fe: The Road to New Mexico and the American Conquest, 1806-1848* (Norman: University of Oklahoma Press, 2002), Chapter 20.

<sup>15</sup> GE to AG, 3/23/1847, *HL*.

<sup>16</sup> AG to GE, 4/6/1847, *AGPG*; AG to GE, 4/28/1847, *AGPG*.

“dissatisfied,” he noted, and would return “as soon as the money went out.”<sup>17</sup>

Soon after Engelmann sent off the letter, Fendler showed up in St. Louis, exhausted, destitute, and penniless. Fendler had used up all his funds during his stay in Santa Fe, and had been forced to cash his watch, gun, and other personal items to get his luggage and collections delivered back to St. Louis by Mexican traders. What he possessed now were crates of dried plants and piles of field notes.<sup>18</sup> He expected that Engelmann and Gray could turn his Santa Fe collections into cash sooner, not later. He anticipated a proper remuneration for his loss. After all, Engelmann had promised that his Santa Fe collections would be sold at the rate of \$10 per hundred specimens, and that he would receive at least \$8 per hundred specimens.

Engelmann immediately applied himself to sorting Fendler’s specimens into sets. He found it formidable work. Unlike Lindheimer’s collections, which were more or less in order, Fendler’s collections were a mess. “All my leisure time has been devoted to assist him in arranging his collections,” he told Gray in October.

[T]hey are beautiful, the specimens mostly splendid, and a great many new things amongst them. But they are not well made—of some he has collected 80 to 100 specimens; of other, equally interesting ones, only 5 to 10 or 15, when he could have collected many more. I have not yet been able to form an opinion about the number of specimens or species, but hope that he may be able to pay his expenses and the advances made to him and have something besides—though that will not amount to much.<sup>19</sup>

And “it took a long time to arrange them,” Engelmann noted, “as sometimes the

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<sup>17</sup> GE to AG, 7/27/1847, *HL*; GE to AG, 9/24/1847, *HL*.

<sup>18</sup> Engelmann afterward estimated that Fendler had “brought home from one season about 12000 specimens.” GE to AG, 4/3/1848, *HL*.

<sup>19</sup> GE to AG, 10/31/1847, *HL*.

specimens of one species were in 10 or 15 different packages—so he has collected perhaps 80 or 100 of one and 10 or 15 only of another species.” Even so he thought that Fendler had done a superb job (“the specimens are fine and mostly very complete”). “I am just putting out a collection for Fendler, for you and for myself, the rest to be distributed first to the few subscribers who have paid before hand.” Now he needed a favor. Could Gray “have a short notice published immediately here and in England” including the following information?

... he [Fendler] offers about 10 sets of something like 400 or 500 (perhaps more) species, 10 more of about 300 and 20 more of 200 species—most of them in the best possible state of preservation, and well made, a few being only incomplete (in some oaks, willows, &c.); that the price is \$10 a hundred; transportation from St. Louis to be paid by the subscriber—that a printed catalogue with description of new species will be sent to every subscriber, similar in every respect to Lindheimer’s coll.<sup>20</sup>

Gray immediately wrote to Sir William about Fendler’s Santa Fe collections, along with Engelmann’s letter. “May I ask you to publish in the *Lond. Jour. Bot.* what you think needful to call attention to the collection—at the earliest opportunity.” Engelmann’s letter (Gray changed the term “made” to “selected”) soon appeared in the *London Journal of Botany* under the title “Notice of Mr. Fendler’s Botanical Journey to Santa Fe, in North Mexico.”<sup>21</sup>

Gray got hold of Fendler’s plants in December. “The specimens are perfectly charming!” he wrote congratulating them both on Fendler’s success. “Better never

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<sup>20</sup> GE to AG, 10/31/1847, *HL*.

<sup>21</sup> AG to WJH, 12/1/1847, Letter 259, Reel 2, *CAB*; William J. Hooker, ed., *The London Journal of Botany; Containing Figures and Descriptions of Such Plants as Recommend Themselves by Their Novelty, Rarity, History, or Uses; Together with Botanical Notices and Information, and Occasional Memoirs of Eminent Botanists, Vol. VII* (London: Reeve, Benham, and Reeve, 1848), 44-46.

were made. In a week I shall take them right up to study—and they are Rocky Mt. forms of vegetation entirely, so I can do it with ease and comfort... If these come from the plains, what will the Mountains yield? F. must go back, or a new collector, now that order is restored there. All Fendler's collection will be sold at once, no fear, such fine specimens and so many good plants."<sup>22</sup>

Engelmann was not optimistic, however.

Fendler is willing to return to Santa Fe and perhaps farther south (he says that in the climate of N. Mexico nearly 6 months of every year are lost for botanical collections on account of the rigorous and long winters), if some generous friends of botany can be induced to advance him a sufficient sum, which he could partly repay by the returns for his present collections, and fully and finally by the collections he is going to make, if he should not be so unfortunate as to lose them, or have other mishaps.—he had about \$400 with him last time, which lasted him one year with the most rigorous economy, and he thinks he must have \$500 now, of which however at least 300, perhaps, the whole could be refunded in the course of this year by the sale of his present collections. What does Mr. Lowell say?—<sup>23</sup>

Gray replied that Fendler "ought to go back, and without delay."

He has gained much experience, and will now work to greater advantage. He makes unrivaled specimens, and with your further instructions will collect so as to make more equable sets. If he will stay and bide his time he can get on to the mountains, and must try the higher ones, especially those near Taos. I like your plans, and will immediately lay the whole before Mr. Lowell. I hope he will engage in it, but, besides what you are aware of, he has already advanced so much to help me along with *Illustr. Genera, &c.*—that I fear he will think there is no end to the wants of botanists. But if Fendler engages in this enterprise with spirit, it must be done somehow. (Let him collect land-shells too, etc.)

On the other hand, Gray found it disagreeable to offer Fendler the "whole outfit" as the collector had desired. "Say he has \$250 outfit, and a credit at St. Louis, so that he can draw \$125 every three months, and meanwhile his collections will come to

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<sup>22</sup> AG to GE, 12/20/1847, *AGPG*.

<sup>23</sup> GE to AG, 2/15/1848, *HL*.

hand to encourage us. Let him stay two years, and if he is energetic he will reap a fine harvest for botany, and accumulate a pretty little sum for himself, and have learned a profession, for so that of a collector now is." He concluded the letter by suggesting to Engelmann that they both took "a share of the ultimate responsibility in case of Fendler's death."<sup>24</sup>

Then Gray mustered all his power of persuasion to gather money to send Fendler back to Santa Fe. In March 1848, he told Engelmann that he had gathered some \$350 for Fendler's outfit and had recruited a new subscriber. He was agitated when he learned that Fendler exhibited little interest in repeating his expedition. "Tell Fendler he is a fool, if he does not go back to St. Fe!" Gray exclaimed to Engelmann.<sup>25</sup>

Fendler might have heard of Gray's words from Engelmann. In a letter to Gray in July 1848, some ten months after his return, Fendler declared that could not yet comply with Gray's scheme for "certain considerations."

When my pecuniary means at Santa Fe were nearly all exhausted, when I had to sacrifice one thing after another of my most necessary effects, to keep up a few days longer the scanty support of our lives, in order to collect something more of the vegetation of that region; I looked forward with the utmost confidence to those gentlemen in the East, who had induced me to go out to Santa Fe, and who would as I hoped, leave me not without assistance as soon as I should have returned with my collections to St. Louis. It was this hope that made me bear all the difficulties most cheerfully under the happy impression that the enjoyment of the fruits of my labor would soon compensate for all.

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<sup>24</sup> AG to GE, 2/29/1848, *AGPG*.

<sup>25</sup> AG to GE, 3/15/1848, *AGPG*; AG to GE, 4/17/1848, *AGPG*. "...Fendler has concluded not to go. He is gone to the western borders of Missouri; where he will establish a small business; if he succeeds, well; if not, he will probably go next spring west as a Collector." GE to AG, 4/18/1848, *HL*.

“But alas! it was to be otherwise,” he lamented. Upon his return, he wrote, he found himself being “obliged to borrow money to keep from starving.” He could not help mourning “how little I have gained after all by engaging in this botanical enterprise.”<sup>26</sup>

**“Collecting without a house produces little for one’s purse”**

Lindheimer, knowing that he could no longer receive a stipend from Gray, aspired to figure the best way by which his family could make a living by collecting specimens. He gradually developed his style. He did not agree that profitable collecting required him to travel to the most remote regions and grab as many novelties as possible. “I have no desire to go there [the Rio Grande] yet for botanizing at least if I am to make a living by collecting,” he told Engelmann matter-of-factly. “In two months (without a house) I could only have a yield of 1,500 plants which, with all the deductions taken from that, would surely not bring more than 15 X 8 dollars=\$120. That does not cover the cost of such an expedition.” But if he collected with a house, as what he was doing now, the equation would be altered.

Usually I am home by 11 or 1 o’clock [midday], prepare the plant specimens by preserving and changing them around during the heat of noon, when I can warm the paper in the sun. By 2 or 4 in the afternoon I am through with the plants and rarely get to botanizing in the evening. If I could devote my time to botanizing, from 5 in the morning until dark in the evening, then I [could] preserve, switch around, and put in order 100 to 200 plants daily in good weather such as we are having now. I am now collecting 50 to 100 specimens daily.

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<sup>26</sup> AF to AG, 7/25/1848, *HL*.

“Collecting without a house produces little for one’s purse,” declared the collector.<sup>27</sup>

The result of this collecting-with-a-house style was stunning. “About a week ago I got a letter from Lindheimer,” Engelmann told Gray in August 1846, “telling me that he had packed and was sending away to me several boxes containing 30,000 specimens collected this spring & summer besides many odd specimens single specimens etc.;—and telling me that they were as well preserved as any he ever sent to me.—I expect them every day and long to feast on them!—Lindheimer also has an assistant and apparently a very industrious one, a wife!”<sup>28</sup>

Delighted by his friend’s happy and *useful* marriage, Engelmann still grew worried that Lindheimer would soon exhaust the stock of botanical novelties in New Braunfels and that a peaceful domestic life would prevent Lindheimer from stepping into the vast, scarcely explored Texan and New Mexican floras. So he asked if Gray could contact authorities in Washington, DC, for Lindheimer’s next expedition.

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<sup>27</sup> FL to GE, 7/8/1846, Goyne, *A Life...*, 159-161. Moreover, he told Engelmann, with the house serving as his headquarters, he could produce specimens of capital quality, along with detailed information about the specimens’ habitats, habits, characteristics, and the like, which he believed would be of great use to Engelmann and Gray:

Drummond could take more time in finding his plants. (Oh, how I would like to do that!) The drying of the plants took less of his time; he did not switch or change the position of the plants, or rarely did it, when preserving them, as well as I can determine. Evidently the plants do not look very good, because the colors of these plants are often unrecognizable and because he used tremendous quantities of paper, wagonloads. He also wrote no notes to accompany his plants, not even concerning where he found them, as I can see from T. & G [Torrey and Gray’s *Flora of North America*].

<sup>28</sup> GE to AG, 8/2/1846, *HL*.



Perhaps Gray could make arrangements akin to what Fendler had received during the Santa Fe expedition, for example, free transportation and protection?<sup>29</sup>

Gray indeed had become increasingly dissatisfied with Lindheimer's rather stationary collecting style. He learned from one of Engelmann's letters (dated January 11, 1845) in which Lindheimer had announced his plan to explore San Saba, "where the plants, I hope, have no Latin name."<sup>30</sup> He loved this expression, but did not think that Lindheimer was capable of judging where such no-Latin-name plants existed. In July 1846, when Engelmann cheerfully informed him about Lindheimer's recent progress in plant collecting in New Braunfels, he replied, "It is said that a corps of troops is to be sent up through Texas towards New Spain. Lindheimer ought to go along, and so get high up into the country, where so much is new,—and the plants have really 'no Latin names'."<sup>31</sup> In September, he sent Engelmann a permission issued by the War Department. Now Lindheimer could "go ahead" with General Wool's party, he noted. "I hope he will get it in time to overtake them, and collect on the western frontiers of Texas."<sup>32</sup>

It turned out that Lindheimer never relied on the U.S. Army to advance his collecting enterprise.<sup>33</sup> He still insisted on his collecting-with-a-house style and did

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<sup>29</sup> GE to AG, 8/2/1846, *HL*.

<sup>30</sup> GE to AG, 1/11/1845, *HL*.

<sup>31</sup> AG to GE, 7/15/1846, *AGPG*.

<sup>32</sup> AG to GE, 9/undated/1846, *AGPG*.

<sup>33</sup> "My last letter from Lindheimer is from the beginning of September; the troops were there in San Antonio, but he did not appear to give any notice to accompany them..." GE to AG, 10/18/1846, *HL*.

not think that the Army would furnish him with anything comparable to a house with an industrious wife. What buttressed Lindheimer's further expedition in Texas was those German immigrants who ventured to make the interior of Texas their homeland. Early in 1847, he traveled with a train of German settlers to Fredericksburg and collected there until September. Then he proceeded to the lands between the Llano and San Saba River (the "middle of the best hunting grounds of the mighty Comanches"), staying with his fellow German immigrants, and collected there until fall of 1848, when the colony collapsed due to internal conflicts and threats from Indians.<sup>34</sup> For most of the year 1849, he stayed with his friend in Comanche Spring near San Antonio. He returned to New Braunfels in the fall of 1849, and continued collecting plants in the neighboring areas for the next two years.<sup>35</sup>

**"We must keep ahead of this!"**

Lindheimer was not the only collector who accompanied German immigrants to explore the hinterlands of Texas. In 1845, Carl Ferdinand von Roemer (1818-1891), a well-trained PhD in geology at the University of Berlin, set sail to Texas, with an introduction letter from Alexander von Humboldt. Roemer's expedition was under the auspices of the Berlin Academy of Science, and was commissioned to survey Texan natural history in general and geology in particular. Early in 1846, Roemer

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<sup>34</sup> See FL to GE, 4/18/1845, Goyne, *A Life...*, 113.

<sup>35</sup> Blankinship, *Plantae Lindheimerianae, Part III*, 138.

reached New Braunfels and immediately set out to seek Lindheimer, the plant collector.<sup>36</sup>

Roemer also wrote to Engelmann about his visit to Lindheimer.<sup>37</sup> Engelmann grew worried, however. It seemed to him that with Lindheimer's help, Roemer might have made "more collecting" and therefore "rob" Lindheimer, Gray and himself of "the priority," causing "blunders and confusions" in nomenclature.<sup>38</sup>

Learning of Engelmann's worries, Lindheimer replied that such worries were "off course."<sup>39</sup> But Engelmann was not convinced. "Are you aware," he asked Gray, "that,—[in] Bonn, Germany a society has been formed for the exploration of the nat.

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<sup>36</sup> Roemer recalled his encounter afterward:

At the end of the town, some distance from the last house, half hidden beneath a group of elm and oak trees, stood a hut or little house close to the banks of the Comal. It furnished an idyllic picture with its enclosed garden and general arrangement and position. When I neared this simple, rustic home, I spied a man in front of the entrance busily engaged in splitting wood. Apparently he was used to this kind of work. As far as the thick black beard, covering his face, permitted me to judge, the man was in his early forties. He wore a blue jacket, open at the front, yellow trousers and the coarse shoes customarily worn by farmers in the vicinity. Near him were two beautiful brown spotted bird dogs, and a dark-colored pony was tied to a nearby tree. The description fitted the man I was looking for. His answer to my question, given in a soft, almost timorous voice, which did not seem in harmony with the rough exterior of the man, confirmed my conjecture. It was the botanist, Mr. Ferdinand Lindheimer, of Frankfort-on-the-Main.

Roemer, Ferdinand. *Texas: With Particular Reference to German Immigration and the Physical Appearance of the Country* (San Antonio: Standard Printing Co., 1935), 106-107.

<sup>37</sup> "Just now Dr. Roemer is also writing to you," Lindheimer told Engelmann. FL to GE, [dated "toward the end of February 1846,"] Goyne, *A Life...*, 151.

<sup>38</sup> GE to AG, 3/9/1847, *HL*.

<sup>39</sup> FL to GE, 7/8/1846, Goyne, *A Life...*, 159.

his. of Texas, which will publish a large work... We must keep ahead of this!"<sup>40</sup>

Afterward, he was content to read Gray's reply, asserting that "We will keep ahead of the Bonn people" and, "By the close of next summer (Deo favente) we may hope to have the botany of Texas pretty well in our hands."<sup>41</sup>

But an unexpected incident occurred. Roemer, eager to secure scientific priority for his discoveries in Texas, recruited a number of competent naturalists to help him enumerate his natural history collections. As for botanical collections, he placed them in the hands of Adolph Scheele, pastor-botanist at Heersum. In 1848, Scheele began publishing a catalogue of Roemer's collections in *Linnaea* (until 1852). At that time, neither Gray nor Engelmann were aware of Scheele's publishing project. In May 1849, Gray sent a note to Engelmann in haste: "I send you (mail) 3 fasc. of *Linnaea*—for 1848,—(they belong to Sullivant. Send to him as soon as you are done with them) by which you will see that one Scheele has been getting hold of Lindheimer's yet undistributed coll. of plants, and has been playing the deuce with them. His names must be known. Pray send me your remarks.—Some he has hit well, and anticipated names I have in print."<sup>42</sup>

Engelmann and Gray's failure to secure scientific priority to name and describe the new species in Lindheimer's collections provoked some uneasiness among American botanists. "Did you receive an article on Texas plants—(mostly

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<sup>40</sup> GE to AG, 12/10/1846, *HL*.

<sup>41</sup> AG to GE, 1/5/1847, *AGPG*.

<sup>42</sup> AG to GE, 5/2/1849, *AGPG*.

Lindheimer's) by Scheele?" Torrey asked Gray. "Has this [publication] made any mischief in nomenclature?"<sup>43</sup>

To Gray and Engelmann, Scheele did make much "mischief" in nomenclature. "Now as to Scheele," Gray told Engelmann in November. "I to-day got two more mos.—with more abominable stuff of the same kind,—more Compositae too—all wrong, and fortunately all preoccupied, I believe. Your Euphorbiaceae too."

Annoyed, he claimed that he would send "a list of real names" along with Scheele's catalogue to Diederich Franz Leonhard von Schlechtendal, editor of *Linnaea*. He would ask Schlechtendal to "admit no more of it into *Linnaea*."<sup>44</sup>

Partly because of Scheele's publication, it was not until 1850 that Gray submitted the second part of *Plantae Lindheimerianae* (1850) to the Boston Society of Natural History for publication. In it he desperately waged a war over scientific priority against Scheele. Besides deriding Scheele's diagnosis of species, he aggressively reduced the German botanist's new species to varieties or synonyms of some known species.<sup>45</sup> His general opinion about Scheele's classification was manifested when he reduced Scheele's new species to his and Torrey's *Filaginopsis multicaulis*: "no great reliance can be placed on this writer's [Scheele's] determination."<sup>46</sup>

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<sup>43</sup> JT to AG, 5/23/1849, *HL*.

<sup>44</sup> AG to GE, 11/7/1849, *HL*.

<sup>45</sup> For example, Gray considered Scheele's *Ranunculus macranthus* *R. repens*, *Linn. var. macranthus*. Gray, "Plantae Lindheimerianae, Part II," 141-142.

<sup>46</sup> Gray, "Plantae Lindheimerianae, Part II," 224.

But if Gray found that Scheele had correctly detected a new species, given it a name, and published it in *Linnaea*, he would accept Scheele's priority (and complain that Scheele had "badly" chosen specific characters)—although he himself, Engelmann, and Lindheimer might have named it in correspondence and manuscripts.<sup>47</sup> Here Gray was to follow the framework suggested by *The Rules of Zoological Nomenclature* (1843). According to *The Rules*, only names published in scientific journals or books would be accepted. That is to say, those names appearing in naturalists' correspondence and manuscripts would only be accepted as synonyms, no matter how early naturalists might have coined such names. Gray was not obliged to follow the framework set up by British zoologists in zoological nomenclature, yet hailed it as a standard and put it in practice in American botany, which, in this particular case, meant that a great portion of those botanical labors he, Engelmann, and Lindheimer had invested in exploring the Texan flora harvested nothing scientifically.<sup>48</sup>

The unpleasant experience of dealing with Scheele's "mischief" might have provoked Gray to publish a review of Agassiz's *Nomenclator Zoologicus* (1842-1846) in *The American Journal of Science and Arts*. He translated a specific paragraph in *Nomenclator Zoologicus* to inform readers of a group of people who "deserved" naturalists' "censure." Agassiz had said, he wrote, that "we [naturalists] brand with infamy those impudent parasites who prowl about museums to pick materials for

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<sup>47</sup> Gray, "Plantae Lindheimerianae, Part II," 184, and *Abutilon holosericeum*, on p. 162.

<sup>48</sup> For example, although Gray considered Lindheimer's *Vitis populifolia* a far better name than Scheele's *V. rupestris*, he adopted Scheele's name.

their *opuscula*, without mentioning the sources whence they have derived their spoil, and sometimes even furtively described the species, the names of which they claim." Gray told his readers that he absolutely concurred with Agassiz's "censure." In actuality, he added, "not less blameworthy are those who purposely pass by, instead of courteously adopting, appropriate names under which naturalists often distribute their species in advance of publication." He found such a case a "felony" and "more atrocious" than Agassiz's "impudent parasites," pronounced Gray, "because remediless, and to be prevented by no rule except that of courtesy; for the public good requires that priority should be conceded to actual publication alone."<sup>49</sup>

Gray's strenuous effort to undermine Scheele's taxonomy was not effective. "Scheele has written to me and requests exchange etc. etc.," Engelmann told Gray in March 1850. "I have given him in answer a list of 40 or 50 of his wrong names with corrections!—If that won't deter him... I don't know what will." Two months later, he exclaimed to Gray that Scheele appeared to have determined to carry on the enumeration to the end. He just received the most recent issue of *Linnaea*, he wrote, and was "astonished" to find that Scheele still continued publishing and causing mischief in nomenclature.<sup>50</sup>

With "Plantae Lindheimerianae, Part II" ready, Gray and Engelmann sorted Lindheimer's 1845-1848 collections into 40 sets, each of which contained some 300 species. Then the two botanists tried their best to "drum up" Lindheimer's previous

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<sup>49</sup> Gray, "Nomenclator Zoologicus...", 305-306.

<sup>50</sup> GE to AG, 3/8/1850, *HL*; GE to AG, 5/28/1850, *HL*.

subscribers to purchase these sets (Figure 4-1).<sup>51</sup> But this time Lindheimer's collections were not well received. At the end of 1849, about 10 sets still remained undisposed of.<sup>52</sup> In other words, the whole four-year labor of collecting plants won Lindheimer less than \$720 before deducting necessary expenses.<sup>53</sup>

Engelmann began to think that keeping a collector in the field by the subscription system was a "bad business." After finishing the distribution of Lindheimer's 1845-1848 collections (about 15,000 specimens), he asked Rudolph Friedrich Hohenacker of Esslingen to take care of Lindheimer's collecting enterprise. He informed Gray of the arrangement in March 1850.

Mr. Hohenacker in Esslingen offers at my request to undertake the distribution and sale of Lindheimer's cheap collections but at what conditions!—L. is to pay freight and insurance to Esslingen; He is there to distribute the plants, print the labels and sell the plants, pay all further expenses, and to receive in recompensation one complete set and the third of the excerpts—Is that fair?—But how do better?—I can not go into the distribution of these distributions—have absolutely no time for it.—If Lindheimer can collect 400 species a year in 40 or 50 specimens he may sell them for \$5.00 a hundred, but not make as much as \$500 on them—but that is doing better than he is doing now.—But I shall I think propose him to accept.—He must first get more independent, and then he may undertake larger further expeditions.<sup>54</sup>

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<sup>51</sup> The term "drum up" appeared in a letter from Gray to Engelmann. AG to GE, 2/25/1849, *LAG*, 362.

<sup>52</sup> See GE to AG, 12/21/1849, *HL*.

<sup>53</sup> For example, the best set of Lindheimer's set was sold at the price of \$38.50. See a letter from Engelmann to Gray undated but postmarked 3/10/1850, *HL*.

<sup>54</sup> GE to AG, 3/27/1850, *HL*.





Figure 4-1. *Abutilon holosericeum* Scheele. Lindheimer discovered the species in 1846, but the right to name it got preempted by Scheele. Gray and Engelmann's common friend Charles Short received the specimen of the species at the rate of \$8 per hundred specimens. (Courtesy of the Diatom Herbarium of the Academy of Natural Sciences of Drexel University, Philadelphia, PA, USA.)

**“We must do something for him”**

The turn of the 1840s and the 1850s also marked a turning point in Fendler’s collecting career. Receiving Fendler’s “alas!” letter, Gray rushed to examine what the collector had harvested in Santa Fe. On November 8, 1848, he sent *Plantae Fendlerianae Novi-Mexicanae* (including some 462 species and varieties) to the American Academy of Arts and Sciences to be published in the Academy’s *Memoir*. He understood that his enumeration was far from complete (up to *Compositae*) but felt obliged to have it published first. Beyond the imperative of securing priority for naming and describing new species, he considered *Plantae Fendlerianae Novi-Mexicanae* an “advertisement.” “It is greatly to be wished that he should receive patronage, in the form of additional subscriptions for his collections, which may enable him to reengage in this arduous undertaking under more favorable circumstances than before.”<sup>55</sup>

In November 1848, Gray reported to Engelmann that he probably could advance some \$300 (\$200 from Lowell; \$100 from Nuttall) for Fendler’s expedition to Santa Fe and probably to Salt Lake, as well. Engelmann, after discussing the matter with Fendler, replied that Fendler was wondering if he (Fendler) could use advanced money for an expedition to California. Gray disagreed. “Tell him botanists (the only ones we can rely on to purchase) are well stocked with Californian plants—that I will have nothing to do with his going there as a collector.—If he wants to go as an adventurer merely—he must on his own hook.”<sup>56</sup>

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<sup>55</sup> Gray, “*Plantae Fendlerianae novi-mexicanae*,” 2.

<sup>56</sup> AG to GE, 11/18/1848, *AGPG*; AG to GE, 11/30/1848, *AGPG*.

Gray did bring Fendler's proposal to Lowell's attention, and he found that Lowell "strongly objected to it, and earnestly hoped Fendler would not be seized with the gold fever." For his part, he told Engelmann afterward, he would not try to dissuade Fendler, for undoubtedly Fendler could make much more money by digging gold than collecting weeds. "I am the more sorry the expedition must be given up."<sup>57</sup>

Gray's words worked. Late in March, Engelmann informed Gray that Fendler would soon embark on a botanical expedition to Salt Lake with the Army. "The understanding is that he remains there at least one year, and that he collects plants and sends them here. Everything is right now, I think."<sup>58</sup> Gray replied, "I was glad to learn that Fendler had at last made up his mind for Mormondom and botany." "I trust he is now on his way, under favorable auspices—Mr. Lowell tells me he has paid the draft in his favor for \$200."

What happened next was so fast and unexpected that both Engelmann and Gray could scarcely respond. In May, Engelmann informed Gray that Fendler had left for Salt Lake.<sup>59</sup> In July, he informed Gray that "Fendler had been obliged to give up the expedition," for an unsuccessful attempt to cross the river had cost Fendler's entire outfit.<sup>60</sup> In August, he informed Gray that Fendler had returned to St. Louis and found that the recent Great Fire had devastated all his [Fendler's] properties,

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<sup>57</sup> AG to GE, 3/24/1849, *AGPG*.

<sup>58</sup> GE to AG, 3/28/1849, *HL*.

<sup>59</sup> GE to AG, 5/13/1849, *HL*.

<sup>60</sup> GE to AG, 7/[undated/]1849, *HL*.

including his botanical specimens, “except the plants of Santa Fe now in your hands or on their way to England.”<sup>61</sup>

But Gray was not yet prepared to give up. In August, he told Engelmann that he had persuaded Henry of the Smithsonian to “render efficient aid, by subscribing \$150, or at least 100 to his [Fendler’s] Salt Lake Exped.”<sup>62</sup> Engelmann replied that Fendler would “try some other business” and was now “thoroughly disgusted with losing months and years before getting in action again and dissatisfied also with the slowness of the returns.” “We have deterred him to start and he has not only lost his time but also money or outlay.” “[W]e owe him, that he should get the money for his plants sent from here last April a year!” he cried in another letter.<sup>63</sup>

Engelmann’s sense of guilt was partly lifted at the end of 1849. As he put it in a letter to Gray, “I find that Fendler has received in cash already \$850 for his Santa Fe collections, is sure of 250 more... making the whole proceed of one year campaign for him and assistant \$1100 with the prospect of 200 more.” For that matter, although he and Gray still owed Fendler some money (for they had not completed the enumeration of Fendler’s entire collections and thus could not collect the subscription fee), he thought this amount of remuneration should be sufficient for Fendler’s current needs.<sup>64</sup> In December 1849, Fendler made his way to Panama. He did not do well there and soon returned to St. Louis. In May 1850, Engelmann

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<sup>61</sup> GE to AG, 8/6/1849, *HL*.

<sup>62</sup> AG to GE, 8/28/1849, *AGPG*.

<sup>63</sup> GE to AG, 9/26/1849, *HL*; GE to AG, 11/9/1849, *HL*.

<sup>64</sup> GE to AG, 12/17/1849, *HL*.

informed Gray, “Fendler appears in distressed circumstances and wants me to buy his charges plants from him.—I have declined it, but will advance him some money on them, and then send the whole either to Hohenacker to sell under the same conditions he offered to Lindheimer, or try to do better.”<sup>65</sup> “I hope you will trouble yourself not too much about the money affairs of Lindh. & Fendl.,” he told Gray, and “save your valuable time for your studies.”<sup>66</sup>

### **Cataloging species**

“St. Louis is certainly as we say here, a great city—the centre of North America if not of the world and of civilization!” Engelmann told Gray in July 1849. But could Gray imagine what had recently happened to this great city? The recent Great Fire destroyed one third of the city’s steamboats and one tenth of the “wealth of our citizens,” he cried, while one tenth of the citizens lost their lives to cholera.<sup>67</sup> As a physician, Engelmann found himself on the front lines of these catastrophes. “It is like a dream to me,” he told Gray in another letter, “and I am astonished how I could get through that immense bodily and mental labor and excitement so well.—Hardly a day during that period that I took a regular meal, or a night that I slept 3 or 4 hours in succession.”<sup>68</sup> Still, he resolved not to put his botanical studies and duties aside. It was as if by burying himself into the mechanical work of numbering, sorting, and

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<sup>65</sup> GE to AG, 5/18/1850, *HL*.

<sup>66</sup> GE to AG, 2/6/1851, *HL*.

<sup>67</sup> GE to AG, 7/[undated/]1849, *HL*.

<sup>68</sup> GE to AG, 8/6/1849, *HL*.

labeling specimens he could gain some sense of peacefulness. About four months after sending off his like-living-in-a-dream letter, he cheerfully reported to Gray: “The distribution of Pl. Lindh. in 40 sets was finished to night (16th) 450 in the best sets, 315 in the last... A good deal of time and labor will it yet require to put out in proper paper these 15,000 specimens and pack them!”<sup>69</sup>

Gray also felt himself to be living in a dream, or, put more precisely, a nightmare in which he was chased and cornered by various duties and obligations. He had a demanding teaching schedule at Harvard College, duties in scientific societies, and necessary labors to be devoted to maintaining the Harvard Botanical Garden. In addition, in January 1848, his younger brother George died of typhoid fever, and soon afterward his wife became dangerously ill. Like Engelmann, Gray sometimes wondered if he could survive such mental and physical turmoil. “Two more years like the last would probably destroy me,” he moaned to a correspondent. “I have labored hard since I saw you, but under many discouragements, my wife having been sick all the autumn, and still quite feeble—though convalescent,—so that I have not had the help I anticipated and needed,” he told Joseph Henry of the Smithsonian.<sup>70</sup>

Gray’s and Engelmann’s common friends began worrying that running the subscription system had drained the two botanists’ remaining energy. Sullivant, receiving a copy of *Plantae Fendlerianae* from Gray, replied admiringly, “What a prodigious worker you are.” “[N]o wonder you are growing gray.” Torrey, on the

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<sup>69</sup> GE to AG, 12/17/1849, *HL*.

<sup>70</sup> AG to CW, 1/17/1848, *LAG*, 353-355; AG to JH, 1/1/1849, *PJH*, Vol. 7, 445.

other hand, became worried that Gray and Engelmann had driven each other too hard. “Really I think you are making a perfect slave of yourself,” he told Gray. “The world will go on just as well if you describe a few scores less of new genera & species.”<sup>71</sup>

What motivated the two botanists to exhaust themselves (and each other) to run a subscription system? Here it is worth noting that neither Gray nor Engelmann charged any fee for their labor devoted to sorting out specimens, checking references, diagnosing species, producing manuscripts, making catalogues, packing specimens, and so on. What they acquired as botanists in charge was a complete set of collections without paying any subscription fee. Compared to their subscribers, most of whom were independently wealthy and hardly noticed the expenditure of some \$150 to subscribe to sets of dried plants, they did not have that luxury. For Gray and Engelmann, running a subscription system turned out to be the only and most effective way by which to acquire specimens. With the goal of completing the project of the North American flora hovering before them, the two botanists could only wrack their brains to produce catalogues to ensure that the subscription system under their charge functioned smoothly.

But compiling a catalogue for subscribers and collectors alike was not as easy as it appeared. Insofar as the subscription system was concerned, European botanists (British botanists in particular) of the mid-1840s had come to agree that the catalogue accompanying distributed specimens should be a catalogue of *species* instead of specimens. That is to say, species—and species alone—should be

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<sup>71</sup> WSS to AG, 3/15/1849, *HL*; JT to AG, 1/11, 1850, *HL*.

regarded as the unit that deserved a distribution number marked in the catalogue, and therefore the unit by which to evaluate a given collection's scientific and monetary value and to calculate the subscription fee.

It took some time for this idea to influence American botanical circles. In 1844, when Engelmann took charge of enumerating a collection made by Charles A. Geyer in Illinois and Missouri, what he produced was a catalogue of specimens (including varieties and "hybrids of different species of *Verbena*").<sup>72</sup> The first part of *Plantae Lindheimerianae* was mostly a catalogue of specimens, as well. In it Gray and Engelmann enumerated some 35 varieties and assigned each variety an independent distribution number. But when preparing *Plantae Fendlerianae Novi-Mexicanae* (1849), both Gray and Engelmann decided to follow the European standard and compile a catalogue of species. It was Engelmann who came up with the proposal. In February 1848, busily sorting Fendler's Santa Fe collections, he suggested what he thought was the appropriate format for *Plantae Fendlerianae Novi-Mexicanae* to Gray.

I propose to follow Hooker's plan in the *Plantae Geyerianae*, namely to mention in the printed catalogue every plant collected, whether there are specimens enough for distribution or not. Even the commonest plants are interesting from a geographical point of view, and the rare or new ones ought not be passed by, even if they can not be distributed.<sup>73</sup>

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<sup>72</sup> George Engelmann, "Catalogue of a Collection of Plants Made in Illinois and Missouri, by Charles A. Geyer; with Critical Remarks, &c.," *The American Journal of Science and Arts* 46, no. 1 (1844), 94-104. The quotation is on p. 99.

<sup>73</sup> GE to AG, 2/15/1848, *HL*.



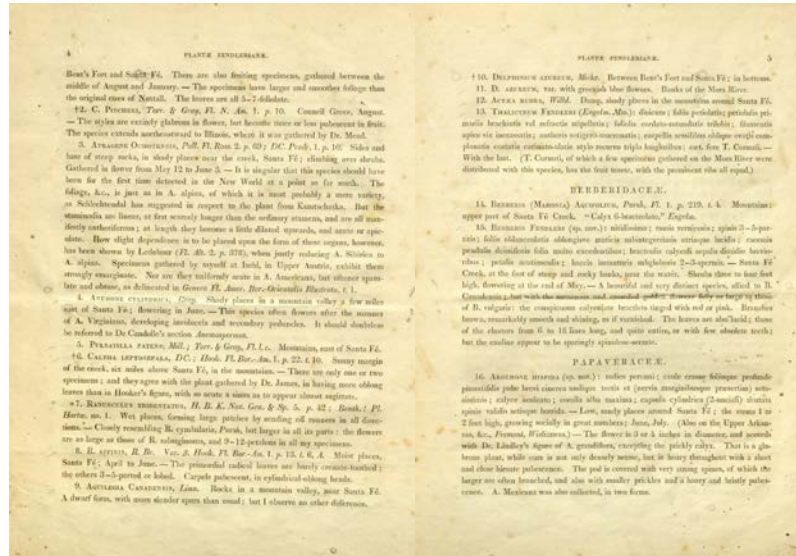


Figure 4-2. *Plantae Fendlerianae Novi-Mexicanae* represented Gray and Engelmann's effort to author a catalogue of species. (Courtesy of the Peter H. Raven Library, Missouri Botanical Garden, Saint Louis, MO, USA.)

"I like the plans you suggest about mentioning all the plants collected, whether enough to distribute or not," Gray replied.<sup>74</sup> The incoming *Plantae Fendlerianae Novi-Mexicanae* would no longer be a catalogue for a particular collection made in certain places during a certain period of time; it would be a synopsis of the flora of the region, with species serving as its essential element (Figure 4-2).

Needless to say, a well-prepared catalogue of species not only satisfied botanists' intellectual interests but also enhanced the value of collections (Figure 4-3). In 1854, for example, when Gray managed to persuade Charles Short to pay \$400 to purchase Jean-Louis Berlandier's Mexican collection, he informed Engelmann how the transaction went. "The specimens are mostly poor and rubbishy; but they are important as being mostly under the numbers that sets of them were distributed (sold) in Europe—which numbers are quoted in D.C. Prodr. and elsewhere—so that

<sup>74</sup> AG to GE, 2/29/1848, AGPG.

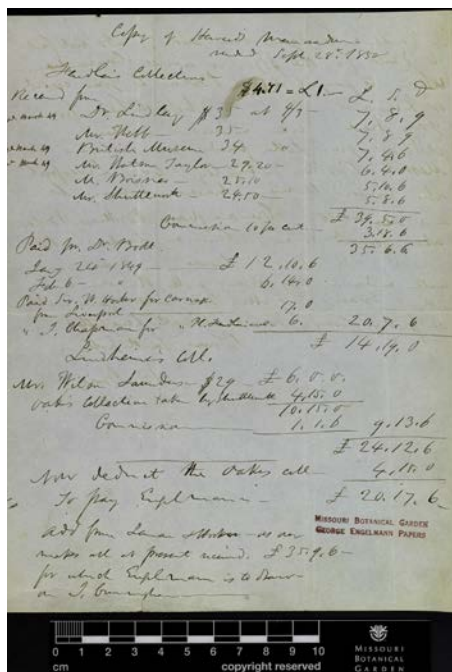


Figure 4-3. In a letter to Engelmann in October 16, 1850 (during which time Gray was studying the Ex. Ex. botanical collections in Europe), Gray reported how much in subscription fees he had collected for Fendler’s and Lindheimer’s collections. (Courtesy of the Peter H. Raven Library, Missouri Botanical Garden, Saint Louis, MO, USA.)

we may identify a great many published species with certainty.”<sup>75</sup> To botanists of the mid-nineteenth century, indeed, a collection without a well-researched and published catalogue of species was but piles of dried *stuff*. No matter how exquisitely a collection was made, it would hardly be worth \$5 per hundred specimens (as Hohenacker’s arrangements with Lindheimer and Fendler show).

But if a catalogue of species played such an important role in the world of botany, what was a species? In the second edition of *The Botanical Text-Book* (1845), published when Gray was occupied with studying and enumerating Lindheimer’s Texan collections, he defined species as follows:

We assemble, under this name, those individuals which we observe or judge to

<sup>75</sup> AG to GE, 8/1/1854, *AGPG*.

have arisen from one parent stock, or which, although met with widely dissociated, resemble each other so closely that we infer them to have had a common parentage. A Species, or particular sort, therefore, embraces all those individuals which, slightly differing perhaps in size, color, or such unimportant respects, resemble each other more nearly than they resemble any other plants, so that we infer them to have sprung from a common original stock, and which preserve their characters unchanged when propagated by seed. All classification and system in natural history rest upon the fundamental idea of the original creation of certain forms, which have naturally been perpetuated unchanged, or with such changes only as we may conceive or prove to have arisen from varying physical influences, accidental circumstances, or from cultivation.<sup>76</sup>

But to define a species was one thing; to *discriminate* one species from others was quite another. Botanists during most of the nineteenth century hardly agreed with each other on where to draw the line between species and varieties. In fact, even Gray and Engelmann, two pillars of the subscription system that connected collectors with subscribers, exhibited almost entirely opposite styles of discriminating species. Gray was on his way to becoming one of the most aggressive lumpers of his day, allying himself with British botanists George Bentham and Joseph D. Hooker.<sup>77</sup> His qualifications to join the prestigious community of lumpers chiefly stemmed from his drastic extension of the collecting networks. A Presbyterian missionary named Henry H. Spalding collected plants for him in Oregon; Charles Wright, a Yale graduate, had for some years constantly sent him

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<sup>76</sup> Gray, *The Botanical Text-Book (2nd Edition)*, 302-303.

<sup>77</sup> A "relentless iconoclast," *The Gardener's Monthly and Horticultural Advertiser* would later call Gray, who audaciously demolished those "new species" reported by authors of local flora. Anonymous, "Obituary: C. R. Overman, of Illinois," *The Gardener's Monthly and Horticultural Advertiser* 7, no. 9 (1865), 271-272. For a notable analysis of the Gray-Bentham-Hooker alliance, see Peter F. Stevens, "J. D. Hooker, George Bentham, Asa Gray and Ferdinand Mueller on Species Limits in Theory and Practice: A Mid-Nineteenth-Century Debate and Its Repercussions," *Historical Records of Australian Science* 11, no. 3 (1996), 345-370.

plants collected from Austin and its vicinity; from Torrey he received specimens made in various governmental expeditions; from Sir William, he could acquire almost every specimen he wanted through exchange. He got hold of ample resources to establish himself as a species lumper.

By contrast, Engelmann was arguably the most hair-splitting discriminator of species of his day. Indeed, in one of Engelmann's earliest writings titled "A Monography of the North American Cuscutineae" (1842), he had exhibited his strong inclination to be a species-splitter. As Gray would later comment, "his [Engelmann's] first monograph was of the genus *Cuscuta* (published in the "American Journal of Science," in 1842), of which when Engelmann took it up we were supposed to have only one indigenous species, and that not peculiar to the United States, but which he immediately brought up to fourteen species without going west of the Mississippi valley."<sup>78</sup>

Engelmann's "Monography" astonished American botanists of the day. "Did Short tell you of Engelman's [*sic*] discoveries, in what Americans have hitherto called *Cuscuta Americana*?" Curtis asked Gray. "He has found two distinct genera & 5 species!! Rafinesquism I fear." Soon afterward, he exclaimed to Gray that he just received Engelmann's announcement of 7 new species of *Cuscuta*. "American botanists will be stumped if he proves his point; & I have no means of gainsaying

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<sup>78</sup> George Engelmann, "A Monography of the North American Cuscutineae," *The American Journal of Science and Arts* 43, no. 2 (1842), 333-345; Asa Gray, "Memorials of George Engelmann and of Oswald Heer," *The American Journal of Science (3<sup>rd</sup> Series)* 28, no. 163 (1884), 64.

him.” Curtis admitted that he was “rather incredulous.” “I mean to examine such specimens as I can find this season, & see how many species I can detect.”<sup>79</sup>

Soon Curtis communicated his doubts to Engelmann. “N. A. Botany is to be greatly indebted to your researches,” he wrote. “If there be anything to fear from you, it is that you may discriminate too nicely, & make too many species.” Gray did not agree with Engelmann’s diagnosis of *Cuscuta* either. In the face of Engelmann’s enthusiasm to seek Gray’s approval, he replied that “the new discoveries you speak of, rather go to show that the characters employed will not be stable enough, nor marked by habit or geographical distribution to divide *Cuscuta*, L. into genera. I confessed I should rather see them arranged as strong subgenera.” “He is very keen—& a capital discriminator of difference,” Torrey commented on Engelmann’s classification to Gray, “though he is apt to make too many species.”<sup>80</sup>

Engelmann’s reputation as a splitter was made internationally known when he published a report based on a collection made by Frederick Adolph Wislizenus in an expedition to northern Mexico (1846-1847). In it he described some 47 new species and 3 new genera.<sup>81</sup> Sir William was concerned. In a notice published in the *London Journal of Botany*, he commented that Engelmann’s want of “large herbaria and public libraries” might have “occasioned some plants to be described as novelties

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<sup>79</sup> MAC to AG, 2/5/1842, *HL*; MAC to AG, 4/25/1842, *HL*.

<sup>80</sup> MAC to GE, 1/28/1847, Folder 2, Box 1, *CMAC*; AG to GE, 1/5/1847, *AGPG*; JT to AG, 6/18/1850, *HL*.

<sup>81</sup> Engelmann’s report can be found in Frederick Adolph Wislizenus, *Memoir of a Tour to Northern Mexico: Connected with Col. Doniphan’s Expedition, in 1846 and 1847; with a Scientific Appendix and Three Maps* (Washington: Tippin & Streeper, 1848).

which have elsewhere appeared in European works; and this, we suspect, is especially the case with the *Cacteeae*, of which a very great number of species are stated to be new.”<sup>82</sup>

Engelmann understood well that his want of large herbaria and libraries would influence his diagnosis of species. He respected Gray’s and other lumpers’ classifications greatly, but he would not be blinded by their authority. In a letter to Gray in October 1845, he protested Gray’s reducing of his new species into varieties: “I have paid more attention to the species and think I know now 6 forms... Now: are these differences in size of nuts in size & shape of wings and color and length and condition naked or rough of any importance, if we can not find some other characteristics.—A thing of some importance in those times, where nativism becomes so important!” In another letter, he challenged Gray’s diagnosis of the genus *Sagittaria* as shown in *A Manual* (1848). “[Y]ou go too far in describing all the northern plants [of *Sagittaria*] as belongs to varieties!” he exclaimed. “Would not some of your north botanists examine all your *Sagittaria* in a living state and with ripe fruit? I should like to compare specimens or exchange.” Curtis, receiving a letter from Engelmann packed with complaints about Gray’s classification, replied that he presumed that Gray always manifested “a temptation to lump species in troublesome Genera.”<sup>83</sup>

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<sup>82</sup> W. Hooker, *The London Journal of Botany...*, Vol. VII, 391.

<sup>83</sup> GE to AG, 10/6/1845, *HL*; GE to AG, 8/22/1848, *HL*; MAC to GE, 10/25/1848, Folder 2, Box 1, *CMAC*.

For Engelmann, his “temptation” when he encountered troublesome genera or species was to consult his field collectors. Take his correspondence with Lindheimer, for example. First, he insisted that every specimen from Lindheimer be clearly labeled. Then, realizing that he and Lindheimer were on the same page (or communicated with each other according to the same system of reference), he packed his letters to Lindheimer with inquiries about plants’ habitats, living states, colors of flowers, tastes of fruits, textures of leaves, and so on. He consulted Lindheimer’s opinion when he needed to decide whether or not a specimen represented an entirely new species; he relied on Lindheimer’s observation to characterize species; he treated Lindheimer as an equal and respected Lindheimer’s judgment as much as Gray’s.

Lindheimer was always touched by Engelmann’s stream of inquiries. “I simply cannot thank you enough for your good counting and list-keeping in connection with the plants I have found,” he once told Engelmann.<sup>84</sup> He himself was also a methodical note-taker, record-keeper, and letter-writer and had been amazed by the natural diversity and harmony he daily experienced in the field.<sup>85</sup> He packed his

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<sup>84</sup> FL to GE, 10/9/1846, Goyne, *A Life...*, 165.

<sup>85</sup> In a letter to Engelmann, Lindheimer described how he put his collections in order.

My plants that I collected this summer are, to be sure, somewhat in order. The species are not lying about intermingled, but often are in from 2 to 4 different fascicles separated from one another. For lack of space I must filter my entire collection through the system in order to put all individual ones of the separate families in storage, so first I always have to put the species of ten families on one pile, so that the tenth, the twentieth, the thirtieth family can be put together. Then I separate the different families, etc. (FL to GE, 10/9/1846, Goyne, *A Life...*, 171-172)

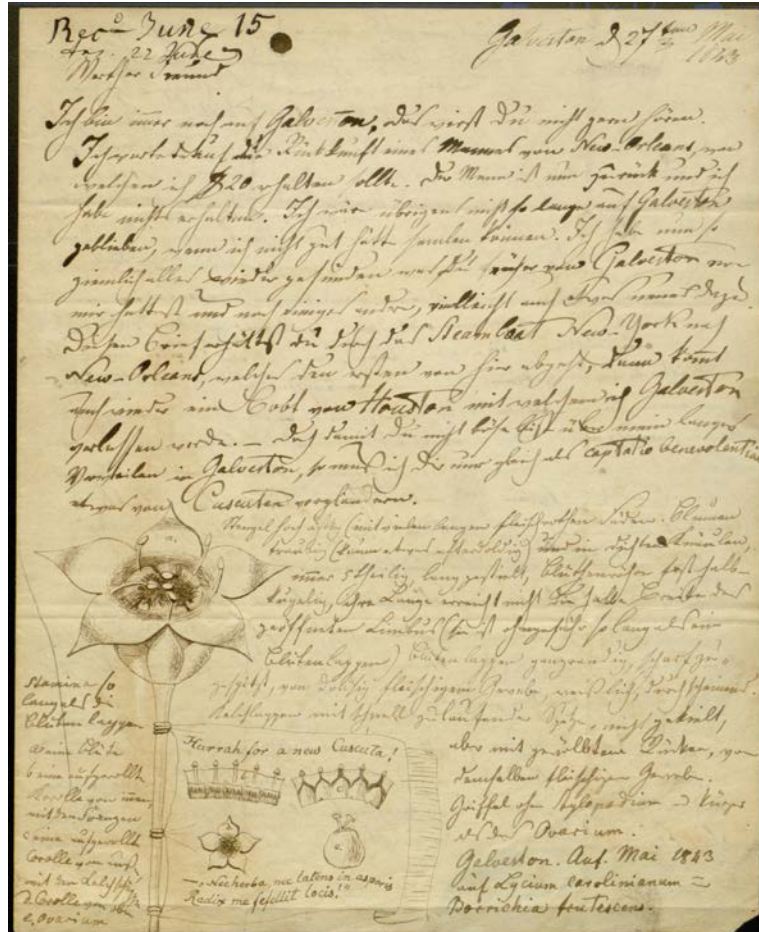


Figure 4-4. In a letter to Engelmann in May 1843, Lindheimer proudly wrote, “Hurrah for a new Cuscuta!” “Nec herba, nec latens in asperis Radix me fefellit locis (Neither a plant nor a root lurking in rough places escaped my notice!)” (See Goyne, *A Life...*, 61. Courtesy of the Peter H. Raven Library, Missouri Botanical Garden, Saint Louis, MO, USA.)

letters to Engelmann with lengthy and juicy descriptions of plants that Engelmann could hardly extract from dried specimens (Figure 4-4). For example, when sending Engelmann specimens of a plant with a local name “mountain grape,” he offered the following description:

A grape plant that grows perpendicular and up to a height of 2’ to 3’ has smooth [a sketch was drawn here], thin, almost kidney-shaped leaves and no vine, with clusters long as a finger made up of 10 to 20 grapes a little thicker than a little yellow pea (the best-tasting grape I have yet encountered in America) It covers



the dry bed of the Cibolo, which consists of chalk boulders, as far as the eye can see and is called *mountain grape* here.<sup>86</sup>

With collectors' field observations in hand, Engelmann produced hundreds of pages of manuscripts for Gray's reference. He urged Gray to treat collectors' field observations seriously ("You will find Fendler's account of the habitat etc. pretty full"<sup>87</sup>) and did not hesitate to express his disappointment if Gray did not pay sufficient attention to the issue of locality. He wanted Gray to understand that lumping was not the only way by which to deal with troublesome genera and species.

Engelmann's methodically compiled and sufficiently digested field information did play an increasingly important role in Gray's classification. To be sure, in the early stages of Gray's collaboration with Engelmann, when he received Engelmann's manuscripts, his first step was always to reduce Engelmann's new species into varieties or to demolish them as synonyms of known species.<sup>88</sup> But as his

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<sup>86</sup> FL to GE, 7/[undated/]1845, Goyne, *A Life...*, 116.

<sup>87</sup> GE to AG, 2/15/1848, *HL*.

<sup>88</sup> For example, in his manuscript sent to Gray in February 1844, Engelmann remarked on a new species that he thought appeared in Lindheimer's collections:

261 *Helianthus asperrimus* nsp.

Covering large patches of the prairies between the Brazos & Colorado, October.—Near *H. giganteus* Lin but easily distinguished by its great roughness low growth, single stem etc... Perhaps too near *H. maximilianii* Schrad. but distinguished by its low simple stem, more canescent, broader and less squanose scale of the involucre—The simple stems rise from a thickened rhizome 2, 3, or 3½ feet high; in the flowering plant they are naked below, with larger rounded leaves about the middle and smaller and fewer ones towards the top; leaves contracted at base, but not petioled, in all specimens conduplicate, and covered backward, mostly entire sometimes slightly denticulate, very rough but more so

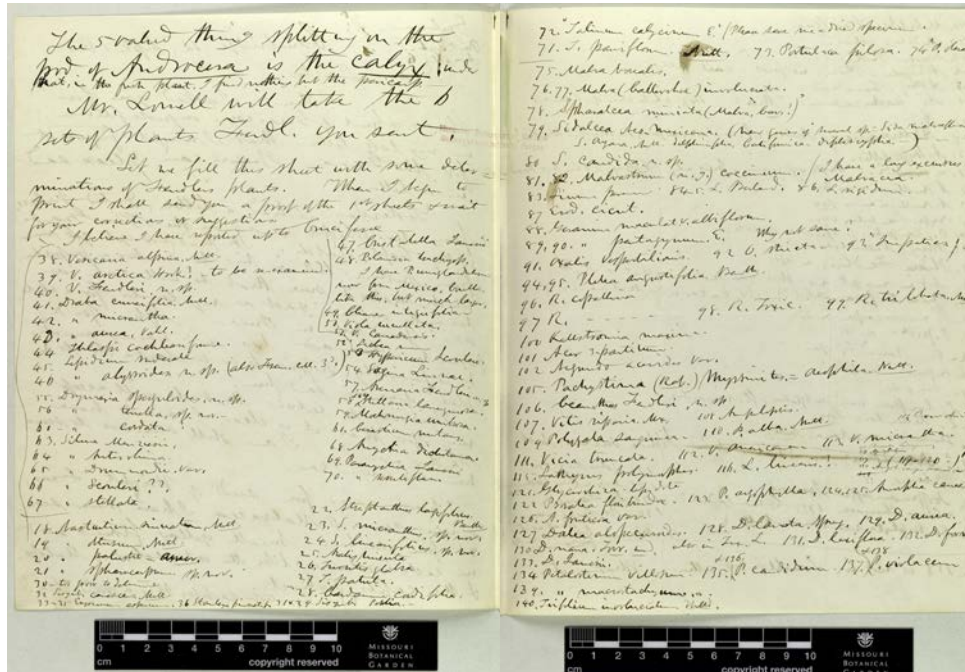


Figure 4-5. In a letter dated September 12, 1848, Gray noted, “Let me fill this sheet with some determinations of Fendler’s Plants. When I begin to print I shall send you a proof of the 1st sheets & wait for your corrections or suggestions.” (Courtesy of the Peter H. Raven Library, Missouri Botanical Garden, Saint Louis, MO, USA.)

on the upper than on the lower surface. Heads from one to five, the terminal one with a long (2 to 4 inches), the lateral ones with very short (1/2 to 1 inches) peduncles. Heads 2 to 2½ inches diameter; involucral scales linear-lanceolate frequently with setaceously. (GE to AG, 2/17/1844, HL)

Then, in Gray’s hand, Engelmann’s above description was reduced as follows:

261 *Helianthus maximiliani* β *asperrimus*

A variety of the last [*H. maximiliani*], as we take it to be, with a simple stem, two to three and a half feet high, bearing solitary or few heads. Prairies between the Brazos and the Colorado, forming large patches. October. (Engelmann and Gray, *Plantae Lindheimerianae, Part I*, 41)

Sometimes Gray’s eagerness to lump Engelmann’s new species could run so high that he did not even give it a second thought. As he put it when combining some of Engelmann’s new species with the known *Opuntia fragilis*, “Though unable to institute a proper comparison, I have little doubt that [Engelmann’s new species] is *O. fragilis* of Nuttall, attaining a fuller growth in that warm region than on the Missouri.” Engelmann and Gray, *Plantae Lindheimerianae, Part I*, 37.

correspondence with Engelmann intensified, and as Lindheimer and Fendler brought back a striking number of novelties, he became more and more dependent on his two field collaborators' knowledge to make judgments on specific distinction (Figure 4-5). For instance, when describing a species named "Negundo aceroides, Maench, var. ?" he nearly copied out the paragraph in Fendler's field note: "The lower part of the trunk generally very knotty, which seemed to arise from the many wounds the tree receives early in spring, in order to draw the sap from it, which is collected in cavities cut into the trunk a little beneath the wounded places."<sup>89</sup> He even found that he could rely on collector's field notes to solve specific botanical questions. For example, for a while botanists had been debating the color of the flower of *Lathyrus polymorphus*, and Gray found the answer in Fendler's field note: "The showy flowers are an inch in length, and deep blue in the dried specimens. Mr. Fendler, however, states, in his notes, that they are of different colors, but generally of a deep crimson, which turns into deep blue in drying."<sup>90</sup>

Gray's appreciation of Engelmann and Lindheimer's field-based classification was made more manifest in *Plantae Lindheimerianae, Part II* (1850). For example, when diagnosing *Algarobia glandulosa*, he copied the entire paragraph from Lindheimer's field note: "Trees from 30 to 40 feet high, with few and large, erect branches; the trunk often from one to two and a half feet in diameter; the heart-

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<sup>89</sup> Gray, "Plantae Fendlerianae Novi-Mexicanae," 29; Shaw, *Augustus Fendler's Collection List*, 22.

<sup>90</sup> Gray, "Plantae Fendlerianae Novi-Mexicanae," 30; Shaw, *Augustus Fendler's Collection List*, 23.



Figure 4-6. One of the maps that Engelmann prepared in coming up with his account of the “botanico-geographical distribution.” (Courtesy of the Peter H. Raven Library, Missouri Botanical Garden, Saint Louis, MO, USA.)

wood dark reddish brown; but often occurring as a small tree or shrubs. Important as furnishing the only firewood in Western Texas; also for its edible fruit.

*Lindheimer*.”<sup>91</sup> Also, for a new species that Lindheimer had suggested be named “*Prunus tawakonia*” (Gray did not adopt this name but accepted Scheele’s “*Prunus rivulari*”), he remarked that the species bore fruit “in about the size of a cherry” and “acidulated,” and “Tawakony Indians boil them and eat them with honey.”<sup>92</sup> More importantly, he relied heavily on Engelmann’s and Lindheimer’s field knowledge to undermine Scheele’s classification. For example, when combing Scheele’s *Rhus verrucosa* into *Rhus toxicodendron* Linn, he remarked that what Scheele had

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<sup>91</sup> Gray, “*Plantae Lindheimerianae*, Part II,” 181.

<sup>92</sup> Gray, “*Plantae Lindheimerianae*, Part II,” 186.

considered the most significant specific characteristic was “merely exudations of resinous juice caused by the puncture of insects on some leaves only, as Dr. Engelmann has pointed out.”<sup>93</sup>

Interestingly, Gray was by no means the endpoint of this stream of information originating from the field. Gray’s diagnosis of species then traveled back to Engelmann to prepare an account of “botanico-geographical distribution” (Figure 4-6). As Engelmann wrote in a letter to Gray in March 1847,

My business now will be to write out a geographical distribution for the *Plantae Lindh. 1845 & 1846.*—then I shall wait for your answer in definitely arranging the numbers for them... I shall then print the labels and distribute the specimens into sets. While I do this, every specimen will pass through my hands and I shall have occasion to make many observations, which I shall write down for your use. In the meantime you will have prepared an account of the whole, description of the new species, remarks etc. to the old ones and a notice of every one with locality and month upon collected, so that it may be copied on the label.<sup>94</sup>

Engelmann had attempted such an analysis when he was helping Wislizenus enumerate plants collected on an expedition from St. Louis through Santa Fe to Chihuahua. Here it is worth noting that Engelmann got involved in Wislizenus’s expedition when American society was eager to look for peculiarities by which to identify a boundary line between the United States and Mexico (a concern that had triggered the U.S-Mexican War and would lead to the undertaking of the U.S.-Mexican Boundary Survey). In his report, Wislizenus offered his ideas on how a

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<sup>93</sup> Gray, “*Plantae Lindheimerianae*, Part II,” 159.

<sup>94</sup> GE to AG, 3/23/1847, *HL*. The term “botanico-geographical distribution” can be found in GE to AG, 3/9/1847, *HL*.

boundary line should be drawn, which inspired Engelmann.<sup>95</sup> With Wislizenus's specimens and maps in hand, he noticed what he would later call the "peculiarity" of the Texan flora, in particular the flora of the Rio Grande valley. "Indeed, the flora of the valley of the Rio Grande connects the United States, the Californian, the Mexican, and the Texan floras, including species or genera, or families, peculiar to each of these countries."<sup>96</sup>

Gray's diagnosis of Texan species—based on intensive and extensive studies at the herbarium and library—offered Engelmann a solid basis to expose his "botanico-geographical" analysis with confidence. He found that he eventually could confirm what he had tried to characterize in Wislizenus's report: "The flora of the Rio Grande connects the North American with the Mexican flora, and has also many peculiar plants of its own."<sup>97</sup> In the history of American botany, Engelmann's analysis represented American botanists' earliest attempts to figure out the U.S.-Mexican boundary from a botanical point of view. It remained authoritative for the next ten years or so, until the U.S. Boundary Commission published a volume titled "Botany of the Boundary" in 1860 (see Chapters 5 and 6).

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<sup>95</sup> "Let us suppose, for instance, that from the mouth of the Rio Grande a boundary line should be drawn up to Laredo, the headpoint of steam navigation on the Rio Grande, and in the latitude of Laredo a line from thence west to the gulf of California, that territory would embrace, besides the old province of Texas, a small portion of the States of Tamaulipas and Coahuila, the greatest part of the State of Chihuahua, the State of Sonora, New Mexico, and both Californias." Wislizenus, *Memoir...*, 84.

<sup>96</sup> See Wislizenus, *Memoir...*, 87.

<sup>97</sup> Engelmann's analysis can be found in Gray, "Plantae Lindheimerianae, Part II," 238-239.

For Gray, Engelmann was one of the best and most erudite sounding boards for his classification. Without Engelmann and his methodical documentation of field information, Gray had little doubt that he would encounter a multitude of difficulties in determining which groups of individuals should be considered species instead of varieties. More importantly, it was Engelmann who consistently reminded Gray of the fine line between species lumping and shoddy species diagnosis. As time went by, he grew increasingly appreciative of Engelmann's field-based classification. In 1854, when commenting on Joseph D. Hooker's *Flora Novae-Zelandiae*, he warned the young Hooker not to underestimate those botanists with incomplete libraries and herbaria:

It is hardly possible that a vast series of apparently confluent forms should receive the detailed examination which the less privileged botanist may concentrate upon his fewer materials; and much is left to the quick, almost intuitive judgment, which is liable to error, indeed, but in which the true genius of a botanist is generally disclosed.<sup>98</sup>

"Nothing escaped his attention," he would comment on Engelmann's taxonomic style afterward. "[H]e drew with facility; and he methodically secured his observations by notes and sketches, available for his own after-use and for that of his correspondents."<sup>99</sup>

### **A metropolitan botanist**

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<sup>98</sup> Asa Gray, "Introductory Essay, in Dr. Hooker's *Flora of New Zealand: Vol. I*," *The American Journal of Science and Arts (2nd Series)* 17, no. 50 (1854), 252.

<sup>99</sup> Gray, "Memorials...," 64-65.

Still, it is worth noting that despite Gray's growing appreciation of Engelmann as a colleague, Engelmann's role in running the subscription system grew minor and marginal. In "Plantae Lindheimerianae, Part II" (1850), for example, Engelmann's role was reduced to a source of information instead of a botanical author—despite Engelmann's heroic efforts in compiling information and producing manuscripts for Gray's reference. In the age when "species" was highlighted as a universal means of transaction in the world of botany, it was Gray—not Engelmann—who defined, refined, and produced species as if he were a mint creating currency. Gray was able to do so because he had connections and was extraordinarily well equipped to produce well-researched catalogues of species. Gray was a "metropolitan botanist," in close touch with great botanists and catalogue-producers like Sir William Hooker, George Bentham, and Augustin-Pyramus de Candolle, but in stark contrast to a field botanist like George Engelmann.

Furthermore, in the late 1840s, due to the influential *Rules of Zoological Nomenclature* devised by the BAAS, American naturalists at large had become less and less interested in debating the essence or philosophical meanings of the term "species." Rather, what concerned them was what competent naturalists and institutions said about species, unambiguously, in a published and accessible form such as catalogues of species and botanical manuals. This attitude was eloquently demonstrated in a letter sent by Carey to Gray regarding his (Carey's) research of *Carex* included in *A Manual*:

As to my doings in *Carex*—If any body questions you on the subject, refer them to me—& I have an answer stereotyped—"What I wrote I certainly thought at





Figure 4-7. William Hooker in 1863 (left) and Augustin-Pyramus de Candolle (right) (Curtsey of the Archives of the Gray Herbarium, Harvard University, Cambridge, MA, USA [left] and the Wellcome Library, London, UK [right].)

the time of writing; whether, if I were now reviewing the ground, I should alter my opinion in my points, I cannot say; but in the meantime, what the Book says, in reference to any & every species, I say, be it right or wrong, till I give equal publicity to a change of opinion”—If any philosopher will insist upon sending me specimens & quiddity to resolve—I shall esteem it a personal insult, & demand satisfaction, or light my pipe with their missives—There is the book. Why should I “eat their dirt”?<sup>100</sup>

In the midst of a heated debate about how to define “species,” there was “the Book,” metropolitan botanists said. Let philosophers “eat their dirt.”

However, being a metropolitan botanist serving as the link between collectors and subscribers required more than large libraries, herbaria, and social networks. In the late 1840s and the early 1850s, when running the subscription system had given rise to such tricky issues as scientific priority, tensions between species lumpers and splitters, and so on, Gray had two role models to guide his actions: Sir William

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<sup>100</sup> JC to AG, 11/17/1847, *HL*.

Hooker and Augustin-Pyramus de Candolle (Figure 4-7). For American botanists of the mid-nineteenth century, Hooker's unparalleled capability to recruit "private collectors" to join his subscription system was legendary. Through Hooker's voluminous publications on the basis of collectors' botanical labor, botanists could gain a clear sense that no matter how much attention this distinguished botanist had paid to plan the expedition, advise the collector, distribute collections, and enumerate species, he did not charge any fee either from subscribers or from collectors. Here it is useful to note that Hooker was by no means rich. As a professor in botany, a relatively low position by Victorian standards, Hooker in the 1830s was always in search of patronage by which to make a decent living. At times, in fact, Professor Hooker had to stand beside the door of the classroom to collect students' attendance fees.<sup>101</sup> But insofar as the running of the subscription system was concerned, Hooker never thought to get paid for his labor. As a nexus that connected collectors with subscribers, he had to prove his trustworthiness and disinterestedness. He could not have behaved as an agent who sold specimens on behalf of the collector and charged a commission. Nor could he have acted as a specimen dealer, who, as often the case, exhibited strong tendencies to multiply the number of the species in their collections to extract more money from buyers (and thus became known as "species mongers"). A botanist in charge of a subscription system ought to be a trustworthy gentleman of science. In spite of (and probably because of) his secular (earthy even) role in running the subscription system, he

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<sup>101</sup> Endersby, *Imperial Nature*, 11. For a thorough discussion about Hooker's relatively low status as a professor in Britain's "gentlemanly science," see pp. 8-12 and Endersby, "A Life More Ordinary."

ought to envelop himself in an aura of disinterestedness by distancing himself from specimen dealers and species mongers. “The single-mindedness with which he gave himself to his scientific work, and the conscientiousness with which he lived *for* science while he lived *by* it, were above praise,” Gray would comment in Sir William’s obituary in 1866.<sup>102</sup>

Gray’s appreciation for de Candolle’s “conscientiousness” was made explicit in a book review (and an obituary, as well) for the Swiss botanist’s posthumous *Mémoires et souvenir* (1862):

To De Candolle’s credit it must be said, not only that his career was remarkably free from controversies about priority and reclamations, but that his example and precepts, his scrupulous care to render due credit to every contributor, his respect for unpublished names communicated to his own or recorded in other herbaria, and the like, have been most influential in establishing both the law and the ethics which prevail in systematic botany (more fully, or from an earlier period than in the other departments of natural history), and which have secured such general coöperation and harmonious relations among its votaries.<sup>103</sup>

As he grew more deeply involved in the subscription system, Gray followed “the law and the ethics” exemplified by Sir William and de Candolle. When enumerating and publishing collectors’ or correspondents’ collections, for example, he always used quotation marks and italics to emphasize his collectors’ and collaborators’ contributions. He was not reluctant to give credit to those enthusiasts working in the field—though he was adamant about retaining his prestige and responsibility as

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<sup>102</sup> Asa Gray, “Sir William Jackson Hooker,” *The American Journal of Science and Arts (2nd Series)* 41, no. 121 (1866), 9.

<sup>103</sup> Asa Gray, “Mémoires et Souvenirs de Augustin-Pyramus de Candolle, Ecrits par Lui-même et Publiés par Son Fils,” *The American Journal of Science and Arts (2nd Series)* 35, no. 103 (1863), 6.

botanical author. Running the subscription system helped Gray rapidly amass “social capital,” secure his reputation as a respected and respectable botanical author, and jump from college professor in the United States to metropolitan botanist. Once his status in the world of botany was fairly secure, Gray had become extraordinary “rich,” getting hold of many *complete* sets of specimens. After all, botanists of the day were living in a world where “species” of plants (not specimens) were widely regarded as currency. Engelmann, for example, paid Lowell for a copy of Gray’s *Genera Florae Americae Boreali-Orientalis Illustrata* by giving him a collection containing 210 species. Fendler, upon learning of this particular rule, asked Gray to send him natural-history books and other facilities. Fendler told Gray that he should be able to make payment using his (Fendler’s) collections then at Gray’s disposal.<sup>104</sup>

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But if running a subscription system helped Gray achieve a status comparable to Sir William Hooker and Augustin-Pyramus de Candolle in botany, what would he do when that system proved to be a “bad business” before such a formidable project as the “complete flora of North America”? In this regard, Gray might have recalled the frequent complaint of Sir William when his subscription system failed and losses for subscribers seemed inevitable: “We regret it, because, as our Government does not patronize and support, like those of the continental nations, similar expeditions in

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<sup>104</sup> GE to AG, 9/1/1846, *HL*; AF to AG, 10/23/1853, *HL*.

behalf of Natural History, it is quite certain that nothing can be done, but through the aid of private individuals, in furtherance of so good a cause.”<sup>105</sup>

The British government must have heard the botanist’s muttering. In 1841, Sir William was appointed as the director of Royal Botanical Gardens at Kew. For Gray, and for many botanists of the day, what happened after Sir William’s appointment was stunning. The mid-1840s witnessed the British botanist eagerly send botanical collectors to join governmental expeditions, transforming the British Empire’s colonial system into a Kew Garden collecting network. He edited numerous botanical magazines and “companions” under such compelling subtitles as *“Containing Figures and Descriptions of Such Plants as Recommend Themselves by Their Novelty, Rarity, History, or Uses; Together with Botanical Notices and Information, and Occasional Memoirs of Eminent Botanists.”* Sir William’s overarching goal was to make botany useful to the Empire and vice versa. Kew Gardens under Sir William’s directorship and Sir William under the government’s patronage made up a steam engine that drove British botany forward with astonishing speed.<sup>106</sup> Sir William was proud of what he had done for British botany

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<sup>105</sup> Hooker made this remark in January 1837, upon hearing of Robert H. Schomburgk’s failure to collect and make qualified specimens in Essequibo. See W. Hooker, *Companion... Vol. II*, 185.

<sup>106</sup> But it should be noted that Sir William soon realized that employed collectors and government-funded expeditions were by no means effective and economic ways of procuring specimens. In a letter to the British government, he explained what he had observed and offered an alternative: “In conclusion I may observe that in consequence of the great number of intelligent & scientific men now resident abroad, we shall find it, as a general principle, more economical to procure plants by correspondence with such persons than to send out special Collectors” (quoted from Endersby, *Imperial Nature*, 87). For a detailed analysis about how Sir William’s

and thought that he knew the secret to his success. As he put it in a letter to Gray, Gray had to “show the commercial value of Botany & to make Botany popular.”<sup>107</sup>

Perhaps no botanists on the American side of the Atlantic admired Sir William’s achievements more than did Gray.<sup>108</sup> In 1842, when Gray took charge of the Harvard Botanical Garden, the Garden was about the same size as Royal Botanical Gardens at Kew (the former was seven acres and the latter eight acres). Five years later, when Sir William had made use of governmental support to make Kew Gardens a truly cosmopolitan botanical institute, Gray found that he oftentimes still had to *beg* for money from wealthy Bostonians to keep collectors in some remote corners of the United States of America. The government’s patronage indeed mattered. The question that remained was how a Harvard professor could secure the government’s patronage to advance such a national project as a flora of North America.

Another equally important question for Gray was how to secure a collector with

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method “to procure plants by correspondence” worked, particularly in the hands of Joseph D. Hooker, Sir William’s son, see Endersby, *Imperial Nature*, Chapter 3.

<sup>107</sup> WJH to AG, 7/7/1853, *HL*.

<sup>108</sup> In 1866, when he composed an obituary for Sir William Hooker, Gray would write: “Our survey of what Sir William Hooker did for science would be incomplete indeed, if it were confined to his published works—numerous and important as they are—and the wise and efficient administration through which, in a short space of twenty-four years, a Queen’s flower and kitchen-garden and pleasure-grounds have been transformed into an imperial botanical establishment of unrivaled interest and value. Account should be taken of the spirit in which he worked, of the researches and explorations he promoted, of the aid and encouragement he extended to his fellow-laborers, especially to young and rising botanists, and of the means and appliances he gathered for their use no less than for his own.” See Gray, “Sir William Jackson Hooker,” 5.

the “right” character. His collaborations with Lindheimer and Fendler had taught him an important lesson: that is, for his ambition to author a complete flora of North America, he needed a collector with Lindheimer’s powers of observation and Fendler’s skill at making excellent specimens, but without the two collectors’ reservations about thrusting themselves into the totally unknown wilderness. This collector, he envisioned, ought to be bold, entrepreneurial, and independent, so that he could not only make a living but also make money by collecting plants—in short, he ought to be a professional collector.

## CHAPTER FIVE

### “Charles Wright of Texas”

The year 1848 had proven a productive year for Asa Gray. Since his arrival at Harvard as a natural history professor, he had published *A Manual of the Botany of the Northern United States* and *Genera Florae Americae Boreali-Orientalis Illustrata*, along with numerous journal articles and book reviews, making himself the most bookish botanist in the United States. Gray’s botanical friends, while admiring Gray’s dedication to disseminating botanical knowledge to academics and the public alike, became worried about Gray’s postponing or even forgetting the project that would truly contribute to American botany: a complete North American flora organized by the natural system. “I am glad to see that both your northern flora [Gray’s *A Manual*] and textbook are in our bookstores and are bought,” Engelmann told Gray in April 1850. “Don’t you forget the flora of N. America!!” he exclaimed. “Too many irons in the fire, eh?” Curtis also teased Gray.<sup>1</sup>

Unsurprisingly, as the United States of America grew into a country that embraced extremely varied floras, Torrey and Gray’s *A Flora of North America*—published between 1838 and 1843—rapidly became outdated. Gray’s colleagues, witnessing how Torrey and Gray struggled to push the project forward, began wondering if they had placed too much on the two botanists’ shoulders. In a letter to Joseph Henry of Princeton College in 1838, Torrey wrote,

You must get the college to subscribe to our Flora. We cannot give away copies.

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<sup>1</sup> GE to AG, 4/16/1850, *HL*; MAC to AG, 5/27/1847, *HL*.



Our bills on Part I. alone amount to more than \$350. We pay as we go, and if any thing is to be made we will have it ourselves.—At any rate no one shall suffer loss by us. If botanists and literary institutions don't patronize the work, we cannot go on.—<sup>2</sup>

Who could patronize a work as immense and invaluable as the flora of North America? Should such a *national* project as the flora of North America rely on some government institutions instead of individual botanists?

Increase Allen Lapham, a dedicated naturalist in Milwaukee, Wisconsin, was among those who gave serious thought to this option. In July 1849, he communicated with Charles Short about the subject. "Can there not be some way desired to effect the completion of Torrey and Gray's Flora?" he asked. As far as he knew, he wrote that the cause of the delay was "the limited sale of the work and consequent lack of funds to pay the printer." He then told Short that he had known a long list of works commenced but never completed "for want of patronage," and he hoped that *A Flora of North America* would not be added to the list. Taken altogether, Lapham thought that the newly established Smithsonian Institution should "take up the work and complete its publication."

A recommendation signed by the principal botanists in different parts of the country might induce the Regents of the Smithsonian Institution to take the matter under their patronage. The botanists of Europe have clubbed together to finish De Candolle's *Prodromus*—Why should not those of the United States do the same in regard to the North American Flora? It should be regarded as a great national work and there should be pride enough to not allow it to be abandoned.<sup>3</sup>

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<sup>2</sup> JT to JH, 8/2/1838, *HL*.

<sup>3</sup> JAL to CWS, 7/30/1849, *CWSC*.

Gray, though staying put in his residence on Garden Street, Cambridge, understood that the geographical scope of the so-called North American flora was fundamentally different from what he had understood when he and Torrey initiated the project. And as a good friend of Joseph Henry and as a member of the committee at the American Academy of Arts and Sciences responsible for examining the practicability of Henry's "Programme," he had little doubt that the Smithsonian Institution would play a crucial role in facilitating scientific studies in the country.<sup>4</sup> Gray was not reluctant to associate himself with the Smithsonian and other governmental agencies—as long as such associations could bring him specimens and information from the infinite "Far West." Early in 1848, a seemingly pure political event gave him an unprecedented chance to place himself and his North American flora under the government's patronage. In February, Nicholas P. Trist signed the Treaty of Guadalupe Hidalgo, officially concluding the U.S-Mexican War. Among those thorny issues that the treaty aimed to settle was the lack of physical boundaries between Texas and Mexico. Realizing how imperative it was to solve the boundary issue once and for all, the treaty stipulated that the U.S. and Mexican governments should appoint their own commissioners and surveyors to survey the boundary line, and should have the two parties meet at San Diego a year after the treaty's ratification. From there, the treaty continued, the surveying parties should

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<sup>4</sup> In December 1847, Gray wrote to Henry on behalf of the American Academy of Arts and Sciences: "Dear Sir, I am directed by the Academy to communicate to you the accompanying report made on the subject of your Programme, laid before the Academy with your note of the 30th September, and to express the great interest which the Society feels in the important subject to which it relates" (AG to JH, 12/8/1847, RH 3180, Box 36, *RSI*). Henry replied that "the Amer. Acay came in good time and served a good purpose" (JH to AG, 1/10/1848, *PJH*, Vol. 7, 256).

“proceed to run and mark the said boundary in its whole course to the mouth of the Rio Bravo de Norte.”<sup>5</sup>

Engelmann paid close attention to the course through which the project of the U.S.-Mexican Boundary Survey took shape. In January 1849, about a year after the ratification of the Treaty of Guadalupe Hidalgo, he told Gray, “A collector should go along [with the Boundary Commission] by all means.” As far as the North American flora was concerned, he noted, “The region of the Gila to the Rio Grande is entirely new.” Enthusiastic as he was, Engelmann thought that he could no longer stand at Gray’s side and support Gray’s botanical endeavors. He was now too busy sorting and distributing thousands of specimens made by Lindheimer and Fendler. Should a collector be allowed to join the Boundary Commission, Gray would have to take full responsibility for this collector’s welfare. Perhaps the recently established Smithsonian Institution could be of some help?<sup>6</sup>

Unbeknownst to Engelmann, Gray had already contacted Henry of the Smithsonian in hopes of securing a position for a botanical collector in the Boundary Commission. Although at the moment he did not recommend any person for Henry’s consideration, he did have a candidate in mind. He wanted a “Charles Wright of Texas” to join the Boundary Commission.

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<sup>5</sup> A body of excellent work has examined the history of the U.S.-Mexican Boundary Survey from multiple points of view. See, among others, Goetzmann, *Army Exploration...*; Rachel C. St. John, *Line in the Sand: A History of the Western U.S.-Mexico Border* (Princeton: Princeton University Press, 2011); Robert V. Hine, *Bartlett’s West: Drawing the Mexican Boundary* (New Haven: Yale University Press, 1968).

<sup>6</sup> GE to AG, 1/5/[1849], *HL*.

### **A professional collector**

Gray's "Charles Wright of Texas" was actually a Yankee from Wethersfield, Connecticut. Though born in a farming family in 1811 and educated at Yale College between 1831 and 1835, Wright opted for an unsteady life. Upon his graduation from Yale, he went to Natchez, a town at the time notorious for vice, profligacy, and crime. He served as a tutor to a planter family there until the family went bankrupt in 1837. He then migrated to Zavala, Texas, and became interested in botany. He soon realized that he could easily establish relationships with noted botanists simply by sending them Texan plants. In 1844, Wright made a decision that would forever change the course of his life: He sorted out his collections and addressed them to Professor Asa Gray.

Wright soon received Gray's encouraging reply. Delighted, he thrust himself into the wilderness in earnest, set up a herbarium, selected the best sets from his collections, and sent them to Gray. "If they should in any wise contribute to your amusement or emolument I shall be pleased," he once told Gray.<sup>7</sup> Living in a time when collecting was by no means a way of making a living, Wright sought every possibility to support his collecting endeavor. He was in turn a surveyor, a member in a company organized for the Mexican War, and a professor at a college in Rutersville. Still, he was by no means a stranger to American society, prospering in mid-nineteenth-century Texas. A member of the Linonian Society at Yale, a longtime resident of Texas with a lofty educational background, a man of property with some

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<sup>7</sup> CW to AG, 4/11/1845, *HL*.

360 acres of land, and an ardent letter-writer who had grown accustomed to maintaining a far-reaching social network, Wright befriended local politicians, planters, and celebrities.<sup>8</sup> To many Texans, he was Professor Wright.

Professor Wright was conscious of his remarkable social status and background. Although he collected plants, he was not the type of collector who sold plants for money. Rather, he was an enthusiast who managed to improve himself by studying natural history. "I am no doctor or son of a doctor and neither have nor wish any handle to my name but plain Mr.," he once told Gray. "I am a graduate of Yale and so am entitled to my A.B. if I want it and I seek no more."<sup>9</sup>

Wright applied every possible strategy to encourage Gray to write him letters. "Neither of us can boast [of] the legibility of our penmanship," he once told Gray. "[H]owever having generally considered myself among the best in deciphering hieroglyphics yours but gave me little trouble." Wright's stream of letters did get Gray's attention. On the Independence Day of 1846, he wrote to Wright to propose "a regular correspondence." Wright was electrified. He immediately replied that he would "cordially embrace" Gray's proposal, and would "enter upon the collection and preservation of plants with renewed zeal." But Gray had to understand that he was quite busy, Wright emphasized. As a college professor, he told Gray that most of his time had been occupied by teaching, and hence his botanical research would be limited. In consequence, he wrote, he would appreciate it if Gray could furnish him

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<sup>8</sup> Regarding Wright's affiliation with the Linonian Society, see CW to JW, 12/18/1854, *CWC*. Wright owned 160 acres in Jackson County, 160 acres in Fannin County, and 40 acres in Ruttersville. See CW to JW, 6/11/1849, *CWC*.

<sup>9</sup> CW to AG, 8/8/1846, *HL*.

with necessary books and instruments. "I ... shall be happy to receive any favor from you that will testify your gratitude for my assistance and that will facilitate my researches in the vegetable kingdom and if I am ever able I will own the 'formidable work' [Torrey and Gray's *A Flora*] which you have on hand."<sup>10</sup>

Wright soon contemplated a botanical excursion that could suit Gray's botanical interests and Wright's wanderlust alike. He revealed his plan to Gray in March 1847, remarking that "as my expenses will be (though not great) something and my income nothing I shall be grateful for any assistance which you and your friends can render me whether in books or money." Could Gray send him "a good supply of paper" he asked in another letter. "I have to buy newspapers out of the P.O. & pay the postage on them which at the recent high rate is quite a tax on me."<sup>11</sup>

Wright considered his intercourse with Gray to be based on liberal and reciprocal gift exchange. Hence he expected Gray to return his favors *in kind* or at least reply to his letters regularly. He was often disappointed on that score. "Since that time I have been waiting with impatience for an answer and I now began to think that my letters are somewhere in the Gulf of Mexico," he once complained to Gray. In 1847, after sending off a collection to Gray and receiving no reply, he grew annoyed. Then all of sudden he received a letter from his sister about her recent correspondence with Gray. Wright was startled. "I am very glad to hear from Mr. Gray even indirectly," he replied. For Gray had not remembered him "in a sensible way," he wrote that he had "given him up for dead or mad or something else

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<sup>10</sup> CW to AG, 3/14/1845, *HL*; CW to AG, 8/8/1846, *HL*; CW to AG, 10/29/1846, *HL*.

<sup>11</sup> CW to AG, 3/8/1847, *HL*; CW to AG, 10/15/1847, *HL*.

unaccountable.” Relieved that his “Dear Doctor” was still alive (and might not have gone mad), he wrote, “I wish you had written me more particularly about your intercourse with Mr. Gray and told me less about these ninth cousins of ours whom I have entirely forgotten.”<sup>12</sup>

Wright also dashed off a letter to Gray claiming that he would continue collecting plants for Gray. “[Since] you are indeed an inhabitant of upper air I have revoked the countermand and have been busying myself to collect you as good an assortment of Cactaceae as I could.” To his surprise, he still heard nothing from Gray. “You did not tell me whether you had seen Mr. Gray or had written to him &c.,” he complained to his sister instead. “I had not heard from him so long that like a drowning man catching at a straw when you mentioned having had some intercourse with him.”<sup>13</sup>

### **“I should prefer to be a collector”**

Gray was glad to have such a knowledgeable correspondent in Texas as Wright. Impressed by Wright’s enthusiasm, he never forgot to keep presentation copies of his various publications for Wright’s use.<sup>14</sup> Also, as Wright wished, he identified Wright’s specimens and sent him the list.<sup>15</sup> What he felt unable to do was to financially support Wright’s collecting endeavor. He understood how much money

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<sup>12</sup> CW to AG, 10/29/1846, *HL*; CW to MAW, 1/19/1848, *CWC*.

<sup>13</sup> CW to AG, 2/8/1848, *HL*; CW to MAR, 5/12/1848, *CWC*.

<sup>14</sup> According to one of Wright’s letters, Gray sent Wright such books as *Genera Florae Americae Boreali-Orientalis Illustrata* (1848) and *A Manual of the Botany of the Northern United States* (1848).

<sup>15</sup> See CW to AG, 3/8/1847, *HL*.

Wright must have had invested to collect plants in Texas. He was perfectly willing to remunerate Wright for his botanical labors—had he had any additional cash in his pockets. In the face of Wright’s polite but frequent requests for money, he begged Wright’s sympathy. If Professor Wright was poor, then Professor Gray had almost gone bankrupt—for he had sunk too much money into his research.<sup>16</sup> He then came up with a solution. He indoctrinated Wright in the etiquette practiced in botanical circles and encouraged Wright to make use of them.

For a microscope, you don't want a Compound Microscope (properly so called) but (what I use) a French simple microscope with doublets. I used to import them for about \$25.—An optician in Boston has one but asks \$35, & I think it has not the right power either. I will see what I can do, but perhaps I shall not be able to get one for you till summer. I have not the money to pay for it, & can not get it from the Garden in advance. If you could send really a copious set of Lichens & of Musci & Hepaticae, these happens to be the very things that I could cash immediately for you, through Tuckerman & Sullivant. A good collection of Mosses, Lichens &c. will bring the microscope you wish. But Tuckerman is not a man to make advance for any one! and as to Sullivant—he is so generous that I have already laid him under contemplation as fully as I have the face to ask.<sup>17</sup>

Afterward, growing embarrassed by his unequal return for Wright’s botanical labor, he suggested that Wright send him seeds and bulbs (but not living plants) for the Harvard Botanical Garden. If Wright agreed to do so, he wrote, he could then use a small fund at his disposal to send Wright cash. “So you can aid me with fine plants I

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<sup>16</sup> AG to CW, 3/10/1848, *AGPG*.

<sup>17</sup> AG to CW, 1/17/1848, *AGPG*. Wright’s inquiry read: “Will a collection of cryptogamic microscope—if so and any cryptogamist will send me one I will make for him next winter as good a collection as circumstances and situation will permit and remit it to him.” CW to AG, 9/20/1847, *HL*.



want; and I can aid you from the Garden, at the rate of \$50 or more a year—to our mutual advantage.”<sup>18</sup>

Finally, as for Wright’s grumbles about not receiving Gray’s letters in a timely manner, he felt sorry about that. In January 1848, after reading a particularly harsh letter from Wright complaining of Gray’s indifference, Gray offered a lengthy explanation for the delay. He had been working like a dog, he groaned.

I wish I could write to you as you wish, all about botany, etc. I wish I could aid you as I desire, but I fear it is impossible. I must have rest and less anxiety. Two more years like the last would probably destroy me. If I had an assistant or two, to take details off my hands, I might stand it; as it is I cannot... Will you wonder that I am a little disheartened when, in spite of every effort, I make so little progress? And in six weeks I begin to lecture in college again; and in April the Garden will require more time than I can give it. Such are merely some of the things on my hands, some of my cares!

“So have mercy, and long patience,” he wrote.<sup>19</sup>

Gray opted for the most straightforward means to overcome this problem of communication. He had learned of Wright’s eagerness to deliver collections to New England in person and to study the Texan flora with him.<sup>20</sup> In June 1848, he made a decision that would forever change his and Wright’s careers: he invited Wright to come to Cambridge to serve as his curator.<sup>21</sup>

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<sup>18</sup> AG to CW, 3/10/1848, *AGPG*.

<sup>19</sup> AG to CW, 1/17/1848, *LAG*, 353-355.

<sup>20</sup> “[I]f I could make a considerable collection that could be made of pecuniary advantage to me,” Wright wrote in March 1847, “I would visit the North and bring my collections with me wishing much to see my kindred who are residing there.” CW to AG, 3/8/1847, *HL*.

<sup>21</sup> Upon sending off this invitation, he wrote to Engelmann with both anxiety and anticipation: “As to Curator I had sent to Wright in Texas and offered it to him—But just before my letter could reach him, I got one saying he was going to be with Capt.

When Gray sent off the invitation, Wright was on a collecting trip to Eagle Pass. As he trudged across prairies looking for novelties, he entertained himself with the thought that he might be granted the chance to study with Gray.<sup>22</sup> Upon returning to San Antonio, he was delighted to find Gray's invitation waiting for him. "I am now fully resolved to embrace your proposition," he immediately replied, "and hurry on to the North with the least possible delay."

I have made a very pretty collection of plants and seeds in Western Texas and Tamaulipas (I believe that is the state which I traveled in). There is still a vast field open here and if I were to consult my own taste I should prefer to be a collector.

"I am in excellent health and think I could brace a northern winter like a bear," Wright concluded the letter.<sup>23</sup>

Gray was equally delighted. He underlined Wright's sentence "I should prefer to be a collector" and marked it with four asterisks. He thought that he might be able to make Wright a professional collector. That is to say: Wright would know how to seek botanical novelties, make well-selected specimens, and make detailed profiles about each specimen. So long as Wright could equip himself for such a profession, he had no doubt that botanists would not hesitate to reward Wright's labors with a handsome sum of money. It was the profession that he had advised the talented

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Veatch, on the Rio del Norte till Oct. I fear my letter has not been forwarded to him, but when he gets it I have very little doubt he will accept my proposition & it is only in case he declines that I could engage Fendler..." AG to GE, 9/6/1848, *AGPG*.

<sup>22</sup> "If we agree that I go to Santa Fe would it not be a good plan for me to come and spend the winter with you and assist you in studying and arranging my collections or would I freeze up to an icicle." CW to AG, 7/21/1848, *HL*.

<sup>23</sup> CW to AG, 9/20/1848, *HL*.

Fendler to choose, and Fendler's reluctance to follow Gray's advice had proved that collecting was not a profession easy to pursue. Late in 1848, while waiting for Wright's arrival, Gray hoped Wright was the person that he had been searching for.

### **"Dog-paradise"**

On a rainy night in December 1848, Wright arrived in Boston. The next morning, for the weather was extraordinary hospitable, he decided to walk to Gray's house on Garden Street, Cambridge. Bathed in the New England winter sun, Wright felt energetic. The walk from Boston to Cambridge was four miles, but to the 37-year-old Wright it must have seemed a shortcut to a new stage in his career.<sup>24</sup>

When Wright arrived at Gray's house, he found that Gray had gone the church. He soon saw a man with a beard "considerably sprinkled with white hairs" stepping into the house, singing "halloo." "You must be Mr. Wright," the bearded man said, and introduced himself as Asa Gray. The two men then sat down and chatted until bedtime. From Gray, Wright learned that his chief mission was to arrange his previous Texas collections and thereby to familiarize himself with "the natural families of the vegetable kingdom." It was essential for a collector nowadays to have such curatorial experiences, he learned.<sup>25</sup>

Wright was glad to have Gray as his mentor, for thus far he had not had any chance to systematically study botany or any branch of natural history. He soon was amazed by Gray's herbarium and library. "Such a quantity of shelves and boxes and

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<sup>24</sup> CW to JW, 12/[undated/]1848, *CWC*.

<sup>25</sup> see CW to JW, 1/22/1849, *CWC*.

packages of plants you never saw and may see from all parts of the known world,” he wrote home. “And more books than you ‘could shake a stick’ from all countries and in all languages full of figures of plants, animals, birds, beasts, insects, fishes, and bones and shells.”<sup>26</sup>

With some humor, he described his and Gray’s daily routine to his brother John:

You may like to know how we live here. Well, the first meal of the day is of course breakfast but you need not think that the last is supper. No such thing. We have no such word in our vocabulary or at least no use for it... We eat breakfast in tolerably good season, for fashionable folks; say at 8 o’clock more or less or sooner or later. This consists merely of coffee with bread or muffin or toast and butter a very temperate and wholesome meal. After breakfast we work till noon when we have lunch consisting of a little bread and butter and a glass of water. Then to work again till about six o’clock when we have the principal meal of the day—dinner. Now we have meats and potatoes and rice and squash finishing with a dessert of stewed quinces or a custard or some apples or some such things and finishing all with a cup of tea—and so ends the eating for the day. Then we work again till 10 or 11 o’clock more or less so that we give dinner a pretty good chance for digestion.<sup>27</sup>

Afterward, when Wright once again thrust himself into the wilderness, he would call Gray’s situation a “dog paradise.”<sup>28</sup>

Gray was impressed by Wright’s potential as a plant collector. It occurred to him that Wright might achieve what Lindheimer and Fendler had failed (or never intended) to do. In December 1848, while Wright darted about his house looking for references and specimens, Gray wrote to Engelmann with satisfaction:

[Wright prefers] exploring to anything else,—will go to Santa Fe, or wherever we choose to send him,—he is used to roughing it, and has long been in the woods and prairies.—As he is used to lower Texas, I am going to try to get him attached

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<sup>26</sup> CW to JW, 12/[undated/]1848, *CWC*.

<sup>27</sup> CW to JW, 12/[undated/]1848, *CWC*.

<sup>28</sup> CW to AG, 6/2/1849, *HL*.

to the southern boundary survey party,—to go west as far as he can and then get into the Sierra Madre, Cosiquisiachi, &c.—or to strike up north and make head quarters at Great Salt Lake.<sup>29</sup>

Then, with Henry of the Smithsonian serving as a messenger, he made his wish felt to William H. Emory, an influential U.S. Army Officer and topographer. Emory had just completed a successful survey that stretched from Fort Leavenworth to San Diego by way of the Arkansas, Rio Grande, and Gila rivers. Impressed, Congress offered Emory the Boundary Commissionership on the condition that he should resign from the Army. Emory declined the offer, but agreed to serve as chief astronomer and commander of the escort of the troop to accompany the Boundary Commission.<sup>30</sup> For Gray and for Gray's botanical circle, Emory's active role in the Boundary Commission was good news. Unlike his predecessors (John Charles Frémont, for example), Emory had exhibited willingness to collaborate with men of science.<sup>31</sup> Gray soon received Emory's promise to secure a position for Wright in his campaign. He was relieved. He trusted that he would get Wright attached to the Boundary Commission, he told Engelmann and Henry.<sup>32</sup>

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<sup>29</sup> AG to GE, 12/4/1848, *AGPG*. Wright definitely looked forward to this incoming expedition. "We are trying to secure for me the privilege of accompanying the expedition appointed for running the boundary line between Mexico and the ceded territory." CW to JW, 1/22/1849, *CWC*.

<sup>30</sup> William H. Emory, *Report on the United States and Mexican Boundary Survey, Made under the Direction of the Secretary of the Interior, Vol. I* (Washington: Cornelius Wendell, 1857), Chapter 1.

<sup>31</sup> Both Torrey and Engelmann were deeply involved in enumerating the collections made in Emory's expedition from Fort Leavenworth to San Diego. Gray, although he did not directly correspond with Emory at the time, helped Torrey enumerate some families that interested him.

<sup>32</sup> AG to GE, 1/24/1849, *LAG*, 360-361 and AG to JH, 1/23/1849, *PJH*, Vol. 7, 464.

Emory, understanding that the current law would not allow him to appoint a botanist in his campaign, decided to hire Wright as an “assistant computer.” He created three or four such nominal positions and asked the Secretary of the State for additional money and facilities. The scheme did not work completely. Early in February, Emory wrote to Gray that he had just received an instruction from Washington, DC, to cut off “two of my computers.” As a result, he wrote, he would not be able to take Wright along. “[T]he rare opportunity which this expedition offers to contribute to our Knowledge... will be disregarded & thrown away.”<sup>33</sup>

Gray was disappointed. “The Secretary of the State will not let a cent of the appropriation go to a botanist,” he complained to Engelmann.<sup>34</sup> Undiscouraged, he promptly wrote to Emory that he and Wright would afford all necessary instruments and facilities for plant collecting. Wright, he pointed out, would ask for nothing from Emory’s campaign except for protection and transportation.<sup>35</sup> Gray also turned to Torrey for help. He knew Torrey had collaborated with Emory during Emory’s previous expedition. If Torrey could say something in Wright’s favor, Gray thought that perhaps Emory might be able to squeeze in Wright in some capacity. Finally, upon learning that John B. Weller, a politician from Ohio, had been appointed boundary commissioner, he dashed off a letter to Sullivant. He hoped that

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<sup>33</sup> WHE to AG, 2/4/1849, *HL*.

<sup>34</sup> AG to GE, 2/24/1849, *AGPG*.

<sup>35</sup> Gray’s proposal can be found in Emory’s reply; WHE to AG, 2/17/1849, *HL*.

Sullivant would convince Weller of the importance of botany and help Wright secure a position on the Boundary Commission.<sup>36</sup>

None of these strategies worked.<sup>37</sup> Gray eventually turned his own network and was glad to learn that two of his close friends and great patrons, John A. Lowell and Benjamin D. Greene, exhibited an interest in subscribing to Wright's expedition.<sup>38</sup> Furthermore, regarding the issue of free transportation and protection, he learned that General William Worth, the commanding Army officer in Texas, was about to

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<sup>36</sup> See WSS to AG, 3/15/1849, *HL*; regarding Weller's appointment, see Goetzmann, *Army Exploration...*, 157-158.

<sup>37</sup> Like Gray, Torrey had been working relentlessly to insert a botanist into the Boundary Commission. At the time Torrey did not know Wright, nor did he know Gray's plan for Wright. He had his own favored collector, Charles C. Parry, a doctor who graduated from Torrey's college and had collected for Torrey during the North West Geological Survey. Emory, learning of Torrey's wish, reserved the post of the "Assistant Computer" for Parry. It was after Emory had settled Parry's appointment that Torrey learned of Gray's plan for Wright. Torrey felt sorry. "Had you not been so anxious to get Wright a place," he told Gray, "I should not have so hailed the appointment of Dr. Parry" (JT to AG, 1/26, 1849, *HL*). "Had I known that Wright would have gone—he should have had my recommendation," he told Gray in another letter (JT to AG, 2/21/1849, *HL*). Sullivant, too, reported to Gray that he did write to Weller as Gray had wished, but it occurred to him that Weller had already been on the way to Texas. Gray had made his wish felt too late, he wrote regretfully. "There might have been something done had I been earlier advised of your wishes" (WSS to AG, 3/15/1849, *HL*).

Afterward, Torrey wrote to Emory to see if there was any chance left for Wright. Emory, sandwiched between Torrey and Gray, made the decision. "I regret it will not be in my power to take Mr. Wright even on the very liberal term he proposes," he wrote to Gray. He would have taken Wright along, he explained, had he not already "all the necessary conveniences, prepared for me by Dr. Torrey." Finally, as if he were concerned that his words might hurt Gray's feelings, Emory assured Gray that Torrey had done the best for the matter. "By the way on my return to Washington I received Dr. Torrey's letter urging Mr. Wright's appointment but I was compelled to give him the same answer." WHE to AG, 2/17/1849, *HL*.

<sup>38</sup> "Lowell and Greene subscribing handsomely," Gray told Torrey, "which is as much better than Emory's as possible." AG to JT, 2/26/1849, *LAG*, 362.

launch a troop to survey the wagon roads from San Antonio to El Paso through Fredericksburg. He thought Wright might be able take advantage of General Worth's project.<sup>39</sup> In a letter to Torrey in February 1849, he outlined Wright's upcoming expedition.

Wright has left me this morning to go to his mother's in Connecticut (Wethersfield); there to make his portfolios and presses; comes on to New York soon; takes first vessel for Galveston (I expect a letter from Hastings telling when it sails), and to reach Austin and Fredericksburg in time to accompany the troops that are about to be sent up, by a new road, across a new country, to El Paso, in New Mexico. Look on the map (Wislizenus [Figure 5-1]) and you will see the region we mean him to explore this summer; the hot valley of Rio del Norte, early in the season, the mountains east, and especially those west in summer. He will probably stay two years, and get to Taos and Spanish Peaks this year or next.<sup>40</sup>

Engelmann was aware that his two close colleagues in the East had made significant progress in securing sources of specimens by associating themselves with governmental expeditions. "You have now Wright on hand," he wrote to Gray, and "Maj. Hendrick will also collect for you; Fremont, Emory's etc. will go to Torrey—But I shall also have several to collect for me—Wislizenus will probably go to California and Dr. Gregg is on his way there!—these collections we must

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<sup>39</sup> "Lowell, Greene and I have got up an expedition for him that will do as well,—and on which he is going right-off—to Texas, in time if possible to join the troops which Gen. Worth is sending to El Paso del Norte by new route directly across from Fredericksburg—if not, get across by best chance he can. Botanizing on the Rio del Norte and especially in the mountains right west and S. W. this summer-winter—or go down to Chihuahua—or back to Texas—and another year try again—perhaps get up to Taos—and the mountains there—or go farther west towards Calif. along the boundary line." AG to GE, 2/24/1849, *AGPG*.

<sup>40</sup> AG to JT, 2/26/1849, *LAG*, 362-363.



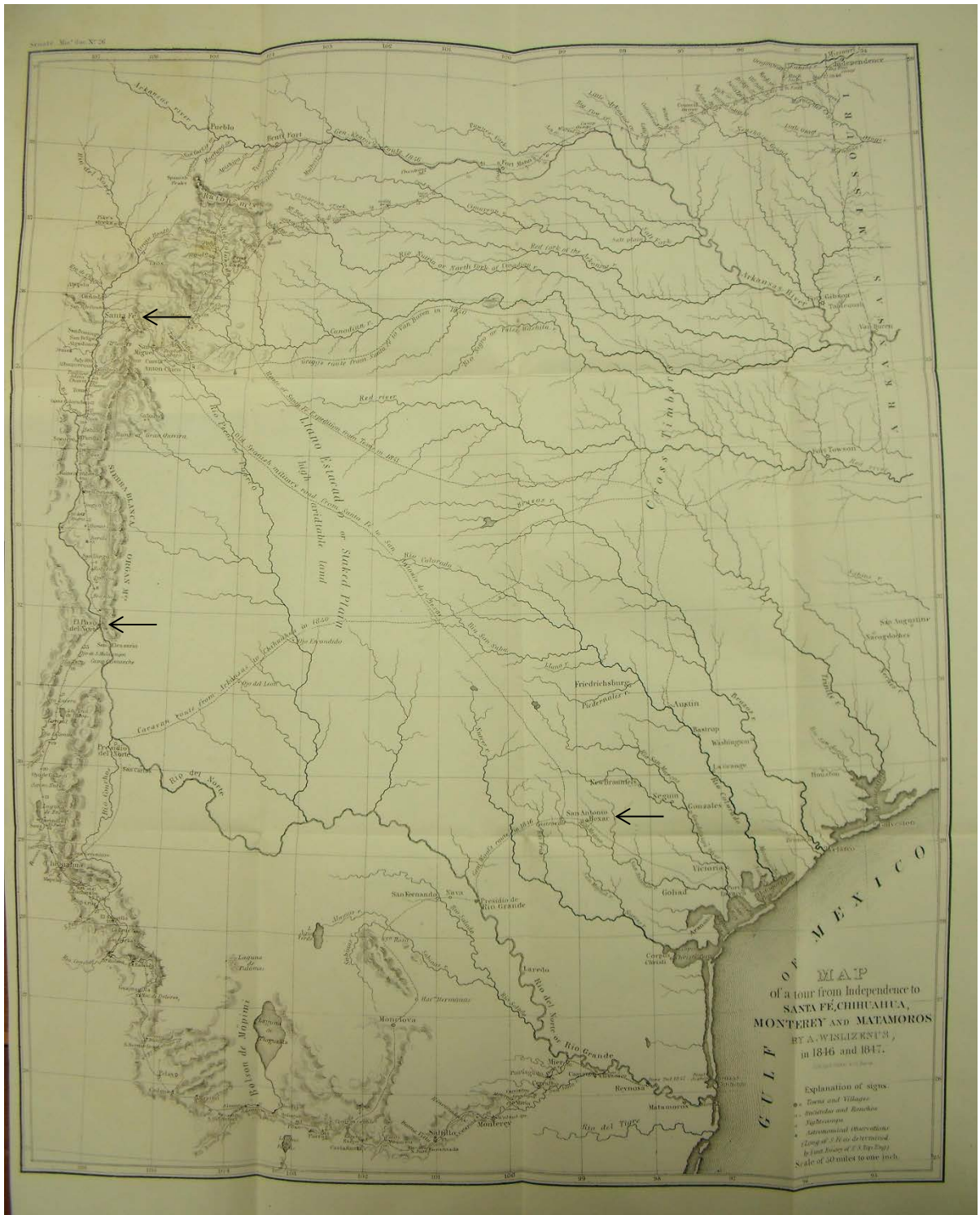


Figure 5-1. The same map Gray used to describe Wright’s 1849 expedition for Torrey’s reference. The arrows show the three major stepping stones (San Antonio de Bexar, El Paso del Norte, and Santa Fe) of the expedition. (From Wislizenus, *Memoir of a Tour to Northern Mexico*, 1848. Courtesy of the Huntington Library, San Marino, CA, USA.)

communicate and exchange with both one another before we distribute specimens to others.”<sup>41</sup>

“Be sure you procure for me full sets of Fendler’s and Wright’s plants,” Sir William also wrote to Gray. “Everything that comes from your newly acquired territories (save the gold—& I would not disprove that, if it came in my way) is most interesting to me.”<sup>42</sup>

### **“Hog- and ass-paradise combined”**

Wright arrived in New York in late March 1849. He visited Torrey to receive reams of drying paper. On April 7, he boarded a ship packed with sixty “men”—including a woman in man’s clothes—sailing for Galveston. Traveling by ship was an entirely fresh experience to Wright. He stared in amazement at whales playing so close to the ship, at flying fishes raising and descending themselves over waves (some appeared to have “two pairs of wings!”), and at such natural wonders as the “Hole on the Wall” (“It looks at a distance like a bridge connecting a rock in the water with the main land”). But as a collector on duty, Wright soon grew impatient. Instead of running “straight down the coast of Florida on account of the Gulf Stream,” he found that the ship “went round through the Bahamas.” “We are crowded together about 20 in a little bit of a room with a grass or hay or straw mattress and pillow the same and nothing to cover with,” he complained to his brother John. The breakfast

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<sup>41</sup> GE to AG, 3/28/1849, *HL*.

<sup>42</sup> WJH to AG, 4/13/1849, *HL*.

consisted of “pickled beef and a slop nicknamed coffee which tastes like a decoction of charcoal,” he noted, whereas the dinner was mostly “salt pork a foot thick,” “stinking beef tongue,” “pickled beef,” and “a few watery potatoes.” With such disagreeable meals, together with the fact that he hardly gained any sea legs, he found that his stomach “gradually became disordered.”<sup>43</sup>

So Wright was overjoyed to see the coast of Galveston materializing on the horizon. He arrived in Galveston on April 28, about three weeks after he left New York. He botanized a little there, and on the early morning of May 1, he boarded “a rickety steamboat with one paddle-wheel” sailing for Houston. Arriving in the evening, he tried to get a stagecoach to deliver his luggage to Austin but to no avail. He then hired a road wagon, which was “(unfortunately) overloaded and with a weak team.”

By a blunder of the driver he started on the wrong road and one as bad as roads ever got to be. The result was that he was obliged to hire himself hauled out of the mud into the right road and on that we had divers unloadings and reloadings to perform on our way. I footed it through mud and water and had a fair chance to botanize for aquatics or amphibian... My paper was considerably injured and diminished in quantity by abrasion when wet in loading & unloading and rubbing against the other articles in the waggon.<sup>44</sup>

Wright remained in Austin for ten days or so (roughly from May 19 to May 28). He met a Mr. Chapman from Springfield, who was running a transportation business between Austin and San Antonio. From Chapman, Wright learned that the surveying troop with which he intended to travel had long gone, and General Worth, the army officer who devised the expedition, had died (on May 7, of cholera). Fortunately,

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<sup>43</sup> CW to JW, 4/17/1849, *CWC*.

<sup>44</sup> CW to AG, 5/26/1849, *HL*.

Chapman told Wright, a surveying troop would soon leave San Antonio for El Paso, and Wright should immediately contact General William S. Harney, Worth's successor, to see if he could travel with the troop and receive free transportation and protection.<sup>45</sup>

Wright took Chapman's suggested course of action. He arrived in San Antonio on May 30 and immediately presented General Harney an introduction letter from Washington D. Miller (1814-1866), secretary of state of Texas. The general was unimpressed, however. The wagons were all heavily loaded, he announced, and Wright should contact Captain Samuel G. French, Assistant Quartermaster, to discuss the matter. Wright followed the order but found that Captain French was equally unwilling to have anything to do with Wright's expedition. Just when he thought that it was time to give up, a Dr. Baker intervened and helped Wright obtain permission from the stone-faced Captain French. Relieved, Wright promptly reported the good news to Gray. "I have been obliged to muster up all the Yankee confidence natural to me (which is and always was but little)" to load his luggage and facilities (amounting to some 400-500 pounds) onto the wagons, he noted.

I am rejoiced that I am in time late as it is, to go with the train and you will doubtless share my joy at the favorable prospect before me. It is supposed we will be three months on the route (—travelling slowly—) of only about 500 miles. It seems hardly possible that so much time will be required to make that distance.

Still, Wright wrote, Captain French promised "the least assurance of subsistence" for Wright's trip to El Paso. "Now can not you get a special order from the head of the Commissary Department to furnish me with support and transportation? The

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<sup>45</sup> CW to AG, 5/26/1849, *HL*.

officers all plead that they had no authority to grant these favors and if they grant them to one for a certain purpose—as to me for botanizing—how can they refuse them to others for other purposes?” “Now I hope you won’t forget me,” Wright concluded the letter.<sup>46</sup>

Captain French’s troops left San Antonio at the end of May. The rain fell in torrents. On June 2, a violent thunderstorm arose, and the troop was forced to camp in Castroville. With Wright curled under a damp tent, enveloped in clothes soaked with water, and helplessly trying to keep his plants and reams of papers dry, the wagonmaster called upon him. Who had permitted Wright to load the luggage in the Army’s wagons? he barked. Captain French, Wright muttered. The wagonmaster refused to believe him and threatened to throw Wright’s *stuff* away. The collector exploded—not before the wagonmaster but in a letter to Gray. Returning to the camp, he grabbed his pen and wrote.

Quihi June 2nd/49

My Dear Dr

I wrote you so recently that if I were not full I would keep silence. But steam is so high that if I do not blow off fearful consequence may follow.

Yesterday morning [June 1, on the banks of the Medina] we had a violent norther[n] cold and accompanied with rain after which and when ready to start my baggage, paper, &c was distributed about into three or four waggons. It was so packed that it was not much injured. This morning about daylight we had another more severe accompanied with hail. My collections were nearly all wet and I have had no time to dry them so they will be much damaged. My paper is nearly all wet. I should not wonder if we have another storm tonight.

Now these are misfortunes attendant on my dependent situation and I can not prevent them. The officers care nothing about my affairs and the waggons have a little curiosity to gratify by looking on while I change my plants and care

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<sup>46</sup> CW to AG, 5/31/1849, *HL*.

no more about it or rather would be pleased if they were sunk in the river and their load would be lightened.

You will recollect I suppose a suggestion made to you that I should be equipped with a waggon and horse from which you discussed instancing the labors of other botanists who had made large collections. But I venture to say that Drummond did not attempt to save 12-15 specimens of each species or if he did he had an art which I do not possess.

The outfit which I proposed seemed to you perhaps large but I am sincerely of the opinion that the entire cost of the outfit might have been clear saved the present year. I would rather have a horse and carriage and ten dollars in my pocket than have five hundred as I am so far as it facilitates my operations. I have money in my pocket but it does me no good. I can buy nothing with it. I sit uninvited and see others eating and it is a severe trial to my feelings to thrust myself among them. The men have their rations and often none to spare and how am I to get along to El Paso I know not. If I had consulted my own feelings alone I should have stopped at San Antonio and turned back. But you and Mr. Lowell had expectations which would not have been realized and I felt reluctant to disappoint you. You wrote to me [Jan. 17, 1848] of working like a dog. I know how you live—then call your situation dog-paradise and mine hog- and ass-paradise combined and you may realize my situation—sleep all night if you can in the rain and walk 12-15 miles next day in the mud and then overhaul [*sic*] a huge package of soaked plants and dry them by the heat of the clouds.

I have been now three or four days in such a state of uncertainty about the possibility of going on that I have no enjoyment and today I have not saved a specimen—have merely collected some seeds as I walked along the way. As for studying the plants I have not attempted it so long that I have almost forgotten how I have been vexed enough to cry or swear when thinking that I have the pleasing prospect of being dependent for six months on a parcel of men who call me a fool and wish me at the bottom of the sea.

There is a man who is bound for California in our company—provided with a carriage and mules provisions and cooking utensils—independent as a wood sawyer and dependent on others only for safety against enemies. If I had such a one my expenses would be very trifling. I could collect twice as many specimens of twice as many species and twice as well preserved I could attend to them at any time. I am pleased in wet or dry weather and have the assurance that the rain at least could be prevented from coming to them. I could also take them to Houston or other seaport and put them on shipboard myself and then I would know they would depend for their forwarding on no careless agent.

I am fully resolved that this season will close my botanical travels on horseback or on foot if I can not operate to better advantage. I will give it up and turn my attention to something else.

I can now only hope that when Capt. French arrives in camp by [*sic*] situation will be improved by an appointment or in some other way.

I have no expectation of hearing from you till about the beginning of winter of course the arrangements for the next year if I spend it on the boundary must be attended to by you. So far as shipment of paper &c. are concerned, what I

wrote in my last about money I recall as I know—not what to do with that which I have. You now know my sentiment [*sic*] on the mode of botanizing in this country & if you wish to continue it on my plan I am ready to do all I can.

It will facilitate the saving of bulbs seeds-cacti &c. in large quantities which if they sell as well as some sold [in] Galveston would go far to defray expenses.

I am in good health. Give my regards to Mrs. G and all enquiring friends.

Affectionately Yours,  
Charles Wright

### **“A mighty industrious man”**

Beginning the expedition with the “hog- and ass-paradise combined,” Wright soon felt compelled to detail some “monetary affairs” to his brother John, which “would require attention in the event of accident happening to me in the long and uncertain journey which I have commenced.”<sup>47</sup>

It turned out that Wright had been wise to make such a financial arrangement in advance. His journey to El Paso was replete with danger. The Indians followed the troop, killing four people and taking some captive.<sup>48</sup> Two men got killed after falling under the wagons. One of Wright’s teammates developed serious fever and died in pain. As for Wright himself, he once fell under the wagon, and felt the wheels roaring past his ears.<sup>49</sup> Late in July, when the troop was crossing from Devil’s River to the Pecos, he became ill (after botanizing the whole morning and then standing naked

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<sup>47</sup> CW to JW, 6/11/1849, *CWC*.

<sup>48</sup> “We want Texas rangers to deal with the red skins.” CW to JW, 6/11/1849, *CWC*.

<sup>49</sup> CW to JW, 6/11/1849, *CWC*.

by the river washing his clothes). He felt fever grab him, together with “troublesome pain,” and was forced to retire to the wagon for 10 days.<sup>50</sup>

But he never stopped collecting. He told Gray that he had been doing well and wished Gray not to become too worried about his physical status.<sup>51</sup> He observed what had happened to his teammates and to himself with detachment. He treated his and his teammates’ sufferings as if they were but trivialities. Afterward, he remarked to John about how he had occupied himself with collecting: “I took a ramble over the hills when a thick fog came on & I could see nothing which I was busied in collecting specimens; the train started on & I fell so far behind that I could not overtake the waggon & had to walk [for one] day. However I found some new plants and was thus compensated for my labor.” To Gray, he wrote with pride: “I don’t eat the bread of idleness & have frequently heard the remark as I passed a company of men at play or sitting in conversation ‘that is a mighty industrious man’.”<sup>52</sup>

But a mighty industrious man still had his worries. In July 1849, when the troop arrived in San Pedro, he informed Gray that he had decided to change the course of his expedition:

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<sup>50</sup> See CW to AG, 9/13/1849, *HL*; also CW to JW, 12/29/1849, *CWC*.

<sup>51</sup> “I suppose I need not tell you that I was in a bad humor when I wrote last & I had good reason to be so. I was going westward in a state of great uncertainty whether I would be permitted to proceed. Now however I have the assurance from Maj. Van Horn the officer in command that I shall have all the facilities which he has power to grant.” CW to AG, 6/18/1849, *HL*; also CW to JW, 6/11/1849, *CWC*.

<sup>52</sup> CW to JW, 12/29/1849, *CWC*; CW to AG, 7/31/1849, *HL*.



I have pretty much made up my mind to return to San Antonio with the train which will stay but a few weeks at El Paso. I have paper sufficient for the present season only and I am perfectly convinced that I shall get no more without going after it unless (which is not very probable) it could be sent as Quartermaster's stores. The indifference and even petty spite shown me and my pursuits by the head waggon master of the train in which I have been thrown lead me to this opinion and the want of interest shown by the Quartermaster himself proves to me that he will give himself no trouble to forward my pursuits.

Moreover, he found the wagonmaster had developed an interest in playing tricks on him. He always found his collecting facilities scattered over five or six different wagons. On top of such annoyances was Captain French's "ill humor." Whenever the Captain felt that the troop under his command was moved too slowly, the Captain would "grumble about the weakness of his teams and the transportation of botanist's trunks." Exasperated, he exclaimed in a letter to Gray,

There are two ways to remedy these evils in any subsequent campaign—to get special permission and power from the Sec. of War to demand transportation and subsistence—or to go entirely independent of the troops as I have heretofore written to you except the protection of their company. I am pretty well determined to stay at home hereafter unless I can travel under more agreeable circumstances. I have been devising a plan to secure the favor and assistance of Government—to present to Prof. Henrie [*sic*] for the Smithsonian Institute a set of my collections—then if he has any influence at head quarters he can secure for me these advantages which will put me above the control of an Ass. Q. Master and an Almighty Waggonmaster.<sup>53</sup>

Wright did not expect that assistance from Gray and the Smithsonian (if any) would reach him in time and thereby dispel the "evils" that had staggered his collecting enterprise. He had to rely on himself. Skillfully, he began showing his collections to Captain French. As an experienced teacher and a field collector who had studied with Harvard Professor Asa Gray, Wright transformed prairies stretching from San Antonio to El Paso into a classroom, where he was a professor

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<sup>53</sup> CW to AG, 7/2/1849, *HL*.

and the captain a student. He hoped that he could smooth his expedition by stimulating in the Captain an interest in natural history. To his delight, the strategy worked. The Captain softened his attitude and exhibited more and more willingness to support Wright. Like a drowning man grasping at a straw, Wright told Captain French that he would be more than happy to donate part of his collections to the Smithsonian in the captain's name. Flattered, the Captain replied that he wanted Wright to make the donation to the Academy of Natural Sciences of Philadelphia instead, because he had prestigious friends whom he was eager to please. On July 10, when camping at San Felipe Creek, some two hundred miles away from San Antonio, Wright dashed off a letter to Gray. He reported the captain's budding interest in natural history. It would be of great help if Gray could send the Captain "some books or memoirs read before the Academy which will not be too great a pecuniary sacrifice for you to make."<sup>54</sup>

Another factor that constantly worried Wright was the depth of his pockets. "I pay now \$12 per month for my board and at that rate by the time I return to Austin I shall be about out of funds," he told Gray.<sup>55</sup> He wondered if Gray could honor his (Wright's) draft so that he could get some \$150 from Dr. Baker. Also, as the troop came to camp near the head of Devil's River, he found a job—not a naturalist of any sort but a cook. He was relieved then, but soon discovered that cooking was not as easy as he had thought. Worse, he found his good friend Dr. Baker's attitude toward him was changing rapidly after tasting the meals he prepared.

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<sup>54</sup> See CW to AG, 7/10/1849, *HL*.

<sup>55</sup> CW to AG, 7/2/1849, *HL*.

It was not long however before the Dr began to find fault with the cookery more than I thought was necessary in a camp. The tea was too strong or too weak the rice was bitter with smoke the bread was dough or burnt to a coal & the coffee was muddy or cold as ice. If any thing fell it was smashed if wood was scarce “there was not enough to put in a humming-birds eye.” If any thing was cooking too slowly then was not a particle of fire under it. If any thing was broken I broke it. So it was with every remark intended for censure—it was couched in the most hyperbolic language he was master of.<sup>56</sup>

Overwhelmed, he complained to home that the doctor was “probably imagining himself at the Astor House or somewhere else.” To Gray, he cried, “Just think of that, Doctor—a literary and College-larnt [learnt] man turning cook for a living!” Darkly, he remarked, “Well it’s some consolation to them [those who had been obliged to eat meals prepared by Wright] that when I get out of this scrape I’ll not get into exactly such another.”<sup>57</sup>

Wright’s despair about his low status in the troop eventually led to his complete estrangement from Baker. The event took place when the troop was making its way back to San Antonio. One day (probably late in October 1849), as the two old friends helped each other overcome the rough road along Devil’s River, they took a rest on a hillside and waited for the rest of the troop. Venturing to break the silence, Baker asked, “One waggon had got stalled, isn’t it, Mr. Wright?” Well, Wright replied, “There was another waggon which had been stalled.” Not accustomed to being challenged, Baker declared that only one wagon got stalled, and Wright insisted that he did notice another stalled wagon. “You lied,” Baker shouted, and Wright responded with a punch to the doctor’s face. Baker, as Wright described it

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<sup>56</sup> CW to AG, [undated; presumably December 1849,] *HL*.

<sup>57</sup> CW to JW, 12/29/1849, *CWC*; CW to AG, 7/31/1849, *HL*.

afterward, “went off abusing me as if I were the very offscouring of humanity and declaring I would repent it.” To Gray he explained why he had decided to make such an ungentlemanly move: “I was heartily sorry that he should forget the gentleman and become rowdy enough to give another man the lie and irritate me to become rowdy enough to resort to fisticuffs to repel the charge.” But Wright did feel sorry about what he had done. Would it be possible to name a species after Dr. Baker, he asked Gray. “Dr. B. brought to me while I was sick the first specimen of a new sp. of *Leucophyllum*. If it should prove to be new let it be named *Bakeri*.”<sup>58</sup>

Wright arrived in El Paso on September 12, 1849. In a camp about five miles below El Paso, he drafted a letter to Gray summarizing the result of the first half of the expedition.<sup>59</sup> “My list of specimens, seeds, &c. (duplicates included) now amounts to more than 1200 yesterday,” he wrote proudly. As soon as he returned to San Antonio, he wrote, he would contrive the means to send the collections to Gray. Though some 600 miles were still ahead of him, he had begun contemplating how he should reward himself as the expedition concluded. “I remember with pleasure the happy days I spent with you last winter and have a desire to repeat them.”<sup>60</sup>

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<sup>58</sup> CW to AG, [undated; presumably December 1849,] *HL*; Wright also described his conflict with Dr. Baker to John; CW to JW, 12/29/1849, *CWC*. Gray did not follow Wright’s suggestion but named it *Leucophyllum minus*. According to Walt Davis and Isabel Davis, the species is called “Big Bend silver-leaf” in Texas, “a common and attractive staple of the desert landscape of far West Texas.” Walt Davis and Isabel Davis, *Exploring the Edges of Texas* (College Station: Texas A&M University Press, 2010), 42.

<sup>59</sup> But it seems that Wright did not have the chance to send the letter off. The letter still remains in *CWC*.

<sup>60</sup> CW to AG, 9/13/1849, *CWC*.

### **“800 salable species”**

On November 23, 1849, Wright arrived in San Antonio, officially completing an expedition that in total took him 146 days (104 days from San Antonio to El Paso via the Southern Road; 42 days back to San Antonio via the Northern Road). He had pushed himself across more than one thousand miles—mostly on foot (673 miles along the Southern Road and roughly the same along the Northern Road; Figure 5-2). He estimated that he had collected some 3,000 specimens containing some 1,400 species, together with numerous packages of seeds. He remained healthy and ready to embark on the next expedition.

But first things first: He rushed to the Post Office to check whether any letters were waiting for him. He had received no letters during his 146-day expedition either from Gray or from Gray’s botanical friends. To his delight, he found two letters from Gray, including one to Captain French aiming to improve Wright’s status in the troop. Wright felt gratified, although this particular letter was now of no use. In the weeks that followed, he settled himself and composed a series of letters to Gray. He reported his current situation (“ragged, dirty, & probably lousy & with very few dollars in my pocket”), reviewed what had happened to him during the expedition (including his fight with Baker), explained why he had decided to return to San Antonio (“The truth is I was obliged to come back or starve—but I suppose you thought I had been living all this time on Uncle Sam’s bread & butter

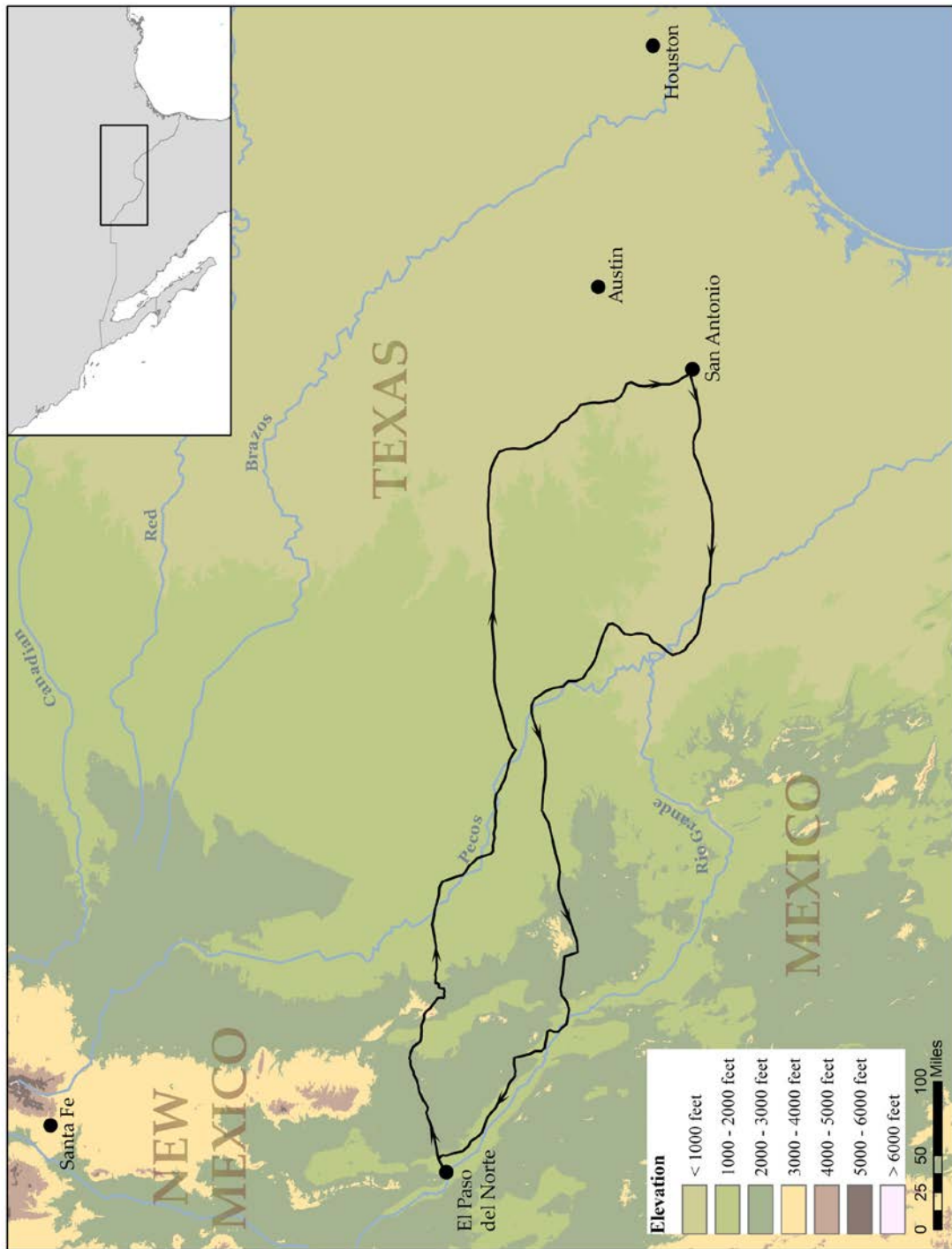


Figure 5-2. The likely route of Wright's 1849 expedition. (The map is based on the maps and geographical information in *SAEP, RR*, and Andrew Wallace, and Richard H. Hevly, *From Texas to San Diego in 1851: The Overland Journal of Dr. S. W. Woodhouse, Surgeon-Naturalist of the Sitgreaves Expedition* [Lubbock: Texas Tech University Press, 2007].)

and saving my money”), and revealed what he had been thinking respecting his future collaboration with Gray.<sup>61</sup>

You may want to know how much money I want. Well \$500 for two years and I don't think I would be willing to go on another campaign like the last and under no more favorable circumstances with less than \$300. Next time I am not going on foot I don't relish the thought of starting afoot on a journey of 1300 miles perhaps including all ramblings 2500 [miles] without a fair prospect of riding a good deal of the way and a good horse or mule would cost from \$60-100.

He also asked Gray to send him more books. “I want the means of studying so that I may hope to become something more than a mere collector.”<sup>62</sup>

Wright found San Antonio a place where “selfishness” prevailed.<sup>63</sup> Exasperated, he packed up his collections and luggage, leaving for San Marcos. There he visited his old friend Colonel Claiborne Kyle and asked if Kyle could offer him free accommodation. He said his stay in San Marcos would not be long. After receiving a new outfit from Dr. Gray, he would be on his way.<sup>64</sup>

Now Wright's remaining task was to deliver his collections to Gray and prepare for the expedition the following year. He understood that he had to proceed fast, for Gray would leave for Europe in June or July. Then Gray's letters came. He learned that Gray could not “distribute—still less study them,” unless Wright's collections

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<sup>61</sup> Wright sent at least three letters to Gray concerning his expedition (two letters were dated 11/25/1849 and 12/6/1849, respectively; one letter was undated, presumably mid-December, when Wright had moved to San Marcos to stay with his friend Colonel Kyle). All three letters are part of the *HL* collection.

<sup>62</sup> CW to AG, 11/25/1849, *HL*.

<sup>63</sup> “[I]f any end is to be gained” in San Antonio, he told John, “it is through money and of that I have but very little.” CW to JW, 11/23/1849, *CWC*.

<sup>64</sup> Wright began his stay with the Kyles on December 8, 1849.

reached Harvard in February or March.<sup>65</sup> Anxiously, he sent off his collection by express, and asked Gray to pay the freight.<sup>66</sup>

Wright felt relieved. To his family, he cheerfully relayed how much he had enjoyed his time spent with the Kyles.<sup>67</sup> He also occupied himself with intensive and extensive activities in the field, in particular deer-hunting and turkey-shooting. “I have killed in the course of the winter half a dozen deer & about 7-8 turkeys and dressed 10-20 deerskins besides collecting some 300-400 species of mosses &[c],” he wrote home with pride.<sup>68</sup> “I have been rather busy of late in killing deer & dressing the skin and Mrs. Kyle is making me a buckskin suit. The pantaloons are complete & she is at work on the hunting-shirt or coat & I am smoking my buckskin for my moccasins—these I make myself.”<sup>69</sup> Wright felt compelled to make an extraordinary outfit for his upcoming expedition. His 1849 expedition had taught him that it was clothing made of deerskin—instead of Indian cotton—that would enable him to survive the rough wilderness of Texas. Wright also wrote to Gray about his daily life. Facetiously, as if to remind Gray of how poor Gray’s daily diet was, he asked if Gray wanted him to address a turkey to Cambridge.<sup>70</sup>

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<sup>65</sup> AG to CW, 1/27/1850, *AGPG*.

<sup>66</sup> See AG to CW, 2/10/1850, *AGPG*, in which Gray announced the arrival of Wright’s collection.

<sup>67</sup> CW to MAW, 8/20/1850, *CWC*.

<sup>68</sup> CW to JW, 3/12/1850, *CWC*.

<sup>69</sup> CW to MAW, 8/20/1850, *CWC*.

<sup>70</sup> CW to AG, 3/9/1850, *HL*.



With his commission to Gray and subscribers completed, Wright felt obliged to undertake some liberal and reciprocal exchanges with Gray's botanical friends. He wrote to Gray in January 1850: "In my last letter to you I enclosed some lichens for Mr. Tuckerman. Tell him that I will put up some more in a few days the names of which I wish him to report to me."<sup>71</sup> In March, he reported that he had crept over some forest grounds and made a collection of mosses, liverworts, and fungi. "I shall separate them into three packages one containing Musci & Hepaticae (intended for Sullivant), one Lichens for Tuckerman, and the other (a small one) Fungi for Bulkley [*sic*; Miles Joseph Berkeley]." He told Gray that he asked for no remuneration except for the three distinguished botanists' suggestions about how to advance his knowledge in these obscure branches of botany.<sup>72</sup> In April, he told Gray that he would soon send some shells to Professor Agassiz. "Would it be too great a favor to ask the Prof. to write to me and enclose pencil & sketches of such things as I have sent him (or a copy of engravings of any should be made) with the names annexed."<sup>73</sup> "My great desire is to improve myself [and] increase [my knowledge] as much as possible," he told Gray.<sup>74</sup>

Wright felt that he could indulge himself with such pleasure-driven undertakings for a while. Although he was now virtually penniless, he thought that he would soon become extraordinarily rich. He had sent off his collections to Gray, and he knew

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<sup>71</sup> CW to AG, 1/[undated/]1850, *HL*.

<sup>72</sup> CW to AG, 3/ 9/1850, *HL*.

<sup>73</sup> CW to AG, 4/11/1850, *HL*.

<sup>74</sup> CW to AG, 3/9/1850, *HL*.

how good Gray was at transforming dried plants into cash (Figure 5-3). As he put it in a letter to John,

You may want to know what I think I have made by my last summer's labor. I collected I suppose at least 800 salable species (exclusion of such as I picked up on old ground) and from 8-12 specimens of each species—that will make say probably 60 sets and at 10 dollars a hundred would be \$800 besides a valuable collection of seeds & living cactuses... [A]s the Dr. is going to Europe and will have opportunity of learning the state of the market for such things and is anxious to keep me at the business of collecting he will doubtless exert himself to effect a sale of as many sets as possible.<sup>75</sup>

**“You northerner!”**

But underlying this idyllic way of life was Wright's rising dissatisfaction with Gray and other metropolitan naturalists. In January 1850, when Gray suggested that Wright “pay some merchants” to get the collections delivered, he exclaimed, “Believe me that I can judge better than you of the case or difficulty of transacting such a business affair as I had on hand in San Antonio.” “If the same business was to be done in Cambridge or Boston or New York,” he wrote, “you could manage it far better than I and you might transact the same business here far better than I could. But I hope you will not enter judgment against me in Cambridge when the court should be held at San Antonio.”<sup>76</sup> In another letter, he protested Gray's suggestion that Wright should send the collections to Cambridge via some stagecoach facilities. “It seems almost impossible to make you who live [in] stage-coach and rail-road facilities comprehend the difficulties I have to contend with.” Gray's suggestion was

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<sup>75</sup> CW to JW, 3/12/1850, *CWC*.

<sup>76</sup> CW to AG, 3/8/1850, *HL*.



Figure 5-3. *Amaranthus fimbriatus* (Torrey) Bentham ex S. Watson, one of Wright's "salable" species. (Courtesy of the Gray Herbarium, Harvard University, Cambridge, MA, USA.)

“conditionally impossible,” he declared, for he could hardly pay “twenty dollars” for transportation.<sup>77</sup>

Besides, Wright thought that Gray had a strong tendency to underestimate his knowledge in the field. “I hope before this you will have learned that I have gumption enough not to ‘pack up the fleshy Cacti with dried plants,’” he told Gray with annoyance.<sup>78</sup> More importantly, he felt hurt when Gray asked why he did not collect enough specimens to make up twenty sets. “Besides in our intercourse face to face & by letter you have never held our prospect of a sale for more than 6-8 sets,” he replied. “Then why [do you] want me to kill myself in collecting more than would be of any use to us? You ought to remember in this hot climate I cannot safely undergo the same fatigue that I could in that of N. England. What I collected last summer ought to be worth 600-800 dollars at the most reasonable estimate and if I am satisfied with that surely you ought to be.” “Now my dear Doctor,” he cried, “if you are not convinced at least give me credit for acting according to the judgment God has given me in good faith towards you & my other patrons and with no unreasonably selfish desires for my own personal benefit.”<sup>79</sup>

But compared to what took place between Gray and Wright regarding the upcoming expedition, such disagreements over numbers of sets were but insignificant skirmishes. Wright, somehow feeling sorry for not being able to reach the mountains as Gray had dreamed, decided not to return to the north, not to see

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<sup>77</sup> CW to AG, 7/1/1850, *HL*.

<sup>78</sup> CW to AG, 3/8/1850, *HL*.

<sup>79</sup> CW to AG, 3/9/1850, *HL*.

his family, and not to study botany at Gray's herbarium, but instead remained in Texas to capture the earliest chance to get himself to El Paso or to Santa Fe. Sure enough, to achieve this meant that he needed a new outfit, sooner not later, for seasons suitable for plant collecting were short (and would be shorter if Wright intended to collect in the mountains), and it still had to take him at least one month to get to El Paso. Wright thought Gray ought to understand how much he had sacrificed and how urgent the whole matter had become.

Late in March, he received a letter from Gray about the new outfit.

Fortunately there is a Boston ship up for Galveston to sail on the 20 inst. I must arrange to send your paper & other things by that vessel, though I know of no one so consigned to there—You ask me to send to Port Lavaca, cosigned to J. Randall; but I think it will be best not to risk this opportunity of sending directly to Texas, which will save time & expense—So please instruct some one at Galveston to look out for a box by the mark Robert Walsh addressed to you—I will send suitable paper & as many of the other things for as far as I can I will look over your letters meanwhile, & see what you have asked for.<sup>80</sup>

Wright felt it extremely unwise for Gray to consign the package to “some one at Galveston.” But before he had time to reply, another letter from Gray came, noting that the package had been sent to “Chas. Wright, Galveston, Texas.”<sup>81</sup> Wright exploded. “I am afraid your neglect to send to a consignee will cause a fatal delay,” he replied. “I gave you sufficient notice early in the winter of the necessity of making timely arrangements for next summer's operations and of the embarrassing delays attending commercial transactions here.”<sup>82</sup>

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<sup>80</sup> AG to CW, 2/10/1850, *AGPG*.

<sup>81</sup> AG to CW, 2/18/1852, *HL*.

<sup>82</sup> CW to AG, 4/11/1850, *HL*.

In April 1850, approximately five months after completing his expedition, Wright had still received no cash for his 1849 collections. Although Gray had authorized him to negotiate a draft in some local banks either on Professor Gray or on the Smithsonian, Wright thought it was impossible to do so. He needed *hard* currency. “My hands are tied so far as making any definite arrangements for hauling is concerned until I receive remittance money,” he told Gray. “It is now too late to write on the matter—if the money is not on the way it need not be sent. I exceedingly regret that I did not go north as I intended.” Helplessly, he told Gray that “I shall go to Austin a few days and will try to sell drafts on you & on Washington though I have no belief that I will succeed. There are few men that understand how a gatherer of weeds & grasses can convert them into money and unless a man has Negroes or land his extensions to mill money are looked upon with suspicion.” For that matter, Wright warned, if Gray received any inquiry from the bank of Austin regarding the draft drawn upon him, “You must honor them or I shall get the character of a villain and I will not be here to vindicate myself.”<sup>83</sup>

“Dr. Gray has certainly acted very strangely with me this year,” he wrote home.

We commenced our correspondence relative to botanical exploration the present year early last winter when I told him he must send me paper & money with all possible dispatch. He had to wait till the arrival of my plants before he could raise any money on them. They reached him early in Feby and he wrote me soon that I must draw on him for money deposited with him. I had however before told him that I did not believe I could negotiate drafts on him & asked him to contrive means to send the money to me. So we have been writing back & forward ever since.<sup>84</sup>

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<sup>83</sup> CW to AG, 4/11/1850, *HL*.

<sup>84</sup> CW to MAW, 7/2/1850, *CWC*.

The event that eventually exhausted Wright's waning patience was the tedious and futile negotiation with the Boundary Commission regarding his appointment as a member. Early in 1849, he and Gray had failed to secure a position in the Commission, for Emory had chosen Torrey's protégé Charles Perry. Then, about a year later, Wright learned from Gray that Congress had appointed a new commissioner, John Russell Bartlett, and Gray had solicited Bartlett to carry Wright along and support Wright's collecting enterprise with necessary facilities. Also, he learned from Gray, Bartlett would bring the check of some \$500 as part of remuneration for the 1849 collections.<sup>85</sup> Delighted, Wright dashed off a letter to Bartlett asking for an interview. Then, having heard nothing from Bartlett, he set off to San Antonio to see if he could catch the silent commissioner there (according to some hearsay). He did not meet Bartlett, but had a glimpse of the extravagantly delicate uniforms of Bartlett's men.<sup>86</sup> Disheartened, he returned to San Marcos and waited for further information. He again sent letters to Bartlett, and again received no reply. And he again set off to San Antonio (again according to hearsay), and this time he found Bartlett had departed for El Paso, probably with the check that belonged to him. Now Wright no longer thought that he could join the Boundary Commission. He wrote home declaring that Bartlett had taken "very particular care not to answer my letter."<sup>87</sup>

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<sup>85</sup> See AG to CW, 5/30/1850, *AGPG*.

<sup>86</sup> CW to AG 10/15/1850, *HL*.

<sup>87</sup> CW to JW, 10/22/1850, *CWC*.

Wright thought he knew why the newly appointed Commissioner had taken “particular care” not to correspond with him. Despite his enthusiasm to join the Commission as a plant collector, he told Gray, Bartlett “had some favorite of his own to appoint a fat office—Dr. [George] Thurber.” “You may have the benefit of Dr. Thurber’s collections, but I question whether he will climb hills and wide ditches and pierce thickets as I would do.” In fact, he added, Thurber would probably “endanger his broadcloth” if Thurber decided to botanize in Texas with such an unsuitable outfit. How could such an unqualified man occupy a position in the Commission? Wright muttered darkly. He felt himself being thrown into a “vast desert of human selfishness,” and an oasis of generosity and sympathy remained nowhere to be seen.<sup>88</sup>

Wright’s despair at his inability to join the Boundary Commission was deepened further when he realized that there were actually *three* botanical collectors on the commission. Besides Parry and Thurber, a Dr. John M. Bigelow had joined the commission as a surgeon-collector under the patronage of Sullivant. Wright could not help wondering if he had chosen the wrong patron; or, as he groaned to Gray, “Perhaps also I put too high an estimate on my labors.” It seemed to him that none of his correspondents had taken his labors seriously. “Some bad luck must befall my attempt at correspondence or he [Sullivant] does not notice a small fig like me,” he told Gray.<sup>89</sup> Also, he noted, “I can not but be vexed when I think of Prof. Agassiz sending [me] so useless a thing as that catalogue of shells.” Agassiz apparently

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<sup>88</sup> CW to AG 10/15/1850, *HL*.

<sup>89</sup> CW to AG, 3/9/1850, *HL*.



treated him as a specimen dealer of some sort. Receiving such a disagreeable reply, Wright told Gray, he simply packed a box full of weeds and sent it back.<sup>90</sup>

In October 1850, almost a year had passed since Wright had completed his expedition, and he still had received no money or outfit. He became uncomfortable with the fact that he had been staying with the Kyle family for so long without paying rent of any kind. So he mobilized his network to see if he could get a job. In late October, Wright told John that he had agreed to serve as a family tutor for a Mrs. Millet for four or five months, for \$25. Mrs. Millet was a keeper of a “public house” in New Braunfels, he wrote.<sup>91</sup>

With the new job secured, he composed a letter to Gray declaring that he could no longer collect plants for him. He was now preparing to “settle down once more at my profession—pedagogism.” “You will probably be surprised and vexed at the unfortunate result of all our plans & negotiations,” wrote Wright, but what he had experienced during the past year altogether suggested that he should avoid having anything to do with Gray. He had become an old man, he wrote self-pityingly, or, to be precise, “an old bachelor.” He had met Lindheimer in New Braunfels, he wrote, and found that this collector-turned-newspaper-editor was now enjoying an agreeable domestic life. In comparison, Wright said, he had no wife, no income, no support, and not even a plan for what he should do for the year of 1851. He found

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<sup>90</sup> CW to AG 10/15/1850, *HL*.

<sup>91</sup> CW to JW, 10/22/1850, *CWC*.

this “vexatious suspense” had prevented him from “putting off old-bachelorism.” He thought it was all Gray’s fault.<sup>92</sup>

The resentment for his current miserable circumstances provoked Wright to make some remarks that he knew would irritate Gray. Wright, though always calling himself a Yankee, was emotionally attached to and politically inclined to the South. He paid close attention to the compromise advocated by Senators Henry Clay and Stephen Douglas in hopes of solving the rising confrontations between the North and South respecting slavery. He found the final result vicious. In his opinion, Henry Clay was but a “miserable straddling fenceriding politician,” who should “go to New York and be married to Seward [William Henry Seward] & show himself in his true color.” As for Stephen Douglas, he thought Douglas should go ahead and “sleep in his life with some big buck-nigger.” To him the so-called Compromise compromised nothing: “the South as usual gave up all & the North gained all.”<sup>93</sup>

Wright knew that Gray was an abolitionist sympathizer, and he hated abolitionists. In the letter that accused Gray of indifference, Wright wrote that he recently came across “a list of things some persons had never known,” and one of them was that “I never have known a political (& I say religious) abolitionist that would put a Negro in his best bed.” Wright found this statement amusing and acute. “I do wish all of them could be made to sleep with a big buck nigger every night till (and it would not be long) they repented of their (political) iniquities.” Wright could not help connecting his current sufferings as a result of Gray’s inattention with the

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<sup>92</sup> CW to AG, 10/15/1850, *HL*.

<sup>93</sup> CW to JW, 12/17/1850, *CWC*.

North's lack of responsibility in the rising tensions with the South. "You northerners (I mean only political abolitionists) may think you are doing right but you know little of the trouble of which you are laying the foundation."<sup>94</sup>

After finishing this "you northerners" letter, Wright composed another letter to Secretary Henry of the Smithsonian expressing his desire to collect natural-history specimens for Henry's honorable institution. He thought that Secretary Henry might be a more responsible patron than Dr. Gray. He then put the two letters in one envelope and consigned it to the Smithsonian. He packed up his luggage, gathered his deerskin pantaloons, said goodbye to the Kyles, and moved to New Braunfels.

**"I have already done the best I can for you"**

October 23, 1850. Pontrilas House, Herefordshire, Britain.

Gray had just finished reading Wright's letter about how a constellation of unpleasant circumstances had tremendously delayed the presumed 1850 expedition. He was dispirited. He could not help recalling how much he had done for Wright. He could hardly believe all his labors were to be in vain.

Since Wright left him in February 1849, Gray thought, he had been working relentlessly to smooth Wright's expedition. He had answered *every* letter of Wright's. He had cleared Wright's debt with his own money (\$150). He had "written a line to Capt. French, letting him know that we all feel a great interest in your welfare, and scientific success." And he had persuaded Henry of the Smithsonian to advance \$150 to support Wright's expedition, with the promise that he himself

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<sup>94</sup> CW to AG, 10/15/1850, *HL*.

would prepare a full set for the Smithsonian, study the collection, and publish the result in *Smithsonian Contributions to Knowledge*. Then, learning that Wright was unable to reach high mountains, and probably would return north and stay with him, he had replied that Wright might rely upon “every kindness and assistance” he could render, although “Mrs. Gray’s health is still so delicate, and an invalid cousin is to spend the winter.”<sup>95</sup> Afterward, when Wright reported that he had quarreled with Baker and eventually had knocked this gentleman down, he had applauded, “I must say that I think you behaved very well indeed under the precautions of Dr. B. and I do not blame you for striking him!”<sup>96</sup>

When Wright settled down in San Marcos, the botanist and the collector at last resumed their regular correspondence. Gray, in the face of Wright’s irascible temper, rough manner, and total ignorance of appropriate epistolary styles, did his best to soothe Wright. Gray suggested that Wright should not “depend too much on Agassiz.” Professor Agassiz was “overwhelmed with work,” he wrote, and “It is quite impossible to get him to give names and drawings of shells &c.” As for Sullivant, he wrote, “I think, [Sullivant] must have written to you; but whether or not you can always depend on him; he will always pay well for collections of mosses when they come to hand.”<sup>97</sup>

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<sup>95</sup> AG to CW, 8/23/1849, *AGPG*; AG to CW, 11/24/1849, *AGPG*.

<sup>96</sup> AG to CW, 1/2/1850, *AGPG*.

<sup>97</sup> AG to CW, 5/30/1850, *AGPG*. Gray did inform Sullivant about Wright’s complaints. To it Sullivant replied, “I have opened a correspondence with Texas Wright as you call him—and have proposed to him to collect for me. Among the mosses from him under cover of yours a few weeks since is a fine new *Lerkea* which I shall first apparently publish as *L. Wrightii*—” WSS to AG, 2/8/1850, *HL*.

As for Wright's complaint about not receiving Gray's package for the next expedition, he felt as regretful and resentful as Wright. The package he sent to Wright in February 1850 contained drying and wrapping papers of great quality and quantity. He also put in the package what he thought might be of interest and of use to Wright, including James D. Dana's *Mineralogy*, Benjamin Silliman's *Chemistry*, and Spanish grammar books, together with two magnifying glasses, two yards India Rubber cloth, and one large India Rubber Camp blanket. He felt that he had done his best for Wright's upcoming expedition. As he told Wright, "I hope you will think I have done pretty well, considering,—since I began to make preparations—my wife's cousin has lain very sick in the house, where her corpse now lies."<sup>98</sup>

As for the transportation of Wright's collection to Cambridge, indeed, he had suggested that Wright pay some merchants or hire stagecoaches to deliver the collection to Cambridge.<sup>99</sup> But the rationale behind this suggestion was that he needed to receive Wright's collections as early as possible. He had agreed to take charge of the botanical collections made during the U.S. Exploring Expedition. To perform this duty satisfactorily, he had persuaded Captain Wilkes to pay him stipends and traveling expenses for a research trip to Europe. Early in 1849, Gray signed a contract with Captain Wilkes and planned to leave for Europe by April or May 1850. He would then stay in Europe for one year and work out the botanical collections of the Exploring Expedition.<sup>100</sup> With this enjoyable but still tremendous

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<sup>98</sup> AG to CW, 2/18/1852, *AGPG*.

<sup>99</sup> See AG to CW, 1/2/1850, *AGPG*.

<sup>100</sup> See AG to GE, 2/25/1849, *LAG*, 362.

duty ahead of him, he naturally urged Wright to send off the collections in the most prompt manner. After all, after receiving the collections, he still had to unpack Wright's specimens, sort them according to species, assign each of them a distribution number, group them into sets, study them, make the catalogue of them, and distribute the sets and the catalogue to subscribers.

Finally, Gray was concerned at Wright's accusations that he had delayed cashing the 1849 collections. The fact was that he deemed it inappropriate to ask Wright's subscribers to pay the fee or advance any money until he had the chance to study Wright's specimens. First, Wright was still a new face in the world of botany, and his collections made along the route from San Antonio and El Paso would have to compete with those made by Lindheimer and Fendler in the specimen market. As the botanist in charge of the subscription system, he had to ensure that Wright had made specimens of good quality so that subscribers' fees would be justified. Second, as his experiences in charging for Lindheimer's and Fendler's collecting expeditions had shown, a collection without a carefully studied catalogue deserved no good price. That is to say, he needed to undertake a thorough examination on Wright's collections to better estimate their scientific and monetary value. "I hope to get this early [in] autumn that I may turn the plants to good account for you," Gray wrote to Wright in May 1849. Somewhat worryingly, he added, "Lindheimer's collection for the last four years are now about to be distributed by Engelmann—So that the market is filled with more Texan plants."<sup>101</sup>

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<sup>101</sup> AG to CW, 5/7/1849, *AGPG*.

Late in 1849, when Wright's collections arrived, Gray immediately looked them over and estimated the number of "salable species." From Wright's field notes and letters, he understood that Wright's expedition probably had yielded some 1,200 species. But he did not think that all 1,200 species were salable. Aside from the fact that Wright did not collect enough specimens for some species, he explained to Wright, "as to distributed sets, we have, it seems to cut off 300 nos from the beginning, for it was no part of our plan to collect from Galveston to San Antonio—the very ground that you & especially Lindheimer has gone over before—Lindheimer's sets of these plants fully supply the market at \$8,—and we cannot put them into yours for \$10." "It should do more harm than profit to the sale of the plants by putting them into the sets (1-308) excepting when they are not in Pl. Lindh." he asserted to Wright.<sup>102</sup>

He began looking for possible subscribers. In March 1850, he wrote to Charles Short inquiring if this independently wealthy physician-botanist wanted to join Wright's subscription list. "Wright's plants will not be very numerous I find," he wrote, "not more than 4 or 500 species after the very first sets, and not more than 12 sets in all. But they are extremely good plants and with considerable novelty. I can have a set for you as soon as distributed, notify you of the number and amount, and you can remit to Sullivant the money; for I shall ask him to look after Wright's pecuniary affairs a little while I am away."<sup>103</sup>

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<sup>102</sup> AG to CW, 1/2/1850, *AGPG*.

<sup>103</sup> AG to CWS, 3/21/1850, Coker, "Letters...", 108.

Short agreed to join Wright's subscription list. In effect, with Gray eagerly spreading the news that Wright had collected many "good and novel" plants, more and more botanists and institutions exhibited interests in subscribing to Wright's 1849 collections. Finally, Gray found that he needed to prepare 20 sets for distribution. Here he encountered a problem. After a close examination of Wright's 1849 collections, he realized that Wright had not collected enough specimens to make up 20 sets. He made this concern felt to Wright, but soon received the collector's harsh reply. Baffled, he decided to adopt a rather extraordinary way of distributing Wright's 1849 collections. That is: although the "selling point" of Wright's 1849 collections was the specimens collected along the route from San Antonio and El Paso in the summer and autumn of 1849, he looked over Wright's other collections (made in Houston, Galveston, Eagle Pass, Austin, and so on), selected appropriate specimens, mingled them with the specimens made in 1849, grouped the specimens into 20 sets, and distributed the 20 sets *as if* all the sets were made during Wright's 1849 expedition. Gray's principle was that it did not matter when or where Wright collected the *specimens*. As long as the specimens represented the *species* distributed in the region between San Antonio and El Paso, he thought it was acceptable to include such specimens in the sets.<sup>104</sup>

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<sup>104</sup> My observation derived from the correspondence between Gray and Wright agrees with Ivan M. Johnston (1898–1960)'s study based on Wright's field notes. Johnston was a botanist affiliated with the Gray Herbarium, specializing in the flora of the American South. In the late 1930s, he was confused by Gray's overly simplistic descriptions of localities, habitats, and other details in *Plantae Wrightianae Texano-Neo-Mexicano: Part I & II*, and delved into Wright's field notes (preserved at Gray Herbarium) for further information. Johnston never published his results. The typed manuscript, under the title "Field Notes of Charles Wright for 1849 and 1851-52," is



Gray understood how inappropriate it was to distribute specimens in such a manner (entirely ignoring the issues of locality and temporality of every specimen). In a letter to Wright in May 1850, he noted that he had been forced to adopt a rather embarrassing strategy to make up 20 sets for distribution. He added a reminder to Wright about how he should proceed in the following expedition (certainly a reminder to himself, as well): “Here after do not send those collected in Texas or any formerly collected as they deteriorate your sets. Every one of our subscribers have them from Drummond’s and Lindheimer’s [collections] before. It was not well thought of to send that Texan bundle, as they are of no use, except that I have used a few to eke out other sets.”<sup>105</sup>

Then he divided this enlarged collections into four groups. The first group was for those who would receive the full set of 828 species, which included himself, John A. Lowell, and the Smithsonian. The second group was for those who would receive

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now preserved at Gray Herbarium. Johnston’s conclusion about how Gray dealt with Wright’s field notes is worth quoting here:

The field-numbers assigned the collections by Wright... were disregarded by Gray when he made up Wright’s plants into sets. The numbers on the labels of Wright’s specimens in herbaria belong to a new series assigned them by Gray, the set of distribution-numbers. Not only did Gray ignore Wright’s field-numbers but he also frequently united and distributed under a single distribution-number, two or even more of the collections which Wright had collected under different field-numbers, frequently at distant stations, and at different seasons. If Gray thought two or more of Wright’s collections represented the same species and if there was any advantage in uniting them, he did so regularly without scruples.

“[A]s a general rule,” Johnston comments, Gray “made little or no effort to preserve Wright’s field-ticket, or to note the proper field-number on the herbarium-label of these collections.”

<sup>105</sup> AG to CW, 5/30/1850, *AGPG*.

less complete sets, including George Bentham (680 species), William Harvey (690 species), William Hooker (650 species), Benjamin Greene (631 species), the British Museum (519 species), P. Barker Webb (453 species), and Elias Durand (498 species). The final group was for Torrey and Engelmann, who had either contributed to Wright's outfit or agreed to help Gray prepare the catalogue for Wright's 1849 collections: Torrey would receive a set of 433 species for his contribution to Wright's outfit (drying paper), and Engelmann would receive "a number of supernumeraries and plants" that Gray wanted Engelmann to study. Finally, upon Wright's request, he also prepared a set containing 410 species for Alphonse Pyramus de Candolle in exchange for de Candolle's *Prodromus Systematis Naturalis Regni Vegetabilis*.<sup>106</sup>

Then, although the catalogue for Wright's collections was not yet ready, Gray persuaded Wright's subscribers to pay the money in advance. He soon received \$513.13, and expected some \$379.11 to come. Then, he contacted the Smithsonian to have Congress issue a check for Wright. He also contacted the new appointed Boundary Commissioner Bartlett to secure Wright's position and had Bartlett agree to deliver the draft to Wright in person. Having been troubled by the loss of the package that he had so carefully prepared for Wright, he could not have allowed similar accidents to occur again.<sup>107</sup>

The last thing Gray did for Wright before he set sail for Europe was to prepare a volume titled *Plantae Wrightianae Texano-New-Mexicanae*. He had arranged it to be

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<sup>106</sup> AG to CW, 5/30/1850, *AGPG*.

<sup>107</sup> AG to CW, 6/7/[1850], *AGPG* and AG to CW, 6/9/[1850], *AGPG*.

published as part of the *Smithsonian Contributions to Knowledge* series. Although by definition the volume was but a catalogue for Wright's 1849 collections, he expected its publication would further his botanical enterprise in particular and American botany in general. First, he expected that *Plantae Wrightianae Texano-New-Mexicanae* would please the Smithsonian enough that this prestigious institution would be more and more willing to "patronize" Wright's and other collectors' expedition under his charge.<sup>108</sup> Second, in return, he planned to help the Smithsonian establish "an important and authentic North American herbarium." As he put it in the volume's introduction,

A full set of the plants here enumerated or described is retained in my own herbarium; another becomes the property of the Smithsonian Institution, which has efficiently patronized this exploration. It will there form, with similar sets of the collections of Fendler and Lindheimer, made in new Mexico and Texas, the nucleus of an important and authentic North American herbarium, destined to be enriched, I trust, by continued accessions, especially from our newly-acquired territories, until it shall comprise representatives of our whole flora, and specimens of all the vegetable products of our wide country.<sup>109</sup>

Early in June, Gray set sail for Europe with 13 large boxes.<sup>110</sup> In addition to the botanical collections of the Exploring Expedition, he also carried Wright's 1849 collections with him. He managed to study these collections at major herbaria in Europe for one year, during which time he would gather sufficient information for a botanical report for the Exploring Expedition and for *Plantae Wrightianae Texano-New-Mexicanae*. In the year to come, he would be a commissioned botanist in charge

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<sup>108</sup> He had arranged for Wright to be placed under "the patronage of the Smithsonian Institution," Gray told Wright. AG to CW, 8/23/1849, *AGPG*.

<sup>109</sup> Gray, *Plantae Wrightianae Texano-Neo-Mexicano, Part I*, 5-6.

<sup>110</sup> JT to JH, 6/20/1850, *PJH, Vol. 8*, 62-63.

of the enormous collections made during the famous United States Expedition Exploration, as well as a humble salesman, carrying a catalogue with him, knocking on the doors of potential customers and asking, "Are you a subscriber to Mr. Wright's Texan and New Mexico expedition?"<sup>111</sup>

With such memories flashing through his mind, Gray made a decision. He asked his wife to come to his side. He would like to write a letter to Mr. Wright, he said, and he needed Jane to serve as his amanuensis. "My dear Mr. Wright," he began.

I received today a letter forwarded to me from you, dated Aug. 30th—from which it seemed you have been doing nothing all summer; & have not received, as I can learn, any of the various necessities which I forwarded to you before my departure of June 11th. Among them were a certificate from the Smithsonian Institution, a recommendation from the Secretary, &c &c. And I also give you permission to draw on Prof. Henry at Washington for \$379.11.

You say you have not written to Prof. Henry, & I think I told you in my letter that you were to draw on him for the money left in hands for you. If you have made no application for it he probably keeps it until you applied. Let me know whether you received the letter containing your account, the amount to your credit with Prof. Henry.

Mr. Sullivant has written once to you, & the reason he does not answer your letters, is probably that you give him no idea where they should be directed.

If you look at my letters you will see that you dated my return from Europe a year too soon. I do not expect to be in Cambridge again until the end of next August. I can have nothing to do with your arrangements—If you make collections, I will endeavour to dispose of them, but your plans you must decide for yourself—According to the arrangement I made for you, if you made good collections you could apply for advance from the Smithsonian Institution, but you must correspond with Prof. Henry about that.

About sets which I take.—if you should make only a small collection from a new country I should be very willing to buy a set of you. But when I take collection to distribute & describe, according to all precedents & in some small recompense for the labour, a set belongs to me—It is not worth your while to collect, except when you can collect 8 or 10 sets.

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<sup>111</sup> Gray would later inform Wright that Wright's collections had been "much prized abroad—that I have a complete set of De Candolle's *Produmus* for you—from De Candolle—and that, as I wrote you particular of, I have \$175 dollars of yours in my hands.—I will soon communicate with your brother, and pay over to him, as you direct." AG to CW, 1/23/1852, *AGPG*.

I have already done the best I can for you & must decline for the present anything more to do with your affair, other than the disposing of collections you may send me, & forwarding to you the money.

I feel so uncertain how & when this letter will reach you, because I suppose you have gone on to New Mexico with the Boundary Survey, & thence into Great Salt Lake basin.—If you have not, & still remain in Texas, you must I think have given up entirely the idea of further excursions, which perhaps will be insist: for time are not promising here for selling plants.

With all my desires I fear I can not assist you as you would wish—& certainly I can give no directions nor take charge of them &c. while away in a distant country.<sup>112</sup>

Then he signed his name, and sent it off.

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December 20, 1850, New Braunfels, United States.

Wright had just finished reading Gray's letter dated October 23, 1850. "At times," he immediately replied, "I can hardly repress a smile, when I think of our sanguine expectations & large calculations for the year now expiring." Gray's letter, wrote Wright, reminded him of "the verse of the poet: parturiunt montes, &c.—ridiculus mus." If Gray felt hurt by what he had said in previous letters, he wrote, "I beg ten thousand pardon (or more if you please)."

Wright then developed a long argument aiming to overthrow every point Gray had made. Regarding Gray's insistence that the package must have had arrived somewhere in Galveston, and it was Wright's responsibility to retrieve the package, he fought back: "Now, what more could I do in such a case? Write to all the merchants of Galveston?" Regarding Gray's words that Sullivan's letter to Wright must have had been lost somewhere between Columbus, Ohio, and San Marcos,

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<sup>112</sup> AG to CW, 10/23/1850, *AGPG*.

Texas, he replied that the “excuse” was “a lame one,” for he had been careful about tracking his correspondence. Finally, he expressed his overall disappointment about Gray’s incapability to serve as the botanist in charge of the subscription system:

You write, “I really, have done the best I can for you, it must decline, for the present, any thing to do with your affairs, other than this disposing of collections you may send me & forwarding you the money.” If it was an offence in me, vexed as I was, about not hearing of the paper, books, &c. to think & say that you could have been a little more particular in the shipment of them, I must plead guilty, & I don’t know anything better, now that the delay has resulted in no great misfortune...

“I am not eager to be rich,” Wright wrote bitterly, “or I would not be running about the wildness to please myself & others.” Looking back, he added, “If twenty-five men were willing to live as cheaply as myself, and, thus, give me what they would save, two or three years, I would not need to ramble any more; and, at the end of the time, perhaps we would all be happier.”<sup>113</sup>

Wright decided that he wanted to be a happier man. He finished up the letter and addressed it to the Smithsonian. He did not know Gray’s whereabouts. He didn’t care. The year of 1850 was almost over, and Wright thought his collaboration with Gray should be over, too.

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<sup>113</sup> CW to AG, 12/30/1850, *HL*.

## CHAPTER SIX

### “A Complete Catalogue of Species of the United States”

Early in 1851, George Engelmann of St. Louis received a letter from Mrs. Gray. He read it with interest. He had rarely heard from Professor Gray since the Grays went to Europe. Among several issues that he was eager to discuss with Gray was how to deal with Wright’s increasing number of specimens. He had agreed to take responsibility for enumerating certain families for Wright’s catalogue to be published by the Smithsonian and distributed among Wright’s subscribers. Gray had announced this publication project roughly two years before, but had been silent about the subject for the past year. “Dear Dr. Engelmann,” Mrs. Gray’s letter began,

Dr. Gray has commissioned me to write to you for him to tell you of the receipt of your last letter and his hope to answer it in full before long.—He met with a very serious accident five weeks ago, from which he has now almost recovered though not yet strong or able to move with his usual activity. After a long and pleasant visit at Pontrilas in Herefordshire, with his friend Mr. Bentham, we were on our way to make Dr. Harvey and his friends a short visit in Dublin, when attempting to cross in the steamboat on reaching Holyhead, Dr. Gray walked down the hatchway, which was opened to receive the luggage. It was quite dark, and there was nothing to warn him of the dangerous neighborhood. He fell about 18 or 20 feet, and providentially escaped with only 2 broken ribs; but was very much bruised. He was taken on shore at Holyhead, where he was detained a week, and at first suffered very much from lameness and soreness, and the bruises; besides the great shock to the system from such a fall. But he has a wonderfully recuperative constitution; in 8 days he was able to move over to Dublin, where he rallied very fast under the care of kind friends.—Dr. Harvey came over to Holyhead when he heard of the accident, and devoted himself to his care. After 2 fortnights in Dublin, we were able to travel slowly on to London, and here to Kew, where Sir William Hooker had kindly engaged lodgings for us, where we have been some 10 or 12 days. Dr. Gray spends all his daylight in Sir William’s Herbarium and is at present studying Wright’s collection. And he wishes you to send him as soon as possible, the Mss. of Portulacae and Cactaceae Wrightianae.<sup>1</sup>

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<sup>1</sup> JG to GE, 1/3/1851, *AGPG*.

News of Professor Gray's unfortunate accident in Dublin soon spread in botanical circles on the American side of the Atlantic. The question of what would happen to Gray's protégé "Charles Wright of Texas" became a public affair that demanded American botanists' attention. Who could take care of the poor fellow who had almost sacrificed everything for the sake of science?

Meanwhile, John Torrey of New York received a letter from Professor Gray. He was glad. For the past several months, he had been tormented by interventions from those "big folks" in Washington, DC, respecting the style of the Ex. Ex. Botanical Report. (Torrey was responsible for examining the portion of the Ex. Ex. collections made in California and Oregon, as well as for assisting William Dunlop Brackenridge, the Ex. Ex. botanical collector, to complete a monograph on the Ex. Ex. collections of ferns). In August 1850, when Gray was away in Europe, he ran into Captain Wilkes in DC and conversed briefly with the Captain. During the conversation, Wilkes remarked that he had seen part of Brackenridge's manuscript and was unsatisfied with its style. Evidently, complained the Captain, Torrey had "deviated from what the Committee had adopted": namely, "what in the Nat. History part of the work of the Ex. Ex. the specific Char. should be printed in both Latin and English—so that every one could read and understand the work."<sup>2</sup> Torrey was taken aback. He sent a letter to Brackenridge to make sure whether or not Wilkes was serious about the style—the English translation of the Latin specific characters would make the Ex. Ex.

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<sup>2</sup> Torrey reported the conversation in a letter to Brackenridge; see JT to WDB, 8/27/1850, Folder 5, Box 1, *WDBP*. The quotation of the principle settled by the Library Committee is from WDB to JT, 10/17/1850, Folder 3, Box 1, *WDBP*.



Botanical Report look unlike any publication in botany. Brackenridge replied to confirm the matter.<sup>3</sup> Torrey exploded. He sent a letter to Henry of the Smithsonian declaring that he expected to have “a blow up with Capt. Wilkes & his man Drayton,” because the style stipulated was “simply puerile.” “I have written to Drayton that I will give up my volume—& turn over what I have done to Dr. Gray rather than be dictated to by persons who are unqualified to judge in the matter.” To Brackenridge, he emphasized that this seemingly abrupt movement would not put an end to the whole affair. “If they accept my proposal, they will have to fight the battle all over again with Dr. Gray—who you may be quite sure, will not yield one inch beyond my demands.”<sup>4</sup>

With such unpleasant memories flashing across his mind, Torrey was delighted to find that Gray firmly sided with him. One paragraph in Gray’s letter particularly called his attention:

I have no objection to write my technical descriptive matter, or supplemental descriptions, where there is any, in English—tho’ in a work only to looked at by Botanists, and of a strict Scientific Character—not at all ad populum,—it would be more useful in Latin. But surely I will not do so silly a thing as to write out English translations of the Latin Specific Characters.

He also noticed Gray’s remark at the end of the letter: “You are at liberty to forward this hasty line to the Library Committee.”<sup>5</sup>

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<sup>3</sup> JT to WDB, 8/27/1850, Folder 5, Box 1, *WDBP*; WDB to JT, 10/17/1850, Folder 3, Box 1, *WDBP*.

<sup>4</sup> JT to JH, 11/1/1850, *PJH*, Vol. 8, 114; JT to WDB, 12/16/1850, Folder 5, Box 1, *WDBP*.

<sup>5</sup> AG to JT, 1/31/1851, Folder 6, Box 1, *WDBP*.

So Torrey forwarded Gray's letter to Brackenridge with a notice: "if you like, it can be shown to the Library Committee."<sup>6</sup> Receiving Gray's letter, Brackenridge promptly copied it and addressed the copy to the Library Committee. "It was perhaps better that the Committee should now be made aware of the Doctor's opinion on the style," he wrote to Torrey with satisfaction.<sup>7</sup>

Captain Wilkes was furious on reading Gray's letter. He wrote to Torrey exclaiming that the Ex. Ex. Botanical Report was prepared and published "not for any class of scientific Men," and he was "truly surprised that Dr. Gray should venture at this day to express such an obsolete notion." He declared that if Torrey and Gray insisted on carrying on such "Anti-American notions" as packing the Ex. Ex. Botanical Report with texts in Latin with no corresponding English translations, he would stop the two botanists' "rations." James Alfred Pearce, Chair of the Library Committee, felt troubled by Gray's letter, as well. "[T]ell me candidly," he asked Henry of the Smithsonian, "if there is any thing in the requirement of the comee [committee] that the botanical work shd contain English as well as Latin descriptions which merits justifies the term 'silly' and the resistance of these learned gentlemen."<sup>8</sup>

"The threat of the Capt. as to stopping my rations does not disturb me in the least—as I do not care a snap of the figures about them," Torrey told Brackenridge.

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<sup>6</sup> JT to WDB, 2/26/1851, Folder 5, Box 1, *WDBP*.

<sup>7</sup> WDB to JT, 3/24/1851, Folder 6, Box 1, *WDBP*.

<sup>8</sup> Quoted from Stanton, *The Great United States Exploring Expedition of 1838-1842*, 336; JAP to JH, 4/30/1851, *PJH*, Vol. 8, 181.

“It is my intention to work on the Expl. Exped. collections for the present, at least, & to wait till Dr. Gray returns, when I shall have a forceful article drawn up & signed by the principal naturalists of the country—to be submitted to the real Library Committee. This I tell you in confidence.” Although he was unsure what steps Gray would take, wrote Torrey, he had little doubt that Gray was “quite able to fight his own battle” and would “never yield” to what he did not “approve of.”<sup>9</sup>

Gray, though not entirely unaware of the storm that his letter had induced, devoted his full attention to finishing two of his most pressing tasks: a catalogue for Wright’s 1849 collections and the Ex. Ex. Botanical Report. To him the two issues were in entirely different realms—he prepared the catalogue for Wright’s subscribers to keep Wright in the field and ensure he would be remunerated appropriately, whereas in the case of the Ex. Ex. Botanical Report, he was a contracted man of research responsible for securing scientific credit that was due to the United States of America. But no matter how different these two tasks were, Gray thought that they should meet the highest standards in botany: that is, he would apply a broad view to lump species together, and his judgment would be made on the basis of his studies undertaken in distinguished botanical institutions across Europe. He expected that he would then raise the quality of the two scientific works to an unprecedented level in American natural history. Although he was still recovering from the accident that had broken his ribs, he was energetic, confident, and optimistic.

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<sup>9</sup> JT to WDB, 3/28/1851, Folder 5, Box 1, RU7189.

He hardly foresaw how violent the storm would become as he presented the two works to the scientific circles of the United States.

**“The poor fellow”**

In May 1850, when Gray was about to set sail for Europe, he asked if Sullivant could take care of Wright.<sup>10</sup> “I wish to see him keep collecting,” Sullivant replied, “but would decline the management of his finances as you suggest (want of time)—I will however advance to the order of Engelmann or any agent you may select \$50 to keep him in the field.”<sup>11</sup> But Sullivant soon learned that he could not even contribute such limited attention to Wright’s welfare. In August 1850, cholera broke out in Columbus and its vicinities, claiming several victims, including Sullivant’s wife. “The most admirable of women my dear wife is no more,” he mourned in a letter to Gray.<sup>12</sup>

Torrey emerged to serve as Wright’s caretaker. Torrey had seen Wright’s specimens and found their quality acceptable.<sup>13</sup> During Gray’s absence, he had established regular correspondence with Wright, and had learned of Wright’s difficulties in acquiring sufficient money to continue the expedition. Torrey thought that his and Gray’s common friend Charles Short might be a great sponsor for

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<sup>10</sup> See AG to CWS, 3/21/1850, Coker, “Letters...,” 108.

<sup>11</sup> WSS to AG, 5/27/1850, *HL*.

<sup>12</sup> WSS to AG, 11/3/1850, *HL*; but according to a note Sullivant left at the end of the letter, he had not sent off the letter until October 12, 1851.

<sup>13</sup> See JT to AG, 6/18/1850, *HL*.

Wright's collecting enterprise. He knew that Short had recently taken possession of some properties in Philadelphia worth half a million dollars. This remarkable fortune was left by Short's brother, he also learned, and Short would be glad to subscribe to any botanical expedition that Torrey recommended. "Are you a subscriber to Mr. Wright's Texan and New Mexico expedition?" he soon wrote in a note to Short. "Several gentlemen made up a purse for him, but the poor fellow is getting quite low in funds, and I, for one am unable to do any more for him."<sup>14</sup>

Short agreed to join Wright's subscription list. He sent Torrey a check for \$100 and asked Torrey to forward it to Wright. "You have been extremely liberal to Mr. Wright, and in thus aiding him have done much for North American Botany," Torrey replied. "Although I do not think you have contributed so handsome a sum to get an equivalent in dried plants, I have no doubt Mr. Wright will send you sets of his choicest specimens."<sup>15</sup>

Besides Torrey, another figure who stood up to pave the way for Wright's expedition was Spencer F. Baird, the newly appointed Assistant Secretary of the Smithsonian (Figure 6-1). A professor in zoology at Dickinson College before joining the Smithsonian, Baird spent his spare time studying botany, and was knowledgeable enough to publish a volume titled *Trees and Shrubs of Cumberland County* (1845; coauthored with William M. Baird). In 1846, he sent Gray some fresh

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<sup>14</sup> See JT to AG, 8/27/1850, *HL*; JT to CWS, 8/24/1850, Coker, "Letters...", 110.

<sup>15</sup> JT to CWS, 9/21/1850, Coker, "Letters...", 111.



Figure 6-1. Spencer F. Baird in 1845. (Courtesy of the Smithsonian Institution Archives, Washington, DC, USA.)

specimens of flowering Papaw, which delighted Gray greatly.<sup>16</sup> In the years to come, the two professors' correspondence flourished. Baird helped Gray acquire some living specimens of *Vaccinium* for the Harvard Botanical Garden and introduced his correspondents to Gray.<sup>17</sup> In 1848, when Gray's *A Manual* was published, he did whatever he could to increase its sales. "Your Manual is a glorious book," he told Gray. "Every man with the least smattering of Botanical, & Horticultural love, I make buy one, and lest they should fail to do so, I generally offer to procure it for them which I in most cases have to do."<sup>18</sup>

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<sup>16</sup> SFB to AG, 5/4/1846, *HL*. Baird's specimens were "servicable [serviceable]," Gray replied. AG to SFB, 6/1/1846, Folder 4, Box 23, *SFBP*.

<sup>17</sup> See, for example, AG to SFB, [dated "Thursday;" postmarked 10/22/1846,] *SFBP*. Baird, in a letter sent late in 1849, introduced Gray to "a very energetic correspondent" and "zealous botanist" living in Anderson County, South Carolina: "Should you wish any thing from that neighborhood let me know and I will promise that you shall have it. She (for it is a lady) is one of my best zoological contributors; I received only a few days ago a tin can full of fish and reptiles from her." SFB to AG, 11/3/1849, *HL*.

<sup>18</sup> SFB to AG, 5/12/1848, *HL*.

Gray, in return, helped Baird initiate a lasting and mutually beneficial collaboration with his new colleague at Harvard, the larger-than-life Swiss naturalist Louis Agassiz. Moreover, in 1847, when he learned of Baird's wish to apply for the curatorship of the Smithsonian, he gave Baird's application his full endorsement. "[I]f you become a candidate, please refer Prof. Henry, whom I know very well, to me, and I will most cheerfully speak of your qualifications for the situation."<sup>19</sup> Backed with the support of men of science like Gray, Baird was appointed as assistant secretary of the Smithsonian in July 1850. Henry designated Baird's duties as "to take charge of the museum, and to render such other assistance as the Secretary may require."<sup>20</sup>

Late in 1850, Baird read Wright's "you northerner" letters to Henry and became aware of the chasm widening between the collector and the botanist. He forwarded Wright's letter to Gray and composed a letter to Wright. Baird suggested that Wright should not lose confidence in Gray, for the delay of Wright's check was not Gray's fault. "[O]wing to the long and unexpected delay by Congress in passing the appropriation bills for 1850, this money which was in the form of a draft by Dr. Gray, on the Library Committee of Congress, could not be attained in time to send by

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<sup>19</sup> AG to SFB, [dated "Thursday;" postmarked 10/22/1846,] Folder 4, Box 23, *SFBP*; AG to SFB, 2/15/1847, Folder 4, Box 23, *SFBP*.

<sup>20</sup> JH to SFB, 7/5/1850, *PJH, Vol. 8*, 67. On Baird's almost unparalleled role in changing the Smithsonian into a natural history institution and a national museum, see, among others, Pamela M. Henson, "Spencer Baird's Dream: A U.S. National Museum," in *Cultures and Institutions of Natural History: Essays in the History and Philosophy of Science*, edited by Michael T. Ghiselin and Alan E. Leviton (San Francisco: California Academy of Sciences, 2000), 101-126; Kohlstedt, "Place and Museum Space"; Laubacher, "Cultures of Collection..."

Mr. Commissioner Bartlett.” “Dr. Gray recently met with a serious accident in England, in falling some 18 feet down the hold of a ship,” he told Wright in another letter.<sup>21</sup>

Baird decided to recruit Wright to join a project that had occupied his thought since his appointment as Assistant Secretary: establishing a “National Museum” at (or in close affiliation with) the Smithsonian. As he put it in a letter to George Perkins March, Regent of the Smithsonian and his intellectual mentor, “I expect the accumulation of a mass of matter thus collected (which the Institution cannot or will not ‘curate’ efficiently) to have the effect of forcing our government into establishing a National Museum, of which (let me whisper it) I hope to be director.”<sup>22</sup> And to ensure the Smithsonian to acquire specimens good enough to satisfy naturalists’ critical eyes, Baird understood, he needed an army of well-trained collectors. He soon occupied himself with preparing a pamphlet titled *Directions for Collecting, Preserving, and Transporting Specimens of Natural History* (1852). In his view, the pamphlet would not only outline the technical details of collecting but also explain them in plain language and with illustrations (Figure 6-2; for some reason, however, when the pamphlet was published in 1852, Baird removed all illustrations).<sup>23</sup>

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<sup>21</sup> SFB to CW, 10/31/1850, *CWC*; SFB to CW, 1/4/1851, *CWC*.

<sup>22</sup> Quoted from Henson, “Spencer Baird’s Dream,” 108-109.

<sup>23</sup> The manuscript of *Directions* can be found in Folder 6, Box 1, *CFGP*.



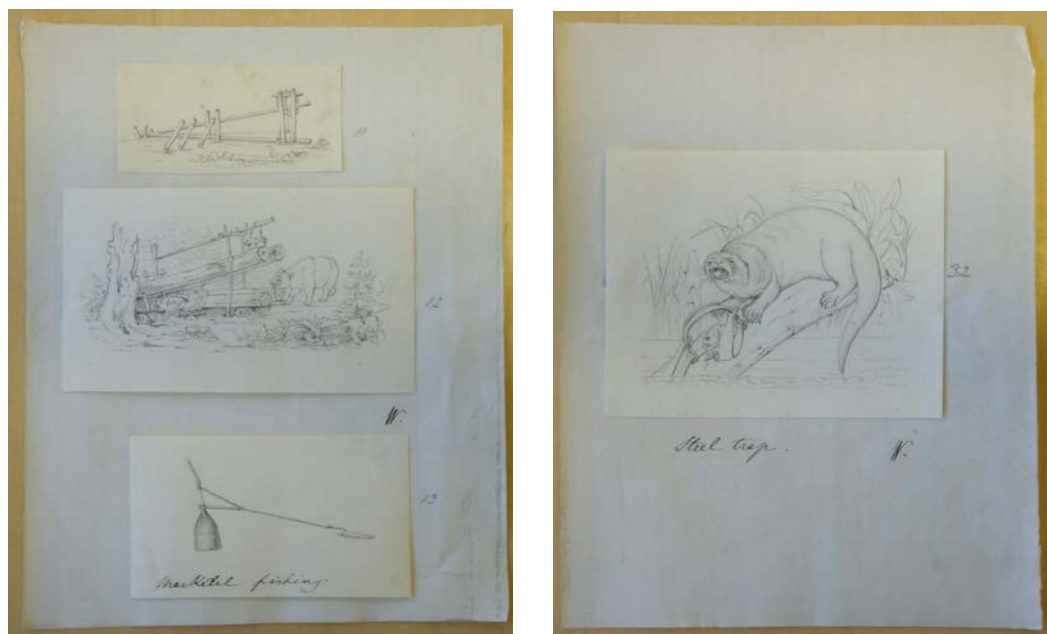


Figure 6-2. The illustrations Baird and his assistant Charles Frederic Girard prepared for *Directions*. (Courtesy of the Smithsonian Institution Archives, Washington, DC, USA.)

In October 1850, Baird sent a long list of desired specimens (“Have you the Peccary near you?”) to Wright with a remark: “You see I am not modest in my requests, and in fact I never had that reputation.”<sup>24</sup> He soon received Wright’s enthusiastic reply. Impressed, he replied, “It is of course not to be expected that you shall be at any expense in collecting,” and “it is equally proper that some compensation at least shall be made [to] you for your service.” For now, he wrote, what he could do was to secure a position for Wright in the Boundary Commission. Baird told Wright that the Smithsonian had contacted Colonel James Duncan Graham, a topographical engineer, who would soon lead the Scientific Corps to San Antonio and join the Boundary Commission. It seemed likely that Graham would afford Wright “every facility in his power,” Baird wrote. Soon afterward he informed

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<sup>24</sup> SFB to CW, 10/31/1850, *CWC*.

Wright that Graham had agreed to include Wright in “his party at his expense; at any rate he will lend all help in the way of transportation &c.”<sup>25</sup>

It turned out that Baird secured not only a position but also a partner for Wright. During his negotiation with Graham about Wright’s appointment, Baird also recommended John H. Clark, his favorite pupil at Dickinson College, to serve as a zoological collector. Baird understood that both Wright and Clark would have to join Graham’s campaign as “assistant computers,” for current laws did not allow Graham to appoint any naturalist in the Scientific Corps. Still he was content. Nominal as the position was, it guaranteed steady salaries and rations for Wright and Clark. For Baird, the U.S.-Mexican Boundary Survey offered an excellent chance to prove his ability as the Smithsonian’s Assistant Secretary and to take a big step toward the eventual establishment of a “National Museum.” He endeavored to serve as the bridge between men of science and governmental agencies and to unite these two parties together in the name of science.

### **“I have influential friends at Washington”**

Wright was electrified by Baird’s letters. Staying put in Mrs. Millet’s public house in New Braunfels, surrounded by exhausted, whiskery, and yet adventurous and ambitious travelers (many of whom were on their way to California to dig for gold), he had become excessively bored by his current job: teaching some ten preschool children. In a letter home, he boasted about his flowering relationship with the

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<sup>25</sup> SFB to CW, 1/4/1851, *CWC*; SFB to CW, 1/9/1851, *CWC*.

Smithsonian and expected that someday he would “get both money & credit & see my name in print.”<sup>26</sup>

I have commenced a correspondence with the directors of the great national scientific institution—the Smithsonian at Washington & I am strongly solicited to turn my attention to other branches of natural history. So I have begun to gather on a small scale bugs & crabs &c. I expect to catch all the bugs & butterflies, frogs & lizards, fishes & snakes, beasts & buds, flies & musketoos, snails & oysters, as well as flowers & grasses. I have already one fresh-water lobster pickled away in whiskey and a few bugs & little fresh-water crabs. The officers of the S. Institution write me that they are now erecting buildings and incurring considerable expense but hereafter they hope to be able to make considerable appropriations for procuring such materials as our new country may supply for the study of Natural History. They want fossils and insects. I may hereafter have my hands full attending to all these matters and may perhaps earn something handsome in the business.<sup>27</sup>

With the needs of his expedition met, and at Baird’s assurance that Gray had done his best to keep Wright in the field, Wright was prepared to forgive Gray. In fact, by December 1850, he had received from Gray more than \$500 in cash, plus a draft of \$100 from Short. “So you see I am rather easy about the pocket,” he wrote home jovially.<sup>28</sup> Moreover, Wright was exceedingly delighted by Gray’s progress in composing *Plantae Wrightianae Texano-New-Mexicanae*. “I am not quite so desirous to see my name in print as a fellow (a little how-come-you-so),” he wrote to Gray in March 1851. “[But] I suppose we are all desirous to a greater or less extent of a reputable notoriety.”

Then Wright poured out what he had observed in the field to make the volume that bore his latinized name perfect:

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<sup>26</sup> CW to JW, 2/19/1851, *CWC*.

<sup>27</sup> CW to JW, 12/17/1850, *CWC*.

<sup>28</sup> CW to JW, 12/17/1850, *CWC*.

I believe & first observed & endeavored to convince you of a fact which you have admitted in Plant. Lindh—that *Condalia obovata* produce its flowers in one summer & ripen the fruit of those flowers the next. Again in a note to Plant. Lindh. part 1 you remark that *Ulmus crassifolia* was collected in Sept. in flower the 2nd time (I quote from memory). Now I authorize you to wipe that out & make it a part of the specific character or the distinctive character of a section of the genus that *U. crassifolia* uniformly flowers not in the spring but in Aug.—Sept. I have been among it now several years have seen nothing but it confirm the species above expressed. You may not now be convinced but I venture to predict that you will be someday if you live long enough.

Now he could not wait to thrust himself into the field once again. “I have as yet nothing of Maj. Graham’s movements,” he complained to Gray. “Am I doomed to throw away the coming summer like the last? If so I must quit the business.” The symbol of the U.S. Army, Wright commented, “ought to be changed and a snail to be put in place of the eagle.”<sup>29</sup>

While waiting for the arrival of Graham’s campaign, Wright found time to pay a visit to the Kyles in San Marcos. Once again, he indulged in deer-hunting and turkey-shooting with Colonel Kyle and enjoyed Mrs. Kyle’s meals. Early in May, upon hearing of the arrival of Colonel Graham’s wagons, he said farewell to the Kyles and set off for San Antonio. “I lodged two nights at a tavern where the fleas kept up such a gallopade upon my carcass that I slept but little,” Wright wrote home after his arrival in San Antonio. But he was energized, particularly when seeing his “truck & botanizing materials deposited on a Government waggon for El Paso.” Once again he boasted of his thriving relationship with the Smithsonian: “I have been for some time in correspondence with the officers of the Smithsonian Institution at

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<sup>29</sup> CW to AG, 3/7/1851, *HL*.

Washington and through them and my own solicitation & patience I have obtained a promise of transportation and subsistence.”<sup>30</sup>

Wright also realized his elevated status as a botanical collector. Unlike his 1849 expedition from San Antonio to El Paso, during which time he was an outsider and a stranger to Captain French’s troop and had to earn a ration by serving as a cook, he was now an officer appointed in the prestigious U.S.-Mexican Boundary Survey. But he was aware of possible role conflict. “I shall receive a salary but my collection will belong to Government,” he wrote home. “I am not very desirous of entering the Government service though it may be greatly to my benefit in future expeditions.”<sup>31</sup>

On the other side of the Atlantic, Gray was glad that Wright had begun quibbling about botany again. It seemed to him that the once damaged relationship with Wright had been repaired. It was now May 1851. After a year of intensive and extensive studies at Europe’s distinguished botanical establishments, he believed that he had gathered sufficient information not only for the Ex. Ex. Botanical Report but also for *Plantae Wrightianae Texano-New-Mexicanae*. He would soon set sail back to Boston. He knew that Wright now had money, had a position, and had been placed under the patronage of the Smithsonian. The second round of his collaboration with Wright would soon begin, he believed, and he resolved to make it an ultimate success.

### **“A trying and painful trip”**

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<sup>30</sup> CW to JW, 5/9/1851, *CWC*.

<sup>31</sup> CW to JW, 5/9/1851, *CWC*.

Under Graham's command, Wright made his first botanical collections between San Antonio and El Paso, along the wagon roads he had lumbered over about two years previously. Leaving San Antonio in late May, Wright arrived in El Paso in late June (June 24 and 25), an expedition that only took him 37 days.<sup>32</sup> "We traveled rapidly and made several night-marches," he wrote home from Frontera. "Riding on horseback has always been very fatiguing to me and when combined with drowsiness almost overpowering."<sup>33</sup>

Meanwhile, Wright could not help comparing the landscape before him with what he had experienced two years ago. The contrast astonished him. "The route to the Pecos presented a pretty flourishing vegetation—but when we reached the Timpio which before furnished me with so great a variety of plants we found it perfectly dry, a recent fire had swept over the whole country to the every summit of the mountains & I found only occasionally a plant worthy of notice."<sup>34</sup> "[O]f all the barren, waterless regions on the face of the earth I want to see no worse than I experienced on the route," the zoological collector Clark told Baird. "There are

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<sup>32</sup> Among them 29 days were spent in marching; see JDG to AG, 7/4/1851, *HL*.

<sup>33</sup> CW to JW, 7/3/1851, *CWC*.

<sup>34</sup> CW to AG, 7/9/1851, *HL*. In his diary documenting his expedition from San Antonio to El Paso between 1850-1851, Electus Backus, major of the 3rd U.S. Infantry, recorded in an entry in his diary dated June 17, 1851: "Col. Graham had lost 16 mules, & was obliged to remain behind." In another entry dated June 12, he recorded: "Mr. Wright, Botanist, today found a large specimen of the Maguey, with fruit upon its stem..." in HM 71478, *DLEB*.

stretches of from 50 to 100 miles without living water, without grass & without wood enough to boil a pot of coffee.”<sup>35</sup>

Yet what struck Wright and Clark more was how quickly the Scientific Corps under Graham’s command deserted. (Graham had hired eight persons in San Antonio, but seven among them quit their positions after arriving in El Paso.) For Clark, this fact spoke volumes about Graham’s disagreeable commandership and manners. “The Col. always addresses me in the most courteous manner,” Clark told Baird, “but I think his treatment sometimes says dog... I have long since prepared myself to take the worst & while he gives me enough to eat I shall pay no regard to what he says other than to obey what is written [to] the bounds of reason & my abilities.” Wright, too, detected Colonel Graham’s abhorrence of civilians serving in the Scientific Corps. But he knew how to turn the circumstance to his advantage. “Col. G has treated me pretty well but I can not say any [of] this of his treatment of others,” he wrote home. “One thing is certain: he can not treat me as he has treated others & I do not think he will attempt it. I have laid him under some small obligation by giving him the use of my money when (I think) he was not very flush & thus enabled him to pay off in cash some men who were anxious to be discharged.”<sup>36</sup>

Wright made his second major set of collections along the route from El Paso through Coppermines of Santa Rita (today’s Santa Rota mine) to Santa Cruz of Sonora (Figure 6-3). The expedition, even by Wright’s standard, constituted an astounding challenge. But what Wright harvested on this particular grounds

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<sup>35</sup> JHC to SFB, 7/4/1851, Folder 21, Box 17, *SFBP*.

<sup>36</sup> JHC to SFB, 7/4/1851, Folder 21, Box 17, *SFBP*; CW to JW, 7/3/1851, *CWC*.

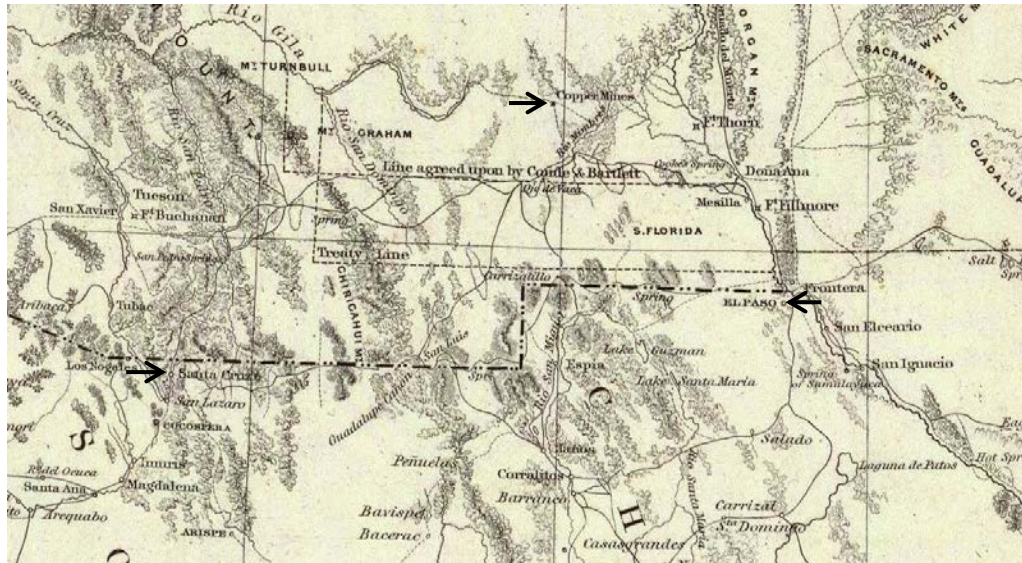


Figure 6-3. Wright's major botanizing ground in 1851. The arrows show the locations of El Paso, Coppermines, and Santa Cruz. (From a map entitled "Map of the United States and Their Territories between the Mississippi and the Pacific Ocean and Part of Mexico," included in Emory, *Report on the United States and Mexican Boundary Survey, 1857*. Courtesy of the Joseph F. Cullman 3rd Library of Natural History, Smithsonian Libraries, Washington, DC, USA.)

was "of exceeding interest." As Gray would later put it, "[it] comprises a larger proportion of new species than any other that has fallen into my hands."<sup>37</sup>

What drove Wright to this particular ground actually was not its scientific significance but instead Commissioner Bartlett's controversial decision regarding where the beginning point of the U.S.-Mexican boundary should be. As Wright informed John, "He [Bartlett] has taken the responsibility of fixing the beginning point of the line much higher up the Rio Grande than that which is generally thought to have been fixed by the treaty in order to secure the Coppermines while giving to Mexican a very valuable settlement above Dona Ana on the side of the river."<sup>38</sup>

<sup>37</sup> See a letter from Graham to Gray, JDG to AG, 3/11/1852, *HL*; Gray, *Plantae Wrightianae Texano-Neo-Mexicano: Part II*, 6.

<sup>38</sup> CW to JW, 8/8/1851, *CWC*.



Graham, given his position as the Head of the Scientific Corps, felt obliged to correct this grave mistake. In July he brought the Scientific Corps to Coppermines, preparing himself to commence a thorough survey to determine the location of the beginning point.

Yet this beginning point of the U.S.-Mexican Boundary was located in the heart of the Chiricahua Nation's territory. Wright, after botanizing some days in Coppermines, wrote to Gray that he had ventured to secure "every thing about the mines [Coppermines] within a moderate distance (for it was dangerous to return & at on account of the Indians)." <sup>39</sup> "There was once a flourishing village here [in Coppermines] & a brisk trade in copper was carried on with the interior," Wright told John. "In 1838 as I have been informed the Indian made an attack on the village and entirely destroyed it killing or taking capture every soul in the place." Despite the danger of losing their scalps, both Wright and Clark continued collecting. Wright vividly described their daily lives to John:

We are living in tents. Mr. Clark and myself occupying one small one in which we have our blankets for bedding he a trunk for his cans of alcohol for preserving reptiles & and his books & instruments & I my press for plants (now a huge package) my bag of clothing and saddlebags for instruments books &c. It sometimes presents a scene of wondrous confusion. In one corner is my press in another Mr. C's trunk with a parcel of books and perhaps a kind of two to be skimmed. Nearby lies a snake destined to the same operation while at the door lies a big mutilated bird and a handful of plants with or without flowers and fruits brought by some friends by hands and laid down for our use. In another corner lie my bags of clothing books & perhaps surmounted by a few quires of paper. You might sometimes see in some part of the house a vial or two insects in alcohol & pretty certainly Mr. C's gallipot of arsenic conspicuously labeled poison—very likely too a box of insects pinned down dead or alive as the case may be—this last business I have not yet the hardihood to undertake. A coat or two shoes also and our hats if not on our heads would be left to make up the

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<sup>39</sup> CW to AG, 11/30/1851, *HL*.

confusion. Nor must I omit to mention my saddle & bridle nor Mr. C's gun which stands tied to the tent-pole while his powder-flask & shot-bay lie nearby. And now if you were to peep in & see ourselves with perhaps a visitor or two you would think we were in all about enough to fill a room some eight feet square & shaped too like a garret.<sup>40</sup>

While arranging their specimens, the two collectors joked about their nominal positions as assistant computers. "I hope my computations will involve higher principles of mathematics than I have been exercised in hitherto." Clark shared some of his and Wright's favorite jokes with Baird, "such as the following: if a horse eats two quarts of corn at one feed, how much will one ration be at 3 feed per day; and if a mule gets lost how long it will take to catch him."<sup>41</sup>

They even met the then-notorious Apache Chief Mangas Coloradas (1793-1863). "A few days ago we had a visit from the Indian somewhere between fifty and a hundred made their appearance in camp early in the morning apart coming up boldly and about as many keeping in the back-ground as if conscious they deserved a flogging," Wright told John.

The chief Mangus Colorado [*sic*] is a stout broadfaced pleasant looking man in the prime of life dressed as far as possible in American style—with blue military coat—a large epaulette not on the shoulder but considerably down on the arm—a brace of pistols belted round him and a gun. A few others were trying to imitate American dress—having some hats—others coats or waistcoats—a few armed with guns—the awkward flintlock pieces of the Mexican—but most having bows & arrows & some spears... The Col. was disposed to give the chief a grand breakfast when the whole crowd by degrees accepted the invitation cautiously entered the tent and quietly took away biscuit after biscuit and piece after piece of meat till Mr. Clark & myself began to look anxiously around to see if there was any in reserve for us anywhere. Finally the cook took us to his tent and spread for us our breakfast. Even then one came in and squatted down before us saying as plainly as action could speak that he wanted some of our

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<sup>40</sup> CW to JW, 8/8/1851, *CWC*.

<sup>41</sup> JHC to SFB, 7/4/1851, Folder 21, Box 17, *SFBP*.

breakfast. We disregarded him altogether and he soon left. They manifest no gratitude and it is doubtful whether they feel any. They are generally a miserable set short in stature & thin. They wore moccasins which come up nearly to their knees and a blanket of skins or a blanket of Indian or Mexican manufacture. This party consisted of Apaches and Navajoes (pronounced Navahoes). Mangus is the Apache chief. His daughter was with him a pretty good looking Indian except that her upper lip turned up too much and her face was too broad. They all ride astride men and women many without saddles.<sup>42</sup>

In the weeks that followed, the U.S. Boundary Commission lost some 150 horses and mules at the hands of Indians.

Commissioner Bartlett, despite (and probably because of) such a great loss, decided to have a meeting with General García Conde, who was now leading the Mexican Boundary Commission somewhere between Coppermines and Santa Cruz. Colonel Graham felt that it was his duty to join the meeting. The whole party, constituting 58 persons, left Coppermines on August 27. On September 5, they caught up with General Conde's party in Rio San Pedro. The next day, when the meeting was convened, Colonel Graham stepped into the meeting tent, and Commissioner Bartlett entirely neglected the Colonel's authority on the Boundary Commission, to the extent that he refused to give Graham a seat. The fact was that Bartlett had grown annoyed by Graham's frequent reluctance to comply with his orders and quarrels with Chief Surveyor Andrew B. Gray. "Never," he would later complain, "in the whole course of my life, have I been placed in so trying a position." Indeed, as Wright reported home, the tension with Bartlett made Graham

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<sup>42</sup> CW to JW, 8/8/1851, *CWC*.

“melancholy & miserable.” “[H]e pulled out a great deal of his beard and did not know it till he was told of it.”<sup>43</sup>

The meeting did not reach any definite conclusion about the location of the Initial Point. The next day General Conde set off for Santa Cruz, aiming to procure provisions there. Soon after General Conde’s departure, Commissioner Bartlett decided to follow suit, with a team of 14 persons. Graham, once again, decided to follow Bartlett’s steps, and he ordered Wright and Clark to accompany him. It turned out that neither Conde nor Bartlett nor Graham knew how to reach Santa Cruz from Rio San Pedro. The three groups of surveyors soon got lost in “a country traversed but little except by savage beasts and still more savage Indians (Figure 6-4).” As Wright told John afterward, “Gen. C [Conde] was first there after traversing a frightfully mountainous region and Mr. B after going a considerable distance in the night but impracticable direction.” As for his (Graham’s) party, wrote Wright, they got lost, as well, but they was fortunate to be picked up and escorted by local travelers and reached Santa Cruz with relative ease. They were also fortunate in the sense that they ran into one bear and one bull en route and killed the two unfortunate animals to furnish themselves. Bartlett’s party was not that fortunate, however. In fact, Wright assured John, with very scanty provisions, Bartlett and his men had been forced to eat purslane.<sup>44</sup>

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<sup>43</sup> John Russell Bartlett, *Personal Narrative of Explorations and Incidents in Texas, New Mexico, California, Sonora, and Chihuahua, Connected with the United States and Mexican Boundary Commission, during the Years 1850, '51, '52, and '53, Vol. 1* (New York, London: D. Appleton & Company, 1854), 348; CW to JW, 10/10/1851, CWC.

<sup>44</sup> Bartlett, *Personal Narrative...*, 381; CW to JW, 10/10/1851, CWC.

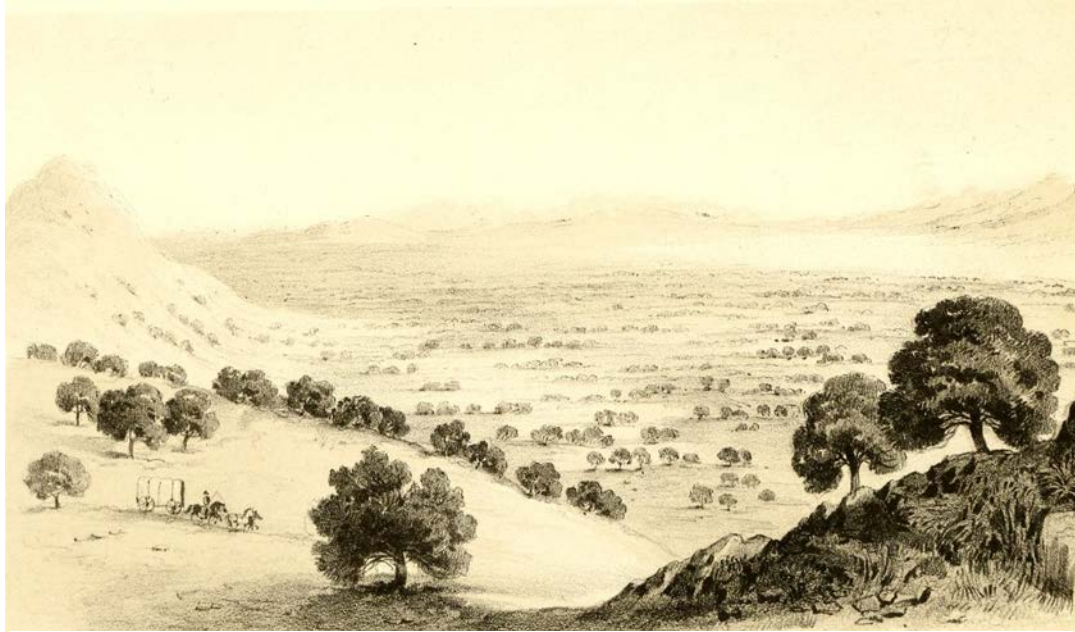


Figure 6-4. "Valley leading to Santa Cruz, Sonora." (From Bartlett, *Personal Narrative...*, between p. 402 and p. 403.)

"It was a trying and painful trip," Clark told Baird. "I hope I shall never be necessitated to endure another such." Although he had only served in Graham's party for less a year, he felt that he had had enough and had seen enough. "I never saw so much indirection and want of purpose in any undertaking as is manifest in the Commission," he told Baird. "I am awfully tired of this country; one sees nothing but degradation and vice under every variety of form & it takes all the nerve I possess to steer clear of them, surrounded as I am by a host of young men who seem to have come here for the sole purpose of dedicating themselves to wine and women."<sup>45</sup>

### **"Little deception"**

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<sup>45</sup> JHC to SFB, 11/13/1851, Folder 21, Box 17, *SFBP*.

After making two significant collections as an assistant computer in Colonel Graham's campaign, Wright became increasingly concerned with an issue that hardly troubled him before: "ownership" of the specimens. Given his status as an officially appointed assistant computer in the Boundary Commission, would he be allowed to distribute his collections to subscribers and gather a subscription fee? What complicated this ownership issue was that he had received Short's check of \$100. Gratified as he was, he found that Short's money put him in a quandary. "I was for some time undecided whether to return it [Short's check] or not owing to the uncertainty of my position with Col. Graham's party," he told Gray.<sup>46</sup>

Moreover, it seemed to Wright that Graham exhibited little interest in collaborating with men of science. As he told Gray, Graham had been reluctant to consign the botanical collections to Gray, for the Colonel was afraid that the collections would "fall into the hands of other botanists than yourself and be published without due credit being given to the Boundary Commission." In fact, Wright added, "He even doubted your claim to reputation as a botanist, because you would not undertake the elaboration of all the Natural Orders—though I believe I satisfied (at least silenced) him by showing him that other well-known botanists did the same."<sup>47</sup>

In July 1851, Wright received an appointment as Assistant Computer in the Boundary Commission at an annual salary of \$500 and one ration.<sup>48</sup> Some four

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<sup>46</sup> CW to AG, 7/9/1851, *HL*.

<sup>47</sup> CW to AG, 7/9/1851, *HL*.

<sup>48</sup> CW to AG, 7/9/1851, *HL*.

months later, however, he wrote to Gray that he had decided to take as much advantage as possible during his service to the Boundary Commission. "If it can be assumed that I will derive some benefit from my labors I will continue to collect as heretofore," he wrote.

My salary is small and my expenses considerable. Besides unlike his other assistants who furnish nothing to the expedition I find my own horse my own paper & botanizing materials and have largely assisted Mr. Clark the zoologist. What I have already collected within the space of two months are worth nearly or quite the whole year's salary. It is therefore reasonable that I should reap a considerable profit from the disposal of the specimens.<sup>49</sup>

Wright therefore demanded Gray "pledge" to "reserve a part of these plants for me and a set for Dr. Short especially." "Col. G. knows nothing of the number of the sets or of the species," he emphasized.<sup>50</sup> In December, he again asked Gray to "look out" for his (Wright's) collections and "prevent them from falling with the Patent Office." Wright was worried that Graham would consign all botanical collections directly to Washington, DC, thus leaving little space for him and Gray to maneuver.<sup>51</sup> Wright's enthusiasm even went so high as to invite other botanists to join his scheme. "Engelmann is in a quandary under the conflicting instructions of Col. Graham and myself—the former directing him to describe the Cactaceae collected by me and send them with his report on them to his address to Washington while I told him to reserve a portion of them for me." "I should have no conscientious scruples in practicing little deception on a man who has shown himself so illiberal to

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<sup>49</sup> CW to AG, 11/30/1851, *HL*.

<sup>50</sup> CW to AG, 11/30/1851, *HL*.

<sup>51</sup> CW to AG, 12/18/1851, *HL*.

me in this matter as Col. Graham has," he declared.<sup>52</sup>

Gray arrived home in June 1851, immediately devoting himself to completing *Plantae Wrightianae Texano-New-Mexicanae, Part I*. (Gray had composed the volume's preface in 1849!) In July, when the manuscript was almost ready, he received Graham's letter about the newest collection made by Wright from San Antonio to Frontera, containing 579 botanical specimens.

My object in sending them to you is that you may, as I request you will, examine them and take them up for describing as the Botanist of my party. In case this duty should be acceptable to you, you will please consider the specimens as belonging to the Scientific Corps of the Boundary Survey Commission, and I request that you will address your report upon them to me, to constitute a part of our proceedings, and to be published with them.<sup>53</sup>

Gray found Graham's request timely. From his perspective, since this collection of 579 specimens covered the same ground as Wright's 1849 collections, he had no doubt that it should be included into *Plantae Wrightianae Texano-New-Mexicanae: Part I* (although the volume's subtitle remained "an account of a collection of plants made by Charles Wright in an expedition from Texas to New Mexico, in the summer and autumn of 1849..."). He resolved to complete *Plantae Wrightianae Texano-New-Mexicanae: Part I* as soon as possible to secure credit for Wright's discoveries in Texan botany. He did not think that "ownership" would be an issue. He knew that Graham would be relieved from duty, and William H. Emory would replace him to

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<sup>52</sup> CW to AG, 1/25/1852, *HL*.

<sup>53</sup> Graham also remarked that "We shall be able I hope to present to you, should you desire them, duplicates of the plants. One complete set will be required by the Government at least, to be deposited in some institutions hereafter to be designated." JDG to AG, 7/4/1851, *HL*.



become the head of the Scientific Corps.<sup>54</sup> For Gray and for his botanical circle, it was a long-awaited change. “It is quite time that some change was made in the Commission,” Torrey told Gray, “for the gentlemen of the party are entirely at loggerheads.”<sup>55</sup>

Gray calculated that the gridlock at the Commission actually had created favorable circumstances for the advancement of botanical science. It seemed to him that now he could just go ahead and publish his enumeration of Wright’s collections made under Graham’s command and distribute specimens without worrying about the issue of ownership. He wrote to Engelmann in October 1851: “Torrey writes me that Graham is recalled and Emory gone to Boundary—and a world of fuss and trouble they have—so you be left to do what you think proper with the Cactee—&c.—”<sup>56</sup> To Short, he wrote,

In Wright’s letters (I had one yesterday) he expressed much anxiety about securing a set of his plants for you, to repay your kind advance and is troubled about it, lest his connexion now with the Boundary Survey (where he has a *small* pay) and the delivery of his packages to Col. Graham, should prevent his having control of them. But Col. Graham as sent the 1st envoi to me, and is bringing the second: and you may rely upon my looking after your and Wright’s interests. I trust that Graham will be content with a single set for Government, when besides yours, there will be several others to turn to Wright’s benefit.<sup>57</sup>

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<sup>54</sup> Bartlett devised the scheme to remove Graham from the Boundary Commission. Bartlett, *Personal Narrative...*, 348. JT to AG, 9/15/1851, *HL*; also see Rodgers III, *John Torrey*, 221-222.

<sup>55</sup> JT to AG, 9/22/1851, *HL*.

<sup>56</sup> AG to GE, 10/21/1851, *AGPG*.

<sup>57</sup> AG to CWS, 1/17/1852, Coker, “Letters...,” 119.

Then he received Graham's second letter. In it the Colonel said that he had been obliged to give "a cursory report" to the Department of the Interior. He wondered if Gray could enumerate Wright's collections made under his command (during the expedition from San Antonio to El Paso). He also remarked that the second of Wright's collections (made during the hazardous trip from El Paso through Coppermines to Santa Cruz of Sonora) would be soon on its way to Cambridge. "It is my wish, after receiving a full set for the government and one for yourself, that the repetitions or additional similar plants should be for the benefit of Mr. Wright who labored so indefatigably amongst many fatigues and exposures, in adding every thing that labor and attention could find, to the collection."<sup>58</sup>

Delighted, Gray asked Graham if he could undertake the enumeration until Wright's entire collections arrived. Graham disagreed. "If something is not shown for the money that has been expended by the Commission, there is rest on to apprehend that another appropriation will not be granted by this Congress."<sup>59</sup> Eager to get hold of more Wright's collections, Gray complied with Graham's order. He rapidly drafted a report listing some 100 new species and told Graham that he would name some new species and perhaps genera after Graham. Afterward, he reported that he estimated that the first set of Wright's collection contained as many as 350 new species and 5 or 6 new or unpublished genera.<sup>60</sup>

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<sup>58</sup> JDG to AG, 3/11/1852, *HL*.

<sup>59</sup> JDG to AG, 3/24/1852, *HL*.

<sup>60</sup> AG to JDG, 8/6/1852, *RSW*, 249-250.

Graham was glad to find such a cooperative botanist in Gray. He included his correspondence with Gray in his report to the Secretary of War. Regarding Gray's intention to name new species after him, he replied, "I must thank you for the complement you have been pleased to pay me in the naming of one of the new plants, though I feel I have hardly a legitimate claim to it. I certainly did all in my power to promote the collections in this branch and also in zoology without any pretence to knowledge in either of those branches in natural science."<sup>61</sup>

In May, Graham informed Gray that he had sent another set of Wright's collections, consisting of "two large leather panniers filled with dried specimens of plants, & some seeds, and a small box containing about 280 packages of seeds."<sup>62</sup>

Gray received the collection on June 4 and immediately informed Wright:

I consider myself very much a gentleman! For your favor of the 12th April reached only this afternoon, and now before the sun has gone down I am answering it! Your letter came very opportunely too. For, tho. Col. Graham has been back so long, it is only yesterday that I got the collection he brought home with him to [from] Indianola (& the seeds); and today I opened it & and had looked over only two bundles. And I was saying to myself, now, if I only had Mr. Wright's list, with localities I should do very well. And when my letters came from the office, yours, with said list enclosed, was among them... I think I shall distribute them thus—1. A full set save up for yourself, 2.—Dr. Short. 3.—Dr. Torrey—selection—what he will need. 4. Mr. Lowell: especially he has not before. 5. Hooker. 6. Mr. Boissier—who had none before and will pay well. 7. Engelmann—especially his favorite families. 8. Mr. Carey—if he will subscribe &

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<sup>61</sup> JDG to AG, 5/17/1852, *HL*. Afterward, he asked if Gray could name some new species or genera after Colonel Sylvanus Thayer. "If you do, please say that it is done at my suggestion, & is intended as a tribute to his long & valuable services to his country & especially in commemoration of his being the founder of the present system of instruction at the U.S. Military Academy at West Point." JDG to AG, 3/14/1853, *HL*.

<sup>62</sup> JDG to AG, 5/17/1852, *HL*.

pay. 9. Bentham—I shall take some for him. 10. Some one of the foreign subscribers etc.<sup>63</sup>

Gray also reassured Wright on the ownership issue. “You may be sure I shall look well after your interest about them. I do not anticipate much difficulty as you collected a good part of them before you were employed by Col. Graham; and as he has been recalled and as the Govt pays little. I shall fight hard before I let more than a single set go to Government & that I hope to Smithsonian.”<sup>64</sup> “You are indeed an invaluable collector,” he told Wright in another letter, “tho’ you do like to grumble now & then,—and I hope the Indians won’t catch you. If they must take a scalp, or a head, there are others I could better spare. So take care of yourself—”<sup>65</sup>

#### **“An invaluable collector”**

An invaluable collector as he might be, Wright still grew worried about his appointment in the Boundary Commission.<sup>66</sup> Emory, arriving in El Paso on November 25, 1851, found that the Commission was in a deplorable state, in particular the Scientific Corps that was once under Graham’s command. “Of all the assemblages of folly, ignorance, and hypocrisy I ever saw congregated together under the title of scientific corps, that turned over to me by Col. Graham exceeds.

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<sup>63</sup> AG to CW, 6/5/1852, *AGPG*.

<sup>64</sup> AG to CW, 1/23/1852, *AGPG*.

<sup>65</sup> AG to CW, 6/5/1852, *AGPG*.

<sup>66</sup> CW to MAW, 11/28/1851, *CWC*.



Figure 6-5. William H. Emory between 1855-1865 (Library of Congress Prints and Photographs Division Washington, DC, USA.)

Think of two hundred thousand dollars expended here and nothing to show but this: The determination of one point in latitude, the running of a parallel one degree, and the partial survey of the river 40 miles! This is all. My God what will become of our appropriations if Congress knows of the follies of the Commissioner and his antagonists too.”<sup>67</sup>

Those who once worked under Graham’s command certainly felt Emory’s fury. “There are more peculiarities about these Topogs [Topographers] than any other set of men I ever had to deal with,” Clark commented in a letter to Baird. “It is frequently remarked here that they are more or less crazy, and that such a state of mind is induced by the moon.” But never fancy that the Smithsonian could have escaped the influence of such moon-induced minds, he warned Baird. “Major says if

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<sup>67</sup> Quoted from Hine, *Bartlett’s West*, 71.

you don't help to make those fellows at Washington send him some money, he'll not have any more snakes caught for you."<sup>68</sup>

Wright grew worried, as well. He demanded that Gray "exert any influence over Maj. Emory to retain me as a collector or computer so that I can continue to botanize as heretofore."<sup>69</sup> Early in December, he had an interview with Emory, and received the promise that he could stay in the Commission as assistant computer.<sup>70</sup> Over the following six months, he continued in his dual roles on the Boundary Commission: a surveyor in charge of one of the surveying teams that worked together to decide the Initial Point of the U.S-Mexican Boundary, and a private collector who collected for subscribers and patrons. Wright's botanical ground during these months was in the valley of the Rio Grande. As Gray put it afterward, Wright's service in Emory's campaign brought the collector "down the Rio Grande for sixty or seventy miles; also up the valley as far as Camp Fillmore, and thence into the Organ Mountains which bound the valley on the east." Toward the end of Wright's service, added Gray, Wright made a "hasty excursion" to Lake Santa Maria and Lake Gusman in Chihuahua. "These vernal collections afford many novelties, no botanist having previously explored this region at that season of the year."<sup>71</sup>

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<sup>68</sup> JHC to SFB, 1/11/1852, Folder 21, Box 17, *SFBP*.

<sup>69</sup> CW to AG, 11/30/1851, *HL*.

<sup>70</sup> CW to AG, 12/18/1851, *HL*.

<sup>71</sup> Gray, *Plantae Wrightianae Texano-Neo-Mexicano: Part II*, 6.

Still, it was difficult to survey and collect natural-history specimens in the Rio Grande. Wright told his sisters of his altered appearance after an almost-three-year separation:

At the close of the day's work I present much the appearance of beggar-boys so often seen in little books especially about the feet and ankles or the latter for my shoes are only three weeks in service and are now much holey. But the nether ends of my buckskin breeches are flying about in all directions and although I patched the knees last night they won't stay patched—I fear I shall have to abandon them to their fate. I have taken a firm resolution if I ever visit Yankeedom again never to leave it without a good supply of clothing for the whole man from top to toe...

But would he settle down with the family upon his return to “Yankeedom”? Wright asked himself: “I often feel as if I ought to come home and work for you and yet I dislike to leave the State [Texas] of my adoption and my favorite studies. Besides I am afraid I should not make a profitable hireling—I should be botanizing the weeds while hoeing & the grass when mowing & I should be shooting birds for ornithological studies or bottling insects for entomology while going to plough in the meadow &, thus occupied, I might not get to work before midday & home again till bedtime.”<sup>72</sup>

As the months passed, however, Wright grew tired of the duties that the Boundary Commission had entrusted to him—and he was glad to find that the commission grew tired of him, as well. “The truth is [that] I am not qualified to do such work and they knew it all the time. I can not sketch in the mountains & ravines

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<sup>72</sup> Wright made this description early in December. But it turned out the state that adopted Wright was about to turn her back on him. In a letter to John, Wright said that he had probably lost his 360 acres of land in Texas because he had not paid the taxes for a long time. Natural-history collecting and studies had consumed all his time and energy, he told John. CW to MAW, 11/28/1851, *CWC*.

& sand hills trees and thickets waters & marshes &c. &c. Consequently I am relieved for which I am rejoicing.”<sup>73</sup> Late in May, he dispatched a letter to his brother: “We shall start for San Antonio about the 15th [of June] which I hope to reach by the last of July and then home in a month—in time for peaches & apples—and sweet cider,— hurrah!!”<sup>74</sup>

Emory understood Wright’s eagerness to go home, but was unwilling to relieve Wright of his duties. In his letter to Wright in June 1852, he instructed Wright to deliver the botanical specimens (collected by Parry, Bigelow, and Wright) to Torrey, a box of cacti to Engelmann, a box of zoological specimens to Baird, and a skull of a Comanche Indian (killed at the Escondido springs in the fall of 1850) to J. J. Abert, head of the Topographical Corps. After completing the deliveries, Emory commanded, “you will aid Dr. Torrey in arranging and collating the Botanical Specimens and preparing notices of all new and rare plants for immediate publication in such form and in such periodicals as may be selected by Dr. Torrey.” “Shall your presence with Dr. Torrey not be required by him,” continued Emory, “you will return to the line and report to me in person.”<sup>75</sup>

Wright adopted what he considered to be the most effective way to fulfill Emory’s order. He first went to the Quartermaster in San Antonio to send his

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<sup>73</sup> CW to MAW, 3/29/1852, *CWC*.

<sup>74</sup> CW to JW, 5/24/1852, *CWC*; Wright had not sent off the letter until June.

<sup>75</sup> WHE to CW, 6/8/1852, *HL*. Regarding the Comanche Indian’s skull, see David J. Weber and Jane Lenz Elder, *Fiasco: George Clinton Gardner's Correspondence from the U.S.-Mexico Boundary Survey 1849-1854* (Dallas: Southern Methodist University Press, 2010), 257.



collections to Indianola. Then he rushed to Indianola (arrived on August 11) to receive his collections, and boarded a steamship to New Orleans (arrived on August 17), where he divided his collections into sets, and addressed them to New York; Washington, DC; or Philadelphia. Then he decided that he would like to deliver the *Cactaceae* collections to Engelmann in person. He boarded a steamboat and arrived in St. Louis on August 18. He stayed with Engelmann for several days, and on August 30, he first took a boat to Cincinnati, and then transferred to a train, traveling all the way to Wethersfield through Columbus and Buffalo. Early in September, he was already in Wethersfield with his family (arriving on September 8). On September 10, he dispatched a letter to Emory reporting the completion of his deliveries. He assured the Commissioner that he would rejoin the Boundary Commission as quickly as possible.<sup>76</sup>

The homeward trip from San Antonio to Wethersfield cost him \$233, an amount that he had carefully saved from the remittances for his botanical labors. Once again he was out of funds. He could only hope that Congress would reimburse him for his traveling expenses, and that Gray would cash his collections made under Graham's and Emory's respective commands. He needed money or he could not even rejoin the Commission as Emory had stipulated.

### **"If I am arrested"**

Gray published *Plantae Wrightianae Texano-New-Mexicanae, Part I* in 1852 as part of the *Smithsonian Contribution to Knowledge*. With the catalogue ready, he

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<sup>76</sup> CW to WHE, 9/10/1852, Folder 43, Box 6, *WHEP*.

immediately began distributing Wright's collections among subscribers and collecting the subscription fee. Sir William Hooker, receiving Wright's specimens and paying £11.5, told Gray that he considered the fee "a cheap pennyworth for such excellent things."<sup>77</sup> The Wright-Gray-Smithsonian collaboration had set a new standard not only for American botany in particular but also for botany in general.<sup>78</sup>

Then Gray received a long letter from Henry of the Smithsonian containing a shaking message: "I am informed confidentially that Capt. Wilkes has refused to certify your account to the library of committee." Gray was stunned. His one-year research trip had cost him \$3,240. He needed Wilkes's certification, or he would be financially ruined.

According to Henry, a number of circumstances had prompted Wilkes to make this extraordinary decision. Of importance was that Gray had devoted "a portion of time to the preparation of a Smithsonian Memoir which, according to contract, ought to have been given to the work of the exploring expedition," and that Gray had "disregarded the prescribed directions of the committee as to the translation of the specific characteristics from the Latin into English." He himself was particularly

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<sup>77</sup> WJH to AG, 1/27/1853, *HL*.

<sup>78</sup> Here it is worth quoting Wright's comment on Gray's volume: "I find some things in *Plant. Wright* needing correction or explanation which may be put in the form of Errata. Thus none of the plants of 1849 & summer of 1851 before I went to the Coppermines were gathered in New Mexico proper although I thought they were at the time. The southern line of that Territory strikes the Rio Grande some twenty miles above here so that the expression "between Western Texas & New Mexico" is not strictly correct. El Paso itself is in the Mexican State of Chihuahua." In other words, in *Plantae Wrightianae Texano-New-Mexicanae, Part 1*, Gray might have ensured his diagnosis of species to be mostly correct but gotten the title misleadingly wrong. CW to AG, 4/12/1852, *HL*.

concerned with the issue of translation, wrote Henry. “The remark made by the Capt that the books were intended for the public and therefore every thing in them ought to be given in ordinary language is ridiculous. The results of science may in ~~general~~ some things be given in ordinary language but not the details or the processes.” Given his status, Henry concluded, “I do not wish to appear to volunteer in this matter but if you will request me by letter to act for you I will do so with much pleasure.”<sup>79</sup>

Gray promptly produced a long reply to seek the money and justice he deserved. First, he assured Henry that when preparing the manuscript for the Ex. Ex. Botanical Report, he had been cautious about ensuring that the English description of every species contained everything mentioned in Latin specific characters (despite lacking a word-by-word English translation). Second, he emphasized, “nothing can be further from the truth than his assertion that I engaged that ‘my whole time was to be given to the preparation’ of the Exploring Expedition—You well know that this would be impossible for me ever to have promised.” Soon he would send Henry necessary documents to prove his points, declared Gray, and, “The copy of the contract I shall send will show that it has not the least foundation; & I solemnly declare that the whole conversation between us at the time, upon which the contract was based, proceeded upon the contrary supposition & statement—.”<sup>80</sup>

Less than two days later, Gray sent Henry a batch of documents, including a copy of the contract he made with Captain Wilkes in 1848 and copies of his

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<sup>79</sup> JH to AG, 5/23/1852, *PJH, Vol. 8*, 318.

<sup>80</sup> AG to JH, 5/31/1852, *PJH, Vol. 8*, 325-326.

correspondence with the Library Committee. He also composed a long letter to explain why he and Torrey had decided to depend on their own judgment in the style of the Ex. Ex. Botanical Report instead of following Captain Wilkes's order: "I cannot but think that my studies & my familiarity with botanical literature should give my opinion, & that of Dr. Torrey, some weight; & that our strong objections (on a matter where we can have no private interests to subversive) are entitled to consideration."<sup>81</sup>

Henry was glad to find that Gray was sincerely trying to repair his relationship with Wilkes. He replied that he would contact the captain about the matter on Gray's behalf. He also gave Gray a lesson about how communication was carried on in the country's capital: "In Washington there is considered a wide difference between a personal and an official intercourse. Two persons who may not be on speaking terms when they meet in their private capacity may have frequent official communication." Taken altogether, Henry suggested that Gray and Wilkes should soon arrange "an interview with each other."<sup>82</sup>

"My dear friend," Gray replied,

No scientific man of my years has laboured more constantly and more disinterestedly than I have—work that ought to be done and done soon thickens upon me as you know, much faster than I can do it; time is most valuable and life is precarious. There is much also that is waiting for my attention. If I can go on with the Ex. Ex. Botany with Comfort, I am willing to push it forward as rapidly as possible. If I am arrested, it may be long before I can return to it.<sup>83</sup>

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<sup>81</sup> AG to JH, 6/2/1852, *PJH*, Vol. 8, 328-331.

<sup>82</sup> JH to AG, 6/16/1852, *PJH*, Vol. 8, 335-336.

<sup>83</sup> AG to JH, 6/18/1852, *HL*; *PJH*, Vol. 8, 337-338.

Henry decided to forward Gray's letter to Wilkes. "The letter is intended solely [sic] for my own eye," he explained to the Captain, "but it is written in such a spirit [sic] of candor and is so true and exposition of Dr. Gray's character that I venture to send it to you for perusal."

I hope you will not write to Dr. Gray until you have had another interview with me. Though the Dr. is a peaceful man of science I know him to have considerable spirit [sic] and requires to be managed somewhat gently. A broad side like that you gave Dr. Torrey would put an end to all attempts at reconciliation. Forgive the freedom of this remark. I make it to prevent farther difficulty.<sup>84</sup>

Soon Wilkes was on his way to Cambridge to meet Gray. The interview took place on July 15 and went well. The same day Gray sent Henry a short note: "I have had an interview with Capt. Wilkes today, of a very satisfactory character: in as much as Capt. Wilkes stated that I am at liberty to give the detailed descriptions of species &c—in place of the translation of the Latin diagnostic character, without having separately to repeat this diagnosis character in English."<sup>85</sup> He had successfully gained the upper hand in negotiations with Wilkes regarding the style of the Ex. Ex. Botanical Report.

But Gray's troubles with government officers were not over yet. He became increasingly troubled by Emory's tight control of the collections Wright made while serving in Emory's campaign (roughly from December 1851 to June 1852). Torrey was concerned that Gray might be annoyed. In August 1852, he explained to Gray why Emory had decided to place all botanical collections—including those made by Wright—in his hands: "It is a mere matter of etiquette—I shall, of course, put them

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<sup>84</sup> JH to CW, 6/21/1852, *PJH*, Vol. 8, 339.

<sup>85</sup> AG to JH, 7/15/1852, *PJH*, Vol. 8, 364.

(I mean those collected by Wright—for Bigelow & Parry will come also) in your hands—only asking a full set for myself. You will share Parry's & Bigelow's, if there be any among them that you want."<sup>86</sup> In another letter to Gray sent about a week later, he wrote,

You understand why they came to me first. It is that Maj. Emory determined from the commencement of his duty on the Boundary Commission, that all the collections made under his command should come to me. Wright having been, since Col. Graham was superseded, employed by Maj. E., & in the pay of Government; trusted one his specimens to me according to the Major's plan. As, however, you had made previous arrangements with Wright, & the poor man has never been properly remitted for his services, I made no claim for any thing but a full single set for myself. It is also understood that another set is to be received for the Smithsonian, & that Dr. Short, out of this, or out of the former collections, is to have a whole supply for the contribution which he made to Mr. Wright's expenses. Of course, I shall give you duplicates of every thing you may want from the collections of the three botanists of the Commission.<sup>87</sup>

Gray was indeed unhappy with Emory's arrangement with Torrey. "Torrey writes me that Mr. Emory required the late coll. to be turned over to him!" he complained to Wright. "Yet he [Torrey] shall at once hand them over to me, only deserving a set for himself—& he will furnish me duplicates of Parry's Bigelow's coll.—You had better bring them on from New York directly here."<sup>88</sup> Afterward, Torrey did send to Gray Wright's collection made under Emory's command, but nothing else. Gray grew annoyed. He complained to Engelmann,

All winter I kept writing to Torrey to send on to me a set of Bigelow's plants—but never saw, even to this day more than 5 or 6.—As Torrey all along expressed perfect willingness to send them, I began to think that Bigelow or Emory did not wish them to be sent, and—that I might do no harm, I therefore did not say, in

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<sup>86</sup> JT to AG, 8/6/1852, *HL*.

<sup>87</sup> JT to AG, 8/13/1852, *HL*.

<sup>88</sup> AG to CW, 9/11/1852, *AGPG*.

the preface of Pl. Wr. What I meant to have said: even tho' I had not seen Bigelow's collections. So it is their own fault. I am quite ready to duly credit any one who sends me plants, but I keep clear and utterly ignore all these petty jealousies,—of which I perceive you to have seen enough. Very contemptible they are:—but, as you say, this small notoriety is all that incites some of these people to collect; and so good to science comes out of these evil passions even. It was short-sighted and mean in Emory to stop Graham's appropriation.—In return Graham's friends may perhaps stop his.—while, with better counsels, we could use advantageously all the appropriations both could ever get. Personally, it only saves me trouble. I can readiest go on, as I have begun, in Smithsonian Contrib.<sup>89</sup>

He promptly published *Plantae Wrightianae: Part II* (1853). In the preface, he buoyantly outlined his future publishing plan:

In the former memoir, the publication of Mr. Wright's large collection made in 1849 was carried as far as to the end of the Order Compositae. On the present occasion I propose to give a similar account of these more recent collections, up to the same point, and hereafter, as soon as other engagements will permit, to combine the account of the remaining portions of these several collections, along with those of Mr. Fendler and Mr. Lindheimer (the publication of which has likewise been arrested at the same point), into one general memoir.<sup>90</sup>

Wright worked briefly with Gray to bring out *Plantae Wrightianae: Part II*. Then he felt that he had fulfilled his duty as a collector under the auspices of Gray's subscription system. In November 1852, he declared to Emory that he had decided to stay with his botanical friends in the East instead of continuing his service in the Boundary Commission.<sup>91</sup> According to newspapers and what he had heard from Baird of the Smithsonian, he wrote, he had been aware of the prevailing negative

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<sup>89</sup> AG to GE, 7/14/1853, *AGPG*.

<sup>90</sup> Gray, *Plantae Wrightianae Texano-Neo-Mexicano: Part II*, 5.

<sup>91</sup> CW to WHE, 11/4/1852, Folder 45, Box 6, *WHEP*.

attitude in DC toward the Boundary Commission.<sup>92</sup> “I shall in a few days be ready as far as depends on myself to return to the line but the prospect is [that] I shall not be able to get any part of my salary,” he explained. “It seems too to be a pretty general opinion that the Commission will have to suspend its labors in consequence of the action of Congress in regard to the appropriation bill and if so I may as well stay here... I will however act according to the best judgment I can form added by such advice as I can get from friends at Washington—if I can procure the means for returning.”<sup>93</sup>

Wright did not care what Emory’s response to his letter might be. He had decided not to have anything to do with the Boundary Commission. In November, about the time when he declared to Emory that he would not rejoin the Commission for now, he informed Gray that he was now looking for a position that might bring him far beyond the American southwestern frontier: “Baird’s last two letters (of 21 & 22nd [of October]) both referred to the Behring’s Strait Expedition and both express anxiety for an answer.” He had replied to express his eagerness to join this particular expedition, he told Gray, and had been “equally anxious to know the result” since then. Indeed, he stressed, thus far he had not received any salary for his

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<sup>92</sup> “Prof. Baird assumes me there is no chance for money until Congress meets.” CW to AG, 11/11/1852, *HL*.

<sup>93</sup> CW to WHE, 11/4/1852, Folder 45, Box 6, *WHEP*. Wright showed his letter to Torrey before sending it off. Torrey was struck by Wright’s rough attitude. But he let Wright’s letter go, and immediately composed a letter to Emory, apologizing for Wright’s etiquette (“He has just written you a letter,— & I consent let it go without adding a few lines,” Torrey explained). Torrey explained that Wright seemed “at a loss,” because the negative opinions about the Boundary Commission in DC had convinced Wright that he would eventually “give up his place & call you [Emory] all back.” JT to WHE, 11/6/1852, Folder 45, Box 6, *WHEP*.



service in the Boundary Commission, and he thought that he might be able to turn this depressing circumstance to his advantage. "My failure to get any money from Washington will be a good excuse for delay to return to the Boundary and yet I don't want to be here idle however pleasant it might otherwise be to stay with my family."<sup>94</sup>

Wright waited for another three months or so to receive the official appointment as the botanist of the so-called Behring's Strait Expedition. He promptly wrote to Emory asking for "a dismissal from the Boundary Commission." "Believing you would have no objection," he wrote, "I accepted the offer & received the appointment and have proceeded to make the necessary preparations for the voyage." Emory did not object to Wright's new appointment. He replied, accepting Wright's resignation and demanding Wright "turn over to Dr. John Torrey" all botanical collections (field notes included) made during Wright's service in the Boundary Commission.<sup>95</sup>

### **"Faithless conduct"**

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<sup>94</sup> CW to AG, 11/11/1852, *HL*. Around the same time, Baird informed Clark of Wright's whereabouts: "I should not wonder much if you did not get Wright back. In the first place he had no money, not having been able to collect of his accounts. Secondly, a naval expedition is about starting for the North Pacific and Behring's Straits under command of Capt. Ringgold, to which he will probably receive the appointment of Botanist..." SFB to JHC, 11/6/1852, Folder 3a, Box 2, *SFBP*; a transcription can also be found in Folder 4.

<sup>95</sup> See CW to WHE, 2/1/1853 and WHE to CW, 2/4/1853, both in Folder 48, Box 6, *WHEP*.

The physician and botanical collector John M. Bigelow was gravely unhappy with Gray and Wright's collaborative effort in bringing out *Plantae Wrightianae: Part II*. Having been dismissed from the Boundary Commission, he was now staying in his hometown in Lancaster, Ohio, dispirited and destitute. In March he wrote to Emory to explore the possibility of receiving some remuneration for his botanical labors performed for the Boundary Commission. "I have been at considerable personal expense in making my collections—Besides, all the pay I have ever received has been in consideration of my services as physician of Surgeon to the Boundary Commission,—I consider it but just I should receive something for my collection as well as my expenses."<sup>96</sup>

A month later, he wrote to Emory about how desperate he had become since reading Gray's *Plantae Wrightianae: Part II*. "I have seen Dr. Gray's Plant: Wright: part 2nd and on examination find that he [Wright] takes entire credit for every new plant gathered in our Lake Guzman excursion without so much as even intimating that I had been in company with him." Worse, he added, he found in Gray's introduction that Gray had aspired to publish a "general memoir" that would bring out every new species contained in Wright's collections made during Wright's service in the Boundary Commission. If Gray truly completed this "memoir," he told Emory, Gray would "deprive" him of much credit that was his and the Commission's due. Why should Wright be so privileged that he could not only keep the collections made during the Boundary Commission but also have Gray publish a monograph on his behalf and receive all of the credit? Bigelow wrote bitterly. In fact, he continued,

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<sup>96</sup> JMB to WHE, 3/11/1853, Folder 49, Box 6, *WHEP*.

Gray hardly mentioned Emory's contribution to advancing Wright's collecting enterprises in particular and American botany in general. Taken altogether, Bigelow begged for Emory's intervention. "You can prevent him [Gray] from bringing out [Gray's general memoir]—until the proper time—those collected under your orders; but whether you have the secure power over those collected under Col. Graham, I do not know."<sup>97</sup>

Emory was furious upon reading Bigelow's letter. At the time he was ill and bedridden in DC, and was unable to seek justice for Bigelow and the Boundary Commission. In September 1853, when he felt able to wield a pen, he wrote to Torrey, enclosing Bigelow's letter. "If it is true that Mr. Wright has acted as Dr. Bigelow stated," he wrote, Wright had been "guilty of a clear breach of honor" and

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<sup>97</sup> JMB to WHE, 4/23/1853, Folder 50, Box 6, *WHEP*. Bigelow also spread his dissatisfaction about the Wright-Gray alliance (if not conspiracy) among his correspondents. To Torrey, he emphasized that Wright ought not to be allowed to have Gray publish his (Wright's) botanical collections independently: "If a union of all our collections could be effected and published altogether in continuation of those contributions giving each collector his due award, it would make something credible to our government and detract nothing from the merits of those engaged in it." After all, he added, "for if we cannot make the music we are necessary in raising the wind so essential in successfully playing the organ of fame" (Quoted from Rodgers III, *John Torrey*, 225-226). To Baird, he wrote that "Should this [Gray's "general memoir] be done without reference to my collections, and before mine are published, it would be an unfortunate continuation of the hardships of which I am with reason complaining." At the end of the letter, he noted that "I will leave it to your own candor to decide, whether it is exactly just that the Smithsonian Institute itself, should anticipate me in the priority of plants I had previously collected, and sent to their fostering care" (JB to SFB, 4/19/1853, Folder 31, Box 14, *SFBP*). After deploying every possible means to stop Gray from publishing his planned "general memoir," Bigelow seemed satisfied. In fact, his experiences in the Boundary Commission and his relationship with Torrey and Baird had won him another great opportunity to explore and collect plants in the vast American West: He would serve as a surgeon in Lieutenant Whipple's Survey of the Southern Railway (JB to SFB, 5/22/1853, Folder 31, Box 14, *SFBP*).

surely “acted unworthy of a scientific man.” Emory reminded Torrey that he had “retained Mr. Wright in the Service of the Boundary Survey on my own responsibility and against great opposition.” From Emory’s perspective, Wright had been “liberally paid and provided with all the means of collecting.” Taking all these facts into consideration, he demanded, “If it was true that Wright had gone in to publish his collection through Gray “without due acknowledgement or authority, I desire you will at once withdraw from Dr. Gray all the collections placed in his hands by Wright and of necessary commence a legal process to recover them.”

From Dr. Gray’s high character I am certain however he would lend himself to no such dishonorable proceeding. A proceeding which will entangle me with the Government and oblige me to pay all the money-advanced to Wright by the Boundary Survey, which is deeply injurious to Mr. Wright’s collaborators and wholly pretending to be a gentleman.<sup>98</sup>

Two days after sending off the letter to Torrey, Emory reported to the newly appointed Boundary Commissioner Robert Blair Campbell: “I find my presence here... as much as our work particularly the Botany has been appropriated, without giving the Survey any credit, and a publication is about being commenced appropriating the whole of it, which I shall arrest. This has happened principally through the faithless conduct of Assistant Wright.”<sup>99</sup>

Torrey was taken aback. “I don’t think there will be the least difficulty about the Mexican Boundary Plants,” he replied, and assured Emory that Gray would not publish “any more of Wright’s plants” until the Boundary Commission had received “all novelties” and had published them as part of the Commission’s scientific report.

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<sup>98</sup> WHE to JT, 9/28/1853, *HL*.

<sup>99</sup> WHE to RBC, 9/30/1853, Folder 55, Box 6, *WHEP*.

Furthermore, he went on, while preparing this ultimate report, he would ask Gray to work out some botanical orders, and he was certain that Gray's participation would significantly improve the quality of the Commission's botanical report. Finally, regarding Gray's failing to give due credit to Emory, he explained that Gray had been "a little awkwardly situated with regard to Col. Graham, who asked him to finish a list of the plants collected by Wright while under his command."<sup>100</sup>

Emory was content with Torrey's reply. "I am very glad to confirm the arrangement you have made with Dr. Gray, and hope you will say so to him," he told Torrey.<sup>101</sup> But he was not prepared to let Wright off the hook. In July 1854, he prepared a report for George C. Whiting of the Department of the Interior regarding Wright's "faithless conduct" performed in the Boundary Commission. In it he demanded that Whiting freeze Wright's account, for Wright had "used the public collections for his own benefit and to the disadvantage of two Drs. Parry and Bigelow." Wright would receive no pay unless he appeared before a special committee, explained his conduct, and swore his innocence.<sup>102</sup> Gray stood up for and defended Wright. He wrote to Emory that he would take all responsibility and would like Emory to drop his charges against Wright. Emory agreed. Wright's family soon received a check for \$233.88.<sup>103</sup>

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<sup>100</sup> JT to WHE, 11/3/1853, Folder 57, Box 7, *WHEP*.

<sup>101</sup> WHE to JT, 11/5/1853, Folder 57, Box 7, *WHEP*.

<sup>102</sup> WHE to GCW, 7/18/1856, *CWC*.

<sup>103</sup> See WHE to GCW, 7/18/1856, *CWC*; JW to SFB, 6/13/1855, Folder 13, Box 66, *SFBP*. Wright was definitely cautious of Bigelow's resentment. In April 1853, he wrote to Gray from Brooklyn (at the time Wright had boarded the Steamer *John*

While helping Wright escape from Emory's charges, Gray felt compelled to follow Torrey's arrangement with Emory. Late in 1853, he complained to Short that he recently received "an intimation from Maj. Emory, that he would like to have some of his Report on Boundary Commission plants in general." To Wright, he explained that "Through Torrey, Emory solicited that I would wait a while before going on with P. Wright.—on the ground that when I did it would lick up almost all they had that was new."<sup>104</sup>

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*Hancock* for his upcoming expedition to East Asia): "Bigelow was here a few days since and began a sort of good natured quarrel with me because you did not give him credit for everything he and I jointly found. I told him he ought to be very thankful to me that he got any credit at all" (CW to AG, 4/4/1853, *HL*). Wright was right about this statement. To be sure, in a letter to Gray dated April 12, 1852 from Frontera, he had commented on Bigelow's character as a botanical collector and had asked Gray to give Bigelow "full credit." "Dr. Bigelow is of the right stamp [as a botanical collector] so far as he goes. As Dr. Torrey wrote me 'he is not much of a botanist' but he goes at it with his whole heart & soul though he makes scrappy specimens & throws them all together in wondrous confusion. Nevertheless his zeal will tell on the flora of the country I hope he will receive full credit for it—" (CW to AG, 4/12/1852, *HL*).

Wright had not been aware of how serious the matter had become until February 1855. He was in Hong Kong at the time, and was about to set sail for the Bering Strait. Over the preceding months, he had entertained the thought that the whole matter might be easily settled by soothing Bigelow's temper. As he told Gray, "I sent him [Bigelow] a real blizzard of a letter—if it don't cure him & put him in a good humor I must give him up as incorrigible and let him take it out in growling" (CW to AG, 8/12/1854, *HL*). Now, when he was so far away from home, the prospect that he would never receive any of his unpaid salary for his service in the Boundary Commission distressed him. Sitting in his cabin in the *Vincennes*, anxious about the imminent voyage to the North Pacific, he composed two letters to Graham and Emory, respectively. In them he cried that it would be utterly unfair for the Boundary Commission to refuse to reimburse him. Given his current whereabouts, he added, it was impossible for him to stand in court and defend himself. Moreover, he remarked, the expedition that he embarked on was replete with dangers and uncertainties: "should I die or be lost on the voyage—would my heirs get it at all?" (CW to JDG, 2/13/1855, and CW to WHE, 2/13/1855, Folder 13, Box 66, *SFBP*).

<sup>104</sup> AG to CWS, 11/24/1853, Coker, "Letters...", 126; AG to CW, 2/19/1854, *AGPG*.

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Gray did not publish the intended *Plantae Wrightianae Texano-Neo-Mexicano: Part III*, nor did he proceed with the long-delayed *Plantae Lindheimerianae, Part III* or *Plantae Fendlerianae Novi-Mexicanae: Part II*. In fact, from 1853 (when *Plantae Wrightianae Texano-Neo-Mexicano: Part II* was published) onward, he hardly published any catalogues designated for subscribers' use. The fact was that Gray no longer relied on the subscription system to acquire specimens from the field. When, in the 1840s, he literally had to *beg* for money from wealthy Bostonians to keep collectors in the field, Gray was now *inundated* by specimens consigned to him by dozens of governmental expeditions. His relationship with the field had undergone fundamental changes.

One of the most important factors that changed closet naturalists' relationship with the field was the widespread spirit of Manifest Destiny. The discovery of gold in California, the country's thriving commercial relations with China, and the technological innovations that made transcontinental and transpacific communications possible, all contributed to this extraordinary spirit. Enthusiasm for moving, migrating, traveling, trading, adventuring, and conquering became so contagious that even a hermit living in a cabin close to Walden Pond felt compelled to address the subject. "[W]hat does the West stand for?" Henry David Thoreau inquired of his readers in *Walden* (1854).<sup>105</sup>

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<sup>105</sup> To Thoreau, the question can be reframed as "does it mean that Americans should thrust into the nation's seemingly unending frontier?" His answer is a definitely "Nay": "be a Columbus to whole new continents and

It was against this intellectually charged and action-packed backdrop that the War Department launched the Pacific Railroad Surveys (1853-1856; hereafter PRS). In the space of three years or so, at least 26 surveying teams traversed the continent (along the 35th, 37th, 39th, 47th, and 49th northern parallels) to look for the best route to build a transcontinental railroad. Although in the end the PRS did not reach any conclusion regarding where the track should be laid, its surveying teams brought back astonishing numbers of specimens covering almost every department in natural history. Baird was involved in the PRS from the outset. Making good use of his dual roles as a man of science and as a critical member of the elite circle of Washington, DC, he helped shape the PRS into not only an engineering but also a natural-history project. With Secretary Henry's support, Baird played many roles over the course of the PRS—from recommending collectors to the War Department, to curating the PRS's natural-history collections, to distributing collections among men of science, to enumerating the zoological collections, to authoring the PRS's zoological report.

Baird knew how eager his fellow men of science were to use the PRS's natural history collections. He soon established regulations of access to the PRS's collections. "I have been in a dilemma as to how I could let you examine the specimens of turtles brought home by the government expeditions without incurring official risk from their commanders," Baird wrote to Agassiz in January

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worlds within you, opening new channels, not of trade, but of thought. Every man is the lord of a realm beside which the earthly empire of the Czar is but a petty state, a hummock left by the ice." Henry David Thoreau, *Walden* (Boston: Houghton, Mifflin and Company, 1897), 494-495.



1856, in response to Agassiz's request to integrating the PRS's collections into his widely subscribed *Contributions to the Natural History of the United States of America* (the first volume was published in 1857). "[T]here is such a precision and technicality in relation to the government collections, that we have to [be] very careful in our movement," he went on.

The following arrangement, if you agree to it, may make the matter perfectly practicable. It is:—for you to consent to prepare a report on these collections, to appear in your own name, of course, as the Chelonians, by Prof. L. Agassiz. This to consist of a description in full of all the species, with specific characters, synonymy, etc.

2. A list of the species, with characteristics of the new ones, to be published in the Proceedings of the Philadelphia Academy, in the same official way as I have published these, to be reproduced with a full description in the reports of the expeditions.

Baird then offered his suggestions as to how Agassiz could get the species published in the most effective and efficient way:

Of these expeditions there are seven or eight entirely different ones. It would be much trouble for you to prepare as many different reports; the best plan will be to make out a single one for all, putting each species with its specific characters on separate sheets. Send this to me, and I will make the assorting by expeditions and arrange the articles for publication in the proceedings of Acad. Nat. Sciences under your name.<sup>106</sup>

Gray knew Baird's rules well. During most of the 1850s, he compulsively produced descriptions of species for the PRS's botanical report. He understood that he needed to do so—for otherwise he could hardly receive sufficient specimens to "calibrate" those species included or to be included in *A Flora of North America*.

(Also, helping the War Department bring out the botanical report gave him a good

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<sup>106</sup> SFB to LA, 1/28[?]/1856, Elmer Charles Herber, *Correspondence between Spencer Fullerton Baird and Louis Agassiz—Two Pioneer American Naturalists* (Washington, DC: Smithsonian Institution, 1963), 114-115.

excuse to ask for duplicates to be deposited in his herbarium.) The work of cataloging the PRS's botanical collections was formidable. At times Gray found that he had to rack his brains to come out with something novel to be included in the PRS's report. "As to Steven's Collection [said to be made in Washington Territory] it stopped just when it began to be of any consequence whatever," he complained to Baird. "I believe none were sent me really from Washington Territory." "Of course," Gray went on, "I might put some botanical value into them by dint of study,—but there is really nothing in the collection worth making a Report about."<sup>107</sup>

But participating in the PRS and other governmental surveys did bring about changes in Gray's view of botanical nature. Late in the 1840s, when he was preparing *Genera Florae Americae Boreali-Orientalis Illustrata* (1848-1849), what concerned him was how to represent the North American flora by a group of specifically selected genera (or representative genera). He believed that a flora could be illustrated as such, for genera represented nature's plan.<sup>108</sup> Toward the end of the 1850s, however, he no longer embarked on any project of this sort. Instead, in the face of the dramatic explosion of knowledge about the North American flora, what he envisioned was "a complete catalogue of the species."

... it would have been most convenient and acceptable to botanists to have cited the numbers of Wright's distributed collections throughout, and also, as far as possible those of Fendler, Lindheimer, and of Berlandier's posthumous distribution. A systematic catalogue of all the plants enumerated and described in these various Western Expeditions, or rather a complete catalogue of the

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<sup>107</sup> AG to SFB, 10/18/1854, Folder 4, Box 5, *CM*.

<sup>108</sup> See Daston, "On Scientific Observation," 102-103.

species of the United States west of the 100th parallel of longitude, including those of the Mexican border, is now very much wanted.<sup>109</sup>

Faced with the prospect of coming up with “a complete catalogue of species of the United States,” Gray mostly did not even bother to offer specific descriptions (except for new species) in the entries he composed for the PRS’s botanical report. A contemporary reader of Gray’s entries would only find references like these: “*Eddya hispidissima*: the same as No. 845 of Mr. Wright’s Texan collection (1849) and No. 1557 of his New Mexico collection.”<sup>110</sup> Undoubtedly, in order to gain a clear idea of what this description actually entailed, the reader would need to secure a copy of *Plantae Wrightianae Texano-Neo-Mexicano: Part I* and, if necessary, contact Gray for duplicates of the specimens No. 845 and No. 1557, or pay a visit to Gray’s herbarium to examine the actual specimen. By reducing specific descriptions to a series of references, Gray maintained a trend that had gradually prevailed among naturalists since the mid-1840s: namely, to concentrate the power and authority to name and describe species in the hands of those competent naturalists (that is, naturalists equipped with comprehensive collections of books and specimens).

Another change took place in Gray’s taxonomic practices during the 1850s was that he consciously promoted a principle that would later become *the* standard in

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<sup>109</sup> Asa Gray, “The Botany of the Mexican Boundary,” *The American Journal of Science and Arts (2nd Series)* 27, no. 83 (1859), 291-292.

<sup>110</sup> John Torrey and Asa Gray, “Report on the Botany of the Expedition: Route near the Thirty-Second Parallel of North Latitude, under the Command of Brevet Captain John Pope, Corps of the Topographical Engineers,” in *Reports of Explorations and Surveys, to Ascertain the Most Practicable and Economic Route for a Railroad from the Mississippi River to the Pacific Ocean, Vol. II* (Washington: Beverly Tucker, 1855), 170.

twentieth-century botanical taxonomy: the categorical importance of type specimens. As historian of science Lorraine Daston points out, toward the end of the nineteenth century, more and more naturalists began considering the so-called type specimen “no longer one herbarium sample among many but ‘the most important material in a museum of natural history’.”<sup>111</sup> This transition was evident in Gray’s taxonomic practices throughout the 1850s. By serving as the “clearing house” for multiple governmental surveys, he was able to ensure that his herbarium housed a great number of type specimens, thus consolidating his herbarium (not yet Harvard’s) as *the* herbarium that represented the North American flora. “My herbarium is getting to be important,” he told Baird late in 1852. “[I]t is important that it should contain, as far as possible, specimens of new genera & species which I characterize.”<sup>112</sup>

Gray was by no means the only naturalist eager to turn the biota of North America into a catalogue of species. Baird promoted and practiced this very principle with enthusiasm, as well. In 1852, he wrote to his good friend J. P. Kirtland of Cleveland, declaring how much he and his assistant Girard had been engaged in the “descriptive catalog business” (Figure 6-6).

I have something to tell which I know will please you. Mr. Girard and I have on hand a descriptive catalogue of the serpents in the Smithsonian Museum, in fact including all of North America. We revise the whole subject, and give original descriptions of all species, old and new. Localities and items (?) of all are given in full... This will be followed by similar catalogues of the other reptiles and fish!!!

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<sup>111</sup> Daston, “Type Specimens...,” 174.

<sup>112</sup> AG to SFB, 12/22/1852, Folder 8, Box 3, *CM*. Regarding the transition from Gray’s herbarium to Harvard University Herbaria, see Wood’s remarkable “Gray’s Herbarium...,” 321-342.



Figure 6-6. This engraving is from *Annual Report of the Smithsonian Institution for the Year 1850*, 41-50, under the title “Cataloging and Classification of Specimens.” (Courtesy of the Smithsonian Institution Archives, Washington, DC, USA.)

All soon as possible. Now I would like to have you bundle together all the specimens of serpents you have or can borrow in Cleveland, and send on immediately by Express. I will examine and label all carefully according to the catalogue, and return all very soon; in less than a month, if you say so. Send even the commonest, and all specimens of a species. No time is to be lost, as the catalogue is in print. I want the benefit of having numerous specimens from which to eliminate the specific character. There might possibly be a new species, although that is hardly probable... We are going strong into the descriptive catalogue business and hope of these days to rival to British Museum series. Our Reptilia ones will be a complete manual, embracing more than twice as many species as Holbrook—we have so many new and old things from the country beyond the Missouri.<sup>113</sup>

Moreover, toward the end of the 1850s, Secretary Henry finally yielded to the government’s pressing requests and conceded that the Smithsonian ought to play an active role in preserving, curating, and displaying collections made in multiple governmental surveys and explorations—in other words, the Smithsonian should

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<sup>113</sup> SFB to JPK, 12/[undated], 1852, Folder 3a, Box 2, *SFBP*; a transcription of this particular letter can be found in Folder 4.

serve as the museum for the United States of America. (Henry accepted this request on the condition that Congress would pay all expenses incurred.) Baird grabbed the chance to establish the Smithsonian's authority over zoological taxonomy and nomenclature (much as Gray did in botany with his herbarium). He evidently applied the type-specimen concept to compose the PRS's and other governmental surveys' zoological reports. As he put it in his introduction to the PRS's zoological report (1857), "I have usually made the entire detailed description of the species from one particular specimen, (often indicating it by number,) mentioning afterwards the variations presented from this type by the others before me." Then, with this principle set forth, he claimed that the "Smithsonian Museum Catalogue" was the cornerstone upon which the PRS's report was based and to which any question about classification should be referred.<sup>114</sup>

Turning the flora and fauna of the United States into a catalogue of species became a project that fascinated a group of practically minded naturalists including Gray, Baird, Torrey, Curtis, Sullivant, Engelmann, and Tuckerman. Enthralled by the number of unknown biological forms that had been unearthed by the PRS and other governmental surveys, these "practical naturalists" did not follow the model of the U.S. Exploring Expedition in composing lavishly printed monographs; nor did they follow the model advocated by prominent naturalists like Agassiz, who preferred to write books about the natural systems of classification derived from specimens, but

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<sup>114</sup> Spencer F. Baird, "General Report upon the Zoology of the Several Pacific Railroad Routes," in *Reports of Explorations and Surveys, to Ascertain the Most Practicable and Economic Route for a Railroad from the Mississippi River to the Pacific Ocean, Vol. VIII* (Washington: A.O.P. Nicholson, 1857), xxvii-xxviii.

hardly dealt with specimens *per se*. Instead, with specimens and field information pouring into their hands, they produced catalogues and published them in scientific journals or even in proceedings of learned societies' meetings. What ultimately mattered to these practical naturalists was the issue of scientific priority—for it elevated the status of American science, pleased governmental officers, increased their “cataloging power,” and made progress toward their common goal: a complete catalogue of species of the United States.

To this group of practical naturalists, the process of compiling species catalogues was by no means dry or grueling, as the term “catalogue” might suggest. On the contrary, they found the whole process enjoyable and teeming with intellectual challenges. To them the fundamental unit that constituted a catalogue of species—generic and specific names—demonstrated the soundness of naturalists' “taste” and judgment. To ensure that those unknown organisms were correctly named, these practical naturalists exchanged opinions, asked for advice, debated and defended their choices of names. “Your geographical generic names are not so bad; but—pray do not run on them too much,” Gray told Baird in April 1853. “Generic names of animals (more by taken of reptiles) taken from those of men, are in my view detestable—for species, very well.—”<sup>115</sup>

In a letter to George Thurber (1821-1890), a Providence-based chemist who joined the U.S.-Mexican Boundary Survey as a botanical collector, Gray made a series of comments about how to correctly coin a specific name. Regarding Thurber's

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<sup>115</sup> AG to SFB, 4/2/1853, Folder 27, Box 3, *CM*.

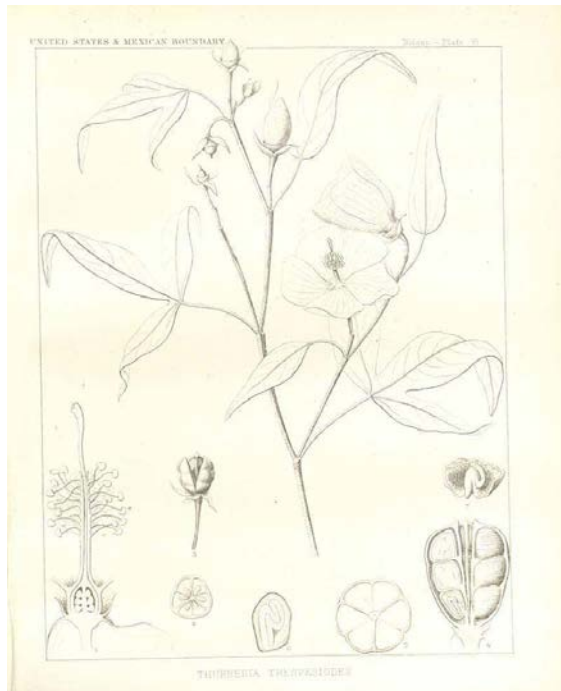


Figure 6-7. *Thurberia thespesioides* (From Emory, *Report on the United States and Mexican Boundary Survey*, 1857. Courtesy of the Joseph F. Cullman 3rd Library of Natural History, Smithsonian Libraries, Washington, DC, USA.)

proposal to name a new species “*Ranunculus Huntiana*,” wrote Gray, “I never liked naming a plant after a person who has had nothing to do with it, as collector, describer, and nothing else; therefore do not like *R. Huntiana*.” As for Thurber’s inquiry about how to name a new species representing the new genus named in Thurber’s honor, Gray’s spirit was enlivened. “*Thurberia* specific name?” he wrote,

“*Thurberia palmata*” might pass, and would anglicize into “the handy Thurber,” but then the hand has only three fingers.

“*T. tridactyla*” would meet this; but only birds are tridactylous; besides, the uppermost leaves are entire.

Taking another tack, from its smoothness, we might say, *T. glabra* or *T. laevis*; or, as I believe you have not a strong beard, *T. imberbis*. But, on the whole, perhaps it would be as well to indicate merely the nearest affinity of the genus, and call it “*Thurberia thespesioides*,” as it is nearest *Thespesia*. Take your choice, though,



of any of the above, to which add “*T. rosea*,” if the color of the flower warrants that name.<sup>116</sup>

After consulting Thurber, Gray published the species under the name *Thurberia thespesioides* in a volume titled “*Plantae Novae Thurberianae: The Characters of Some New Genera and Species of Plants in a Collection Made by George Thurber, Esq., of the Late Mexican Boundary Commission, Chiefly in New Mexico and Sonora*” (1854; Figure 6-7).<sup>117</sup> In it, besides diagnosing this “elegant,” “herbaceous, doubtless perennial herb,” he named a “curious,” “small,” and “wholly thorn-brush” shrub, with “perfect spinous branches throughout” “*Holacantha Emoryi*.”<sup>118</sup>

What did “a complete catalogue of the species of the United States” mean to botanists of the day? Notably, in 1859, when Gray had just published his views on the necessity of producing a complete catalogue of species of the North American flora, he published a notice for a volume titled *Catalogue of the Phaenogamous and Acrogenous Plants Contained in Gray's Manual of the Botany of the Northern United States, Adapted for Marking Desiderata in Exchanges of Specimens, Etc.* (1859).<sup>119</sup>

A help of this sort in making exchanges has often been asked for, and the enterprising publishers of Gray's Manual have responded to the demand by

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<sup>116</sup> AG to GT, 4/20/1854, LAG, 408-409.

<sup>117</sup> The species is now considered a variety of *Thespesia lampas*. See <http://www.theplantlist.org/tpl/record/kew-2522429>

<sup>118</sup> Asa Gray, *Plantae Novae Thurberianae: The Characters of Some New Genera and Species of Plants in a Collection made by George Thurber, of the late Mexican Boundary Commission, Chiefly in New Mexico and Sonora* (Cambridge: Metcalf and Co., 1855), 312.

<sup>119</sup> The volume's author is Horace Mann (1844-1868), one of Gray's favorite students at Harvard.

publishing, at a low price, this neat Catalogue, for which good office they deserve the best thanks of our scattered botanists. The species are numbered consecutively, from No. 1, *Atragene Americana* to 2421, *Azolla Caroliniana*. The list, in double columns, fills thirty-two pages of the same size as those of the Manual itself. A cent stamp will pay the postage of the pamphlet to any part of the United States; and the sender has only to indicate to his distant correspondent, by marking or by copying the numbers, the species which he desires to receive or is able to furnish. Moreover, the names of the orders, which are printed in bold type, and even those of the genera, may serve another useful purpose: they may be cut out and used for labels in the herbarium.<sup>120</sup>

A well-ordered catalogue of species thus served as the unit of account in a world where species were widely regarded as the medium of exchange. Understandably, only a limited number of individuals and institutions were privileged enough to produce catalogues of species. By the mid-1850s, as his herbarium became home to a great number of type specimens of American plants, Gray secured such a privilege. Toward the end of his life, however, when American botany became increasingly decentralized, and multiple individuals and institutions competed for this “cataloging power,” he grew worried.<sup>121</sup> In public and in private, he commented on the current worrisome status of American nomenclature as if he were a secretary of the treasury facing an economic crisis and compelled to revive the public’s confidence in the nation’s currency. Species were what competent naturalists said

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<sup>120</sup> Asa Gray, “Catalogue of the Phaenogamous and Acrogenous Plants Contained in Gray’s *Manual of the Botany of the Northern United States*, Adapted for Marking Desiderata in Exchanges of Specimens, Etc.,” *The American Journal of Science and Arts (2nd Series)* 27, no. 83 (1859), 292-293.

<sup>121</sup> See Kingsland, *The Evolution...*, Chapter 2.

they were. The scientific public needed to rely upon competent naturalists' judgment of species to insure that transactions functioned effectively.<sup>122</sup>

Moreover, as his flourishing association with the government brought him research material and information from the field, thus rescuing him from sinking deeply into the subscription system, he grew more excited about his future as "Professor" and as "an American Botanist." In November 1853, with his forty-third birthday around the corner, he wrote to Charles Short about his plan for the next decade. He had been serving as a Harvard professor for a decade, and he thought that the year 1853 was an excellent moment to contemplate his next step. He listed an ambitious array of research and publishing projects. He was thrilled by his own vision, Gray told Short, but he was confident that he would eventually prevail.

My next 10 years, if it please God that I have them here, should be my best ones, I am 43 years old this week! I can just barely get on comfortably with my salary—having no children, and my dear wife some \$3000 capital accumulating interest for her future, and ultimate expectations of perhaps \$8000 more, we have no anxiety on her future account—So that the doing the work incumbent on me to do as Professor—and as an American Botanist, is just the work before me—which I engage in with alacrity, and with satisfaction, and single-mindedness.<sup>123</sup>

Aside from Gray's strikingly candid words about the conditions necessary to be a Harvard professor and an American botanist (with a rich wife and with no children), Gray's above letter is revealing. As is well known in the history of science, by the end of 1863, Gray had emerged as the most famous botanist in the United States, had shown that the theory of evolution by natural selection could be applied to botany,

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<sup>122</sup> See, for example, Asa Gray, "Remarks Concerning the Flora of North America." *The American Journal of Science (3rd Series)* 24, no. 143 (1882), 321-331.

<sup>123</sup> AG to CWS, 11/24/1853, Coker, "Letters...," 128.

had got the better of the charismatic Swiss naturalist Louis Agassiz in evolutionary debates, and was about to begin his ten years of service as president of the American Academy of Arts and Sciences. In the words of his close colleague Charles Sargent, Professor Asa Gray had become a “philosophical naturalist.”<sup>124</sup>

Two Charleses prompted Gray’s remarkable transition in the mid-1850s. The first Charles, Charles Darwin, staying put in Down House in Kent, relentlessly drove Gray to investigate an array of critical issues that Gray had for a while refrained from touching upon: the origin and distribution of species. The second, Charles Wright, circumnavigating the globe as a botanist appointed to the U.S. North Pacific Exploring Expedition, made comprehensive collections that represented the floras en route. The place that made a research team out of Gray and the two Charleses was Japan, which offered “the greatest interest,” as Gray would later remark.<sup>125</sup>

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<sup>124</sup> Sargent, *Scientific Papers of Asa Gray, Vol. I*, 125.

<sup>125</sup> Gray made this remark in a letter dated April 27, 1859 to Alphonse de Candolle about taking de Candolle’s son on a botanical expedition. Gray suggested that the expedition should begin in Oregon and British Columbia, followed by an exploration of the interior of the Sandwich Islands. The climax of the expedition, however, should take place in the “the Kurile Islands, and all the northern part of Japan, Yesso [Hokkaido], and the islands northeast of it.” In Gray’s opinion, this region—fragmented as it might be—offered “the greatest interest.” “The expedition you have suggested strikes my fancy as the best that could be,” Gray admitted. “[We] would take your son through regions full of interest, safe to explore, and healthy. Certainly I can suggest nothing better” (AG to ADC, 4/27/1859, *LAG*, 451-452; regarding de Candolle’s inquiry, see ADC to AG, 1/23/1859, *HL*). Unfortunately, however, upon receiving Gray’s reply, de Candolle could not help worrying whether a trip to the farthest corner of Northeast Asia was too much for his son. Gray’s proposed excursion never got off the ground.

**PART III**  
**TWO CHARLESES**

How does one organize an expedition: what equipment is taken, what sources read; what are the little dangers and the large ones? No one has ever written this.

—John Steinbeck

As I cannot go upon a Northwest Passage, then I will find a passage round the actual world where I am.

—Henry David Thoreau

## CHAPTER SEVEN

### “Empire of Commerce and of Science”

Washington, DC. October 1852.

Secretary of the Navy John Pendleton Kennedy was composing his annual report to Congress. It was 1852, and he had no doubt that the Navy Department under his leadership had made extraordinary achievements during the past year. Of importance was the task of employing the East India Squadron for a special mission to Japan. He was convinced of one thing: commanded by Commodore Matthew C. Perry, and equipped with four steamers (*Susquehanna*, *Mississippi*, *Princeton*, *Alleghany*), three sloops (*Plymouth*, *Saratoga*, and *Vandalia*), two store-ships (*Supply* and *Southampton*), one ship-of-the-line (*Vermont*), and one corvette (*Macedonian*), the Japan expedition would open negotiations with Japanese authorities, gather firsthand information about the country, and thereby render itself a cornerstone upon which the United States could “establish relations of amity and commerce” with this self-secluded empire.<sup>1</sup>

Kennedy went on to remark that the Navy Department was now occupying itself with a project “somewhat allied in character and importance” to the Japan expedition. Thanks to the naval appropriation bill of the last session of Congress, he wrote, the Navy Department now had a budget of \$125,000 “for the building or purchase of suitable vessels, and for prosecuting a survey and reconnaissance, for

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<sup>1</sup> Kennedy’s report can be found in *MPUSTH*, 295–297. The sentence about Japan’s “gross oppression and cruelty” is on p. 295.

naval and commercial purposes, of such parts of Behring's Straits, of the North Pacific ocean, and the China Seas." Though earnestly concurring with Congress's decision and highly appreciative of Congress's support, he continued, the department found that Congress's money would hardly suffice for an expedition of such geographical and intellectual scale. The fact was that \$125,000 could purchase only "one steamer of an inferior class, and perhaps two small brigs," he noted. Had the department followed Congress's directions, he doubted if enough money would be left for procuring scientific instruments.<sup>2</sup>

Kennedy then made explicit what he had in mind. Instead of "the building or purchase of suitable vessels," he stated that he preferred to use the department's existing vessels, which could be "fully manned and equipped, and furnished with all the necessaries appropriate to the hazardous nature of their cruise, constitute the material elements of the expedition" (Figure 7-1).

I have accordingly put the Vincennes, one of our staunchest and best sloops-of-war, in the lead of the expedition. I have added to this the propeller John Hancock, which, being found to have an engine of the strongest construction, needed only some alterations in her size and frame, and the addition of new boilers, to make her in every respect a most efficient contribution to the force required. She has, with this view, been placed in the hands of the naval constructor, who is now assiduously at work, and I am happy to report with all desirable success, in fitting her out with every accommodation which her future operations may demand. Besides these two vessels, the brig Porpoise has been detailed for the expedition, and put in condition for all the exigencies of her employment. A small pilot-boat, adapted to speedy navigation and shallow waters, will be added to the squadron.<sup>3</sup>

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<sup>2</sup> *MPUSTH*, 297-298.

<sup>3</sup> *MPUSTH*, 298.

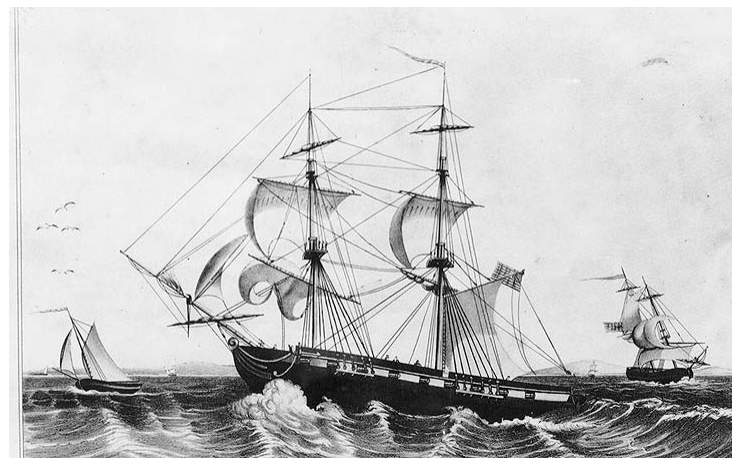
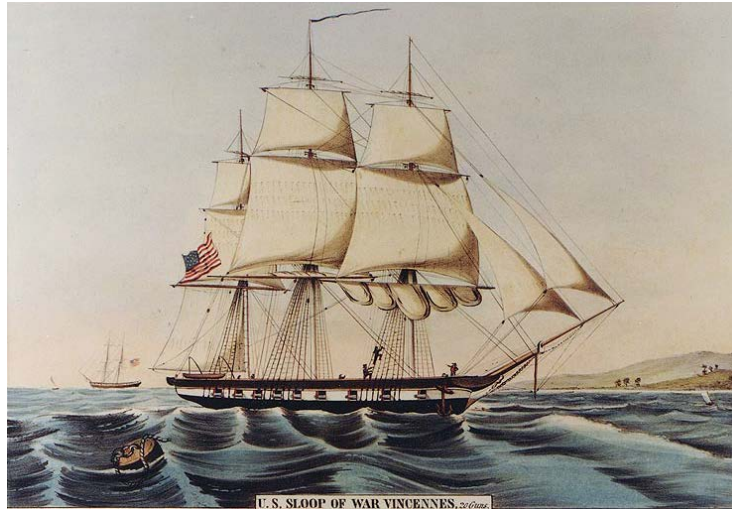


Figure 7-1. The three major vessels in the NPEE: *Vincennes* (above), *John Hancock* (middle), and *Porpoise* (below). (The drawings of the *Vincennes* and the *Porpoise* are from the U.S. Naval Historical Center Photograph; the drawing of the *John Hancock* is from Habersham, *My Last Cruise*, between p. 156 and p. 157. Courtesy of Naval History and Heritage Command and the National Museum of Natural History Library, Smithsonian Libraries, Washington, DC, USA.)



The Secretary went on to note that the money thus saved would be employed to procure scientific instruments and to appoint competent men of science. Thus far, he continued, he had “supplied the squadron with an astronomer and hydrographer of known ability and accomplishment, and also with a naturalist and botanist, who were charged with the duty of collecting and preserving specimens of such natural productions as may be interesting to science and commerce.” As for the commander of the expedition, he wrote that he had appointed Cadwalader Ringgold, a veteran of the U.S. Exploring Expedition, to command the expedition. “Particular attention will be given to the survey of the seas and coasts through and along which our whaling ships pursue their perilous trade, looking carefully to the coast of Japan, the Kurile Islands, the sea of Okhotsk, and the unexplored shores of Northern Asia.”<sup>4</sup>

After putting the finishing touches on the report, Secretary Kennedy felt relieved. He had no doubt that the scientific and commercial value that might be derived from Ringgold’s expedition would be an appropriate contribution to his beloved country. Kennedy understood that he would soon bow out as Franklin Pierce began his presidency. Uncertain of whether or not his successor would share his enthusiasm for securing the country’s commercial and navigational interests by a well-planned and executed scientific expedition, the retiring secretary resolved to render

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<sup>4</sup> *MPUSTH*, 298–299. Regarding the life and career of Cadwalader Ringgold, see Alan Fraser Houston, “Cadwalader Ringgold, U. S. Navy: Gold Rush Surveyor of San Francisco Bay and Waters to Sacramento, 1849-1850,” *California History* 79, no. 4 (2000/2001), 208-221.



Figure 7-2. Cadwalader Ringgold between 1860 and 1870. (Library of Congress Prints and Photographs Division, Washington, DC, USA)

Ringgold's expedition so firmly grounded that no one could easily alter or undermine it.<sup>5</sup>

### **“Not for conquest but discovery”**

Commander Ringgold (Figure 7-2) was content that the Navy Department had selected him to command a recently initiated expedition to the China Seas and the North Pacific Ocean. After hearing about Congress's naval appropriation, he had promptly made his desire to command the expedition felt in the Navy Department. “In organizing the Expedition, I beg leave, very humbly, to tender you my service, for the command of it,” he had written. “My long service in command of one of the vessels of the late Exploring Expedition, and consequent familiarity (I would hope), with duties necessary to the successful accomplishment and execution of the

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<sup>5</sup> Henry wrote to Gray in November 1852: “Mr. Kennedy the present secretary of the navy, is a man of more liberal views, than his predecessors, and is inclined, during his short reign, to make his marks, so deeply that they cannot be readily effaced.” *PJH, Vol. 8, 402.*

important enterprise, I trust will be favorably remembered and merit the consideration of the Department.”<sup>6</sup>

Ringgold’s carefully worded letter worked. His appointment soon arrived, along with a letter from Secretary Kennedy stating that the expedition was “not for conquest but discovery,” and that Ringgold should ensure that the expedition contributed to the expansion of “the empire of commerce and of science.”<sup>7</sup> Kennedy had tried to impress upon Ringgold the *axioms* that the U.S. government had established since the Exploring Expedition. Indeed, in July 1840, when Ringgold was serving in the Exploring Expedition (Ringgold was the lieutenant commanding the brig *Porpoise* at the time), he led an attack against villages in Fiji in retaliation for Fijians’ murder of two crewmen. It was Commander Charles Wilkes who issued the order, and the efficiency with which Ringgold carried it out probably exceeded Wilkes’ expectations. Some eighty Fijians were killed, two villages were entirely demolished, and one Fijian leader was captured and shipped back to the United States. (This Fijian would die on the way. His head would be cut off, and the skull would be preserved and become one of the trophies of the expedition’s scientific collection.) Not one of Ringgold’s men suffered serious injury during the attack.<sup>8</sup>

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<sup>6</sup> CR to JPK, 9/16/1852, F372-373, R3, *REENP*.

<sup>7</sup> Quoted from Cole, *Yankee Surveyors...*, 7.

<sup>8</sup> Wilkes’ order to Ringgold (dated July 25, 1840) can be found in Charles Wilkes, *Narrative of the United States Exploring Expedition, during the Years 1838, 1839, 1840, 1841, 1842, Vol. III*. New York: G. P. Putnam & Co, 1856), 423–424. It is worth noting that Wilkes would stand before the Judge Advocate at the Court of Inquiry convened for investigating Ringgold’s “insanity” during the first half of the NPEE. Wilkes would give his full endorsement of Ringgold’s capacity as a Navy officer, and his order and Ringgold’s report regarding the attack against the Fijians would be

Ringgold began pondering how an exploratory project could maximize its contribution to science. During his service in the Exploring Expedition, he had witnessed precious natural-history specimens become spoiled. Moreover, when the expedition concluded, and its natural-history collections were transferred to corresponding institutions in Washington, DC, for examination, preservation, and display, he was struck by these institutions' incompetence. With these memories in mind, he was determined to tackle the thorny issues of specimen collecting and preservation as early as possible. "Experience suggested to me, the propriety of making provision, before the departure of the Expedition," he informed the newly appointed Navy Secretary James C. Dobbin.<sup>9</sup>

Ringgold's first step was to contact Henry of the Smithsonian. He wondered whether the Smithsonian could furnish the expedition with necessary instruments for natural-history collecting and serve as the expedition's "depository." In the history of U.S. Navy expeditions, Ringgold's proposal was almost unprecedented.

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presented at the court as a testimony to Ringgold's "soundness" in the midst of a stressful military operation. In fact, by the time Ringgold made his wish to command the NPEE known to the Navy Department, he had acquired a reputation for effectively protecting his men and vessel from harsh circumstances. Navy Department, "Record of Proceedings of the Court of Inquiry Convened Under the Act. Of Jan. 16, 1857, in the Case of Comdr. Cadwalader Ringgold, Oct. 27–Nov. 27, 1857," in the Roll 27, *REENP*.

<sup>9</sup> CR to JCD, 4/22/1853, F60-62, R2, *REENP*. Regarding the incapability of the scientific institutions in Washington, DC, respecting the preservation of the specimens procured during the Exploring Expedition, see Stanton, *The Great United States Exploring Expedition of 1838–1842*, Chapter 18. For a comprehensive and systematic introduction to the scientific achievements of the Exploring Expedition, see Herman J. Viola and Carolyn Margolis, eds. *Magnificent Voyagers: The U.S. Exploring Expedition, 1838–1842* (Washington, DC: Smithsonian Institution Press, 1985).

Afterward, when facing a challenge from Secretary Dobbin, he explained that he had decided to do this after consulting a number of men of science. “[T]he Smithsonian Institution is in communication with the various scientific associations throughout the Continent, and possesses advantages which are not enjoyed by similar associations or individuals in this country in procuring such works as will be useful to the Expedition frequently long before they are finally published.”<sup>10</sup>

Henry decided to recommend Assistant Secretary Baird to Ringgold for the task of settling the terms and conditions needed for the collaboration between the Smithsonian and the Navy Department. He knew of Baird’s enthusiasm for improving the country’s standards of natural-history collecting and preservation and appreciated Baird’s diplomatic skills in negotiating with governmental agencies. Then, with Baird’s assistance and the Navy Department’s money, Henry borrowed, rented, and purchased natural history instruments for Ringgold’s expedition. In December, when the instruments arrived, he invited Ringgold and the expedition’s scientific corps to visit the Smithsonian. He ordered Baird and other staff to put their daily duties aside and concentrate on Ringgold’s visit. He wanted Ringgold and the scientific corps to be completely aware of the expedition’s crucial role in advancing “the increase and diffusion of knowledge.” “The cost of these labors to the Institution, if estimated by the salaries paid the officers during the time they were employed in this duty, would amount to several hundred dollars,” he remarked afterward.<sup>11</sup>

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<sup>10</sup> CR to JCD, 4/22/1853, F60-62, R2, *REENP*.

<sup>11</sup> JH to JCD, 12/28/1853, *PJH*, Vol. 8, 506.

Baird met Ringgold for the first time in October 1852. He was glad to find that the Commander would give him full authority to proceed with the preparations for natural history collecting and preservation.<sup>12</sup> In November, after another meeting with Commander Ringgold, he prepared a memorandum for Ringgold's reference. Baird first tried to dispel Ringgold's possible worries about recruiting civilian men of science to participate in a Navy expedition. "To avoid unnecessary criticism on the part of illiberally disposed individuals," he wrote, it seemed inevitable to appoint men of science under "some appellation other than that of naturalist," for example, "computer" and "assistant astronomer." Second, he urged that Ringgold should enforce the order that the "specimens which may be collected should be handed over to you as soon as catalogued and in a condition for permanent preservation." Then Ringgold should contrive to consign the collections to the Smithsonian, Baird emphasized. "As there are a number of expeditions already in possession of your proposed field of operations, in entering upon it, it will be necessary to use the greatest possible dispatch in bringing the scientific results before the world, placing them in immediate record, and thus securing the credit of priority as far as possible." Baird guaranteed that upon receiving the specimens, the Smithsonian would lose no time in unpacking, arranging, and undertaking "a judicious and careful examination." Needless to say, Baird noted that the Smithsonian would be careful about preserving labels, journals, notebooks, and other textual information that accompanied the specimens, thus ensuring that no such critical information as

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<sup>12</sup> See Baird's letter to John Rodgers in October 1852; SFB to JR, 10/23/1852, Folder 2, Box 1, *NPEEC*.

the geographical distribution of species would be in disarray. "Everything will be done which the time of the offices will allow to dispose of the accumulating material for the best interests of science, and the reputation of the expedition," he assured Ringgold.

But would the Smithsonian charge any fee for its service? To this rather embarrassing question, Baird's answer was a definite "yes." In his opinion, the Navy Department should not only pay all expenses that the institution would incur in the course of transporting, arranging, preserving, and curating specimens, but also appoint a supervisor to oversee the whole operation. "As a large amount of labor will necessarily be required to carry out the above suggestions properly," he wrote, "it would be but fair that the expenses of supervision should include (in addition to transportation, cost of preservation materials, taxidermy &c) a moderate compensation, say 500.00 dollars per annum, to some one under the direction of the Institution." He concluded the memorandum by asserting that he would "cheerfully accept the trust."<sup>13</sup>

Ringgold was delighted to find Baird a knowledgeable and cooperative man of science. He took Baird's scheme to heart.<sup>14</sup> He eagerly recommended Baird's favored

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<sup>13</sup> SFB to CR, 11/12/1852, Folder 2, Box 1, *NPEEC*; also see SFB to CR, 12/14/1852, Folder 2, Box 1, *NPEEC*.

<sup>14</sup> Ringgold reported to Secretary Kennedy in February 1853 that "all expenses incurred by the Smithsonian Institution, under the direction of Prof. Baird, for the transportation, care and preservation of the specimens and collections sent home by the Expedition, be paid out of the specific fund, set apart for that purpose" (CR to JPK, 2/28/1853, F35, R2, *REENP*). In the following letter, he reported that he had authorized Baird to procure scientific publications "as in his opinion may be of interest," for the use of the expedition (CR to JPK, 3/1/1853, F37, R2, *REENP*).

naturalists to the Navy Department, including botanist Charles Wright, zoologist William Stimpson, and collector or “assistant naturalist” Alfred Ames.<sup>15</sup> More importantly, he reinterpreted Baird’s scheme for natural-history collecting along the lines of military order. He wanted Baird’s scheme to be correctly undertaken and robustly pursued. In an order addressed to officers commanding vessels of the squadron, for example, he required “Officers of the Expedition” to keep “careful and minute journals during the cruise to contain the daily position of the ship at sea, distance, bearings, and remarks and observations such as may present themselves in regard to occasions and objects of interest and science and natural history manners and customs of the inhabitants of islands and other places visited.” In another order, he required that “all fishes and invertebratae, all parasite animals and plants adhering to weeds or floating bodies must not fail to receive attention and specimens of them must always be obtained when practicable.” In fact, he noted in another order, “Fish caught must be preserved until opened in the presence of the Zoologist and their stomachs carefully examined so that if anything of interest is found in them it may be taken care of.”<sup>16</sup> He had resolved to make the surveying expedition under his command exact and exhaustive.

Baird was satisfied with what he had achieved in bringing the Smithsonian and the Navy Department together in the name of science. “Thanks to our liberal Secretary of the Navy, Mr. J. P. Kennedy,” he wrote to his father-in-law, Army

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<sup>15</sup> Ringgold’s recommendation can be found in CR to JPK, 12/15/1852, *FAC*. Kennedy’s reply can be found in JPK to CR, 12/17–18/1852, *FAC*.

<sup>16</sup> Both orders can be found in F12–14, R1, *REENP*.



Inspector General Sylvester Churchill, "I had full authority to prepare at the expense of the operation whatever apparatus was necessary to capture all sorts of Sea Devils and Water Kelpies." Thus far he had secured positions for "two enterprising and able naturalists [Charles Wright and William Stimpson]," he told Churchill. "[W]e together ransacked our brains, and made out tremendous lists of nets, kettles, dredges, etc. amounting to near \$2,000, all of which were authorized and paid for without flinching. They go much better prepared than the old expedition (Wilkes)."<sup>17</sup>

### **"A pleasant voyage"**

Wethersfield, Connecticut. December 1852.

Charles Wright was delighted that the Navy Department had decided to appoint him to the position of botanist in Ringgold's expedition. From his new correspondent, Baird of the Smithsonian, he learned that the appointment would continue for three years and the annual salary would amount to \$1,000. Having submitted his application for the position in October, he had been impatiently waiting for the result.<sup>18</sup> He had just been dismissed from the U.S.–Mexican Boundary Survey and was now staying with his family. Although he was glad to be with his family and leave all dangers, confusions, and discomforts behind, he found life in Wethersfield dreary. In fact, within less than forty-eight hours after his arrival at home, he had

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<sup>17</sup> SFB to SC, 3/8/1853, Folder 3b, Box 2, *SFBP*; a transcript of this letter can be found in Folder 4.

<sup>18</sup> See CW to AG, 11/11/1852, *HL*.

grown bored. While listening to his sisters' chickens clucking and scratching on the door, he thought that if he could not secure a new job as quickly as possible, he would not only have to deal with such noises and nuisances indefinitely, but also labor on the family farm incessantly—a sort of occupation that had scarcely interested him since his departure for Yale College in 1831. He would have to “beg a job of Prof. Baird” to stave off “idleness,” he had told his brother.<sup>19</sup>

But to be away from home for a three-year overseas expedition was still a difficult decision to make. He felt sorry that he once again had to oblige his brother John to take care of their widowed mother and two unmarried sisters, Mary Ann and Abigail. What would be their response when they heard that their little Charlie would journey to the other side of world from New England, eventually setting foot upon the soil of Macao, Hong Kong, and Japan? He entertained himself with questions of this sort. He was almost sure that his sisters would mutter something like “you once told me that you would ‘hardly expect ever to cross the “big water” and shall stop somewhere this side of the Pacific,’ and now are you going to break your promise?”<sup>20</sup> John, Wright thought, would probably support his decision. After all, since he devoted himself to botanical collecting, he had regularly sent John money and had at times rescued John from financial crisis. Moreover, aware of John's eagerness to befriend scientific luminaries, he had encouraged John to send

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<sup>19</sup> Of all things I cannot endure a hen cockling under my very nose.” CW to JW, 12/17/1850, *CWC*; CW to JW, 12/1/1852, *CWC*.

<sup>20</sup> CW to MAW, 5/5/1845, *CWC*.

botanical specimens to Harvard botanist Asa Gray. "Botanists you know are mostly indebted to farmers & others for such floral curiosities," he told John.<sup>21</sup>

Yes, Wright understood, it was such the pleasure and profit of collecting and discovering botanical novelties that drove him to step into the field again and again. By the end of 1852, he had devoted himself to botanical collecting for about ten years. Thanks to his friend and correspondent Asa Gray, he now had dozens of species named after him, and his name had been Latinized and published as the title of one of the *Smithsonian Contributions to Knowledge*. His devotion to plant collecting also brought him associations with established naturalists like Gray, Torrey, and Baird. Just as his network of contacts was expanding, so too was the influx of money coming his way. Again thanks to Gray, Wright had some ten gentlemen and institutions who regularly subscribed to his botanical collections. His previous experiences in Texas and New Mexico had taught him that insofar as he could hunt down botanical novelties in the field and make exquisite specimens of them, a summer of labor could readily earn him about \$500. Natural history collecting, after all, was a profession, as Gray had once told him.

As his excitement at getting a new job ebbed, Wright began worrying about whether he could properly carry out the duties that the Navy Department and the Smithsonian had entrusted to him.<sup>22</sup> He only had a remote idea about the

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<sup>21</sup> CW to JW, 5/29/1854, *CWC*.

<sup>22</sup> As a member in the scientific corps in Ringgold's expedition, Wright was invited to visit the Smithsonian for the training session hosted by Henry and Baird. "Tomorrow I shall meet Mr. Brackenridge, Curator of the Botanical Garden and view his curiosities," he wrote home from Washington, DC. "Prof. B [Baird] showed me the fishes & snakes & lizards and all arranged and says there are many entirely

geographical scale that Ringgold's expedition would cover and an even remoter idea of what kind of flora he would encounter en route.<sup>23</sup> So he sat down and composed a letter to Gray. "It is desirable to provide many things for the whole three years such as shoes and other articles of clothing," he wrote.<sup>24</sup> "Can you suggest any books for me as a supplement to De Candolle? ... Would Endlicher be of much use to me?" Also, he demanded that Gray distribute his previous collections to subscribers and collect the money. "If you could give me a check on a New York bank perhaps I could get gold more conveniently than with one on Charles River Bank," he wrote.<sup>25</sup>

Gray kept in close touch with Wright as the collector endeavored to secure a position in Ringgold's expedition. He shared most of Wright's hopes and worries. As early as October 1852, when Wright first revealed his intention to join the NPEE as a collector, Gray replied that the expedition was "just the thing!" and expressed his delight that Wright had applied for the position "so promptly."<sup>26</sup> In November, shortly after Wright's letter arrived confirming the appointment, he replied that he

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new." As for the cityscape, he commented, "I am rather pleased with Washington. It is a kind of combination of city & country. The Smithsonian building is a fine one and surrounded by a lance lot planted with many kinds of trees though quite young most of them" (CW to JW, 12/1/1852, *CWC*).

<sup>23</sup> When directing John to send letters to "Singapore," Wright confessed, "I have so forgot my geography that I do not know where it is" (CW to JW, 5/26/1853, *CWC*).

<sup>24</sup> It turned out that Wright in total carried 112 pieces of clothes with him. See CW to JW, 3/20/1854, *CWC*.

<sup>25</sup> CW to AG, 11/13/1852, *HL*.

<sup>26</sup> AG to CW, 10/28/1852, *AGPG*.

would soon send Wright money and necessary instructions.<sup>27</sup> Also, when learning that the Smithsonian would purchase books, instruments, and other facilities on behalf of Wright, he promptly drew up a list for Wright's reference. The paper used for drying plants should be of high quality, he noted, and Wright could purchase plenty of such paper in New York for a reasonable price. He also suggested that Wright should ask for additional sets of collecting devices, so that Wright could recruit other people to join his undertaking. As for reference books, he suggested that Wright should at least ask for Augustin Pyramus de Candolle's *Prodromus Systematis Naturalis Regni Vegetabilis*, Stephan Ladislaus Endlicher's *Genera Plantarum Secundum Ordines Naturales Disposita*, William J. Hooker's *The Botany of Captain Beechey's Voyage*, and William Henry Harvey's *A Manual of the British Marine Algae*.<sup>28</sup> Afterward, with the financial support of the Navy Department and with the Smithsonian dealing with administrative matters, he helped Wright establish a botanical closet onboard the steamer *John Hancock*.<sup>29</sup> A list Wright sent to Ringgold in April 1853 indicates how well equipped he was for the expedition: "20 reams paper for drying plants, 4 presses for drying plants with straps &c., 1 simple microscope, 2 pocket lenses, 6 memorandum books for recording observation, 1 ream post writing paper, 6 pounds strong twine for tying packages, 25 pounds Manila paper for wrappers, and 4 portfolios for collecting plants."<sup>30</sup>

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<sup>27</sup> AG to CW, 11/17/1852, *AGPG*.

<sup>28</sup> AG to CW, 11/19/1852, *AGPG*.

<sup>29</sup> AG to SFB, 12/22/1852, Folder 8, Box 3, *CM*.

<sup>30</sup> CW to CR, 4/5/1853, Folder 21, Box 1, *NPEEC*.

Gray supported Wright's appointment wholeheartedly. Afterward, when Joseph Henry told him about Wright's visit to the Smithsonian (Wright "appeared himself a well preserved specimen of a variety of the genus Homo") and expressed his concern that Wright seemed "not a model explorer or rather I should say collector," he replied, "I rejoice at Mr. Wright's appointment on Mr Capt. Ringgold's Expedition. He is, as you say, a collector rather than a savant, but he is the very best collector I know, and will do well." Also, Gray was glad that the Smithsonian was now extending its influence to the Navy Department. He congratulated Henry on securing the Smithsonian's position as "scientific advisor to the Government." He promised that when he came to examine the botanical collections that Ringgold's expedition might harvest, he would give grateful acknowledgment to Secretary Kennedy's "liberal views and zeal."<sup>31</sup>

Late in December, upon Baird's request, Gray composed instructions "about drying plants in the best & readiest way for a traveller" and addressed it to Baird to be distributed among the botanical collectors in Ringgold's expedition and some other Navy expeditions. "Instruct the collector to record the proper notes on the plants he collects," he wrote, "not those points which the specimens will always tell themselves—but those they will not—such as nature of locality, size of tree or plant, color of flower, fruit, &c.—native use &c. if any.—&c." Another crucial point was that collectors should collect "at least 4 specimens of every thing," he noted.

I will look over, & report on those collections, if desired—& give characters of new things... But when there are specimens to spare, I should have one to place in my own herbarium. In case of such collections if 4 or 5 specimens of each

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<sup>31</sup> JH to AG, 12/3/1852, *PJH*, Vol. 8, 413; AG to JH, 12/6/1852, *PJH*, Vol. 8, 415.

plant are gathered one duplicate set should I think be given to the Paris Museum,—& one to the Hookerian Herbarium.

This manner of distributing specimens would best serve the interests of science, he emphasized.<sup>32</sup>

Gray soon informed Sir William Hooker of Wright's appointment. Sir William replied that he was certain that Wright would be "a good acquisition to every Expedition, but especially to Japanese & the Sandwich Islands." Aware of the strong national characteristics of Ringgold's expedition, he expressed his hope that the Navy Department would allow Wright to "dispose of his duplicates" and that, if this allowance were granted, the Royal Botanical Gardens would receive "a lion's share."<sup>33</sup> Afterward, anticipating that Gray's and Wright's participation in Ringgold's expedition would significantly advance current knowledge about East Asian botany (and the Kew Gardens' collections as well), Sir William aspired to make Ringgold's expedition known to the world of botany. On the basis of an essay published in the *New York Journal of Commerce*, he edited and published a notice in his *Journal of Botany and Kew Garden Miscellany*. Besides outlining the scientific and commercial significance of Ringgold's expedition, the notice remarked on the vast geographical scale of the expedition, the high quality of the officers in command, the competency of the scientific corps, the well-outfitted squadron, the carefully manufactured surveying instruments, and so on. It also mentioned that the U.S. Navy Department had wisely installed a library containing over 1,000 volumes on board the flagship

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<sup>32</sup> Baird's request can be found in SFB to AG, 11/16/1852, *HL*; Gray's instructions can be found in AG to SFB, 12/22/1852, Folder 8, Box 3, *CM*.

<sup>33</sup> WJH to AG, 1/27/1853, *HL*.

*Vincennes*. In review, the notice announced, “every care has been taken to render this long voyage interesting, not only to those in command, and to the scientific gentlemen, but also to poor Jack.” Ringgold’s expedition had been furnished with “every means of success,” it asserted.

### **Striking analogies**

Gray was optimistic that the NPEE would turn out to be a successful scientific operation. He knew how desperately botanists had been yearning for plants from Japan. In the early 1850s, the most comprehensive collection of Japan’s plants was made by German naturalist Philipp Franz von Siebold (1796–1866) during his eight-year service (1823-1829) in Dejima, Nagasaki, as a physician in the Dutch East India Company; and the most authoritative account of Japan’s flora remained Siebold and Joseph Gerhard Zuccarini (1797–1848)’s *Flora Japonica* (first volume was published in 1835). “There are few better countries [than Japan] in the world,” Sir William had told Gray.<sup>34</sup>

Gray was by no means a stranger to the flora of East Asia. As early as the 1830s, when his friend Samuel W. Williams left for Canton, he had asked him to send back specimens and had afterward enjoyed studying “a nice little parcel of weeds from

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<sup>34</sup> WJH to AG, 11/30/1853, *HL*. Regarding Siebold’s study of Japan’s flora and its influence, see, among others, Kimura Yōjirō 木村陽二郎, *Shi-boruto to nihon no shokubutsu: Tōzai bunka kōryū no gensen* シーボルトと日本の植物：東西文化交流の源泉 (Tokyo: Kōwa Shuppan, 1981); Maki Fukuoka, *The Premise of Fidelity: Science, Visuality, and Representing the Real in Nineteenth-Century Japan* (Stanford: Stanford University Press, 2012).



China.”<sup>35</sup> But it was not until his trip to Europe that he had the chance to systematically study the East Asian flora. In February 1839, when studying at the British Museum, he noticed a specimen identified by Thunberg as *Taxus nucifera*. “It is very like *Torreya* [a peculiar genus in North American flora]! and doubtless a congener,” he noted in his journal.<sup>36</sup> In June, he paid a visit to German botanist Zuccarini’s house, examined Siebold’s plant collection now in Zuccarini’s hands, and helped Zuccarini name the dubious American *Cyperaceae*. Delighted, Zuccarini presented Gray a set of publications, including the first volume of *Flora Japonica*. Gray was thrilled. Returning to the hotel, he recorded in his journal the meeting that had brought him the “most bountiful offers for exchange.” “He [Zuccarini] gave me some of his publications and even offered me his ‘Japan Flora’ (Siebold’s), which is an expensive work, but it is very desirable for us to have, though it will be rather difficult for me to give him an equivalence.”<sup>37</sup> Upon his return, he promptly published a review of Siebold and Zuccarini’s *Flora Japonica*. “The flora of Japan,” he

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<sup>35</sup> LAG, 54. Williams must have felt obliged to meet Gray’s request. In September 1833, when still sailing on the Indian Ocean, he wrote to Gray apologizing that he might be unable to procure as many specimens as Gray might have expected. “The immediate vicinity of Canton did not allow any pursuit of this sort,” he explained. “Being entirely alone in such pursuits, and perhaps laughed at by Chinamen and questioned by countrymen, and moreover having little or no ground to survey, you will not expect much yet awhile.” As far as he knew, continued Williams, the spots remarkable for their botanical wealth were often guarded by Chinese cultivators. “I probably would get a spear,” he wrote half-jokingly. Certainly Williams must have figured ways to reckon with the unfriendly conditions. SWW to AG, 9/3/1833, HL; AG to MG, 9/28/1835.

<sup>36</sup> Asa Gray, “A pilgrimage to *Torreya*,” *The American Agriculturalist (New Series)* 34, no. 7 (1875), 266-267.

<sup>37</sup> LAG, 235.

noted, “presents such striking analogies to that of the temperate part of North America as to tender this work of more than ordinary interest to American botanists.” Then he went through 46 species recorded in Siebold and Zuccarini’s book, and juxtaposed them with “related North American forms.” “While about half the species thus far published are nearly related to (chiefly characteristic) North American plants,” he noted, “only eight, besides those given above, belong to genera which have no representatives in this country.”<sup>38</sup>

Gray understood that he was now facing a theme that had captivated European and American botanists for the past century: the similarities between the flora of East Asia and that of eastern North America. Why was ginseng, for example, found in East Asia and eastern North America, but nowhere in between? What was the *plan* underlying this remarkable pattern in nature?

Indeed, almost since the moment naturalists began closely examining the natural history of North America and that of East Asia, the similarities between the floras of these two landmasses had caught their intensive attention. Early in the 1710s, Pierre Jartoux, a French Jesuit missionary in Peking, communicated with the French Academy of Sciences in Paris about his encounter with the famous and highly valued Chinese medicinal plant named ginseng during an expedition to China’s border with Korea (dispatched by the Kangxi Emperor [1654–1722]). He expected that the ginseng plant could also be found growing in French Canada, because of how similar

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<sup>38</sup> Asa Gray, “Dr. Siebold, *Flora Japonica*; sectio prima. Plantas ornatui vel usui inservientes; digessit Dr. J. G. Zuccarini: fasc. 1-10, fol.,” *The American Journal of Science and Arts* 39, no. 1 (1840), 175-176.

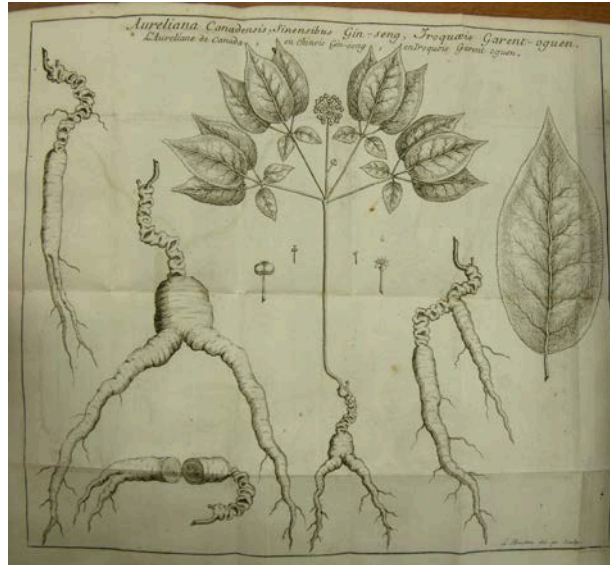


Figure 7-3. Lafitau's illustration showing the ginseng discovered in North America. (From Lafitau, *Mémoire...*, 1718. Courtesy of the Huntington Library, San Marino, CA, USA.)

its environment was to northern China. Jartoux's hypothesis was soon verified by Joseph François Lafitau, a French Jesuit missionary in Quebec. Early in 1716, Lafitau reported the discovery of the native Canadian ginseng between Montreal and Ottawa (Figure 7-3). For Lafitau, this extraordinary discovery, together with the similar pronunciations of the words for the plant in the Chinese and Iroquois languages, suggested that once upon a time America and Asia must have had been united with each other and formed a single continent, so that intensive cultural and biological exchange could take place. French Academicians were electrified by the two missionaries' discoveries. They fell into deep discussions about the taxonomic status of ginseng, and concluded that Korean, Chinese and Canadian ginseng belonged to one and the same species. It was a diagnosis that carried significant commercial implications, as well. Soon afterward French merchants began sailing their ships loaded with Canadian ginseng to China, to exchange for tea, silk, and

other Chinese exotics. The trading strategy worked beautifully over the decades to come. Although Chinese people were for the most part indifferent to foreign goods (except for silver and later on opium), to them ginseng was irresistible.<sup>39</sup>

Anglo-American naturalists did not allow knowledge and profit from ginseng to remain the exclusive domain of their French colleagues for too long. Thanks to such avid and knowledgeable field naturalists as William Byrd (1674-1744) and John Bartram (1699-1777) in North America, leading Anglo-American naturalists such as Peter Collinson (1694-1768) and Benjamin Franklin (1706-1790) became convinced that the distribution of the so-called Canadian ginseng was not confined to Canada but extended to the mountains of Virginia and Pennsylvania. On July 22, 1738, Franklin published a notice in *The Pennsylvania Gazette* (based on a discovery by Bartram): “We have the pleasure of acquainting the World, that the famous Chinese or Tartarian Plant, called Gin seng, is now discovered in this Province near Susquehanna.” About three months later, on December 7, 1738, Peter Collinson read a paper before the Royal Society with two complete specimens on display (collected and sent by Bartram). Besides reporting Bartram’s discovery, he referred to the plant that Kaempfer had described in *History of Japan* as “Nindsen,” claiming that all ginseng plants discovered in China, Japan, and North America should be regarded as

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<sup>39</sup> Lafitau reported this extraordinary discovery in his *Mémoire présenté a son altesse royale Mgr. le duc d’Orleans, régent de France, concernant la precieuse plante du ginseng de Tartarie, découverte en Amérique par le Père Joseph-François Lafitau* (A Paris: chez Joseph Mongé, ruë S. Jacques vis-à-vis le Collège de Louis le Grand, à Saint Ignace, 1718). Plenty of studies have dealt with the discovery of ginseng in North America. See, among others, John H. Appleby, “Ginseng and the Royal Society,” *Notes and Records of the Royal Society of London* 37, no. 2 (1983), 121-145; Linda L. Barnes, *Needles, Herbs, Gods, and Ghosts: China, Healing, and the West to 1848* (Cambridge: Harvard University Press, 2005), Chapter 4.

one and the same species (Linnaeus called it *Panax quinquefolius*). “It is observable that the same Species of Plants are found under the same degrees of Latitude in different parts of the World: as this Plant is a remarkable instance being found in those parts of China and Japan, that agree with the Latitudes of Virginia and Pennsylvania.”<sup>40</sup>

Ginseng was by no means the only plant to stimulate intellectual and commercial curiosity due to its discovery in both eastern North America and East Asia. Another plant that provoked focused discussions among Anglo-American naturalists was *Toxicodendron vernix* or *Rhus vernix*, commonly known as the poison sumac. In 1756 two letters appeared in the Royal Society of London’s *Philosophical Transactions* under the title “Two Letters Concerning Toxicodendron.” The first letter was addressed by Guillaume Mazéas to Stephen Hales regarding the special effect produced by the juice of a plant native to Carolina in North America (dated August 16, 1754, Paris; the letter was originally written in French and was translated by James Parsons [1705-1770] into English). In it Mazéas announced that François Boissier de Sauvages de Lacroix of the Royal Society of Montpellier had made a “discovery of a plant, the juice of which adheres, without the least acrimony, to a cloth, with more force than any other known preparation. The color is black, and the plant, which produces it, is the *Toxicodendron Carolinianum foliis pinnatis, floribus*

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<sup>40</sup> Franklin’s note is quoted from Barnes, *Needles...*, 174; Collinson’s statement is quoted from Appleby, “Ginseng and the Royal Society,” 132. As far as I know, the most recent and comprehensive study on the ginseng trade during the late eighteenth century and the early nineteenth century belongs to Michael D. Block, “New England Merchants, the China Trade, and the Origins of California” (PhD Dissertation, University of Southern California, 2011).



Figure 7-4. *Toxicodendron vernix*. (From Gatesby, *The Natural History of Carolina...*, Vol. 2, between p. 4 and p. 5. Courtesy of the Joseph F. Cullman 3rd Library of Natural History, Smithsonian Libraries, Washington, DC, USA.)

*minimis herbaceis.*” Mazéas thought that this particular American *Toxicodendron* had great potential as varnish. Over the past decades, he understood, European naturalists and manufacturers had spent enormous time looking for plants that could produce varnish with which to make European wares as delicate as Chinese and Japanese lacquerwares. Mazéas thought that the American *Toxicodendron* could be a likely candidate (Figure 7-4).<sup>41</sup>

The second letter was addressed by renowned British horticulturist Philip Miller (1691-1771) to Thomas Birch in response to Mazéas’s letter (dated March 18, 1755, Chelsea). On the basis of Kaempfer’s description and illustration of a tree called by

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<sup>41</sup> Guillaume Mazéas and Philip Miller, “Two Letters Concerning *Toxicodendron*,” *Philosophical Transactions, Giving Some Account of the Present Undertakings, Studies, and Labours, of the Ingenious, in Many Considerable Parts of the World* XLIX, Part I (1755), 157-160.



Figure 7-5. Kaempfer's "sitz dsju." (From Kaempfer, *Flora Japonica*, 276.)

the Japanese "sitz dsju" or "urus no ki," Miller's letter acknowledged Sauvages's discovery but claimed that the discovery was nothing new (Figure 7-5).<sup>42</sup>

As the use of this dye is at present but little known in Europe, this may appear as a new discovery; but whoever will give themselves the trouble to turn to the books, in which this plant is described, will find, that this *American Toxicodendron* is the same species of plant, from which the inhabitants of Japan procure the varnish, with which they stain all their utensils; and the Calicuts are also painted with the juice of this shrub.

As a result, Miller's letter went on, the "American Toxicodendron" definitely held the key to unlocking the secret of the delicacy of Japanese lacquerwares. The letter then furthered the point by quoting the statement made by German botanist Johann Jakob Dillenius (1687-1747) in *Hortus Elthamensis* (1732):

[Dillenius] makes no doubt of its being the same with that of Japan, which, he says, should not seem strange, that a varnish-tree should be found in America,

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<sup>42</sup> Kaempfer's description and illustration can be found in Engelbert Kaempfer, *Flora Japonica: 1712* (Wiesbaden: F. Steiner, 1983), 76-77, 276. It is worth noting that this reprinted Kaempfer's *Flora* is closely annotated by Wolfgang Muntzschick, to accompany a list of plant names arranged by the modern classificatory system.

near the same latitude with Japan; since the Genseng [*sic*], the Bignonia, commonly called Catalpa, with many other plants, are found to be natives of both these countries. And he questions, if the Tea-tree might not be discovered in America, if persons of skill were there to search for it. And he is surprised, that the inhabitants of the English colonies in America have not attempted to procure the varnish, whereby a considerable profit may arise to them, as the plant grows naturally in so great plenty there.

According to Carolinian naturalist Mark Catesby (1683-1749)'s *Natural History of Carolina, Florida and the Bahama Islands*, Miller's letter pointed out, it was very likely that many trees and shrubs in "our northern and southern colonies of America" produced varnish. "[I]t were to be wished, that the inhabitants of both would make some experiments to collect this varnish, which may not only produce much profit to themselves, but also become a national advantage."<sup>43</sup>

The founding members of the American Philosophical Society might have had heard Miller's (or other similar European naturalists') call. In 1771, when they composed the preface for the Society's official journal *Transactions*, they wrote at length about turning the floristic relationship between East Asia and eastern North America to the "national advantage." "If we may trust to the report of travellers," it began, "this country, in the same degree of latitude, very nearly resembles *China*, or the tract of land that forms the eastern side of *Asia*, in soil, climate, temperature of the air, winds, weather, and many natural productions." Then, after a paragraph elaborating the remarkable resemblance between the climate of Philadelphia and that of Beijing, it went on:

This resemblance is manifest not only in the weather and climate, but is also remarkable in the soil and natural produce. Tobacco, *Phytolacca*, (or Poke) the Persimmon tree, the Mulberry tree, with several others, are natives of *China*;

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<sup>43</sup> Mazéas and Miller, "Two Letters...", 162, 165-166.



they are also the natives of this part of *America*. Ginseng is gathered to the westward of *Pekin*, and, as far as we know, has not been found in any other part of the world, except within the same degrees of latitude in *America*. These observations give grounds to hope that, if proper enquiries were made, many more of the native plants of *China*, and very possibly, the Tea, to [*sic*] much in use amongst us, and now become to [*sic*] necessary a part of our diet, might be found in *America*.

The savants of the American Philosophical Society then drew up a blueprint for the future of North America based on this extraordinary biogeographical pattern. That is, “by introducing the produce of those countries, which lie on the east side of the old world, and particularly those of *China*, this country may be improved beyond what heretofore might have been expected.” As long as American society could figure out a way to introduce “the industry of the *Chinese*, their arts of living and improvements in husbandry, as well as their native plants,” the preface wrote wishfully, “*America* might in time become as populous as *China*, which is allowed to contain more inhabitants than any other country, of the same extent, in the world.”<sup>44</sup>

About thirteen years after the statement appeared in *Transactions*, in February 1784, a vessel named the *Empress of China* left New York for Canton with “four hundred peculs of Genseng.” Arriving in August, the two business agents (Samuel Shaw and Thomas Randall) onboard promptly made arrangements with a Hong merchant (known as “Shykinkoa”) and sold the ginseng cargo at the rate of “one

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<sup>44</sup> American Philosophical Society, “Preface,” *Transactions, American Philosophical Society, Held at Philadelphia, for Promoting Useful Knowledge* 1 (1771), iii-vii. A number of studies have paid close attention to China’s cultural and economic influences on early American history; see, among others, Alfred Owen Aldridge, *The Dragon and the Eagle: The Presence of China in the American Enlightenment* (Detroit: Wayne State University Press, 1993); Eric Jay Dolin, *When America First Met China: An Exotic History of Tea, Drugs, and Money in the Age of Sail* (New York: Liveright Publishing Corporation, 2012).

hundred & fifty five dollars the pecul.” The *Empress of China* returned to New York in May the following year. The voyage was a great success. It marked the beginning of the young nation’s commercial relations with China and the materialization of the dream that the American Philosophical Society had elaborated in 1771: to turn North America’s unusual natural relationship with East Asia into a national advantage.<sup>45</sup>

Throughout 1839, as Gray was traveling from one herbarium to another and coming across more and more East Asian plants that reminded him of the flora of eastern North America, such an aged but hardly outdated thesis must have had filled his mind. The issue of how the flora of eastern North America related to its East Asian counterpart appeared in his botanical writings with increasing frequency. For example, in an essay on his botanical excursion to North Carolina, he deliberated that the “number of genera which are either divided between North America, Japan and the mountain region of central Asia, or have nearly allied species in these countries or in the two former, is very considerable: in other cases a North American genus is replaced by a nearly allied one in Japan.”<sup>46</sup> In 1846, when Zuccarini and Siebold published an enumeration of selected genera and species in Japan’s flora, he again published a review stating that “It is interesting to remark how many of our characteristic genera are reproduced in Japan, not to speak of

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<sup>45</sup> Details about the United States’ first attempt to trade ginseng with China can be found in a letter from Thomas Randall to Alexander Hamilton; Harold C. Syrett, ed. *The Papers of Alexander Hamilton, Vol. 10* (New York: Columbia University Press, 1965), 38-55. Also noteworthy is Block’s close study on the subject: Block, “New England Merchants...”

<sup>46</sup> Gray, “Notes of a Botanical Excursion...,” 39-40.

striking analogous forms.”<sup>47</sup>

Gray grew convinced that a close inspection of the East Asia flora would shed fresh light on the flora of North America. This understanding struck him as he came to criticize previous botanists’ diagnoses of *Astilbe decandra*, a shrub peculiar to the eastern United States. Combing through the literature, he found a striking “discrepancy” (some figured it to have “linear-spatulate petals,” while others thought it “apetalous”), and sought to compose a more accurate description. He soon ventured beyond the confines of North America and came across *Hoteia japonica*, a Japanese species known for its resemblance to *Astilbe decandra*. Renowned naturalist Carl Thunberg had collected the plant in Japan in the eighteenth century, but had mistaken it for *Spiraea aruncus*, a European species. Gray’s colleague in Paris, Joseph Decaisne, corrected Thunberg’s diagnosis by coining a new name, *Hoteia japonica*, placing it under the genus *Hoteia*.

Gray found Decaisne’s correction problematic. It occurred to him that the resemblance between the American species and the Japanese species was so

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<sup>47</sup> Asa Gray, “Analogy between the Flora of Japan and That of the United States,” *The American Journal of Science and Arts (2nd Series)* 2, no. 4 (1846), 135. Probably not by coincidence, the series of Gray’s reviews and notices appeared at the moment when American horticultural circles became swept up in a tidal wave of fascination with Japan’s floral wealth across Europe. It was Siebold who provoked the wave. With his *Flora Japonica* progressing, Siebold cultivated seeds and bulbs that he had brought back from Japan and sent grown plants to chief horticultural markets and exhibitions. At the time what particularly intrigued horticulturalists were Japanese lilies. “These [Japanese] Lilies are,” the *American Journal of Agriculture and Science* commented, “we think, without exception, the richest floral gems that modern zeal and research after novelty has brought to notice. Combining the most striking and beautiful combination of colors, with an exquisite perfume, and at the same time being of the most easy cultivation, either in the open border, or in pots in the house, they must become universal favorites.” See Marshall Wilder, “The New Japan Lilies,” *American Journal of Agriculture and Science* 7, no. 3 (1848), 133-137.

conspicuous that they belonged to the same genus. He soon found a similar species named *Spiraea barbata* in the flora of Nepal, which in his view suggested an undeniable generic tie with its Japanese and American counterparts. In 1840, Gray included this research in *A Flora of North America*. Rather than keep the three species in separate genera, he established a new description for the genus *Astilbe*, and placed the three species, *Astilbe decandra* of the eastern United States, *Hoteia japonica* of Japan, and *Spiraea barbata* of Nepal under it.<sup>48</sup> “I have no doubt that these three species belong to a single and very natural genus,” he later explained.<sup>49</sup>

Gray soon established his principle. That is, whenever he came across similar species with their respective distribution in North America, Japan, and even central Asia, he followed what he had showed in *A Flora of North America*: namely, to group those similar American or Asian species under the same genus. But why did Gray prefer to *split* those similar American and Asian species? If, as he frequently stated, in many cases American and Asian species were utterly “undistinguishable” and frequently “confounded” at herbaria, why didn’t he lump them together as single species or as varieties of species?

Two factors may explain Gray’s preference to split similar American and Japanese species. First, Gray in the 1840s lacked sufficient knowledge on how species might be dispersed all over the earth; therefore, if he assumed those similar American and Asian species to be identical, the assumption would undermine his foremost definition of species as an assemblage of individuals descending from one

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<sup>48</sup> Torrey and Gray, *A Flora of North America, Vol. II, Part I*, 588-589.

<sup>49</sup> Gray, “Notes of a Botanical Excursion...,” 37-40.

common parent or stock. Second, Gray had made clear that congeneric resemblances did not suggest shared origin; therefore, he could readily suppose that those similar American and Asian species—no matter how similar they appeared to be—were descended from different origins or stocks. This particular approach reflected Gray's reluctance to accept that the same species might originate or be created in multiple places respectively, the so-called multiple creation theory of the origin of species. Various records indicate that Gray once consulted but was not convinced by the view advocated by Danish botanist Joakim Frederik Schouw. In *The Earth, Plants, and Man* (1852), for example, Schouw argued that because there had been no satisfactory explanation for the "means of diffusion" by which the same species could land in widely separated spots, say, the Alps and Pyrenees on the one hand, and the Scandinavian and Scotch mountains on the other, the hypothesis that "each species of plant first made its appearance in one place, from which it subsequently became diffused over larger or smaller areas, sometimes over very extensive tracts" was highly "improbable; in certain cases altogether inadmissible."<sup>50</sup>

But what was the rationale for Gray's lumping of American and Asian species under the same genus if he considered that congeneric species did not suggest a shared origin? What mattered here was Gray's definition of genera: "a genus is a group of species which present the same particular plan." He was convinced that there must have been a plan in shaping the analogies between the flora of East Asia

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<sup>50</sup> Joakim Frederik Schouw, *The Earth, Plants, and Man: Popular Pictures of Nature* (London: Henry G. Bohn, 1852), 17.

and that of North America.<sup>51</sup> Here it should be noted that Gray's view represented a common understanding among botanists of the first half of the nineteenth century. Take Jacob Bigelow, for example. A retired professor in *materia medica* at Harvard and an acclaimed author on the flora of New England, Bigelow knew that similar or even identical species might be found in East Asia and North America. In Bigelow's opinion, the pattern could be explained "by supposing that originally a zone, or isothermal belt, which existed in each climate, contained all the species capable of flourishing in that climate so long as the climate remained stationary." "In the lapse of ages," Bigelow argued, "a great portion of these plants had disappeared or died out... some having disappeared altogether, and others remaining only in localities, defined by longitudes, in different parts of the same zone." To Bigelow, this was why "a sufficient number of species might be extent in, and common to, both hemispheres, to represent a part of the original growth."<sup>52</sup>

### **"A natural scale"**

By the end of the 1830s, a visitor from Japan had settled in the yard of the Leiden Museum for about five years. Although Leiden at the time had grown into an international city, this visitor still attracted the residents' attention. The visitor's skin color was dark, he preferred to live close to water, and he enjoyed eating fishes and frogs. He was a male giant salamander, three feet in length. It was Siebold and

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<sup>51</sup> Gray, *The Botanical Text-Book (2nd Edition)*, 305–306.

<sup>52</sup> Anonymous, "Four Hundred and Fifty-Eighth Meeting. January 11, 1859—Monthly Meeting," *Proceedings of the American Academy of Arts and Sciences* 4 (1860), 134.



Figure 7-6. *Salamandra maxima* (From Siebold, *Fauna japonica*, 1833-1850, Vol. 3, Saurii et Batrachii Tab. VII. (Courtesy of the Museum of Comparative Zoology, Harvard University, Cambridge, MA, USA.)

his zoological colleague Hermann Schlegel who helped this remarkable creature settle both in Leiden and in its place in the scheme of species. In the first volume of *Fauna Japonica* (1833), Siebold and Schlegel named the species *Salamandra maxima*, along with a series of illustrations that vividly captured the creature's morphology and anatomy (Figure 7-6). Siebold's *Salamandra maxima* would soon become a sort of celebrity. Visitors would elbow each other to see the salamander, feed him fishes or frogs, and see him "snapping at the fingers of any man that attempts to touch it."<sup>53</sup> It turned out that the creature's life-span was equally impressive. It died in 1881, about 52 years old.<sup>54</sup>

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<sup>53</sup> Anonymous, *Abstracts of the Proceedings of the Ashmolean Society, from 1843 to 1852 Inclusive, Vol. II* (Oxford: J. Wright, 1854), 96. The description appeared in the Society's meeting held on April 28, 1845.

<sup>54</sup> Hans Gadow, *Amphibia and Reptiles* (London: Macmillan and Co., 1901), 99-100. In it Gadow called it *Cryptobranchus japonicus*. Gadow's volume is published as the Volume VIII of *The Cambridge Natural History*, edited by S. F. Harmer and A. E. Shipley.

The arrival of the Japanese salamander in Leiden quickly piqued the curiosity of European naturalists.<sup>55</sup> Two facts about the Japanese salamander particularly drew attention. First, no similar creatures could be found anywhere in Europe—yes, there were many native species of salamanders in Europe, but none of them were comparable to this Japanese creature in size and in shape. Second, although nothing like Siebold’s *Salamandra maxima* could be found in Europe, fossil records showed that such creatures had once inhabited the streams and rivers of central Europe. These two facts immediately raised questions: Why had God decided to create one or multiple species of giant salamanders in Europe, and then allowed His creation to die out? Why had God put a similar creature in Japan so far away from Europe and let it survive and thrive?

Accompanying such vexing questions was perhaps one of the best known tales in natural history during the eighteenth and early nineteenth centuries. In 1726 the Swiss naturalist Johann Jakob Scheuchzer (1672-1733) named a set of fossil remains unearthed from Oensingen quarries of Switzerland *Homo diluvii testis*, meaning “man who witnessed the deluge.” He believed that the fossil remains—three feet in length, with a big skull, short ribs, and outstretched arms—were from a “sinner” who got killed by the Biblical flood. In 1811, however, George Cuvier (1769–1832), arguably the most competent naturalist of the first half of the nineteenth century, found Scheuchzer’s identification doubtful. Based on his extensive knowledge of comparative anatomy, Cuvier claimed that the remains had

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<sup>55</sup> John Edward Gray, “The Gigantic Japanese Salamander,” *Annals and Magazine of Natural History: Zoology, Botany, and Geology* 1, no. 5 (1838), 413.



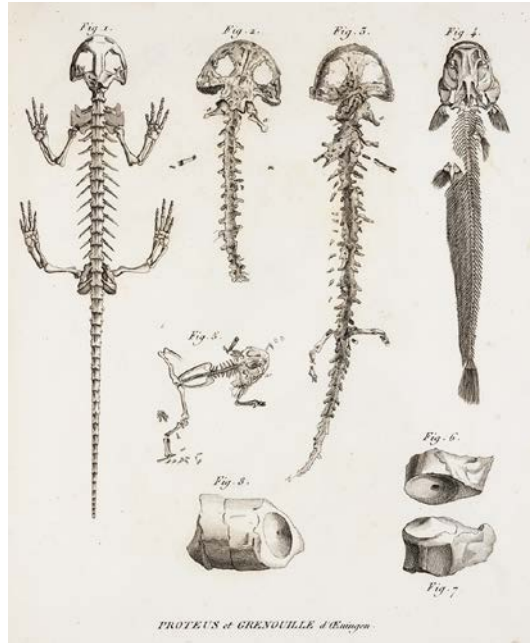


Figure 7-7. Scheuchzer's *Homo diluvii testis* (left) and Cuvier's correction (right) (From Johann Jacob Scheuchzer, *Homo diluvii testis*, 1726 and Georges Cuvier, *Recherches sur les ossemens fossiles*, 1812. Courtesy of the Linda Hall Library, Kansas City, MO, USA.)

nothing to do with the genus *Homo* but belonged to a gigantic salamander (Figure 7-7). In 1831, following Cuvier's identification, Friedrich Holl renamed the species *Salamandra scheuchzeri*.<sup>56</sup> It should be noted that Siebold and his zoological colleagues were working on *Fauna Japonica* at the time. The scientific name that they assigned to the Japanese giant salamander, *Salamandra maxima* (*Salamandra scheuchzeri*'s congener), thus implied an intellectual challenge: How could naturalists make sense of such trans-temporal and trans-continental connections between the two species?<sup>57</sup>

<sup>56</sup> Afterward, in 1837, Johann Jakob von Tschudi changed Holl's *Salamandra scheuchzeri* to *Andrias scheuchzeri*, which remains in use today.

<sup>57</sup> Regarding the discovery of *Homo diluvii testis* and its subsequent debates, John Read has offered an excellent and intriguing account in his *Missing Links: In Search*

For naturalists of the 1830s, what made the connections between *Salamandra scheuchzeri* and *Salamandra maxima* even more intriguing was the fact that Japan was not the only country that produced extant species of giant salamanders. In fact, even before Siebold's *Salamandra maxima* made its appearance in Leiden, European naturalists had known that giant salamanders (or "hellbenders") could be found in eastern North America. Benjamin Smith Barton (1766–1815), naturalist of Philadelphia and vice president of the American Philosophical Society, was among the earliest naturalists who made this American giant salamander known to the world of science. He called it *Salamandra horrida*, which, in the 1820s, was renamed *Menopoma alleghaniensis* by Richard Harlan (Figure 7-8).<sup>58</sup>

European naturalists did not fail to notice the striking resemblance between Siebold's *Salamandra maxima* and Harlan's *Menopoma alleghaniensis*. French naturalist Charles Lucien Bonaparte (1803-1857) suggested that naturalists place *Salamandra maxima* under a new genus he called *Sieboldia* and renamed Siebold's giant salamander *Sieboldia maxima*, closely allied to Harlan's *Menopoma alleghaniensis*. But Bonaparte's eagerness to honor Siebold had been preempted.

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*of Human Origins* (Oxford and New York: Oxford University Press, 2011), Chapter 3. Because Read's focus in his book is human paleontology, he does not discuss how the discovery of *Homo diluvii testis* challenged the contemporary thinking in zoology, in particular the fossil's resemblance to Siebold's *Salamandra maxima*.

<sup>58</sup> See, for example, Benjamin Smith Barton, *A Discourse on Some of the Principal Desiderata in Natural History and on the Best Means of Promoting the Study of this Science, in the United States; Read Before the Philadelphia Linnean Society, on the Tenth of June, 1807* (Philadelphia: Denham & Town, 1807), 23.

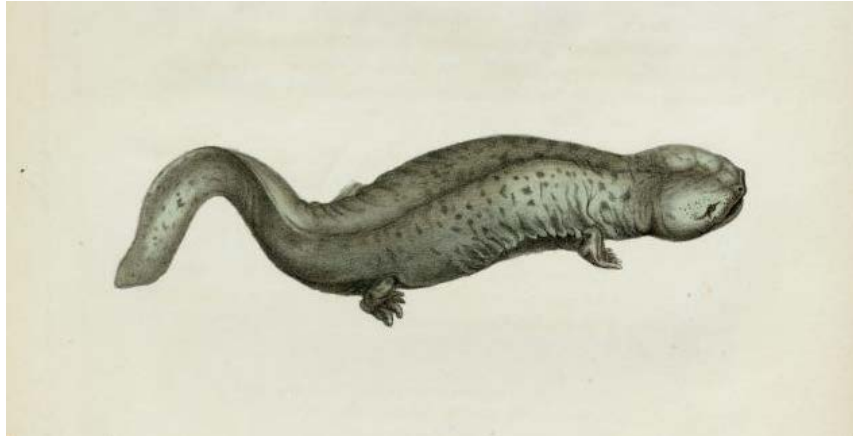


Figure 7-8. *Menopoma alleghaniensis*. (From Holbrook, *North American Herpetology*, 1842, plate 32. Courtesy of the Joseph F. Cullman 3rd Library of Natural History, Smithsonian Libraries, Washington, DC, USA.)

Johann Jakob von Tschudi had come up with the same proposal and renamed Siebold's giant salamander *Megalobatrachus sieboldii*.<sup>59</sup>

Louis Agassiz (Figure 7-9), by then having established his reputation as Cuvier's most capable disciple, an expert in fossil fishes, a rising geologist known for his groundbreaking glacier theory, and an ambitious zoologist hoping to revolutionize contemporary taxonomy, was well aware of the intellectual turbulence that the

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<sup>59</sup> According to British zoologist J. E. Gray, the chief difference between *Sieboldia* and *Menopoma* was that *Sieboldia* had "no lateral opening on the side of the neck," which was "so distinct in *Menopoma*." See J. E. Gray, "The Gigantic Japanese Salamander," 413. Regarding the history of naturalists' efforts to locate Siebold's *Salamandra maxima* properly in taxonomy, a concise analysis can be found in Henry C. Chapman, "Observations on the Japanese Salamander, *Cryptobranchus maximus* (Schlegel)," in *Proceedings of the Academy of Natural Sciences of Philadelphia, Part II*, edited by Edward J. Nolan (Philadelphia: The Academy of Natural Sciences, 1893), 227-233. According to Chapman's essay, Tschudi's diagnosis of Siebold's *Salamandra maxima* was published in *Iconografia della Fauna Italica* (1832-1842).



Figure 7-9. Louis Agassiz (1807-1873). Oil on canvas (1844) by Fritz Züberbuhler. (Harvard University Portrait Collection, Gift of G. R. Agassiz, Max Agassiz, and R. L. Agassiz, 1910, Cambridge, MA, USA.)

Japanese giant salamander had provoked.<sup>60</sup> Remarkably, the debate came to Agassiz's attention when he was absorbed in writing his multi-volume *Nomenclator Zoologicus*, wearily mustering all his remaining energy to overcome his financial and marital crises, and above all preparing himself for a research visit to the United States. Agassiz was granted this precious chance thanks to the great Alexander von Humboldt's recommendation and the King of Prussia's financial support. Equally

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<sup>60</sup> Both Cuvier and Bonaparte were Agassiz's correspondents. Piotr Daszkiewicz, and Keith Philippe, "The Correspondence between Louis Agassiz and the French Naturalists Georges Cuvier, Lucien Bonaparte and Alexandre Brongniart in the Manuscript Collections of the Muséum National d'Histoire Naturelle and Institut de France." *Archives of Natural History* 28, no. 3 (2001), 327-335.

importantly, through an introduction from his geological friend, Charles Lyell, Agassiz secured an appointment to give the Lowell Lectures in Boston.<sup>61</sup> Arriving in Boston in October 1846, and finishing the lectures, Agassiz made his way to the field. One day, observing a species of *Chelydra* (or snapping turtles) in the swamps under the shade of trees, he was struck by a sense of *déjà vu*. He found that the trees before him appeared similar to the fossilized fragments of trees discovered in the ancient soils of Oeningen (the quarries of Switzerland where Scheuchzer found his *Homo diluvii testis*). Thrilled, he promptly composed a letter to his friend, renowned British geologist Sir Roderick Impey Murchison, claiming that he had a “lucky and quite an unexpected hit”:

I think I made a lucky and quite an unexpected hit, by tracing the close analogy between the fossil Flora of the European miocene deposits, (*molasse*) and the living Flora of the temperate parts of the United States of North America. The correspondence extends to all the types of organized beings. After having seen the *Chelydra* alive in the swamps here, under the shade of trees analogous to those which cover the ancient soil of Oeningen, (so celebrated for its profusion of terrestrial and freshwater fossil remains,) I cannot help thinking that the climate could not have been tropical in Europe at the time when the strata of Oeningen were deposited. Again, I may observe that there is the closest affinity between the flora of the Atlantic shores of North America and that of Japan; where we have the *Megalobatrachus*, the corresponding living type of the *Andrias*, or great fossil salamander of Oeningen. As I am unable to write a paper now, I would thank you to make these remarks known before I can publish them *in extenso*.<sup>62</sup>

Agassiz decided to recruit his American colleagues to work with him to explore

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<sup>61</sup> On Agassiz’s life and career before he left for the United States, see, among others, Irmischer, *Louis Agassiz*, Chapter 3; Louis Menand, *The Metaphysical Club: A Story of Ideas in America* (New York: Farrar, Straus, and Giroux, 2002), Chapter 5.

<sup>62</sup> Agassiz’s “lucky and quite unexpected hit” was soon published in *Athenaeum* and then *The American Journal of Science and Arts*. Agassiz, “Analogy between the Fossil Flora of the European Miocene and the living Flora of America,” *The American Journal of Science and Arts* 4, no. 12 (1847), 424-425.

such unexpected analogies further (that is, 1. the analogy between the living flora of eastern North America and that of East Asia; 2. the analogy between the living floras of eastern North America and East Asia and the fossil flora of Europe; 3. the analogy between the living fauna of eastern North America and that of East Asia). In April 1847, through the introduction of his newly acquainted colleague, botanist Asa Gray, Agassiz sent a letter to Spencer F. Baird, by then Professor in Natural History at Dickson College, known for his enthusiasm in botany. Agassiz first expressed his eagerness to establish a regular correspondence with Baird. "Though I am not much a botanist," he wrote, "there is nevertheless one branch of that science in which I take the deepest interest and for which I would also ask your assistance."

The study of trees has become so important to paleontologists, that no one who has paid some attention to fossils can any longer make progress in this department without studying the fossil trees and comparing them with the actual flora. Now, I know you have paid much attention to this subject, and to me it has acquired a new degree of interest since I have ascertained that the arborescent flora of the European Miocene Tertiary deposits has the greatest affinity with the actual flora of the temperate regions of the United States, a result entirely unexpected, and quite contrary to most of the prevailing notions about the temperature of the continent of Europe during the Tertiary epoch. I am now very desirous to make the most extensive collection of all the trees and shrubs of the United States in order to trace as far as possible this analogy. But such a collection cannot be found in any herbarium, it must be made anew with the peculiar view, and if you feel the least inclined to help me in this inquiry, you would not only help me, but really help advancing one of the most interesting geological questions.

Agassiz then gave Baird lengthy instructions on how to prepare sets of specimens that might be made useful for paleontological studies.<sup>63</sup> Although Baird

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<sup>63</sup> "If you find it too troublesome, forgive me for having asked for it. As fossil plants are mostly found in parts, it would be necessary to have 1st, young branches with the buds as they are now before opening; cut such specimens as can be dried between paper; 2nd, branches with young bark, one or two inches in diameter; cut cylinders of about six inches length; 3rd, similar cylinders of the stems; old wood

eventually chose zoology instead of botany as his expertise, Agassiz soon secured a talented Swiss naturalist Leo Lesquereux (1806-1889), who came to the United States with him, to conduct the study. As he wished, Lesquereux would become one of the founding figures in American plant paleontology.<sup>64</sup>

In 1847, Agassiz was appointed as Professor in zoology and geology in the Lawrence Scientific School, Harvard College. One of the new professor's public services was to give twelve lectures on comparative embryology (from December 1848 to January 1849) at the Lowell Institute in Boston. Delighted by the chance to share his thoughts with the residents of his new hometown, Agassiz focused on a topic that perfectly matched his current status and expressed his expectations about the future of American science: the natural history of North America *in relation to* that of the rest of the world.<sup>65</sup>

Agassiz opened his first lecture by making the following statement:

The time has past when it was possible to doubt that there is order in Nature, when the existence of a general system regulating the whole creation could be

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with old bark; then mark the tree to collect at a later period, flowers, fruits, and leaves, and of the last, a great number of all the varieties of form from different branches and recent shoots. It is almost late to begin, but I could not write earlier, and in fact, it is rather imprudent for me to go to-day through the exertion of writing; but perhaps in the thickest of the woods, you will still find specimens of all your species in a leafless state, with the buds in the winter state." LA to SFB, 4/10/1847, Herber, *Correspondence...*, 24-25.

<sup>64</sup> Regarding Lesquereux's life and scientific career, see Rodgers III, *American Botany*.

<sup>65</sup> Louis Agassiz, *Twelve Lectures on Comparative Embryology, Delivered before the Lowell Institute, in Boston, December and January, 1848-9* (Boston: Henry Flanders & Co., Redding & Co., Gould, Kendall & Lincoln, James Munroe & Co; New York: Dewitt & Davenport, Tribune Building; Philadelphia: G. B. Zieber & Co., 1849).

questioned. However, it has been only step by step that man has acquired an insight into this plan.<sup>66</sup>

Then, in the middle of the lecture, he presented his audiences with two sets of illustrations: one showed various types of animals which naturalists commonly regarded as batrachians (including salamanders, hellbenders, Siebold's *Megalobatrachus*, toads, treetoads, frogs, naked snakes, and *proteus*), and the other demonstrated the metamorphoses of frogs and salamanders (Figure 7-10). He then pointed out how much zoologists had stumbled and struggled to arrange such a wide array of animals into a natural system of classification: Some insisted that the naked snakes (or *Caecilia*) should be placed at the head of batrachians, while others argued that naked snakes were actually a kind of snake, and hence frogs were more qualified to be placed at the head of batrachians. The fact was that, remarked Agassiz, "from want of a principle, all these details differ in the various authors." Worse, he argued, each and every natural system of classification—no matter how natural its author had declared it to be—was based on its author's "impression." Agassiz then asserted to his audience that he had pondered the issue again and again, and had come up with a principle that had "nothing arbitrary" and was "given to us by nature."<sup>67</sup>

Agassiz then directed the audience's attention to the set of illustrations that showed the metamorphoses of frogs and salamanders (Figure 7-10). He asked them to note the remarkable coincidences in these two sets of illustrations.

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<sup>66</sup> Agassiz, *Twelve Lectures...*, 5.

<sup>67</sup> Agassiz, *Twelve Lectures...*, 10.



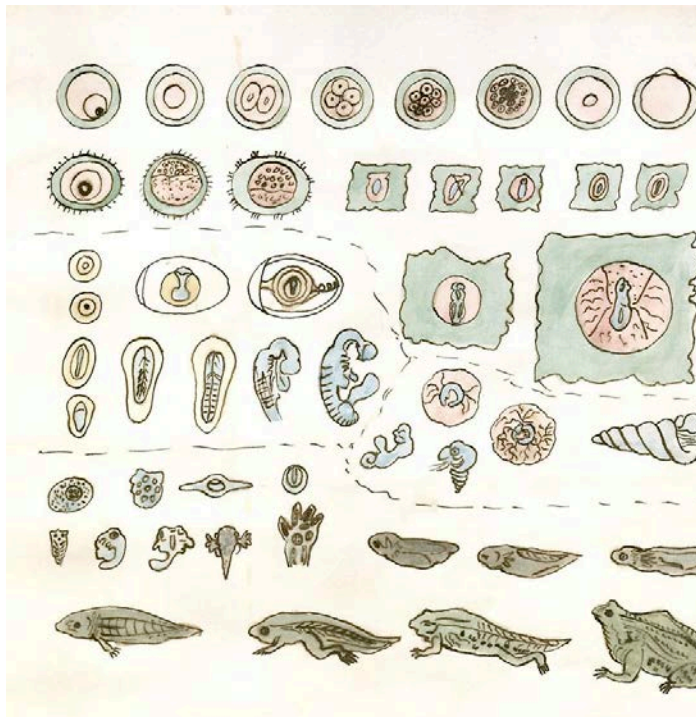
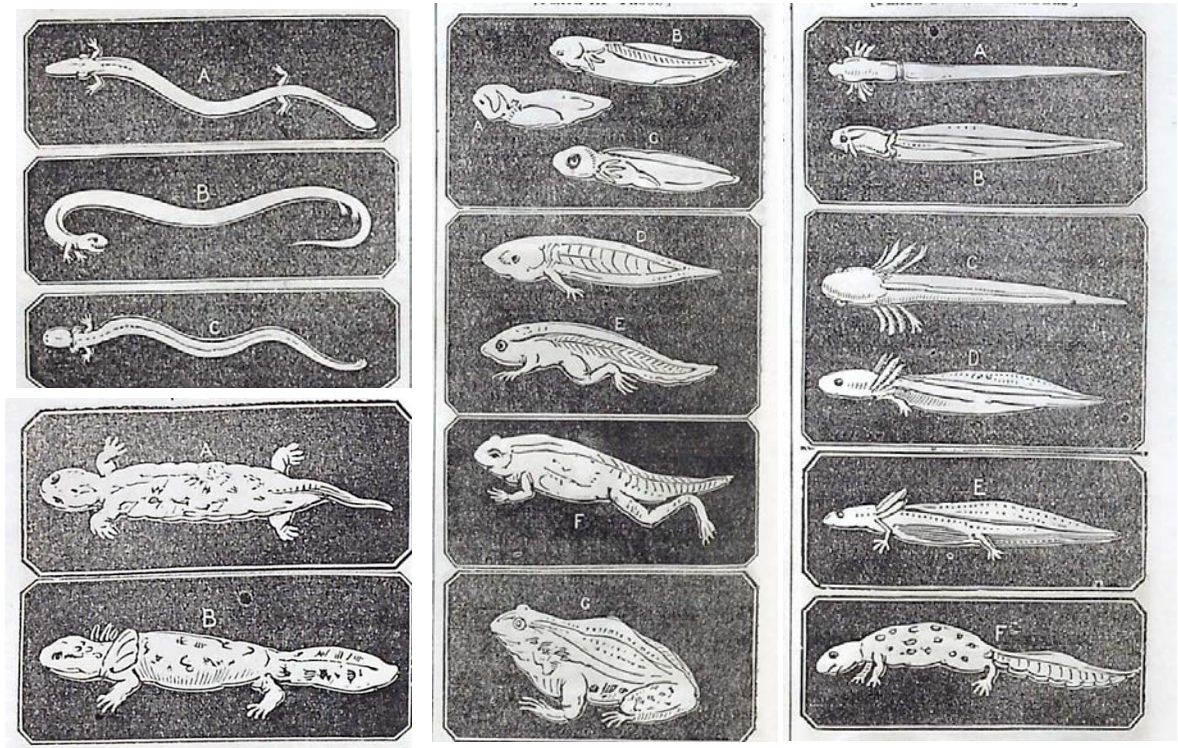


Figure 7-10. The illustrations Agassiz used in his *Twelve Lectures...*, and the drafts Agassiz (or his assistants) prepared for the illustrations. (Courtesy of the Dibner Library of the History of Science and Technology, Smithsonian Libraries, Washington, DC, USA.)

Here, in these facts we have not only the history of the transformation of salamanders and frogs, but we have a natural system of batrachians, and there is no longer any arbitrary arrangement in our system possible. Every thing is indicated in the metamorphoses of the animals.<sup>68</sup>

Then, after spending more time impressing his audience with the striking correspondence between different forms of batrachians and different stages of frogs' and salamanders' metamorphoses, Agassiz argued that frogs should be placed on the top of the natural system of classification, whereas giant salamanders (such as the United States' hellbenders and Japan's *Megalobatrachus*) should be put on the bottom. In sum, stressed Agassiz, "I will say, that in studying the metamorphoses of animals, we may find in the transformations—in the different formations through which they pass, from the first formation up to the full grown condition, a natural scale by which we can measure and estimate the position to ascribe to any animal belonging to this family."<sup>69</sup>

But for Agassiz, the importance of figuring out this "natural scale" was not confined to establishing a natural system of classification. How could one species of giant salamander be discovered in Japan, while a closely allied species was found in eastern North America? Agassiz thought that the only reasonable answer was that the geographical distribution of animals resulted from "the action of the intelligent Author of all these things":

We read here the intelligent action of the Creator in the production of these animals; and we read more than the intelligent invention of his creation. We read the omnipresence of his action, as his action is developed on all parts of the globe, in the United States, in Europe, in Japan, in South America, and in all the

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<sup>68</sup> Agassiz, *Twelve Lectures...*, 11.

<sup>69</sup> Agassiz, *Twelve Lectures...*, 11.

portions of the globe.—And when developed in that way in its actual condition, we see that every one of them, when reproducing its species, passes through these different changes—the higher one, through more of the changes; the lower one, undergoing only the earlier modifications.<sup>70</sup>

Agassiz promptly added this novel discovery to the revised edition of *Principles of Zoölogy* (1852). “It is not uncommon to find,” he claimed, “that there is the closest resemblance between species that dwell far apart from each other, while species of the same genus, that live side by side, are widely different.” In effect, he emphasized, “a single example” would be sufficient enough to illustrate this strange fact in the geographical distribution of animals:

The Menopoma, Siren, Amphiuma, Axolotl, and Menobranchnus, are Batrachians which inhabit the rivers and lakes of the United States and Mexico. They are very similar in external form, yet differ in the fact that some of them have external gills at the sides of the head, in which others are deficient; that some have five legs, while other are only provided with two; and also in having either two or four legs. Hence we might be tempted to refer them to different types, did we not know intermediate animals, completing the series, namely, the Proteus and Megalobatrachus. Now the former exists only in the subterranean lakes of Austria, and the latter in Japan. The connection in this case is consequently established by means of species which inhabit continents widely distant from each other.

He went on to challenge the common view that the geographical distribution of animals resulted from “the effect of external influences.” “We must... see in it the realization of a plan wisely designed, the work of a Supreme Intelligence who created, at the beginning, each species of animal at the place, and for the place, which it inhabits,” he asserted.<sup>71</sup>

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<sup>70</sup> Agassiz, *Twelve Lectures...*, 11.

<sup>71</sup> Louis Agassiz and Augustus Addison Gould, *Principles of Zoölogy: Touching the Structure, Development, Distribution, and Natural Arrangement of the Races of Animals, Living and Extinct: With Numerous Illustrations: Part I, Comparative*

Agassiz's intense interest in using the geographical distribution of American species to show "the work of a Supreme Intelligence" was made manifest in a volume titled *Lake Superior: Its Physical Character, Vegetation, and Animals, Compared with Those of Other and Similar Regions* (1850). He shocked his readers by declaring that the flora of eastern North America was as "old-fashioned" as that of Japan.

... what is quite extraordinary and unexpected, is the fact that the European fossil plants of that locality resemble more closely the trees and shrubs which grow at present in the eastern parts of North America, than those of any other part of the world; thus allowing us to express correctly the differences already mentioned between the vegetation of the eastern and western coasts of the continents, by saying that the present eastern America flora, and I may add, the fauna also, and probably also that of Eastern Asia, have *a more ancient character* than those of Europe and of Western North America.<sup>72</sup>

In a sense, the pattern that Agassiz unveiled was nothing new. After all, it was the pattern that had intrigued and been elaborated by American naturalists for the past century or so and most recently by Asa Gray. (Agassiz mentioned none of these intellectual predecessors, however.) What distinguished Agassiz's account from most of his predecessors' was his outright rejection of any connection between physical environments and geographical distribution of species. "[W]e must... conclude from the very fact that there are so many special thoughtful adaptations for so long successive periods in their distribution, that those manifold relations

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*Physiology: For the Use of Schools and Colleges (Revised Edition)* (Boston: Gould and Lincoln, 1852), 209-210.

<sup>72</sup> Louis Agassiz, *Lake Superior: Its Physical Character, Vegetation, and Animals, Compared with Those of Other and Similar Regions* (Boston: Gould, Kendall and Lincoln, 1850), 150.

could only be introduced, maintained and regulated by the continuous intervention of the Supreme Intelligence.”<sup>73</sup>

Agassiz’s interest in the origin and distribution of species had been kindled. “Were these organized beings created in pairs, as is generally thought and believed?” he asked in an essay published in *The Christian Examiner and Religious Miscellany*. His answer to this question was a definite “no.” The cases of the disjunct distribution of species were so numerous, he argued, that “we are at a loss for substantial arguments for believing that either one or the other place has been the primitive location of such animals, or for denying their simultaneous creation in both.” In Agassiz’s opinion, the following conclusion seemed inevitable—no matter how it contradicted the all-living-beings-originated-from-a-single-pair story as shown in *Genesis*, “most animals and plants must have originated primitively over the whole extent of their natural distribution.”<sup>74</sup>

For the next decade, the “natural scale” that Agassiz unveiled in his Lowell Lectures of December 1848 became one of his favorite examples when illuminating the Creator’s plan in nature. More importantly, in a decade when transmutationist and materialist theories of nature were gaining enthusiasts, he at times employed his natural scale as a firewall to defend his well-ordained and contrived nature from the encroachment of such heretical thoughts. In his *Contributions to the Natural History of the United States of America* (1857), for example, he wrote, “Why is it,

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<sup>73</sup> Agassiz, *Lake Superior*, 151-152.

<sup>74</sup> Louis Agassiz, “Geographical Distribution of Animals,” *The Christian Examiner and Religious Miscellany* (4th Series) 13, no 2 (1850), 189-190.

then, that this very reptile *Proteus anguinus*, forms, with a number of other reptiles living in North America and in Japan, one of the most natural series known in the animal kingdom, every member of which exhibits a distinct grade in the scale?"

"After we have freed ourselves from the mistaken impression that there may be some genetic connection between physical forces and organized beings, there remains a vast field of investigation to ascertain the true relations between both, to their full extent, and within their natural limits."<sup>75</sup>

Unsurprisingly, since Agassiz had been convinced that the analogical relationship derived from his extensive observations of salamanders, frogs, turtles, lizards, and liquidambar could be extended to "all the types of organized beings," he was confident that he could readily apply his "natural scale" to measure various human races' places in nature, as well. Agassiz acquired this confidence only after he arrived in the United States, and he considered his newly acquired confidence to be the greatest gift that American men of science had given him. In December 1846, after a visit to the anthropologist Samuel George Morton's residence in Philadelphia, where he examined Morton's enormous collections of human skulls, Agassiz felt that he had experienced a Eureka moment. He began pondering polygenism: the idea that each human race had its own origin. (Some extreme polygenists even went so far as to claim that there were no such things as human races—each commonly regarded human race was an independently created species.) Until then Agassiz had hardly given the issue a second thought. Indeed, although he had adamantly argued

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<sup>75</sup> Louis Agassiz, *Contributions to the Natural History of the United States of America, Vol. 1* (Boston: Little, Brown and Co.; London: Trübner & Co., 1857), 15-16.

that every single species of animals and plants must have appeared several times in different geological ages and geographical locations, as far as the origin and distribution of human races were concerned, he was a monogenist.

Another American experience that drove Agassiz to polygenism took place in a hotel in Philadelphia, when, for the first time in Agassiz's life, he encountered black people. "I scarcely dare tell you the painful impression I received," he wrote to his mother, "so contrary was the sentiment they inspired in me to our ideas of the fraternity of humankind and the unique origin of our species."

As much as I try to feel pity at the sight of this degraded and degenerated race, as much as their fate fills me with compassion in thinking of them as really men, it is impossible for me to repress the feeling that they are not of the same blood as us. Seeing their black faces with their fat lips and their grimacing teeth, the wool on their heads, their bent knees, their elongated hands, their large curved fingernails, and above all the livid color of their palms, I could not turn my eyes from their face in order to tell them to keep their distance, and when they advanced that hideous hand toward my plate to serve me, I wished I could leave in order to eat a piece of bread apart rather than dine with such service. What unhappiness for the white race to have tied its existence so closely to that of the negroes in certain countries! God protect us from such contact!<sup>76</sup>

In his public lectures, Agassiz began discussing the origin and distribution of human races along with those of frogs, salamanders, lizards, birds, turtles, and so on, particularly in a talk given at the Charleston Literary Club in 1847 (in which he argued that the embryological development of "negroes" was so different from that of Caucasians that he felt compelled to conclude that the two "races" were distinct species) and another one given at the meeting of the American Association for the Advancement of Science in Charleston in 1850 (in which he claimed that viewed

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<sup>76</sup> This particular letter of Agassiz's has been cited widely. My quotation is from Menand, *The Metaphysical Club*, 105.

zoologically, the several races of men were well marked and distinct... These races did not originate from a common centre, nor from a single pair"). Agassiz spread his view through publications as well. In July 1850, he published an essay titled "The Diversity of Origin of the Human Races" in *The Christian Examiner and Religious Miscellany*. He had found the creation story of Adam and Eve utterly unacceptable, he argued. "We maintain, that, like all other organized beings, mankind cannot have originated in single individuals, but must have been created in that numeric harmony which is characteristic of each species; men must have originated in nations, as the bees have originated in swarms, and as the different social plants have at first covered the extensive tracts over which they naturally spread."

Those who hailed polygenism might find Agassiz's following statement revealing:

The character of the Indian race has been so well sketched out by Dr. Morton, in his able works upon that subject, that we need not repeat what he has said. We would only ask, Does not that Indian race present the most striking contrast with the character of the negro race, or with the character of the Mongolian, especially the Chinese and Japanese? The indomitable, courageous, proud Indian,—in how very different a light he stands by the side of the submissive, obsequious imitative negro, or by the side of the tricky, cunning, and cowardly Mongolian!<sup>77</sup>

### **"We should not receive it"**

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<sup>77</sup> Louis Agassiz, "The Diversity of Origin of the Human Races," *The Christian Examiner and Religious Miscellany (4th Series)* 14, no 1 (1850), 128, 143-144. Regarding Agassiz's view on the origin and distribution of human races and its relationship to racism and slavery in the United States, see, among others, Walls, *The Passage to Cosmos*, Chapter 4; Irmscher, *Louis Agassiz*, Chapter 6; Menand, *The Metaphysical Club*, Chapters 5 and 6; Desmond and Moore, *Darwin's Sacred Cause*, Chapter 9.



In October 1846, the thirty-five-year-old Harvard professor Asa Gray attended a party to celebrate the arrival of the renowned Swiss zoologist Louis Agassiz in Cambridge, Massachusetts. While mingling with the crowd around the round-faced Swiss newcomer and paying his respects, he heard a question regarding Agassiz's definition of the term "species." Gray held his breath. As a botanist who had spent enormous time naming, describing, differentiating, and cataloging species, Gray naturally found this question intriguing. Then he heard Agassiz answer that a species was "a thought of the Creator." He was impressed. Thus far he had never applied the term "Creator" (or any other allied term such as God) to define what botanists meant by species. Although he had no doubt that all things "not only *had* a Creator, but *have* a Governor," whenever he needed to define the term species, he preferred a more observable, empirically based one: "A species... proved to be the produce of a common parent."<sup>78</sup>

As time went by, Gray grew increasingly suspicious of Agassiz's views on species and on the geographical distribution thereof. In a letter to Torrey in January 1847 regarding Agassiz's recent talk in Boston, he wrote that Agassiz opined that "animals and plants were originally created in numbers, occupying a considerable area, perhaps almost as large as they now occupy." Furthermore, he added, Agassiz made clear his opposition to "Lyell and others who maintain that very many of the Tertiary species are the same as those now existing," and believed that "there was

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<sup>78</sup> Gray recalled this encounter with Agassiz in a review of Elizabeth Cary Agassiz's *Louis Agassiz: His Life and Correspondence* (1885); Gray, "Louis Agassiz," *The Andover Review* 5, no. 25 (1886), 38-44. Regarding Gray's account of the "Governor" in nature, see Gray, "Explanations..." 470; the quotation of Gray's definition of species is from Gray, *The Botanical Text-Book*, 179-180.

an entirely new creation at the commencement of the historical area, which is all we want to harmonize geology with Genesis.” He found it irritating when Agassiz speculated that the “Negro and Malay races descended from the sons of Noah, but had a distinct origin,” he told Torrey. “We should not receive it, rejecting it on other than scientific grounds, of which he does not feel the force as we do.” “I will say that A. and I always get on perfectly well,” he told Engelmann, “tho’ perhaps we might not if we worked in the same field.”<sup>79</sup>

In 1850, with Agassiz’s inclination to polygenism manifested through his publications and public lectures, Gray’s close botanical correspondents at times made their disagreements felt to Gray. “Agassiz has been making himself Enemies, in certain quarters, by his published views on the diversity of origin of the human race,” Carey told Gray.<sup>80</sup> Curtis, too, told Gray that he was sorry to “hear that Agassiz is on the plurality side.” “He [Agassiz] may be right in his notions of this subject, so far as the lower animals & vegetables are concerned, but analogy does not demand, though it may support, the conclusion touching the human race,” he noted in another letter.<sup>81</sup>

Gray, despite his disagreement with Agassiz’s polygenism, consciously kept the issue of the origin of species at bay. A respectable and responsible naturalist, he believed, should refrain from speculating on such subjects. As he ranted in his review of *Vestiges*, “Are we supposed that the force which keeps a watch going, and

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<sup>79</sup> AG to JT, 1/24/1847, *LAG*, 345-346; AG to GE, 8/28/1849, *AGPG*.

<sup>80</sup> JC to AG, 8/26/1850, *HL*.

<sup>81</sup> MAC to AG, 3/15/1850, *HL*; MAC to AG, 3/29/1850, *HL*.

the arrangements by which it measures time correctly, are in any respect identical with the efficient cause that made the watch?" "The laws which we study and admire," he declared, "throw no light upon the origin of the bodies" and the "phenomena of life, however profoundly studied, have afforded no clew [*sic*] to the origin of life, or to the origin of the species through which the life is manifested." After all, he claimed, "it is not our [naturalists'] business to maintain, that the Deity does not work through what are called secondary causes, even in the act of creation."<sup>82</sup>

Gray was thus struck by the introductory essay to the *Flora Novae-Zelandiae* (1853), where British botanist Joseph D. Hooker (Figure 7-11) advanced four propositions concerning the origin and distribution of species.<sup>83</sup> Gray knew he had to take Hooker's species theories seriously. After receiving Hooker's first letter in 1844, he understood that the British botanist had traveled farther and had made broader observations than probably any other living botanist. "If you do not accomplish something worth the while, you ought not to bear the name of Hooker," he had told the young Hooker.<sup>84</sup> He therefore reprinted much of Hooker's essay in *The American Journal of Science and Arts*, made clear how his views differed from Hooker's, and then suggested how the two views might be taken together to

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<sup>82</sup> Gray, "Explanations...", 471-472.

<sup>83</sup> Joseph Dalton Hooker, *Introductory Essay to the "Flora Novae-Zelandiae"* (London: Lovell Reeve, 1853).

<sup>84</sup> See AG to JDH, 3/1/1844, *LAG*, 324-325.



Figure 7-11. Joseph Dalton Hooker (The Wellcome Library, London, UK.)

formulate a more comprehensive understanding of species and distribution than was available in the research of Agassiz. In the opening of this essay, he summarized Hooker's propositions as follows:

1. That all individuals of a species (as I shall attempt to confine the term) have proceeded from one parent (or pair), and that they retain their distinctive (specific) characters.
2. That species vary more than is generally admitted to be the case.
3. That they are also much more widely distributed than is usually supposed.
4. That their distribution has been effected by natural causes; but that these are not necessarily the same as those to which they are now exposed.<sup>85</sup>

With respect to the first proposition, Gray agreed that the “perennial succession of like individuals... sustaining to each other the relation of parent and progeny” should remain essential topics for naturalists to consider when trying to clarify the meaning of species. He also pointed out that “the evidence on that side of the question strongly favors the inference, that plants, at least, have been distributed

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<sup>85</sup> Gray, “Introductory Essay...,” 243.

each species from a single and specific primordial area.” Despite all agreements, he found Hooker’s stress on “one parent (or pair)” was too extreme to be considered true. In a manner similar to his review of *Vestiges*, he related, “How things go on, and how they began, are two different things; and it is seldom, if ever, that the facts and deductions which account for the former, can be made to throw much direct light on the latter.” “We know not what scientific evidence makes it needful to maintain the doctrine of the single creation of species in the restricted form of a single initial individual or pair,” he went on. “The whole section [in which Hooker brought forth the first proposition] is, in fact, mainly devoted to the illustration of the second cause, namely, that species retain their distinctive characters from age to age.”<sup>86</sup>

In Gray’s opinion, to define species as individuals proceeding from a single pair therefore brought about unnecessary confusion. “To say nothing of the imminent risk of the premature destruction of a single individual,” he argued, “in many cases both of animals and vegetables, a considerable number would be required at the outset to fulfil [*sic*] the relations established between the individuals.” Hence, he insisted, “it would be more satisfactory... if the proposition were stated in the more general form; viz., that all the individuals of a species have proceeded from a common stock, assigned to a limited primordial area;—in which form its bearing upon the other propositions would not be altered.”<sup>87</sup>

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<sup>86</sup> Gray, “Introductory Essay...,” 244.

<sup>87</sup> Gray, “Introductory Essay...,” 243-244. Gray apparently managed to direct readers’ attention to the definition that he had been eagerly advocating since 1845. In the third edition of *The Botanical Text-Book*, for example, he had written that “a species... embraces all those individuals, which... resemble each other more nearly than they resemble any other plants, so that we infer them to have sprung from a

Gray also found that Hooker's second and third propositions should give rise to a logical dilemma:

If species generally are much "more widely diffused than is usually supposed," then the theory of the double or multiple origin, as maintained by Schouw and others becomes all the more likely, at least until adequate natural means of dispersion are clearly shown. And if, at the same time, "species very [*sic*] in a state of nature more than is usually supposed," while what we term their specific characters are permanent, though the amount and value of these characters differs greatly in different species, then it may with the more probability be supposed that many of these differences were aboriginal.

"These two inferences," Gray continued, if taken together, "would perhaps necessitate the conclusion, maintained we believe substantially by Prof. Agassiz, that species are ideal, as much so as genera, and even that they may have been represented from the beginning by as many individuals, and distributed over as wide an area, as at any subsequent time." From his perspective, he argued, this was a view that seemed to "leave these [naturalists' concepts of species, geographical distribution, and alike] no ground of objective reality to rest upon." Hence, he continued, "We are reluctant to rest the very basis of natural history upon a priori conceptions, and therefore cling to the material view, that a species is not only a primordial form... but one represented in nature by the perennial succession of essentially like individuals." Naturalists might "mentally trace them backward to a common and probably local stock of homogeneous individuals, if not to a single

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common original stock, and which preserve their characters unchanged when propagated by seed." Gray, *The Botanical Text-Book: An Introduction to Scientific Botany, Both Structural and Systematic (3rd Rewritten and Enlarged Edition)* (New York: George P. Putnam, 1850), 350; also see, Gray, *The Botanical Text-Book: An Introduction to Scientific Botany, Both Structural and Systematic (4th Edition)* (New York: George P. Putnam, 1853), 358.

individual or pair," he concluded.<sup>88</sup>

Finally, regarding Hooker's proposition on the distribution of species and natural causes, Gray found Hooker's analysis satisfactory and stimulating. He declared that it would "destroy one of the strongest grounds upon which the doctrine of the multiple origin of species (at least in the form maintained by Schouw) is supported." For that matter, he encouraged readers to pay attention to a table in which Hooker listed 228 phaenogamous species, "which may be said to represent each other in two or the three South temperate masses of land: namely, New Zealand, Australia, and extra-tropical South America." This table convincingly showed the "cases of real and usually very close botanical affinity, to the exclusion of analogical resemblances," he noted.<sup>89</sup>

Gray greatly admired Hooker's boldness in applying a broad concept of species to tabulate distinct floras. He understood that Hooker could do so in confidence because of his extensive work at herbaria and botanical excursions. "Young as he still is," Gray noted, "no living botanist has investigated on the spot so many and so widely separated floras, and few like him have had constant access to the largest and best determined herbaria in the world," which, unquestionably, endowed Hooker with authority to "cancel nominal species" and to take "a more enlarged view of specific characters."<sup>90</sup> Yet, he warned, Hooker's engagement with those "species-splitters" might result in a "vicious circle."

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<sup>88</sup> Gray, "Introductory Essay...", 248-249.

<sup>89</sup> Gray, "Introductory Essay...", 350.

<sup>90</sup> Gray, "Introductory Essay...", 252.

One thing [is] clear, and important to be enforced: namely, that if determinations of species are to be of any value, especially in their bearing on general questions, they must rest solely upon observed characters of admitted value, irrespective of all theory. We pronounce such and such individuals, from a certain habit, to belong to a distinct species, only because we find them possessed of certain adequate distinctive marks. If we at length ascertain that particular species are peculiar to particular stations or parts of the world, we have a sound and valuable deduction. But to assume that certain plants, or certain animals, from widely sundered localities belong to different species, notwithstanding their resemblance, until the contrary is proved, and even to announce this as a principle for general adoption, as has been done, is surely a gross instance of reasoning in a vicious circle.<sup>91</sup>

### **“An awful staggerer”**

Gray believed that Hooker was open-minded and could stomach his critical comments. Though he had not corresponded with Hooker for a while, he decided to make sure of their friendship by dispatching a letter to Hooker, making clear his opinions and disagreements. It is evident that besides those comments in print, Gray seized the chance to discuss a case with which he had been familiar and yet which Hooker had not included in his comprehensive discussion on species and geographical distribution: the floristic analogies between two or more localities geographically far-flung in relation to each other.

Hooker was overjoyed to receive Gray's response. "Such Essays attract so little attention in this country," he replied, "that one feels, at least I did, that I was writing for the dead more than for the living, though amongst other men Agassiz had a prominent seat in judgement before me." As for Gray's comments, he admitted that "your turning my arguments against myself on the point, that two originally created distinct species so similar as to be almost undistinguishable, may exist in two widely

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<sup>91</sup> Gray, "Introductory Essay...", 339.



sundered localities, is an awful staggerer, and I have always felt it to be the most impracticable objection of any to the possibility of determining what is and what is not species.” Still, he continued, “I combat this theory more upon principle than upon facts;—once admit it and the flood gates are opened to species-mongers, and it is cast in your teeth every moment, as an argument for making every slight difference, if only accompanied with geographical segregation, of specific value.” In this regard, he concluded, although “I am quite aware that such species must exist; I do not deny, nor would I blink [at] the evidence in favour of it, nor that it is the gravest of all objections to the pronouncement upon species in our present state of knowledge. I therefore admit its application to practice only in exceptional cases.”<sup>92</sup>

To Hooker, this approach to disjunct distribution was an unavoidable compromise. “If you admit two centres [of creations of species] you may as well admit all Agassiz, you cannot draw the line.” More importantly, he warned, “Geographical distribution is hence a vain study, the connection of life with the revolutions of our globe and with all the physics of nature is naught, and nothing can come of its pursuit but the temporary gratification of taste and ingenuity.”<sup>93</sup>

Gray promptly replied to clarify the points “boggled” in his former letter. He seized the chance to share his experience dealing with those similar American and Asian species. “If species were originally given each to a circumscribed... local area (which you maintain, even in the strictest form);—if species were created with almost infinitely various degrees of resemblance among each other (which is just

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<sup>92</sup> JDH to AG, 1/26/1854, Porter, “On the Road...,” 14-15.

<sup>93</sup> JDH to AG, 1/26/1854, Porter, “On the Road...,” 14-15.

what scientific Systematic Botany rests upon), and if it be true that congeneric forms are often found in two widely separated areas of similar climate (as in the U.S. & Japan, Arctic & Antarctic regions &c. &c.), then the occurrence of two closely resembling, yet originally distinct species in widely separated places of similar climate is just what I should *apriori* expect.” “Is it not just what you should maintain so long as you deny the double origin of species?” he asked. “I see no real contradiction between them, viz. the general and fundamental law of genetic resemblance, and the exceptional, in explicable [*sic*]... origin of races, which, once originated, equally follow the law of genetic resemblance, or show the strongest tendency to reproduce the parental features or peculiarities.”<sup>94</sup>

Notably, soon after Gray introduced Hooker’s species theory to American readers, Agassiz presented his updated view on species and geographical distribution. The Swiss naturalist was now at the apex of his productivity. He had just announced his plan to compose a volume titled *Contributions to the Natural History of North America*, and promptly received 2,500 subscriptions.<sup>95</sup> In 1854, he composed an essay titled “Sketch of the Natural Provinces of the Animal World and Their Relation to the Different Types of Man,” to be included in Josiah Nott’s and George R. Gliddon’s *Types of Mankind or, Ethnological Researches*. The thrust of Agassiz’s essay was that “the species belonging to the western countries of the old world are not identical with those of the eastern countries.” As he put it, “It is true that they often resemble each other so closely, that until very recently they have

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<sup>94</sup> AG to JDH, 2/21/1854, Porter, “On the Road...,” 16-17.

<sup>95</sup> See Dupree, *Asa Gray*, 223-230.

been confounded.” Furthermore, in Agassiz’s view, such meticulous distinction suggested that the “Asiatic zoological realm differs essentially from the European and the American,” which in turn evidenced the undeniable racial distinctions that to him existed in these realms. Finally, he argued, “if we now ask what are the nations of men inhabiting those regions [Asiatic zoological realm], we find that they all belong to the so-called Mongolian race, the natural limits of which correspond exactly to the range of the Japanese, Chinese, Mongolian and Caspian faunae taken together, and that peculiar types, distinct nations of this race, cover respectively the different faunae of this realm. The Japanese inhabiting the Japanese zoological province; the Chinese, the Chinese province; the Mongols, the Mongolian province.”<sup>96</sup>

Gray might have expected Agassiz to drive his theories on geographical distribution this far. In a letter to Alphonse de Candolle in June 1854, he enclosed a reprint of Hooker’s essay with a remark: “Agassiz here is committed to the view opposite to Hooker’s, in an equally extreme form,” and expressed enthusiasm about de Candolle’s forthcoming *Géographie botanique raisonnée*, “expecting very much from... fairness of mind.”<sup>97</sup>

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<sup>96</sup> Louis Agassiz, “Sketch of the Natural Provinces of the Animal World and Their Relation to the Different types of Man,” in *Types of Mankind: or, Ethnological Researches, Based upon the Ancient Monuments, Paintings, Sculptures, and Crania of Races, and upon Their Natural, Geographical, Philological and Biblical History*, edited and authored by Josiah Clark Nott and George Robins Gliddon (Philadelphia: Lippincott, Grambo & Co.; London: Trübner & Co., 1854), lxiv-ixvi .

<sup>97</sup> AG to ADC, 6/1/1854, LAG, 410.

## Disjoined species

But Gray might have experienced a sense of *déjà vu* as he read de Candolle's long-awaited *Géographie botanique raisonnée* in October 1855.<sup>98</sup> De Candolle, because he could hardly provide a satisfactory account for why identical species could be found in widely separated spots—the so-called disjoined-species problem—was forced to admit that disjoined species might result from multiple creations. But he insisted on the general accuracy of the single origin of species. Above all, he argued, disjoined species were so rare and so exceptional that naturalists could readily put the problem aside.

But de Candolle's compromise was exactly what Gray could not do. He noticed that de Candolle's example was *Phryma leptostachya*, a species distributed only in eastern North America and in central Asia. At some point he might have recalled Hooker's warning: "if you admit two centres you may as well admit all Agassiz, you cannot draw the line."<sup>99</sup>

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<sup>98</sup> In October 1855, just as he returned from a visit to Europe, Gray proudly reported to de Candolle that, *in addition to* Joseph D. Hooker and Thomas Thomson's voluminous *Flora Indica* (1855), he had finished 1,087 pages of de Candolle's *Géographie botanique raisonnée*. "I was obliged by the close of the voyage to break off, at a very interesting point." AG to ADC, 10/27/1855, LAG, 420.

<sup>99</sup> See Asa Gray, "Alph. DeCandolle: Géographie botanique raisonnée; ou, Exposition des faits principaux et des lois concernant la distribution géographique des plantes de l'époque actuelle," *The American Journal of Sciences and Arts (2nd Series)* 22, no. 66 (1856), 429-432. "With regard to the *Phryma*," Hooker commented in his review of de Candolle's volume, "M. de Candolle inclines to the hypothesis of a double creation... that in the present state of our knowledge there appears to be no direct solution of the problem of their [disjoined species'] dispersion." Joseph D. Hooker, "Géographie botanique raisonnée, ou, Exposition des faits principaux et des lois concernant la distribution géographique des plantes de l'époque actuelle," *Hooker's Journal of Botany and Kew Garden Miscellany* 8 (1856), 116-117.

Also finding de Candolle's treatment problematic was Yale geologist James D. Dana. Like Gray and Agassiz, Dana had been intrigued by the issue of disjunct distribution, especially when he came to examine the zoological collections made during his service in the U.S. Exploring Expedition. Dana was also aware that the flora and fauna of Japan might hold the key to this extraordinary pattern in nature. As he put it in his report on the collections of *Crustacea*,

The close resemblance in species and genera from Britain and New Zealand, and from Japan and the Mediterranean, and the actual identity in some species among the latter, proves there that, as regards the species of two distant regions, identity as well as resemblance may be attributable to independent creations, these resemblances being in direct accordance with the physical resemblances of the regions.

In Dana's opinion, there was only one way left to explain why identical species could have occurred in two or multiple mutually separated locations. "The Creator has in infinite skill, adapted each species to its place, and the whole into a system of admirable harmony and perfection."

In his wisdom, any difference of physical condition and kind of food at hand, is sufficient to require some modification of the intimate structure of species, and this difference is expressed in the form of the body or members, so as to produce an exactness of adaptation, which we are far from fully perceiving or comprehending with our present knowledge of the relations of species to their habitats.<sup>100</sup>

Afterward, Dana would claim that naturalists need not be puzzled by such peculiar patterns in nature as the disjunct distribution of species. In his opinion, the geographical distribution of species would become perfectly comprehensible if

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<sup>100</sup> James Dwight Dana, *On the Classification and Geographical Distribution of Crustacea: from the Report on Crustacea of the United States Exploring Expedition under Captain Charles Wilkes, U.S.N., during the Years 1838-1842* (Philadelphia: Printed by C. Sherman, 1853), 1587-1588.

naturalists took the distribution of minerals as an analogy. Thus, he would argue, it was normal for naturalists to discover a species distributed in two or multiple widely separated locations.<sup>101</sup>

Gray decided not to get involved in this increasingly tangled dispute between the camp of the single specific creation and that of the multiple specific creations. If this was such a slippery issue that even such experienced naturalists like de Candolle and Dana could not handle it properly, the matter might have been utterly unsolvable given current knowledge of species. Hooker seemed to agree with Gray. After Agassiz's "Sketch" was published, the two botanists often joked about their extraordinary discreetness in not confronting Agassiz. In June 1855, when Hooker enthusiastically suggested that Gray should include "species,—their distribution in space & time, their relations" in the lecture, Gray replied, "I am not likely to have time next year for it [the lecture], so shd. be glad to give you my place."<sup>102</sup> Hooker declined the invitation, and Gray replied innocently that "I am sorry that you will not take the bait I throw out to you." "I do not like popular teaching myself... I have no gift," he explained.<sup>103</sup>

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<sup>101</sup> James D. Dana, "Thoughts on Species." *The American Journal of Science and Arts (2nd Series)* 24, no. 72 (1857), 305-316. Gray would find Dana's analogy unacceptable. Although he agreed with Dana that naturalists should adopt a broad definition of species to consider those similar Japanese and American species identical, he would write to Dana, the geographical distribution of species and that of minerals were entirely two different subjects (see Chapter 9). Gray's comment on Dana's essay can be found in AG to JDD, 11/7/1857, *LAG*, 430-432.

<sup>102</sup> AG to JDH, 7/2/1855, *GCK*.

<sup>103</sup> Gray also told Hooker that, in his opinion, a public lecture was by no means an appropriate approach to scientific discussion. Agassiz's public lectures, he pointed

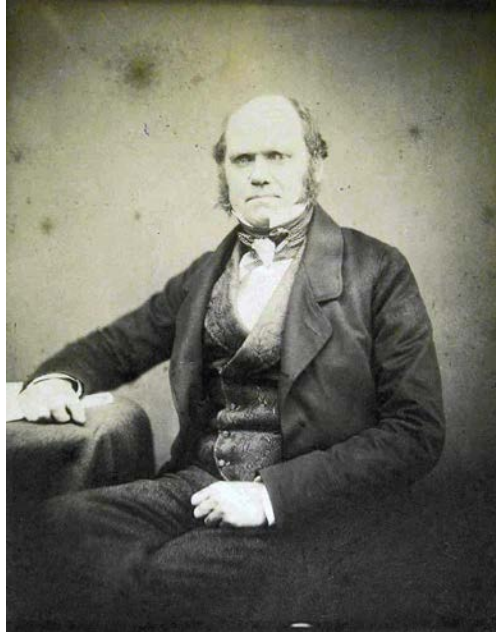


Figure 7-12. Charles Darwin in 1855. (Courtesy of the Archives of the Gray Herbarium, Harvard University, Cambridge, MA, USA.)

Interestingly, Gray and Hooker's reluctance to confront Agassiz about the origin of species, or even to debate with him the origin of disjoined species, called attention to another reluctant naturalist, Charles Darwin, who at the time had kept his species theory secret for more than a decade (Figure 7-12).

These three men became friends and correspondents in the mid-1850s. Gray's letters to Hooker were often forwarded by Hooker to Darwin. Reading some of these in 1854 that referred to Agassiz, Darwin wrote to Hooker, "I cannot quite understand why you & he [Gray] think so strongly that it 'does more harm than good to combat such views'."<sup>104</sup> The British gentleman had just completed his project on barnacles. With refreshed perspectives on how organisms varied and

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out, "injured Agassiz [by] tampering with strict veracity for the sake of popular effect." AG to JDH, 10/6/1855, *GCK*.

<sup>104</sup> CRD to JDH, 3/26/1854, *CCD*, Vol. 5, 186.

with sharpened insights into the larger issues of taxonomy, Darwin felt ready to approach his ultimate goal: to explain how species arise in nature without divine agency.

On November 1, 1854, Hooker wrote to Gray, "I have been staying three days with Darwin, who asks how it is that no American has ever written a popular or scientific work on the general characters of American vegetation as compared with European; is it so?" Gray could not have mistaken Hooker's intention. He promptly replied, explaining that he had decided to compare the "N. America Flora with that of Europe, when the Flora of N. America was completed." "But when will that be?" he groaned. "I shall not be glad if I can ever bring even to a lame conclusion the undertaking I have somewhat too rashly engaged in." "The work Darwin suggested would be most interesting & useful," he confirmed. "I wish there were some fit person on this side of the water to undertake it."<sup>105</sup>

In April 1855, Darwin decided to dispatch a letter to Gray. "I hope that you will remember that I had the pleasure of being introduced to you at Kew," he began.<sup>106</sup> He hardly foresaw that the letter would initiate an engaged dialogue about species, genera, and geographical distribution and would henceforth help him identify a crucial principle in the theory of natural selection on the one hand, and push the reluctant Gray to openly criticize Agassiz's work on the other.

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<sup>105</sup> JDH to ARG, 11/1/1854, Porter, "On the Road to the *Origin...*," 18; AG to JDH, 11/21/1854, *GCK*.

<sup>106</sup> CRD to AG, 4/25/1855, *CCD*, Vol. 5, 322-323.



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Gray felt compelled to communicate with Wright. Since Wright's departure for East Asia in May 1853, he had replied none of Wright's letters. He could tell that Wright had become annoyed by this indifference. So on February 19, 1854, he settled himself and composed a long letter. "Sinner that I am," he began, "I have four letters of yours unanswered; the last from Simon's Bay, November 4th." He then poured out a wide variety of news—foreign and domestic alike—that a traveler like Wright would want to know. He even offered his opinions about how Wright should behave onboard a Navy surveying ship: "Captains at sea are very apt to get a little crusty, which should be minded just as little as possible. I expect to hear that, after getting well settled and at home in Vincennes, you find yourself comfortable and all pleasant. Gentlemanly conduct and devotion to one's pursuits will at length make one respected, anywhere." The North Pacific Exploring Expedition would make Wright's name, Gray emphasized.

When you return, I trust you will yourself prepare the Botanical Report of your cruise. I hope so—for your own sake, both scientifically & because your doing so will keep you on pay, some years longer on shore. I will aid you, if I live, most willingly, over knotty points &c.—perhaps would like to do certain families,—further than that, not, if you will take hold of it yourself—as you ought to do—

As long as Wright could establish himself as a trustworthy and gentlemanly man of science, Gray was convinced, there would be plenty of chances for Wright to explore and to make a living. The United States of America was expanding and needed good men of science to cope with the explosion of information in every department of natural history.<sup>107</sup>

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<sup>107</sup> AG to CW, 2/19/1854, *AGPG*.

Gray had decided to let Wright take all the credit as the naturalist who joined the expedition, collected specimens, compiled field information, published results, and thereby won himself a reputation as a scientific savant, a career-making course akin to James D. Dana or Charles Pickering of the U.S. Exploring Expedition. With this view in mind, he refused to publish any discovery that might be derived from Wright's NPEE collections while Wright was away. In June 1855, when Baird informed him that a portion of Wright's collections had arrived at the Smithsonian and wondered if Gray wanted to examine it, he replied, "Wright's packages of plants ought to be kept undisturbed till his return, kept dry and camphorated—and they are safer in your fire proof building than they would be here by far." He let Baird understand that he had hoped that Wright "might be able to work up the greater part of his botanical collections himself, with help in some families." "There is a general understanding between us," he explained.<sup>108</sup>

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<sup>108</sup> AG to SFB, 6/18/1855, Folder 22, Box 7, *CM*.

## CHAPTER EIGHT

### “The Field of the Work of the American Surveying Expedition”

In the summer of 1854, when Wright read Gray’s letter dated February 19, he groaned that the dear doctor had once again underestimated his sufferings in the field. According to what he had experienced during the past year, he had little doubt that Commander Ringgold had not just gotten a “little crusty” but pretty much had gone crazy. In a letter to John (dated July 25), he reported that within the past twenty-four hours Ringgold had been “as crazy as a bedbug.” With malignant delight, he listed Ringgold’s “vagaries” of all sorts: “he hugged and kissed his steward a long lank nigger as black as darkness distilled”; “he mounted his boy William and rode poose-back round the cabin at midnight”; “he is going to have his boats anchored gilded and he gave the instrument maker a dozen pearl shirt-buttons with orders to turn them smaller.”<sup>1</sup> The fact was that Ringgold eventually collapsed under the duties that he *alone* felt obliged to perform. Since his arrival in Hong Kong in March 1854, he directed the NPEE squadron to undertake a variety of special missions, including chasing pirates, protecting American residents from the threat of the Taiping Rebellion, rescuing shipwrecked sailors, and so on. What he scarcely proceeded was the mission that Congress and the Navy Department had entrusted to him: surveying the routes that might connect the United States with China and Japan through the North Pacific. Ringgold soon encountered increasing cases of disobedience. Enraged, he sent six NPEE officers to court martial. Worse, in

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<sup>1</sup> CW to JW, 7/31/1854, *CWC*.

June, when he directed his men and vessels to Canton for a mission restoring “confidence to the residents of Canton and preventing violence on the part of the lawless bands,” he felt a fever come on.<sup>2</sup> In the months to come, he suffered tremendously from the so-called intermittent fever, now known as malaria. As he later recalled, “The spasmodic affection continued increasing, until my power of speech was partly interrupted,—causing me to stammer, and I suffered great pain and lost strength.”<sup>3</sup> The NPEE was in jeopardy and on the brink of collapse.

It was Commodore Perry who put Ringgold’s expedition back on track in motion. Arriving in Macao late in July after completing his expedition to Japan, Perry came to examine the progress of the NPEE. He hardly believed his eyes. “[E]verything connected, with the Exploring Squadron, in a state of confusion,” he reported to the Navy Department.<sup>4</sup> He immediately assembled a medical board to examine Ringgold’s health. The result was shocking. Ringgold’s mental disarrangement only remotely related to the “intermittent fever,” but had everything to do with some physically rooted insanity, the board reported. Given Ringgold’s proneness toward mental disorder, the board strongly recommended Ringgold’s “immediate return to the United States.”<sup>5</sup>

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<sup>2</sup> Ringgold, “Report,” F25, R3, *REENP*. “Unfortunately, as it turned out, I became myself the first victim to the unhealthiness of the locality,” he reported to Dobbin; see F29.

<sup>3</sup> CR to JCD, 9/4/1854, F363-369, R3, *REENP*, from Hong Kong.

<sup>4</sup> MCP to JCD, 8/9/1854, F344-346, R3, *REENP*.

<sup>5</sup> DSG, SPW, and GA to MCP, 8/1/1854, F319, R3, *REENP*. On Perry’s activities in China during his Japan expedition, see Chester A. Bain’s pioneering “Matthew Perry,

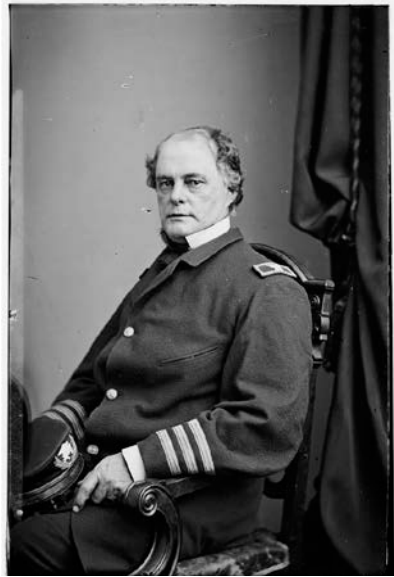


Figure 8-1. John Rodgers between 1860 and 1865. (Library of Congress Prints and Photographs Division Washington, DC, USA.)

Perry promptly took action. He relieved Ringgold of duty; he canceled all of Ringgold's orders demanding alterations be made to the NPEE vessels; and he suspended all of Ringgold's court martial charges.<sup>6</sup> As the Commodore maneuvered through these intricate matters, standing firmly at his side was Lieutenant John Rodgers, an NPEE officer commanding the steamer *John Hancock*, son of Commodore John Rodgers (1772–1838), a critical member of Perry's extended family, and the new commander of the NPEE (Figure 8-1).<sup>7</sup>

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Humphrey Marshall, and the Taiping Rebellion," *The Far Eastern Quarterly* 10, no. 3 (1951), 258-270.

<sup>6</sup> "I shall therefore do the best I can do remedy the evils, which have grown out of these events," Perry wrote to Secretary Dobbin; MCP to JCD, 8/9/1854, F344-346, R3, *REENP*.

<sup>7</sup> Perry's sister married the brother of Commodore Rodgers; Lieutenant Rodgers's brother married Perry's daughter.

The change of command hardly surprised Wright. Looking back, he told Gray, “Ever since leaving Norfolk (perhaps long before) all the mechanics on the ship (and when in part many from the shore) have been almost incessantly at work on doors, windows, ports, skylights, binnacles, gangways, gratings... and all with reference apparently to making each thing pretty or if already pretty prettier.”<sup>8</sup> To John, he wrote that he had little doubt about the diagnosis of Ringgold’s insanity: “He unquestionably began the difficulty by insisting on the observance of all the little niceties and punctilios... Well when he commenced hazing the lieutenants for such trifling matters as the trimming of their beards and putting them under arrest for refusing to shave according to his prescribed form—does it not appear as if his present derangement was then manifesting itself?”<sup>9</sup>

Still, Wright’s participation in Ringgold’s expedition had facilitated his transition to a traveling savant (Wright was “a collector rather than a savant,” Henry of the Smithsonian once commented). Of importance was that Wright became a frequent visitor to the library onboard the *Vincennes*. Wright cultivated this habit when the *Vincennes* set sail from Simon’s Bay to Sydney (in November 1853). At the time utterly annoyed by Ringgold’s commandeering style, he sought refuge in the vessel’s library to see if he could find some enjoyment there. He picked up a volume called *Journal of Researches into the Natural History and Geology of the Countries Visited during the Voyage of the H.M.S. Beagle Round the World, under the Command of Capt. FitzRoy, R.N.* by British naturalist Charles Darwin. Wright found it a good read. Later

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<sup>8</sup> CW to AG, 8/12/1854, *HL*.

<sup>9</sup> CW to JW, 8/2/1854, *CWC*.

on, when Commander Ringgold painstakingly maneuvered the *Vincennes* through the Coral Seas, he found that he got plenty of chances to study Darwin's novel theory of how coral insects built reefs.<sup>10</sup> Wright found the experience of examining nature through an accomplished naturalist's eye enlightening (it was a pleasure that he had never enjoyed during his past expeditions to Texas and New Mexico). After finishing Darwin's book, he picked up Samuel Wells Williams's *Middle Kingdom* for his upcoming trip to China. Again he found it a notable book and recommended it to his brother with enthusiasm. "It shows well the heartless nature of the English Government if such an organ as a heart can be attributed to a Government." In another letter, he urged John to "take books from the Library and follow me through the medium of these in my visits to the different countries whither I may go."<sup>11</sup>

The inspiration that Wright took from great travelers' narratives fundamentally shaped Wright's style in compiling his field notes and observations. He began recording longitude and latitude, wind direction, and sea depth in his journal. He found that he could easily acquire such navigational information by consulting logbooks and by asking the astronomer John M. Brooke. Moreover, to fully grasp such navigational information, he checked maps in the library, transcribed relevant information, and made sketches of the regions where he had conducted or was about to conduct botany (Figure 8-2). This was the first time that Wright had felt both delighted and able to make such a profile of information. Between 1849 and

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<sup>10</sup> CW to JW, 3/20/1854, *CWC*; Wright called *Journal of Researches* "Darwin's Voyage of a Naturalist," and recommended it to John, "though it may contain rather too much geology for you." CW to JW, 12/18/1853, *CWC*.

<sup>11</sup> CW to JW, 4/4/1853 and 12/18/1854, *CWC*.



Figure 8-2. Wright's sketch showing the track of the cruise of the *Vincennes* from Hong Kong to Japan. (Courtesy of the Connecticut State Library, Hartford, Connecticut, USA.)

1852, while collaborating with Gray on the flora of Texas and New Mexico, he had recorded habitats, localities, and local names of his specimens, but hardly cared to render such local information understandable to Gray. As might be expected, Gray at times wondered where on earth Wright had initially put together the collections and had been careful about offering any biogeographical implication that might be derived from Wright's Texan and New Mexican collections. For example, in *Plantae Wrightianae Texano-Neo-Mexicano*, concerning Wright's expedition from San Antonio to El Paso, he claimed that the Army Department would likely soon publish



a set of maps covering Wright's route. Hence, he added, he would not offer "any such account from Mr. Wright's disjointed and necessarily imperfect memoranda."<sup>12</sup>

Moreover, as an officially appointed botanist, Wright was required to enumerate his collections, compile a list, and submit it to Ringgold.<sup>13</sup> For the first time, he realized how demanding botanical studies could be. "I have not succeeded in determining many of them specifically for want of books and convenient place for study," he confessed in his first report to Ringgold concerning his collection made in Madeira.<sup>14</sup> "I have been hard at work nearly every day rumbling over the Mts and valleys—studying by night sometime till the beginning of the new day puzzling over green things till I was tired and going to bed no wiser than before," he wrote to Gray during his botanizing in Simon's Bay (Figure 8-3).<sup>15</sup>

Still, thanks to this repeated circle of collecting, classifying, and cataloging, Wright became no longer a plant hunter but a field botanist capable of surveying a flora entirely foreign to him. He established his two principles in plant collecting. First, he would collect specimens that covered as many provinces as possible in the botanical kingdom. "I am often asked what particular families I collect which I commonly answer by 'everything,'" he wrote to Gray from Hong Kong. "So Mr. Curtis [interested in fungi], Mr. Tuckerman [lichens] and Mr. Sullivant [mosses and

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<sup>12</sup> Gray, *Plantae Wrightianae Texano-Neo-Mexicano... Part I*, 6.

<sup>13</sup> Ringgold's order pertaining to Wright's duty can be found in F19-20, R1, *REENP*.

<sup>14</sup> CW to CR, 9/21/1853, Folder 21, Box 1, *NPEEC*.

<sup>15</sup> CW to AG, 10/23/1853, *HL*.

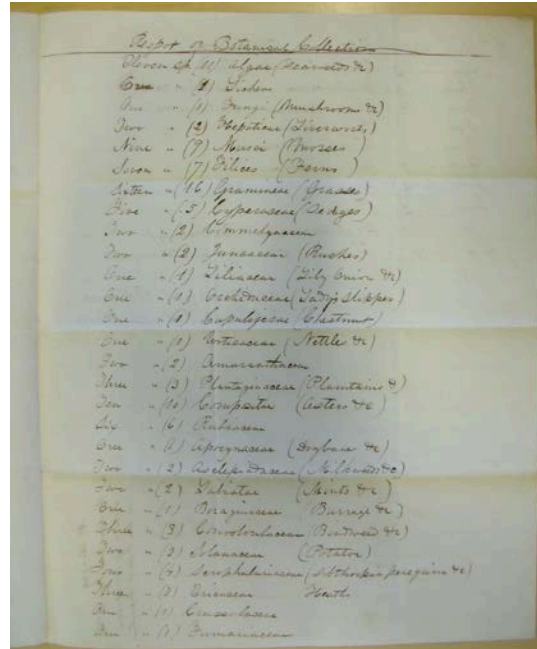


Figure 8-3. A page of Wright's "Report of Botanical Collections," prepared in the early part of the NPEE. (Courtesy of the Smithsonian Institution Archives, Washington, DC, USA.)

liverworts] may feel an interest in my labors.”<sup>16</sup> Second, he would only collect plants in “a state of nature.” That is to say, he would refrain from touching upon any plant in cultivation, which, in his view, had been modified beyond its natural form. So during his botanizing in Hong Kong and its vicinities, including Canton, Whampoa and Cum-sing-moon (from March to September 1854 and then from January to April 1855), though he had plenty of chances to encounter plants unknown to him (and to botanists in general) in farms, gardens, or markets, he ignored them all. Instead, he pushed himself to the remotest corners of the densely populated British colony,

<sup>16</sup> CW to AG, 8/12/1854, HL.

including mountains, riverbanks, and local Chinese graveyards.<sup>17</sup> Afterward he estimated that he had collected some 800 species in Hong Kong and its vicinities.<sup>18</sup>

Wright's collection made in Hong Kong would eventually make his name known internationally in botany. In November 1857, when the expedition concluded, Gray placed Wright's Hong Kong collection in the hands of British botanist George Bentham.<sup>19</sup> Based on this collection, together with collections made by his botanical correspondents, Bentham published *Flora Hongkongensis* in 1861 as part of the Kew series of colonial floras. Bentham was definitely impressed by what Wright had harvested in Hong Kong. In his introduction to *Flora Hongkongensis*, he noted that Wright had "proved himself as zealous and active on this as on other occasions, for he brought away specimens of above 500 species, several of them of great interest, and not received from any other source."<sup>20</sup> It should be noted that Bentham's

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<sup>17</sup> Regarding Wright's expedition to local Chinese graveyards, see CW to JW, 5/1/1854, *CWC*. "They [the local Chinese] seem to have the custom too of exhuming the bones or of collecting them when exposed by the rain or other causes and putting them in stone jars," he told John. Unbeknownst to Wright, here he had encountered a landscape that had been shaped and reshaped by the system of "secondary burial," widely practiced by the Chinese in Hong Kong, Canton, Fukien, Taiwan, and other regions in southeastern China; see, among others, Rubie S. Watson, "Remembering the Dead: Graves and Politics in Southeastern China," in *Death Ritual in Late Imperial and Modern China*, eds. James L. Watson and Evelyn S. Rawski (Berkeley: University of California Press, 1988), 203-227.

<sup>18</sup> CW to AG, 10/14/1854, *HL*.

<sup>19</sup> "There is a Hongkong collection [and] there may be some of these he would like to ask you to name, as far as you may offhand." AG to GB, 11/16/1857, *LAG*, 436.

<sup>20</sup> George Bentham, *Flora Hongkongensis: A Description of the Flowering Plants and Ferns of the Island of Hongkong*. London: L. Reeve, 1861), 10-11.

remark is not as plain as it appears to be. After all, Hong Kong Island did not constitute a vast botanical ground, and British travelers, colonial officers, and professional collectors had been exploring its flora since 1841. So Bentham was somewhat surprised that Wright could still discover new species in Hong Kong's well-explored botanical ground. In *Flora Hongkongensis*, he estimated that Wright had added at least four new species to the flora of Hong Kong: *Vitis corniculata* (now known as *Cayratia corniculata*), *Glochidion wrightii*, *Apocopsis wrightii*, and *Ficus wrightii* (now considered a synonym of *Ficus sarmentosa* var. *impressa*).<sup>21</sup>

Gray benefited from Wright's botanizing in Hong Kong, as well. Late in 1858, when he began studying Wright's collections made in Japan, a letter arrived from Hong Kong, bringing him information he had longed for. It was addressed by Henry F. Hance (1827–1886), a British diplomat and plant enthusiast then serving in Hong Kong. From one of Wright's letters, Gray had learned the name and botanical interests of this British diplomat and had received Hance's letter asking for specimen exchange.<sup>22</sup> He had sent back a set of selected American plants and was

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<sup>21</sup> Bentham, *Flora Hongkongensis*, 54, 313-314, 329, 421-422.

<sup>22</sup> In a letter to Gray in August 1854, Wright reported he had befriended a Mr. Hance, who had lent him books and helped him determine plants. "Mr. Hance has a kind of taste the antipodes of yours—a Compositae-phobia—and asked me to determine for him the most common—almost cosmopolite—sp. of *Ageratum*" (CW to AG, 8/12/1854, *HL*; also CW to JW, 5/29/1854, *CWC* and CW to JW, 8/2/1854, *CWC*). Hance sent his first letter to Gray in February 1858 to propose regular correspondence and specimen exchange. He also noted that "I was so fortunate the other day as to get a glance at a proof sheet of an enumeration from your pen of some plants collected in Japan by Drs. Morrow and Williams." "If you could take the trouble to supply me with a copy of this, and when published of your list of Mr. Chas. Wright's Chinese and Japanese collections, you would be conferring a great favor" (HH to AG, 2/3/1858, *HL*).

now glad to find that Hance was a valuable and responsible correspondent. A paragraph in Hance's 1858 letter particularly drew Gray's attention: "My interests in N. American botany have greater increased since I made a careful examination of some Manchurian & Japanese plants, collected during the Russian War, and found amongst others,—Onoclea sensibilis (not hitherto recorded from Japan)."<sup>23</sup> Gray found the statement useful to substantiate his disjunction thesis, for botanists had long thought that *Onoclea sensibilis* could only be found in eastern North America. In his 1850 paper, he reported that the "most peculiarly American Fern, viz. *Onoclea sensibilis*" had been discovered in Japan.<sup>24</sup>

In the summer of 1854, however, Wright could hardly foresee all such honors, acknowledgments, and intellectual interests in association with his botanical labor in Hong Kong. In fact, like Commander Ringgold and most of his teammates, he eventually got knocked down by Hong Kong's "most villainous climate" and became dangerously ill.<sup>25</sup> In his letter to John, he cried that he had been suffering from "the

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<sup>23</sup> HH to AG, 12/14/1858, *HL*.

<sup>24</sup> Gray, "Diagnostic...", 64.

<sup>25</sup> CW to AG, 4/9/1854, *CWC*. In a letter to his sister written some weeks after his arrival in Hong Kong, Wright described the island's very disagreeable climatic conditions:

We are now beginning to experience some of the discomforts of Hong Kong for which it is notorious and which contribute to give it the name of being a very unhealthy place. For a number of days a dense fog has hung over the moon turns descending oftentimes nearly to their base and again almost disappearing from the summit and on the other side of the harbor almost resting on the surface of the water. This continuing all day and day after day makes the atmosphere damp and unpleasant giving a gloomy appearance to every thing. Our clothing feels damp externally and when the weather is warm sticks to the skin causing a sensation any thing but pleasant. CW to MAW, 3/31/1854, *CWC*.

influenza and a low continuous fever” and had not had “the sensation of hunger” for the past two weeks. Wearily, he wrote that he had taken some “quinine battery” but the side effects were so severe that he could hardly bear it. “[M]y neck got tied up in a knot or appeared to be so and was so stiff and sore that I used to hold my head with both hands when I turned it for fear it would break off.”<sup>26</sup> “The sick-list of the ship is increasing in numbers,” he wrote fearfully in another letter. He wished that Rodgers, the new appointed NPEE Commander, would soon get them out of this particular corner of East Asia and sail northward, so that they could “breathe the healthful air from the ocean.”<sup>27</sup>

### **“A scientific man”**

“In Hong Kong 1854 I succeeded to the Command of the U.S. Surveying Expedition and went on board the corvette Vincennes,” Lieutenant John Rodgers wrote in the first line of his journal for 1854. He was forty-two years old, probably too young to be entrusted with such an honor and duty.<sup>28</sup> As everyone in the expedition knew, he

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“All in all the climate at this season in Hong Kong is more changeful, in its extremes & suddenness than that of New York; the crew and officers have suffered exceedingly from cold & catarrh,” a NPEE officer named Francis Asbury Roe also noted in his journal (Roe, “Journal USS Porpoise 1855,” Folder 3, Box 1, *FARP*. The statement is under the entry of March 1854).

<sup>26</sup> CW to JW, 8/24/1854, *CWC*; under the entry of September 8.

<sup>27</sup> CW to JW, 7/31/1854, *CWC*.

<sup>28</sup> By comparison, Ringgold took command of the NPEE at the age of fifty-one, and Perry commanded the Japan expedition at the age of fifty-eight. In the history of U.S. overseas expeditions, a remarkable exception was Charles Wilkes, who was appointed commander of the U.S. Exploring Expedition at the age of forty; but

became Ringgold's successor *not* because he was experienced in surveying expeditions but because he was second in rank to Ringgold. To be sure, thus far Rodgers's chief experience in surveying derived from his three-year participation (1849-1852) in Alexander Dallas Bache's Coast Survey.<sup>29</sup> The late Commander Ringgold might have been mentally weak, but his experience in surveying was unparalleled. How could a novice like Rodgers command an expedition that was expected to expand the country's empire of commerce and science? Rodgers asked himself this question, which became even more vexing when placed next to the resources at Rodgers's disposal. How could a *lieutenant* like him carry on a surveying expedition when his predecessor had almost exhausted the expedition's budget and his superiors' patience? To name only the most obvious, he did not even have a storeship under his command, despite the fact that this North Pacific Exploring Expedition barely touched the Pacific, let alone the North Pacific.

While pondering such questions, Rodgers pulled out a piece of paper and began drafting his first report to Secretary Dobbin. He first outlined his plan for the expedition's future operations. Unlike Ringgold, who had considered the route from Australia to Hong Kong essential for advancing the commercial intercourse between the United States and China, Rodgers stated that he preferred the other way around.

I deem that the field of the work of the American Surveying Expedition lies principally in the North Pacific Ocean and its bays and inlets. The route between Australia and the British possessions in the East is more particularly a British interest. No one but ourselves has any desire to survey the particular fields of

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Wilkes's appointment was roundly criticized by his contemporaries chiefly because of the commander's relatively inferior status in the Navy's chain of command.

<sup>29</sup> John Rodgers, "U.S. Surveying Expedition, Vol. 1," Folder 1, Box 8, *RFP*.

our commercial enterprise. Our shops from San Francisco to China and our Whalers in the Pacific, and waters which empty into it, whiten the Ocean. For them, if I judge rightly, was this expedition fitted out.<sup>30</sup>

“I think that the Pacific Rail Road, and Steamers to China, will turn the tide of commerce this way,” he told Dobbin in another letter. “We shall carry to Europe their teas and silk, from New York,” he declared, and he believed that such a “tide of commerce” was “inevitable.” Moreover, he thought that the NPEE under his commandship would contribute substantially to the country’s share in the common stock of human knowledge:

By far the great part of the charts in use by navigators, have been made by the English. We, through California, inherit the trade of the Pacific; for we are the only nation upon it which cultivates foreign commerce. In the Pacific we can scarcely hope that the English will make charts, from which we must reap so much more advantage than they [must do].—Self interest would rather cancel their putting an impassable barrier, were it possible, between our country and China.

“I hold then,” he concluded, “that the routes to China must be surveyed by the United States; the outlay cannot be avoided.”<sup>31</sup> The expedition now under his command was part and parcel of Americans’ Manifest Destiny.

Then Rodgers tried to convince Secretary Dobbin that he was perfectly capable of commanding the expedition. He reminded Dobbin of his close associations with eminent men of science, including Charles Henry Davis of the Nautical Almanac, Matthew Fontaine Maury of the Naval Observatory, Alexander Dallas Bache of the Coast Survey, and Louis Agassiz of Harvard College. When asked, Rodgers wrote that these scientific celebrities would not hesitate to give their endorsement of his

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<sup>30</sup> JR to JCD, 8/21/1854, Folder 5, Box 7, *RFP*; also see Cole, *Yankee Surveyors...*, 24.

<sup>31</sup> JR to JCD, 1/29/1856, [frame number illegible,] R5, *REENP*, from San Francisco.



capability.<sup>32</sup> Unlike Ringgold, who acquired his scientific knowledge chiefly from long, focused, and devoted shipboard service, Rodgers believed that he belonged to a new generation. That is to say, he would collaborate with men of science with enthusiasm and without reservation, and would have them accept him as an equal. He would be vindicated. In 1863, Rodgers was elected into the National Academy of Science, then a brand new scientific organization created by an act signed by President Lincoln, under the leadership of Agassiz, Bache, and Davis, and other members of the elite Scientific Lazzaroni.<sup>33</sup>

Finally, given his other duty as the Head of the expedition's scientific corps, Rodgers wanted to know precisely how to deal with the natural history collections that his two collectors, Charles Wright and William Stimpson, would make in the Okinawa Islands, the Bonin Islands, and Japan. He had heard rumors that the Navy Department had abolished its arrangements with the Smithsonian. So he called upon Stimpson to clarify the subject. In his reply, Stimpson assured Rodgers that the Navy Department had restored its arrangements with the Smithsonian. In other words, he confirmed, Rodgers should continue to consign natural historical specimens to the Smithsonian, and the Smithsonian would take good care of and conduct a judicious

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<sup>32</sup> JR to JCD, 8/21/1854, Folder 5, Box 7, *RFP*; also Cole, *Yankee Surveyors...*, 24-25.

<sup>33</sup> According to a letter Senator Henry Wilson sent to Rodgers, Rodgers was actually one of the funding "corporators" of the academy. HW to JR, 3/5/1863, Folder 2, Box 10, *RFP*.

survey of the collections to make the NPEE's scientific contributions known to the world.<sup>34</sup>

The rest of August 1854 was then spent in a flurry of preparation. On September 8, Rodgers wrote to Secretary Dobbin claiming that the squadron would set sail the next day. As for the finalized surveying scheme, he wrote that he had decided to make the expedition cover as much grounds as possible. He had received inquiries from Commissioner Robert McLane and Secretary Peter Parker, he wrote, in which these two gentlemen of diplomacy expressed their desire to have the NPEE's *John Hancock* and the *Fenimore Cooper* escort them to northern China to open negotiations with Chinese authorities.<sup>35</sup> He had agreed to the proposal, he told Dobbin, because the two vessels could meanwhile gather valuable information about the rivers that connected Shanghai with important cities in northern China like Tientsin. Besides China, Rodgers wrote that he also had ordered Captain Stevens to lead the *John Hancock* to gather information about coal mines recently discovered in northern Formosa. As for the *Vincennes* and the *Porpoise* under his immediate command, Rodgers wrote that the two vessels would survey the islets that connected Formosa and Japan, in particular the Bonin Islands and Ryukyu Islands. He estimated that they would reach the southern end of the Japanese Archipelago by the middle of November, and he planned to undertake a series of surveys there. Then he would bring the two vessels back to Hong Kong, rendezvous

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<sup>34</sup> WS to JR, 8/15/1854, Folder 10, Box 1, *NPEEC*.

<sup>35</sup> RMM to JR, 9/2/1854, Folder 5, Box 7, *RFP*; Rodgers's reply can be found in JR to RMM, 9/6/1854, [frame number illegible,] R4, *REENP*.

with the *John Hancock* and the *Fenimore Cooper*, acquire a new outfit, and set sail again for Japan in February 1855.<sup>36</sup>

“I am happy to say that Captain Rodgers is a scientific man... and gives me every facility of my researches,” Stimpson wrote to Baird with satisfaction.<sup>37</sup> Wright, too, wrote home that “Under Rodgers I never fear a reproof for cooking my finger or for having a hole in the seat of my trowsers if such a thing should happen.”<sup>38</sup> “He has promised Mr. Stimpson and myself that he will whenever practicable and safe land us on such shores and islands as we may pass where while the ships are occupied in surveying we will often be able to spend two or three days very pleasantly and profitably.”<sup>39</sup>

### **“We did not see her again”**

The *John Hancock* and the *Fenimore Cooper* departed on September 9 for Shanghai. The *Vincennes* and the *Porpoise* remained in Hong Kong for another three days and left the harbor for the Bonin Islands on September 12. Seeing Hong Kong vanishing on the horizon, Rodgers understood how risky it was to depart during the monsoon season. He felt compelled to do so, however. As he reported to Secretary Dobbin, “My object in going to the North against the monsoon was that our crews had

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<sup>36</sup> JR to JCD, 9/8/1854, Cole, *Yankee Surveyors...*, 25-27.

<sup>37</sup> WS to SFB, 9/5/1854, quoted from Vasile *et al.*, *William Stimpson’s Journal*, 108.

<sup>38</sup> CW to JW, 12/18/1854, *CWC*.

<sup>39</sup> CW to JW, 8/2/1854, *CWC*, under the entry August 13.

become somewhat enervated by the heat and unhealthiness of the Southern part of China and required the bracing atmosphere of a more Northern climate.”<sup>40</sup>

Rodgers’s decision proved controversial. On the one hand, although a sailor still died of dyspepsia when the *Vincennes* arrived in Ryukyu Islands, the officers and crewmen onboard the *Vincennes* and *Porpoise* quickly regained their energy.<sup>41</sup> As for Wright, he soon resumed his correspondence with Gray and John. (“I devote a small portion of my valuable time to enlighten you respecting the above matters,” he told Gray.<sup>42</sup>) Wright also surveyed books and maps at the library, and above all studied his Hong Kong collections. If the circumstances rendered these three undertakings impossible, he studied German. “I have taken up the systematic study of German,” he wrote to Gray. “[I]f we have as much salt-water work proportionally as we have already had and as little opportunity for botanizing I expect to be a pretty good Dutchman by the time I return.”<sup>43</sup>

On the other hand, however, Rodgers found that he had underestimated the power the monsoon. The gales and currents provoked by the monsoon made it difficult to steer the vessels. In fact, as Wright reported to John, an incident that

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<sup>40</sup> JR to JCD, 8/21/1854, Folder 5, Box 7, *RFP*.

<sup>41</sup> According to Wright, the victim was “a petty officer a quartermaster and a very steady old man.” CW to JW, 10/22/1854, *CWC*.

<sup>42</sup> CW to AG, 10/14/1854, *HL*.

<sup>43</sup> CW to AG, 10/14/1854, *HL*. But it seems that Wright’s newly kindled enthusiasm in language study soon died out. About a week after he informed Gray about his “systematic study” of German, he wrote to John that “You needn’t be afraid however that you won’t understand me when I return I have dropped it just now when it interferes with my botanical labor.” CW to JW, 10/22/1854, *CWC*.

occurred right after the *Vincennes* and the *Porpoise* left Hong Kong almost put an abrupt end of the expedition:

We made pretty good progress for the first two days nothing occurring worthy of notice till after dark of the [September] 13th when a light was reported ahead and soon after a large ship found exactly on the opposite course came fearfully near us even if it had been broad day light and much more so in the darkness. She evidently had no lookout or a sleepy or careless one and descried us only when very close—when seeing the *Porpoise* on our port quarter (or left hand side) the direction in which she should have turned she suddenly steered to larboard (or to the left) coming on our starboard or right hand side—the way which it was our privilege and duty to turn—thus embarrassing our movements and causing us and herself too imminent risk. Had we struck the collision would have been fearful and caused the destruction of both ships. Mercifully we did miss each other. It must have been gross and culpable negligence that on the wide ocean two ships should come so fearfully near each other especially at night when the danger is so infinitely magnified.<sup>44</sup>

Rodgers first tried to break out into the Pacific through Bashi Channel and from there reach the Bonin Islands. The attempt proved futile. The currents were so strong, and Rodgers soon found that the *Vincennes* and the *Porpoise* got stuck in the middle of Formosa Strait. As the gales showed no sign of abating, Rodgers decided to seek shelter under the southern end of Formosa. He signaled the *Porpoise* to follow him. Then, on September 21, nine days after the two vessels departed Hong Kong, a tragedy took place. The *Porpoise* was blown away as she made her way to Formosa, and the brig, with fifty-two people onboard, was never to be seen again. Rodgers reported this incident to his mentor Maury of the Naval Observatory:

We kept company until the morning of the 21st of September—We were inside of Formosa, in mid channel, just to the Nd of the Pescadores—All night it had been raining and blowing hard from the NE with a high barometer—We were under close reefed main topsail, main trysail & storm staysail—wearing from time to time to keep in mid channel—The *Porpoise* was under storm sails also, and kept company by wearing when we did upon a signal shown by lanterns—At

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<sup>44</sup> CW to JW, 10/22/1854, *CWC*.

day light we were in mid channel as I have said between Formosa and China, just to the Northward of the Pescadores Is.—The wind was to the N.E. I determined to run to the Sd & Ed under the lee of the Southern part of Formosa, and wait until the gale abated—I wished to signal asking how she was getting—and searched through the signal book, but found that I must run several signals to ask the question. I saw no reason for asking it, it was bad weather for common-place signaling and I refrained—How much I wish I had made the signals—I put up our helm—The Porpoise attempted to follow—The sea was a short angry one but not very high. I saw the Porpoise buff to again—She was lying to, when a rain squall hid her from us—We were running rapidly under reefed foresail and close reefed main topsail and we did not see her again.—<sup>45</sup>

At first, Rodgers was not worried about the *Porpoise's* whereabouts. He was confident that the brig's commander, Lieutenant William K. Bridge, would steer the brig either to the Bonin Islands or to the Ryukyu Islands or return to Hong Kong. He did not even try to look for the *Porpoise*. It had been common for vessels to part with each other in extreme weather conditions.<sup>46</sup> More importantly, Rodgers considered that the *Vincennes* was in great danger. The sea had become rough. Two days after he lost the sight of the *Porpoise*, Rodgers scribbled some lines in the journal: "On the night of the 23rd we were standing to the Sd where the weather became very threatening.—The lightning flashed and the rain fall, we prepared for a squall—but the continued gloomy appearance of the sky, the confused sea, and the augmented force of the fitful squalls induced me to think that we were on the Northern edge of a typhoon."<sup>47</sup> In following six days, the *Vincennes*, with 121 officers and crewmen onboard, drifted up and down the Formosa Strait. On September 29, as the weather

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<sup>45</sup> JR to MFM, 3/5/1855, Folder 3, Box 8, *RFP*.

<sup>46</sup> "Two vessels in a gale are safer for not attempting to keep tight, for each can less to the best advantage..." Rodgers, "U.S. Surveying Expedition, Vol. 1," Folder 1, Box 8, *RFP*.

<sup>47</sup> Rodgers, "U.S. Surveying Expedition, Vol. 1."

calmed, Rodgers got control of the vessel and steered her into the Pacific through the Bashi Channel. The *Vincennes* had been trapped in the Formosa Strait for seventeen days, a voyage that under favorable circumstances would only take less than a week.<sup>48</sup>

### **Perfect state of nature**

The first destination for the *Vincennes's* northward voyage was the Bonin Islands, or, to be more precise, Port Lloyd of Peel Island, the most commodious harbor located in the largest island in the middle group of the Bonin Islands (the Bonin Islands comprise three groups of islets: the northern, middle, and southern groups). In the history of geographical discovery, it is Captain Frederick William Beechey (1796-1856) who won credit as the discoverer of the port, the island, and the Bonin Islands as a whole. In 1827, commanding the H.B.M. ship *Blossom*, Beechey arrived at the islands and took possession of them in the name of his king. He affixed a copper plate to a tree, punched with the inscription: "H.B.M. ship Blossom, Captain Beechey, R. N., took possession of this group of islands in the name and on behalf of His Majesty King George, the 14th of June, 1827." Beechey named the harbor "Port Lloyd," after the Bishop of Oxford, and the largest island "Peel" after Sir Robert Peel. With the ceremony completed, Beechey went on to survey the port and collect natural-history specimens. On leaving these islands, he set sail for the Sandwich

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<sup>48</sup> "... the lubberly old ship [*Vincennes*] was unable to steer for seventeen days," Wright wrote John; CW to JW, 10/22/1854, *CWC*; also CW to AG, 10/14/1854, *HL*; "The distance from Hong Kong to Formosa under favorable circumstances could be over in three days," Wright told Gray; CW to AG, 10/14/1854, *HL*.

Islands, and probably spread the news there. The year of 1830 witnessed a party of colonists (including one Genoese, one Englishman, two Americans, one Dane, and twenty-two male and female Sandwich Islanders) departed from Honolulu for Port Lloyd. Under the auspices of Captain Charlton, the British consul at Honolulu, they were furnished with livestock, seeds, and all necessary appliances and requisites, and endeavored to build up a settlement on Peel Island. In the meantime, Beechey published his discovery of the Bonin Islands in *Narrative of a Voyage to the Pacific and Beering's Strait, to Co-operate with the Polar Expeditions* (1831). Her Britannic majesty's sovereignty over the Bonin Islands was fairly secured.<sup>49</sup>

Twenty-seven years after Beechey's discovery of the Bonin Islands, on October 19, 1854, the *Vincennes* approached Port Lloyd. A man sailed a canoe toward the vessel and introduced himself as a resident of Peel Island who would serve as the *Vincennes's* pilot. He then came aboard and ushered the vessel into the port. After the *Vincennes* anchored, Wright joined a group of officers to visit the settlement and found it unimpressive. "They cultivated a few sweet potatoes (which above all now procurable) and other vegetables on which and on fish they principally subsist," Wright told Gray, and appeared to be "a miserable drunken set when they can get any thing to get drunk." To John, he wrote that the colony was thriving despite the

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<sup>49</sup> Frederick William Beechey, *Narrative of a Voyage to the Pacific and Beering's Strait, to Co-operate with the Polar Expeditions: Performed in His Majesty's Ship Blossom, under the Command of Captain F.W. Beechey, R.N. F.R.S. &c. in the Years 1825, 26, 27, 28* (London: Henry Colburn and Richard Bentley, 1831). This paragraph is based on a report titled "the Bonin Islands" in *CRUS*, 517-529.



settlers' drunkenness: "They have wives from the Sandwich Islands and are bearing a dusky progeny."<sup>50</sup>

But Wright found the flora of the Bonin Islands dear to his heart. "Went ashore after dinner and had my first fair view of tropical vegetation the most striking specimens of which are palm of various kinds," he wrote home. After spending some five months collecting plants in what he considered an over-cultivated botanical ground in Hong Kong, Wright was delighted to find that plants in the Bonin Islands were preserved in a perfect state of nature.

You may easily imagine that in so new a field I find plenty to do and yet this is as with you the autumn and though we have not here the "sere and yellow" leaf and the many tinted hues of your dying foliage yet there is no small variety of colors to cheer the eye and to afford subject for speculation. One abundant palm is of a very light green and a common shrub small tree has its leaves especially the lower side of the color of iron rust—the two combine giving the appearance of the passage of fire over the island. When palms and trees grow thickly the fallen leaves of the former obstruct the way and vines too tough to break come athwart the face the body or legs and you must stoop climb over or seize the knife and sever them. When you get out of the forest you come to what appeared from the ship nice grassy slopes and you are among (if you can get into it) coarse ready grass up to your neck and too thick to stick a butcher knife in. Here you can climb over the top or turn back—the latter course I have taken not being able to see ahead any object worthy of perseverance under such difficulties. There are naked places but they are not so easy to traverse as one looking from a distance might suppose. They are steep and if rocky the partially decomposed fragments crumble beneath the feet and one is always in danger of a tumble of greater or less extent—if these fragment still more reduced and comminuted into soil have formed layers on the steep hillside the rains have made them soft and they give way beneath the feet bringing the climber now and then to a halt on his stern sheet or stretched at full length lucky if his head is uphill.<sup>51</sup>

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<sup>50</sup> CW to AG, 10/14/1854, *HL*; CW to JW, 10/22/1854, *CWC*. Stimpson had the same observation: "They appear however to be very idle, and intoxicated most of the time." Vasile *et al.*, *William Stimpson's Journal*, 66.

<sup>51</sup> CW to JW, 10/22/1854, *CWC*.

Wright thought that he had been better equipped for a thorough and scientific study of the Bonin flora. First, he had a volume titled *The Botany of Captain Beechey's Voyage* in hand (published in 1841; a book recommended by Gray). In it two distinguished botanists, Sir William Hooker and G. A. Walker Arnott, had enumerated some 42 species, with illustrations of new species (*Sideroxylon ferrugineum*, *Elaeocarpus photiniaefolius*, and an *Evonymus*, later considered a new species, under the name *Evonymus boninensis* Koidz). Second, during his stay in Hong Kong, Wright had devoted himself to studying the tropical and subtropical flora of East Asia. He was confident that now he could do a better job than he had in the earlier part of the expedition. Wright exuded this newly acquired confidence in a letter to Gray (dated October 28). He wrote that he had undertaken a "thorough examination" of the northern end of the Peel Island, and was confident that he could "bring away a better list of plant than Beechey did." He then happily enumerated those species that seemed legible to him: *Zanthoxylum piperitum*, *Euonymus japonicus*, *Hydrocotyle asiatica*, *Sideroxylon ferrugineum*, one species of *Corydalis*, one species of *Rhaphiolepis*, two species of *Apocynaceae*, one or two species of *Callicarpa*, one species of *Euphorbia*, one species of *Pandanus*, four species of *Solanum*, and so on. In addition, he wrote, he had procured "a fair proportion of lichens and a very large number of fungi." He estimated that he had collected some "three hundred [species] including one hundred and twenty Fungi (the number of sp. pretty much guess work)."<sup>52</sup>

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<sup>52</sup> CW to AG, 10/14/1854, *HL*.

Gray, when getting hold of Wright's Bonin collection, decided to keep the right to name new Bonin species for himself. He considered the flora of the Bonin Islands part of the flora of Japan, and he had envisioned that the floristic relationship between Japan and eastern North America would turn out to be the most important discovery in Rodgers's expedition. In his 1859 paper on Japan's flora, Gray reported that Wright's Bonin collection contained at least four new species, including *Psychotria homalosperma*, *Hedyotis leptopetala*, and *Stylocoryne subsessilis* (now known as *Tarenna subsessilis*). More importantly, he declared that Wright had collected a Californian species *Photinia arbutifolia* in the Bonin Islands, thus revealing a peculiar phytogeographical connection between Japan and the United States.<sup>53</sup> (In 1888 Russian botanist Carl Maximowicz [1827-1891] considered Gray's "Californian species" a new species, and named it *Photinia wrightiana*.)

Stimpson found the fauna of the Bonin Islands stimulating, as well. What particularly intrigued him was the issue of disjunct distribution of animals. "What

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<sup>53</sup> Here it is worth noting that for the next six decades or so, Wright's Bonin collection remained the most comprehensive one that Western botanists had ever procured or had access to. The next significant Bonin collection made by Western botanists had to wait until 1917, when Ernest Henry Wilson (1876-1930) of the Arnold Arboretum visited the Bonin Islands. From the last quarter of the nineteenth century onward, it was Japanese botanists who played a critical role in surveying, cataloging, and rendering the richness of the Bonin flora known to the world. (In 1875, Japan officially claimed the Bonin Islands as part of the country's territory, on the basis that Japanese people had discovered the islands far before Captain Beechey, and that, indeed, the English word "Bonin" was a derivation of the Japanese term *mujin*, meaning "no people" or uninhabited.) In 2011, one hundred and eighty-four years after Captain Beechey's "discovery" of the Bonin Islands, the World Heritage Committee inscribed the Bonin Islands (now more often known as the Ogasawara Islands) on UNESCO's World Heritage List for "the wealth of their ecosystems which reflect a wide range of evolutionary processes." See <http://whc.unesco.org/en/news/766>.

must one do when he finds species apparently the same in extremely distinct localities, or under circumstances which forbid them directly? for instance a freshwater Palaemon in Java and at the Bonin Is.," he wrote to Yale geologist James D. Dana:

I think I have data to show that many species formerly supposed to be identical (particularly among the shells) are really distinct. Nature seems to have distributed animals as she did the stars, in clusters and nebulae, crowded at some points, scattered at others; we resolve nebulae with our telescopes, and see clearly between closely approximate species by minute examination. So and thus and also a knowledge of geographical distribution, I never pass by even a fragment of a shell or coral, where no little specimens is to be obtain, as is often case in hurried visits to some of the islands.<sup>54</sup>

Here Stimpson was to confirm what Louis Agassiz had attested in various talks and publications concerning the faunal similarities between Japan and eastern North America during the late 1840s and the early 1850s. Having studied with Agassiz for two years before joining the NPEE, Stimpson was delighted to find fresh evidence for his mentor's thesis. He would then keep the issues of disjointed species in mind and would again explore the subject as he collected in Japan for months afterward. Dana would find Stimpson's discovery in the Bonin Islands revealing and discussed it in his "Thoughts on Species" (1857).<sup>55</sup>

Even Commander Rodgers made his contribution to the cause of advancing science. On October 28, nine days after the *Vincennes* arrived in the Bonin Islands, a violent typhoon swept over the Islands and neighboring seas. Officers and crewmen onboard the *Vincennes* felt blessed, because this typhoon came so abruptly, without

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<sup>54</sup> WS to JDD, 2/21/1855, F0737-0738, R3, DFP; also see Vasile et al., *William Stimpson's Journal*, 12.

<sup>55</sup> Dana, "Thoughts on Species," 313.

any significant meteorological signs in advance. They could really have been wrecked had Commander Rodgers decided to set sail for the Ryukyu Islands at that particular moment.<sup>56</sup>

Rodgers was delighted that he had once again led his men and vessel to escape a direct attack by a typhoon. On October 28, as the typhoon approached, landed, and departed, he observed and recorded what was happening in Port Lloyd, and had the expedition's assistant astronomer Anton Schönborn record the rise and fall of the barometer reading during the typhoon's assault. Afterward, he communicated this information to William C. Redfield (1789-1857) of New York, a distinguished meteorologist known for his theory predicting the movements of hurricanes. "I am a firm believer, out of my own experience, in the truth of your theory of hurricanes," wrote Rodgers. "I think you have enabled me to avoid storms, into whose centres I should have been unwilling to be involved, and I feel, therefore, that I am under personal obligations to you for your happy meteorological discoveries." "You have conferred by them a great good to the nation and to the world," he assured Redfield.

Redfield was apparently delighted to receive Rodgers's first-hand observations. He read Rodgers's letter to a meeting of the American Association for the Advancement of Science in August 1856, and then published it in the Association's *Proceedings* and then *The American Journal of Science and Arts* under the title "On

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<sup>56</sup> Alexander Wylly Habersham, *My Last Cruise, or, Where We Went and What We Saw: Being an Account of Visits to the Malay and Loo-Choo Islands, the Coasts of China, Formosa, Japan, Kamtschatka, Siberia, and the Mouth of the Amoor River* (Philadelphia: J.B. Lippincott & Co.; London: Trübner & Co., 1857), 165-166.

the Avoidance of the Violent Portions of Cyclones, with Notices of a Typhoon at the Bonin Islands.”<sup>57</sup>

For men of science researching in their herbaria, museums, observatories, and libraries, the expedition under Rodgers’s command became more and more interesting and relevant to science. And it occurred to them that the climax of the expedition would take place when Commander Rodgers led the squadron to Japan, and the scientific corps landed and undertook close observations and surveys. They looked forward to being furnished with fresh and firsthand information about the country that had very recently been opened by Commodore Perry.

On November 6 the *Vincennes* left Port Lloyd with a considerable quantity of sweet potatoes and fish, as well as a few living turtles onboard.<sup>58</sup> Commander Rodgers, while saying farewell to the “Colony of Peel Island,” ordered the *Vincennes* to set sail for the Ryukyu Islands, at the time ruled by an independent kingdom. Early in the year (on July 11, 1854), Commodore Perry had had the Ryukyu Kingdom sign a “compact” with the United States, which, in Perry’s words, bounded “the government and people of Lew-Chew to treat with kindness and friendship all Americans visiting the ports of the island.”<sup>59</sup> With a copy of Perry’s treaty in hand,

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<sup>57</sup> John Rodgers and Anton Schönborn, “On the Avoidance of the Violent Portions of Cyclones, with Notices of a Typhoon at the Bonin Islands,” *Proceedings of the American Association for the Advancement of Science (Tenth Meeting, Held at Albany, New York, August, 1856)* (Cambridge: Joseph Lovering; New York: G. P. Putnam & Co., 1857), 222-230.

<sup>58</sup> CW to JW, 10/22/1854, *CWC*.

<sup>59</sup> MCP to JCD, 7/19/1854, *MPUSPJ*, 169.

Rodgers regarded himself as a representative of the U.S. government entrusted with the duty to inspect the extent to which the Ryukyu Kingdom had fulfilled the treaty.

**“The most perfect state of cultivation”**

“The contrast between this [Ryukyu] and the Bonin group when all is in a state of nature is very striking,” Wright exclaimed to Gray. Whereas in the Bonin Islands the vegetation was preserved in a perfect state of nature, he commented, he found that in Ryukyu “every level spot of soil as big as a blanket is in the most perfect state of cultivation so much so that a weed is hardly to be found [*sic*].” To John, he offered a more detailed account: “The soil appears good and is admirably cultivated. Every spot ten feet square among the rocks on the hill is dug up and planted with sweet potatoes or something that can be raised without irrigation which all the vallies [*sic*] and hillsides are terraced and ditched and divided off into patches of all sizes (not very large) and shapes. These are planted with rice, taro, bananas, potatoes, turnips (large and excellent) &c. many of them at this season sown with rice and covered with water.”<sup>60</sup>

Again Wright paid little attention to those cultivated plants. He first took excursions to “ravines and cliffs and rocky patches” and was glad to find some “scanty vegetation” (“which man has no motive for destroying”). Second, he paid a visit to the grounds where Ryukyu people buried their dead and found the flora there relatively satisfactory. After eagerly pulling out the weeds thriving beside tombs, he examined the tombs with interest. “They bury their dead in tombs similar

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<sup>60</sup> CW to JW, 10/22/1854, *CWC*.

to those of the Chinese choosing the rocky hills for this purpose,” he wrote home. “They manifestly must pay considerable respect to the dead though I see less signs of it than among the Chinese.”<sup>61</sup>

To Wright’s surprise, the best botanical ground that he could find in Ryukyu was in the densely populated capital town of Shuri. One day, while carrying his portfolios through Shuri, he spotted “a space enclosed and apparently in a state of nature.” Delighted, he jumped over the wall and eagerly filled portfolios with plants. (“There I had a good quiet time and found an oak, a cherry, an orange.”) Reflecting on the experience afterward, he suspected that this space belonged to some sort of royal hunting ground (though he found no deer or other quadrupeds wandering there). As he put it in a letter to Gray, “it is probably tabooed, for when I rashly jumped over the wall no one of the crowds of inquisitive men and boys who had been following me not even the official spy ventured to follow.” To John, he repeated how much he had enjoyed collecting plants in this enclosed space in Shuri, “though I could see many pairs of curious eyes peering over it at me.”<sup>62</sup>

Wright was not alone during his various excursions in Ryukyu. He had local collaborators. Another day, hauling his portfolios full of natural-history specimens through the street of Shuri, he wrote home:

As I walked slowly along the road looking for plants on each side crowds of men and children would congregate at each by—Some chattering and laughing but they would all or most of them scatter and disappear on my near approach, though on my second tour they became somewhat bolder and a few were

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<sup>61</sup> CW to JW, 10/22/1854, *CWC*.

<sup>62</sup> CW to AG, 10/14/1854, *HL*; CW to JW, 10/22/1854, *CWC*.



inquisitive enough to peep at my portfolio and into my satchel when they saw some snails and enquired if they were to be eaten.

Inspired, Wright tried to recruit these curious and inquisitive fellows to join his collecting enterprise. He had applied this strategy in Hong Kong, and it had yielded satisfactory results. He was delighted that the strategy worked in Ryukyu, as well.

I collected a good many spiders and grasshoppers and received great assistance from a crowd of men and boys who entered zealously into the business when they understood what I wanted. They took a lesson in philology enquiring the name (English) of most of our prominent or guns, eyes, nose, ears, beard, hands, fingers, &c &c.<sup>63</sup>

But when Wright offered money to these local assistants in exchange for the specimens, they shook their heads. Due to the “disposition of the Government,” Wright informed Gray afterward, Ryukyu people had been thrilled about getting involved with “any thing like traffic with [foreign] people except through a licensed comprador.”<sup>64</sup> He offered a more detailed account to John:

When I have offered them money for anything they could procure for me or catch such as insects snakes or lizards they have almost uniformly refused it. The Government discourages the use of money through jealousy of foreign influence and the people are forbidden to receive it. Hence taxes are paid in kind and the Ruler receives from one man a pig from another a goat and from a third picul of potatoes &c. Hence we can not go into the city and purchase any thing and all fresh provisions furnished the ship come through a comprador who alone is authorized to transact such business—.<sup>65</sup>

Wright’s interest in this strange set of people was kindled. He packed his letters with details about Ryukyu people’s physicality (“I think they look better than the Chinese”), hairstyles (“The hair of the gentlemen is very neatly done up and is as

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<sup>63</sup> CW to JW, 10/22/1854, *CWC*.

<sup>64</sup> CW to AG, 10/14/1854, *HL*.

<sup>65</sup> CW to JW, 10/22/1854, *CWC*.

smooth and shining as if it had been greased starched and ironed”), architectural style (“In the house I saw no furniture but the floor was covered with mats”), entertainment (“They offered us tea and pipes to smoke. I tasted the former and was convinced that I had met with much better at home”), and social manners (“Comparatively few of the people salute as they meet but when they do they bow very low... I have been occasionally saluted in the same way”). Stimpson, too, packed his journal with vivid observations.

But it seemed that even these two observant collectors hardly deployed their powers of observation as much as Commander Rodgers did. During his intercourse with Ryukyu officers and common people, Rodgers developed a hypothesis that “a striking difference” could be found “in the size of the people, according to the class.” As he put it in the journal, “[People in] the ruling class are strikingly larger than the peasants who are apparently ill fed.” Aspiring to verify this hypothesis, he asked astronomer John M. Brooke to select samples from the “ruling class” and “peasants,” and to measure their respective height. Rodgers recorded the results in the journal: “The indiscriminate measure of 14 of the ruling class by Lt. Brook gave 5 feet 233 hundredths or 5 feet 2 $\frac{3}{4}$  inches as their average height and of the peasants the average height was only 4 feet 8 $\frac{3}{4}$  inches or 4.729 feet.” From Rodgers’s perspective, these numbers spoke profoundly of not only Ryukyu people’s physical conditions but also their social and political situations. As he put it, “This difference of inches in a small people proves I think that the Loo Chooans of the laboring class are in circumstances [unfavorable] to their physical well being and since the island is much of it uncultivable, that they have not a free scope for their labor.” In fact,

remarked Rodgers, the laboring class was living under the government's "despotic" regime. In his journal, Rodgers recorded a tale from Moreton concerning how cruel the Ryukyu government could be in punishing those who dared to interact with foreigners:

This man feeling unwell, had one day complained of being sick and Mr. Moreton had given him some medicine a rough dose. This was reported to the authority who takes the man away for several days—Some time after he returned he told Mr. Moreton that he had been punished by having two bamboos with the ends tied together, opened in the middle and put over his head, then forced by repeated efforts from his neck to his heels, violently disturbing the viscera, and tearing off the skin in places as it moved—This was a sinner punishment but the man was sick for several days after its inflection—It may be presumed that the severe punishments are really severe.<sup>66</sup>

#### **"The best thing which had ever been done in Loo Choo"**

In the meantime, officers and crewmen of the expedition were met with gazes that betrayed mingled feelings of curiosity, suspicion, amusement, and horror among the locals. In Ryukyu, Rodgers noted in his journal, "I was either moved by the cries or frightened by the screams of the children." With some annoyance and amusement, he went on, "We were never invited into a dwelling house—We went into some of the houses of the lowest class and found them to the last degree in all their apartments primitive, sometimes but rarely clean, generally far otherwise." In fact, he claimed that when they entered these houses, all they could find was "a half starved pig."<sup>67</sup>

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<sup>66</sup> Rodgers, "U.S. Surveying Expedition, Vol. 1."

<sup>67</sup> Rodgers, "U.S. Surveying Expedition, Vol. 1."

A particular group of Ryukyu people that continuously piqued Rodgers's and his men's curiosity was the Ryukyu women. "The women [here] are very shy or carefully secluded," Wright told John.

When I came in sight of any they invariably ran away if they could and entered some house or by-lane. I say "if they could" for when traveling along the road with a burden they cross to the opposite side to that which I occupy and look straight forward or downward—or whenever practicable turn off by some by-path. I was going along such a path and saw a woman sitting at the root of a pine tree with her face from me I went silently along till I passed within four or five feet of her when she looked round and darted away as if she thought "Old Nick" was after her.<sup>68</sup>

Rodgers recorded one such field observation in his journal: "our officers saw a woman entirely naked working in a field—near the Road to the Sheudi—She ran when she saw them, but I presume she would not have been alarmed at natives."<sup>69</sup>

An NPEE officer named Alexander Wylly Habersham (1826–1883) would produce an illustration showing a Ryukyu woman utterly scared running away from American visitors in his *My Last Cruise* (Figure 8-4).

But there was another group of Ryukyu people who never ran away from their American visitors. On the contrary, they hid themselves in the dark or in crowds, followed their visitors' movements, and kept detailed records. As Wright put it in a letter home, "I was accompanied (or imagine I was) all through the city (the park excepted) by one or more persons apparently gentlemen who walked when I walked and stopped when I stopped hanging around me but very civil and interfering with none of my movements nor opposing my free progress in any

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<sup>68</sup> CW to JW, 10/22/1854, *CWC*.

<sup>69</sup> Rodgers, "U.S. Surveying Expedition, Vol. 1."

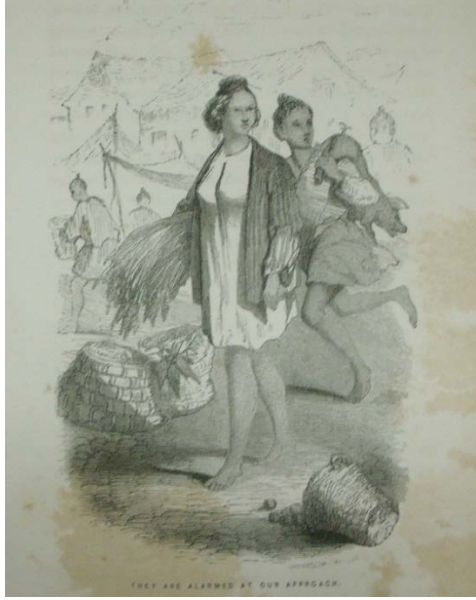


Figure 8-4. “They are alarmed at our approach.” (From Habersham, *My Last Cruise*, between p. 192 and p. 193. Courtesy of National Museum of Natural History Library, Smithsonian Libraries, Washington, DC, USA.)

direction. I suppose they were a kind [of] spies appointed to watch my conduct and report upon it (I have since been assumed my opinion was correct)—”<sup>70</sup>

For Rodgers, however, the presence (if not omnipresence) of Ryukyu spies represented how insincere Ryukyu authorities had been in their treaty with Commodore Perry. His dislike of Ryukyu’s “despotic” governing body swelled. “The government of Lew Chew had when we first arrived promised us fresh provisions for the crew every other day—They did not furnish them however; when remonstrated they said they were a very poor people and needed their cattle for agriculture, but that we should receive supplies twice every week I said very well

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<sup>70</sup> CW to JW, 10/22/1854, *CWC*.

twice every week they rejected to furnish theirs and said once a week, I said very well once a week.”<sup>71</sup>

What happened to these Ryukyu people? Didn't they understand that the treaty was a serious document? Rodgers tried to find the answer. One day, when a Ryukyu interpreter directed his men to carry provisions aboard the *Vincennes*, Rodgers called upon him. Come here, he gestured. Rodgers then opened an ornithological atlas and pointed at an illustration, asking the interpreter if this particular bird could be found in Ryukyu. Rodgers recorded the interpreter's response as follows: “He was disconcerted, then answered after a moments reflection ‘No have got that bird—’ I said nothing—but some 5 or 6 minutes afterwards he said—‘Yes have got that bird—’ ‘On yes you have sent it on board many times,’ said I.”<sup>72</sup>

On the basis of this short interview, Rodgers developed a notion concerning the “national habits” of not only the Ryukyu government but also all governing bodies of Asian countries. “This incident small as it is shows the habit of duplicity in a striking light—The first impulse and act was to answer untruly—and afterwards to reflect whether untruth was the best reply under the circumstance.” “Such is I imagine the Asiatic habit of diplomacy,” he reasoned. “[S]uch evasions of direct dealing have led to the difficulty which Europeans have with the nations of Asia.”

I believe however that this habit of untruth marks rather a peculiar phase of civilization, their natural moral obliquity. In Europe I apprehend that during the middle ages good faith in treating was more rare than at present—The Europeans then as the Asiatics now followed...

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<sup>71</sup> Rodgers, “U.S. Surveying Expedition, Vol. 1.”

<sup>72</sup> Rodgers, “U.S. Surveying Expedition, Vol. 1.”

Rodgers could not help applying this generalization to explain China's drastic decline after the Opium Wars. "I think of the Chinese Empire that it is in the position of the Roman Empire before its final extinction." He believed that the Ryukyu Kingdom would soon follow suit, and hence, "It would I think be a great blessing for the Lew Chewans were some enlightened government to take possession of the island and to hold it with a regiment." The Ryukyu Islands and the islands' ill-fed peasants ought to be ruled by some enlightened "European Nations," he noted, an entirely new regime that would "introduce civilization and industry as practiced in Europe." "[C]hanges in the government are desirable as a measure of humanity," he claimed.

It is a nice question whether a nation so simple so submissive to its ruler, so contented, and with so few wants would be benefitted by the manners wants, and way of thinking which belong to Europe—I think myself that the question should be answered in the affirmative—the fact perhaps that each of us for a [*sic*] enhancement in knowledge is proof that nature intended us to move onward.<sup>73</sup>

But how dared the Ryukyu government refuse to comply with the treaty that the regent had signed with Commodore Perry? Rodgers exclaimed in his journal. A refusal of this kind, in Rodgers's view, was to put him "out of the pale of humanity, to make war upon him, to throw him upon the right of self defense—and to justify his country who owes him protection in declaring war in order to inforce such a measure of humanity." In the era when civilized nations maintained mutual relationships on the basis of treaties, he thought, the Ryukyu Kingdom was behaving like a child. In consequence, Rodgers reasoned that if he pressed the Ryukyu

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<sup>73</sup> Rodgers, "U.S. Surveying Expedition, Vol. 1."

government to comply with the treaty by means of military force, the means *per se* should be considered “a means of education.” Was he a belligerent person? Rodgers asked himself. No, he denied it. “Virtue must maintain her rights with the sword, as well as with soft words.”<sup>74</sup>

What particularly irritated Rodgers at the moment was the Ryukyu government’s reluctance to supply the *Vincennes* with sufficient quantities of wood. On December 11, almost a month after the *Vincennes* arrived in Naha, Rodgers felt he could not wait any longer. He informed Ryukyu officers that if he did not receive wood by the next day, he would take a force and pay a “visit” to the “Regent” and the other nobles residing in Shuri. The wood did not come. Francis Asbury Roe, First Lieutenant onboard the *Vincennes*, took the lead in making Rodgers’s dissatisfaction felt. He went ashore and cursed Ryukyu officers. Roe’s outburst provoked angry consternation.<sup>75</sup> Now Rodgers felt that he had had enough. He immediately summoned a band of some fifty armed seamen and mariners, together with Dahlgren’s field pieces, and marched toward the capital.<sup>76</sup> Several hours later, Rodgers and his men had already crossed over the plains between Naha and Shuri, passed through the streets of Shuri, entered into the palace, stood before the regent of the Ryukyu Kingdom, and prepared themselves for the meeting. Rodgers was proud of how swiftly he had commanded the force. In his journal and afterward in a

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<sup>74</sup> Rodgers, “U.S. Surveying Expedition, Vol. 1.”

<sup>75</sup> Roe had developed very strong opinions toward Ryukyu people: “The people are generally very ugly and repulsive, but kind and inoffensive”; Roe, “Journal USS *Vincennes* 1855,” Folder 4, Box 1, *FARP*.

<sup>76</sup> Rodgers, “U.S. Surveying Expedition, Vol. 1.”



report to the Navy Department, he said that the Ryukyu government had deployed every possible means to deter his movement, including proposing a meeting somewhere between Naha and the palace, preparing a feast by the wayside, and so on. He ignored all of them, Rodgers said, and the team “marched steadily on.” And when they arrived in the palace, Rodgers wrote, he was glad to find that the authorities had opened the gate for his visit (“I should scarcely have known what to do had they been closed,” Rodgers admitted in the journal).<sup>77</sup>

When Rodgers came to speak, he made it clear to the Regent that violating the treaty would be considered a “cause of war” in the European context. Then he enumerated what he considered to be the Ryukyu government’s violations of the treaty.<sup>78</sup> The Regent apologized. He explained that Ryukyu people were “timid sailors” and felt sorry that they did not usher the *Vincennes* properly on the day of the vessel’s arrival. As for insufficient supplies of wood and other articles, the regent explained that the Ryukyu Kingdom was very poor, that people needed all their cattle for agriculture and all their food for themselves, and that wood was scarce and had to be brought from a distance. Rodgers’s reply, as he put it in his journal:

To this I replied that they had plenty of cattle, and plenty of provisions and plenty of wood—and that such excuses were not sufficient... I said that it appeared they did not understand how to keep their treaty with the United States and I should recommend to my government to appoint a consul for them.

Rodgers was glad to find that his words worked. The Ryukyu authorities hastily “begged” Rodgers not to do so. Rodgers softened his attitude. After enjoying the

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<sup>77</sup> Rodgers, “U.S. Surveying Expedition, Vol. 1.”

<sup>78</sup> JR to JCD, 8/21/1854, Folder 5, Box 7, *RFP*; Rodgers, “U.S. Surveying Expedition, Vol. 1.”

“impromptu and simple collation” (including cakes, tea, and sake) offered by Ryukyu authorities, he offered a rifle and a carbine in return as souvenirs. It was his sincere wish that “whenever they looked upon them they would remember the treaty,” he remarked.<sup>79</sup>

Rodgers was delighted that he had taken such a decisive step to ensure Perry’s treaty was uncompromisingly and unmistakably carried out. “It certainly probable produced a profound impression [upon the Ryukyu authorities],” he wrote proudly in the journal. In his report to Secretary Dobbin, he remarked that when he led his men back to the *Vincennes*, he found that “long lines of men were passing wood to the water edge.” Still, as if he were concerned that his superiors would accuse him of a lack of tact, Rodgers wrote defensively, “It will probably be said that my course was rude and ingenious—It was meant to be rude—it was intended to assert a right to the obeisance of the treaty.”<sup>80</sup>

Wright did not join Rodgers’s march to the palace. In fact, he regarded the whole affair as “rush and unnecessary.” As he put it in a letter to John, “I think sufficient allowance has not been made for the customs of a half-civilized people under a despotic government while our means of communicating our wishes to them are so imperfect and our meaning so easy to be misinterpreted.” To him the whole

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<sup>79</sup> Rodgers, “U.S. Surveying Expedition, Vol. 1.”

<sup>80</sup> Rodgers, “U.S. Surveying Expedition, Vol. 1”; JR to JCD, 8/21/1854, Folder 5, Box 7, *RFP*.

operation merely satisfied those who had been eager for a peek at the interior of the palace.<sup>81</sup>

On December 13, the *Vincennes* left the Ryukyu Islands for Japan. “A pilot in a canoe was ahead of the vessel with an American flag on a long bamboo pole to show us the way out of the harbor the bills had all been paid and we were underway,” Rodgers vividly portrayed the scene in his journal. Young and inexperienced as he was, Rodgers thought that he had made an extraordinary contribution to the welfare of the United States of America in particular—if not that of human civilization in general. “The English missionary Mr. Moreton [residing in Ryukyu] said that it was the best thing which had ever been done in Loo Choo,” he remarked.<sup>82</sup>

**“More beautiful scenery can scarce exist”**

On December 28, 1854, the *Vincennes* approached Kagoshima Bay, one of the most commodious harbors along the southern end of the island of Kyushu. Breathlessly, officers and crewmen stared at the magnificent views that gradually materialized before their eyes. “More beautiful scenery can scarce exist,” Stimpson commented in his journal, “and our enjoyment of it is scarce equalled by anything which has yet occurred in this cruise.”<sup>83</sup> Wright, too, was captivated by the scene, in which plants appeared to be in a perfect state of nature. “On the land is an abundance of pine trees and the steep hills are densely wooded,” he told John. “All the tillable land

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<sup>81</sup> CW to JW, 10/22/1854, *CWC*.

<sup>82</sup> Rodgers, “U.S. Surveying Expedition, Vol. 1.”

<sup>83</sup> Vasile, *et al.*, *William Stimpson’s Journal*, 71.

appears terraced to the tops of the hills and many of the areas so formed are as green as if covered by a growth of young wheat.”<sup>84</sup>

But echoing through this lovely scene at Kagoshima Bay was one of the most dramatic encounters between the United States and Japan during the first half of the nineteenth century. About two decades earlier (in 1837), when the American merchant ship *Morrison* approached the same bay, offering to repatriate seven shipwrecked Japanese in exchange for the chance to negotiate with local authorities about future commercial and diplomatic relations between the United States and Japan, the Kagoshima garrison fired on the vessel.<sup>85</sup> For Americans of the day, the attack (afterward known as the Morrison Incident) was held up as important evidence of the savagery and uncivilized status of the Japanese government. Indeed, it could even be said that the incident catalyzed Perry’s expedition to Japan. Yet, as it turned out, Perry did not open Kagoshima Bay to the United States despite his success in signing the Treaty of Kanagawa. On the contrary, according to the Treaty, Americans were only allowed to enter Shimoda and Hakodate, and any attempt to go beyond these two harbors would be considered a violation of the treaty.

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<sup>84</sup> CW to JW, 12/18/1854, *CWC*. In effect, Wright’s eagerness to throw himself into forests of Kagoshima Bay must have run so high that it prompted Rodgers to add a remark in his report to Secretary Dobbin. “Our botanist was tantalized with what seemed to him new plants, just without his reach,” he noted. JR to JDD, 2/15/1855, Cole, *Yankee Surveyors...*, 41.

<sup>85</sup> It should be noted that the Kagoshima garrison’s attack was launched on the basis of “the Shell and Repel Order” issued by the bakufu in 1825. The order was repealed in the wake of the British victory in the Opium War in 1842. Coincidentally, among those who were onboard the *Morrison* and were startled by the garrison’s attack was S. Wells Williams, an old friend of Asa Gray. Williams had been in China since the 1830s and occasionally sent Gray plants.

The impression that officials in Kagoshima were excessively “jealous” about any contact with foreigners was enhanced by two books that anyone who aspired to know more about Japan could check out from the library onboard the *Vincennes*: Vasili Mikhailovich Golovnin’s *Japan and the Japanese* (1853) and Charles McFarlane’s *Japan* (1852). “I have been reading some works on this country—how Golovnin and his fellow captives were caged and McFarlane’s compiled account of the same,” Wright wrote home.<sup>86</sup>

Rodgers was fully aware of the possible danger to his crew and his diplomatic status if he sailed the *Vincennes* into Kagoshima Bay. But he resolved to test the extent to which the recently signed Treaty of Kanagawa had changed the Japanese people’s manners in dealing with Americans. A number of concerns conspired to cause Rodgers to take this risky move. First, like many of his contemporaries, Rodgers considered Japan’s self-seclusion immoral and unethical. As he wrote in his journal, “I think no country has a right to isolate itself—as no individual holds his property for his sole benefit—but must in every civilized country contribute his assessed share of produce or if taxes to the general welfare, so no country possesses its soil and its productivity for its sole use, but must contribute its share to the community of nations.”<sup>87</sup> Second, after a preliminary examination of the topographical features of Kagoshima Bay, Rodgers had little doubt that this bay would play a critical role in advancing communications between San Francisco and Shanghai. As he put it in a letter to his mentor Matthew Fontaine Maury, “I wish

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<sup>86</sup> CW to JW, 12/18/1854, *CWC*.

<sup>87</sup> Rodgers, “U.S. Surveying Expedition, Vol. 1.”

particularly to call your attention to Kago-Sima Bay—It is one of the most beautifully marked sheets of water in the world; on the west side is Mount Horner little less than three thousand feet high.” In fact, he continued, the topographical features of Kagoshima Bay were so conspicuous that “no man awake” could have driven his ship onto the rocks “except intentionally.” Insofar as the United States’ future commerce with Japan was concerned, Rodgers told Maury, he could foresee that Kagoshima Bay would play a strategically important role: “It [Kagoshima Bay] is in a highly cultivated country, and near Tanegasima from which much of the ship timber of Japan is brought—It is near Nagasaki where a valuable coal mine has recently been discovered near the water, indeed in the hills looking into the harbor.”<sup>88</sup>

The *Vincennes* sailed into Kagoshima Bay on December 28. Immediately after their arrival, Rodgers’s men were met by a boat carrying some twenty Japanese swordsmen. Through gestures, the swordsmen ordered the *Vincennes* to get out of the bay right away, and the officers gestured back that they were in need of provisions. Rodgers, realizing that the issue at stake was too complex to be conveyed by gestures, invited the Japanese to board. The Japanese officers accepted the invitation. Rodgers, delighted, ordered his men to serve their guests glasses of wine. The Japanese officers, under the Americans’ curious gaze, took the glasses, lifted them to their foreheads, lowered them, then sipped and savored the contents.

Then the negotiations began. With a Chinese-English dictionary (compiled by an English missionary to China named Walter Henry Medhurst) in hand, Rodgers’s men

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<sup>88</sup> JR to MFM, 3/5/1855, Folder 3, Box 8, *RFP*.

ventured across the language barrier. They first pointed to the Chinese phrase that should stand for “what is your name” and waited for the swordsmen’s responses. The swordsmen read the phrase, nodded politely, not said a word, and wrote some characters in the air. Encouraged, Rodgers’s men gestured, asking if they could take a look at the swords, and if possible, exchange some swords for American goods or curiosities. The swordsmen replied with no words but shook their heads. Finally, Rodgers produced a list of provisions and handed it to the swordsmen. The swordsmen read it, nodded, and gestured that they would provide the provisions according to the list. Then they pointed at the ship, then at the shore, shook their heads, and drew a line across their throats with their fingers. The message was clear: if the Americans came ashore, their superiors would punish them (the Japanese officers) by cutting off their heads. The cruelty inherent in the message provoked some excitement among Rodgers’s officers. “[W]hether this supposition will deter our Capt from making an excursion remains to be seen,” Wright wrote home.<sup>89</sup>

Soon after the meeting, numerous boats loaded with fowl, eggs, oranges, vegetables, and water approached to the *Vincennes*. Meanwhile, officers of Kagoshima Bay sent boats to surround the *Vincennes* to discourage any landing operation by the Americans. Rodgers directed his men to load the provisions onto the *Vincennes*, but did not care about whether or not the officials had prepared them according to the list (After all, about two weeks before reaching Kagoshima Bay,

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<sup>89</sup> CW to JW, 12/18/1854, *CWC*.

Rodgers had received sufficient provisions from the Ryukyu government). His truly intention was to land and undertake scientific observations.

Rodgers made his first landing at Kagoshima Bay on December 29. He sent an officer onshore to take observations, and Wright boldly joined in. They found the Japanese whom they encountered not unfriendly (compared to what they had learned from books). "The natives that day offered us no molestation and I wandered about on a steep ridge of pine trees and bushes without any hindrance," he told John.<sup>90</sup>

Encouraged, Rodgers led another boat to land the next day. This time Kagoshima officials responded with unmistakable disagreement. They made a sketch of a ship and blew it away. They gestured that since they had offered provisions according to Rodgers's list, the Americans should get out of their bay.<sup>91</sup> As Rodgers put it in a report to the Navy Department,

In the morning we rowed on shore to take observations. Some twenty men collected around, each with two swords, and motioned to us to go away. We went quietly on with our work. They came nearer, and we showed our revolvers to them as curiosities, explaining how they turned and how they took apart. They were most struck with the Maynard primer, as resembling their own matches. We [They] seemed unconscious of our display of force for we showed our arms with smile.<sup>92</sup>

Wright joined this landing operation, as well. In his letter to John, he described the rising hostility of the Kagoshima officials: "The next day I landed again with the Capt. and collected a few plants but now the officials had become pertinacious in

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<sup>90</sup> CW to JW, 12/18/1854, *CWC*.

<sup>91</sup> Rodgers, "U.S. Surveying Expedition, Vol. 2," Folder 2, Box 8, *RFP*.

<sup>92</sup> JR to JDD, 2/15/1855, Cole, *Yankee Surveyors...*, 39-40.



their efforts to induce us to return. They hang about us and obstruct our way making frequent gestures that we should go away." Nevertheless, he went on, "the people to the number of several hundred assembled and observed us at some little distance and I dare say gratified to see strangers and indifferent as to the success of their superiors in their efforts to be rid of us."<sup>93</sup>

Rodgers had no intention of leaving Kagoshima Bay at that moment. He asked the officers for an interpreter and was disappointed to find that he would need an interpreter to interpret this message. So he ordered his Chinese cabin boy "Kang" to try to communicate. Kang failed. In his journal, Rodgers complained that Kang "held an unsatisfactory talk with them, by means of imaginary Chinese characters traced upon the palms of their hands."<sup>94</sup>

Rodgers eventually became tired of dealing with these "petty officers" of Kagoshima Bay. It occurred to him that if he wanted to make any progress in surveying the southern end of the Japanese Archipelago, he had to communicate directly with the central government of Japan. So he composed a letter to "the Honourable Secretary of State for Foreign Affairs Kingdom of Japan" and asked Kagoshima officers to deliver the letter. Rodgers outlined the origin of the NPEE as follows:

The Government of the United States sent five vessels, of which this is the chief, to examine the dangers of the Ocean. We have been round more than half the Globe. We have at last arrived at one of the Japanese ports. If the Islands of Japan with the rocks and shoals which surround them, were out of the paths which our vessels follow across the Ocean, the world could say nothing, but as these

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<sup>93</sup> CW to JW, 12/18/1854, *CWC*; also see Vasile, *et al.*, *William Stimpson's Journal*, 72.

<sup>94</sup> Rodgers, "U.S. Surveying Expedition, Vol. 2."

dangers remain in the road of ships, we must examine them, and tell our countrymen where they lie. Otherwise our vessels would be wrecked, and many valuable lives might be lost.

Rodgers concluded the letter by claiming that the *Vincennes's* visit to Kagoshima Bay would not its last in Japan: "I say this because in a few months my own vessel, or one or more of those under my command, will, in the prosecution of our duties, probably stop at some port in Japan, and I earnestly hope no too ardent officer will by his overzeal bring himself and me into trouble."<sup>95</sup>

The letter did not produce any effect, however. As time went by, both parties grew impatient. On one occasion, when astronomer John M. Brooke landed with a group of sailors to undertake scientific observations, a Japanese swordsman called upon him. Brooke, occupied by his work, did not pay attention to the call, which annoyed the swordsman exceedingly. The swordsman then jumped before Brooke, and conflict seemed inevitable. But just as Brooke's companions were about to intervene, they were stopped short by what happened next. As Rodgers described it in a letter to Matthew Fontaine Maury, the swordsman "bared the plumpest part of his body to Mr. Brooke's astonished gaze. He slapped it emphatically—and before Mr. Brooke knew what it all meant, he was out of sight." "[I]nteresting is it not?" he asked.<sup>96</sup>

On another occasion, when Brooke was directing a landing operation, a sailor ("a remarkably athletic negro and as black as night," Rodgers commented in his journal)

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<sup>95</sup> JR to "Honourable Secretary of State for Foreign Affairs Kingdom of Japan," 1/4/1855; also Cole, *Yankee Surveyors...*, 49-49.

<sup>96</sup> JR to MFM, 3/5/1855, Folder 3, Box 8, *RFP*.

struck a Japanese swordsman's head and knocked him down into the water. The boat then retreated to the *Vincennes*, and Rodgers responded by leading a team of sailors and officers, fully armed, to resume the landing. Rodgers decided to show something extraordinary to the Kagoshima officers. He ordered his men to practice shooting onshore.<sup>97</sup> The Kagoshima officers winced, but did not retreat. They gestured that if the Americans dared to continue, they would have their *own* throats cut or bowels ripped open. These very peculiar threats made some members of the expedition anxious. As Wright wrote to John, "Yesterday Mr. Stimpson and myself went out to dredge and when we approached the shore the guard boats came off to us with the well-known order to return. We went farther out in the Bay where we worked unmolested. I intended to go ashore but thought it best not to insist upon it so much against their wishes."<sup>98</sup>

The fact was that Wright had gradually grown unsatisfied with Commander Rodgers's "intrusion" into this particular Japanese bay.

Some attempt to justify our intrusion on them under the plea of necessity—as the want of wood and water and other supplies the importance of taking observation for verifying our positions & correcting the errors of our instruments for rating chronometers &c. &c.—but under the treaty I question whether we have any right to be here. Had we made no treaty with the Emperor and acting according to our democratic notions we might justify any intrusion on these territories by taking it for granted that we were not contravening the wishes of the people—But by making a treaty with the Emperor by which we are allowed to trade [in] certain ports and to enter others in case of distress we have virtually acknowledged his supremacy and our pertinacity in landing here can hardly be looked upon in any other light than as a violation of the treaty.<sup>99</sup>

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<sup>97</sup> Rodgers, "U.S. Surveying Expedition, Vol. 2."

<sup>98</sup> CW to JW, 12/18/1854, *CWC*.

<sup>99</sup> CW to JW, 12/18/1854, *CWC*.

“It appears pretty plain that we shall have no privileges here unless we obtain them by force,” Wright informed John. “It is but justice to say however that their treatment of us has been very considerate and that they have used only persuasion to induce us to leave merely crowding around us and beckoning us away.”<sup>100</sup>

Despite his disagreement with Rodgers’s intrusion, Wright was disappointed at not being able to fully explore the flora of Kagoshima Bay. Due to the Kagoshima officers’ unthreatening but still effective hindrance of his movements, Wright complained to John, “My botanical collection made here is small now amounting to more than seventy-five species probably.”<sup>101</sup>

The *Vincennes* left Kagoshima Bay on January 6, 1855, and proceeded to explore the southern end of the Japanese Archipelago. Rodgers, somewhat disappointed by his unsuccessful negotiations in Kagoshima, became increasingly aggressive in his commanding style. On January 9, when the *Vincennes* anchored in Tanega Sima, he ordered his men to land with arms. The Japanese onshore quickly assembled and hindered the Americans’ movements. Undeterred, Rodgers ordered his men to push through the crowd to move forward. The Japanese, acknowledging their inability to hold off these resolved American surveyors, matched the Americans’ steps. The tension between the two parties underwent a dramatic shift as Rodgers ordered his men to ascend to the highest point of the island. Under a harsh tropical sun, Rodgers’s men quickly grew tired and asked if their Japanese followers could help

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<sup>100</sup> CW to JW, 12/18/1854, *CWC*.

<sup>101</sup> CW to JW, 12/18/1854, *CWC*.

them carry the instruments. The Japanese nodded. Afterward, when this U.S.-Japan surveying team was about to reach the highest point, the Japanese even showed the Americans the best spots from which they could take observations. Rodgers was delighted. When the task was completed, he presented some embroidered silk pocket handkerchiefs to those who appeared to be the leaders of the crowd. They accepted.<sup>102</sup>

Rodgers applied this same method when the *Vincennes* dropped anchor close to Kikai Island on January 19. Once again, he ordered his men to arm themselves and follow him to land. Upon setting foot onshore, Rodgers held a conference with local officers. He gestured that his duty obliged him to bring his men and ascend to the highest point on the island to make observations. The motive behind this request was fully scientific, he declared. Then he was disappointed to find that the Kikai officials fell into a long discussion. Anticipating that the discussion would not reach any conclusion, Rodgers left the conference, stood before his men, and ordered them to proceed. Then he looked back and saw a man on horseback with spears leading about a hundred people, aiming to separate him from his men. He was annoyed. Threateningly, he walked up to the horseman, shaking his fingers in the horseman's face and pulling out the revolver. He lifted the revolver and directed the horseman's attention to the barrels. It worked. The horseman "looked at first calmly at the weapon, and then with a slight shudder slowly turned his head away."<sup>103</sup>

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<sup>102</sup> Rodgers, "U.S. Surveying Expedition, Vol. 2."

<sup>103</sup> Rodgers, "U.S. Surveying Expedition."

A more dramatic encounter took place in Amami Ōshima on January 22, 1855. As Rodgers reported to Secretary Dobbin:

The village of Sima-u [Uke-shima] is composed of the most miserable population which I have ever seen. They were full of jealousy, however, lest we should see more of their land than was proper. With a companion I walked a few yards from the boat. A man ran before us and with a bow put his hands opposite to our breasts. I motioned, and said in as stern a tone as I could, "Go away." He repeated his bow, and advanced his hands closer to our breasts. I several times told him to go away; finding words of no avail, I presented a pistol from under my cloak. I do not think he quite comprehended me, for the instrument was nearly covered with cloth. Finding he did not understand I showed the weapon more clearly. He ran away, and an older man came. To avoid further importunity, I made a circle, and shot into a larger tree, and then put my finger upon the hole. They gave us no further trouble.<sup>104</sup>

Wright landed right after Rodgers's display of power took place. He was struck by the scene. Nearly every Japanese onshore carried a stick of some kind ("many were mere pieces of cane and yet a good many were rather formidable clubs," he told John). He guessed that these sticks were some sort of weapon and was glad that the Japanese did not actually attack. In fact, his collecting that day went well and was fruitful.

We started on track of the Capt. up the hill. The officials followed endeavoring to induce us to return but I did not intend to be cheated out of a botanical ramble unless by force. We could ascend the hills faster than the Mandarins on their ponies and seeing that we (myself and another) were resolved to go ahead which intention we manifested by turning away from the track of those who had preceded us when the greater portion went after these bearing only a few of the common people to accompany us. They examined curiously our movements imitated our language inquired the names of things in our tongue and gave the same in theirs. They manifested a desire to please by collecting and offering me flowers when they saw what I was anxious to procure—also shells when I was looking for those.<sup>105</sup>

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<sup>104</sup> JR to JDD, 2/15/1855, Cole, *Yankee Surveyors...*, 44-45.

<sup>105</sup> CW to JW, 12/18/1854, *CWC*.

“The 22nd [of January] was occupied in successful botanizing on Ousima [Amami Ōshima],” Wright remarked in his journal.<sup>106</sup> Afterward, when Gray came to examine Wright’s Ōshima collections, he would name a new species *Vaccinium wrightii*, a new species that might not have been discovered without Rodgers’s display of arms and the local Japanese’s assistance in botanizing.

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On January 24, 1855, the *Vincennes* left Amami Ōshima for Hong Kong and arrived there six days later (on January 29). The first half of the expedition under Rodgers’s command was thus completed. According to Rodgers’s plan, the *Vincennes* would remain in Hong Kong until late February (Figure 8-5). During this period, he would supervise the scientific corps, which would produce charts on the basis of navigational data thus far collected, make catalogues of natural history specimens to be consigned to the Smithsonian, and contact men of diplomacy who had experience of interacting with the Japanese government. He would outfit the squadron (comprising the *Vincennes*, the *John Hancock*, and the *Fenimore Cooper*) and set sail for Shimoda and Hakodate. In this second northward voyage, Rodgers expected to

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<sup>106</sup> Charles Wright, “Journal of the North Pacific Exploring Expedition, March 18, 1854-Oct. 13, 1855,” *CWP*.



Figure 8-5. The route of the *Vincennes's* two cruises to Japan and its neighboring areas (1854-1855). (The latitude and longitude coordinates of the route are based on Vasile *et al.*, *William Stimpson's Journal.*)

open negotiations with officers in these two harbor towns. He hoped that what he had encountered during his negotiations in Kagoshima would not occur again.

On February 22, 1855, a Chinese businessman in Hong Kong invited Commander Rodgers and other NPEE officers to a special dinner (chiefly because the day before was the Chinese New Year). Wright was invited, as well. In his letter to John, he wrote joyfully about his first encounter with what would be later known as Chinese dim sum.



We attended a dinner yesterday prepared in pure Chinese style and got up for our special benefit. The table was profusely strewn with flowers besides several small vases of bouquets. A variety of stationary dishes were on the table throughout the dinner such as oranges pears pickled eggs and ginger birds nests sharks-fin, "biche de mer" preserved oranges, crystallized sugar (I ate nearly all of it) a kind of almond and watermelon seeds. That embraces the greater part. Then came the courses the courses. And first was a tiny cup of saw-shoo or Chinese rum containing about two thimblefuls—mine was not refilled. A succession of hashes, steels, &c. followed, till we had tasted of thirty, or more dishes all served in small shallow bowls. No knives or forks were on the table & I for one took my first lesson in the use of the chopsticks. You may guess we formed a somewhat laughable spectacle I could commonly secure a thing after several trials & great unsightly seaslug six inches or more in length and proportionally large corresponding to the common terrestrial slug or snail without a shell. It is an important article of traffic with the Chinese and though common on their coasts is not cured there but is brought from the tropical islands where the manufacture or curing of it is carried on to a large extent principally for the Chinese market I tasted of nearly every thing & if I did not eat dog, cat, rat & satisfied I care not to partake of another such feast. Scarcely one of the dishes could be called palatable and none excellent. A band of music formed part of the entertainment. I can't describe it but by comparison. Take two cornstalk fiddles, a willow whistle with three or four finger holes a tin pan & stick along oxhorn and a club and set them all going and you will have a pretty correct idea of our music. It was a sort of mix tone of saw-filing and tin-trumpet. The whole however passed off quite pleasantly and with much good humor.<sup>107</sup>

Wright was right about the good humor that was now prevailing in the upper echelons of the expedition. Rodgers, although deeply grieving the loss of the *Porpoise*, was delighted that he had successfully gotten the expedition back on track. As he told Matthew Fontaine Maury, "We are getting on very well—indeed confidence seems restored, and the temper is excellent of all the officers—and of the men too—There has not a man deserted since we have been here." "[O]ur success has been signal," he added.<sup>108</sup>

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<sup>107</sup> CW to JW, 2/18/1855, *CWC*, under the entry of February 23.

<sup>108</sup> JR to MFM, 3/5/1855, Folder 3, Box 8, *RFP*.

Looking forward, Rodgers thought he had discovered the key an effective surveying project on the shores of Japan. As he put it in his journal, he found that “the display of arms were a passport—They [the Japanese] were unwilling to have us on shore, but not so unwilling as to risk a difficulty with people who were doing no harm and had formidable means of resisting attack.”<sup>109</sup> “I have had to show arms whenever we have been, as a passport to observation,” he reiterated to his mentor Maury.<sup>110</sup> However, to Secretary Dobbin, who had explicitly expressed his desire that the expedition avoid any unnecessary involvement in diplomatic matters, not to mention the deployment of arms in diplomatic matters, Rodgers offered a more elaborate explanation: “We are not war-vessels, and can not display much force, but I have come to the conclusion that the [United States] government would not be unwilling I should risk a collision with the Japanese in endeavouring to carrying out our right. The trade is desirable, but the survey is a necessity. Under these circumstances it appears to me a ‘perfect right’.”<sup>111</sup>

Still, Rodgers was not naïve. He did not think that he could carry on his survey of the Japanese shoreline simply by displaying arms. In January 1855, he sent his letter written in Kagoshima Bay to Peter Parker, secretary and interpreter of the U.S. legation to China. He wondered if Parker could help him translate the letter into Chinese and Dutch (the two major languages Japanese officers used to communicate with foreigners). Although he seriously doubted that such a position as “Secretary of

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<sup>109</sup> Rodgers, “U.S. Surveying Expedition, volume 2,” Folder 2, Box 8, *RFP*.

<sup>110</sup> JR to MFM, 3/15/1855, Folder 3, Box 8, *RFP*.

<sup>111</sup> JR to JDD, 2/15/1855, in Cole, *Yankee Surveyors...*, 45.

State for Foreign Affairs” existed in Japan’s governmental system, he told Parker that he was confident that his letter would be delivered to “the proper hands”—so long as he avoided dealing with petty officers and submitted the letter through superior officers like the governors of Shimoda or Hakodate. He also called Parker’s attention to the title which he signed under his name. “I wrote the good old English word friend instead of ‘very humble servant,’ which I am not,” he wrote. “They [the Japanese] have always kicked their European humble servants.”<sup>112</sup>

About a month later, Rodgers produced a long letter to the “Honourable Secretary of State of the Empire of Japan,” making clear the necessity for him to survey the coast of Japan and gather information about Japan’s natural history. Understanding that the Japanese government might use the Treaty of Kanagawa to turn down his request, Rodgers elaborated upon his mission’s necessity from multiple angles. First, he called the attention of the “Secretary of State of the Empire of Japan” to the tenth article of the Treaty of Kanagawa: American vessels had a right to enter Japanese ports in case of absolute distress. Then, he reasoned, if this particular article symbolized amity and friendship between two nations, he wondered how the Japanese government could not permit him to conduct essential surveys and observations. “To tell a friend that he might enter the nearest harbor in case his vessel were sinking or on fire or dismasted or the crew starving, but to hide from him the position of the harbor, and the way to enter it, would seem more like mockery than good faith.” In effect, he went on, it had been a shared understanding among civilized nations that unveiling those geographical secrets of the world by

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<sup>112</sup> JR to PP, 1/11/1855, F142-143, R6, *REENP*.

surveying sciences and diplomatic means was part of the “natural right,” “in common with every private individual.” “Humanity and policy here go hand in hand,” he added. Finally, Rodgers claimed that his arguments were so complete that he dared not “anticipate the consequences of refusal.” And were he refused, he warned, “Certainly the President of the United States can not regard such refusal as a proof of good will on the part of the government of Japan.”<sup>113</sup>

“I am not quite sure about what success I shall have in Japan,” Rodgers told Maury of Washington, DC. Regardless, he claimed that “I have come to the conclusion to survey even though they should shoot at us—Of course I shall be gentle, and quiet, and temperate—I shall avoid difficulty by every concession except that of swerving from my purpose.”<sup>114</sup>

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<sup>113</sup> JR to “Honourable Secretary of State of the Empire of Japan,” 2/7/1855, Cole, *Yankee Surveyors...*, 51-52. But Rodgers was still concerned about whether this assumed “Honourable Secretary of State of the Empire of Japan” would have enough power to grant permission for his expedition. So he dispatched a letter to Samuel Wells Williams, a missionary residing in Canton who had served as an interpreter on Perry’s expedition to Japan. In his reply, Williams said that Commodore Perry, although not being able to survey any significant portion of Japan’s coastline, had out of necessity permitted American surveyors to pursue similar projects known to the Japanese government. As a result, wrote Williams, he was optimistic about the eventual success of Rodgers’s negotiations with the Japanese government. “Their principal fear of permitting your surveys arises, I think, from the hazard of some collision; and there may have been outrageous cases of this in bygone years from reckless whalers. If this fear is resolved, they would not, I think, absolutely oppose the survey, tho’ they would keep you out of the villages & towns & confine you to the beach as much as possible.” SWW to JR, 2/26/1855, Folder 3, Box 8, *RFP*.

<sup>114</sup> JR to MFM, 3/5/1855, Folder 3, Box 8, *RFP*.

## CHAPTER NINE

### “A Nut for Agassiz to Crack”

Cambridge, Massachusetts, May 1856.

Professor Asa Gray was now busy composing his “essay on species.” For the past year or so, he had been thinking on the subject. “I should like to write an essay on *species*, some day,” he told Darwin in June 1855. “But before I should have time to do it, in my plodding way, I hope you, or Hooker, will do it, and much better far... and I think we may, or rather *you* may, in a few years settle the question as to whether Agassiz’s—or Hooker’s views are correct: they are certainly widely different.”<sup>1</sup> He thought that now he was ready to place himself between Hooker and Agassiz, and address the questions respecting “the geographical distribution, the mutual relations, the nature, and the origin of the existing species of plants.” Questions of this sort, as he admitted afterward, were “so difficult that they are not likely to be conclusively answered in our day,” but he had no doubt that certain elucidations could be drawn, if naturalists engaged in the “critical comparison of the flora of any one considerable region with the vegetation of other parts of the world.”<sup>2</sup>

To be sure, Gray’s enthusiasm in confronting such thorny issues as the origin and distribution of species bespoke a gear shift in his research. After all, throughout most of the 1840s, he had refrained from touching upon any topic of the sort.

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<sup>1</sup> AG to CRD, 6/30/1855, *CCD*, Vol. 5, 363.

<sup>2</sup> Asa Gray, “Statistics of the Flora of the Northern United States [Part I],” *The American Journal of Sciences and Arts (2nd Series)* 22, no. 65 (1856), 204.

Entering into the 1850s, however, the origin and distribution of species rapidly became a heated issue among American naturalists. A statement made by the Boston Society of Natural History in late 1853 captured the spirit that might have tempted Gray to compose his “essay on species”:

Observers in every department of Natural Science are multiplying at the present day with unparalleled rapidity. They may be found distributed throughout the length and breadth of North American Continent. Researchers into the Geological and Mineralogical resources of newly explored tracts go hand in hand with the pioneer, who for the first time opens a path through the wilderness. Collectors in all the departments of Natural History are not slow in following, and naturalists whose pursuits limit their labors to home investigations, are constantly supplied with new materials to work upon. New views of the relations, origin and nature of species, new contrasts between the old and recent ones, new observations of structure of development, new opinions of the bearing of one science upon another, new theories in all departments show that the well trodden field may be explored over and over again without exhausting its wealth.<sup>3</sup>

Unsurprisingly, as “Observers in every department of Natural Science” began considering the “North American Continent” as a whole, how to characterize the North American fauna and flora emerged as a critical issue. Late in 1853, Samuel Batchelder, a Boston-based author and natural-history enthusiast, asked Gray why “nothing resembling the varied hues of an American forest, is ever witnessed in Europe.” Batchelder said that he just came across a notice in a newspaper stating that “there is a mistake often attributing the variegated appearance of an autumnal forest to frosts.” The notice read:

It is the ripening of the leaves, that give the crimson, golden and other hues, as the blush of the peach, the crimson of the plum, the golden appearance of the apple, and not the result of frost, but of the ripening of the fruit. The “frosty”

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<sup>3</sup> Charles Frederic Girard, “Testimonies in reference to the Bibliography of American Natural History for 1851,” Folder 8, Box 1, *CFGP*.

fingers are indeed to those delicate classes of vegetation “fingers of death.” At their touch all this beauty, alike in fruit and leaf, and flowers, disappears.

Gray was delighted by Batchelder’s inquiry. He replied, “It is quite true that the autumnal tints of leaves are not owing to the action of frost; nor are they owing to the coldness of the atmosphere producing a sluggish circulation of saps.” “It is a chemical change,” he emphasized, “that occurs in the juices or other contents of the cells of the parts in which the color changes.”<sup>4</sup> Definitely there was a physiological law that distinguished American forests from their European counterparts, he noted.

But even the so-called North American flora embraced its own internal diversity. As physician-botanist John Strong Newberry pointed out in his report for the Pacific Railroad Surveys, “The botanist going from the valley of the Mississippi to the Pacific coast, will be immediately struck by certain general differences which he will perceive to exist between the vegetation of the region he has left and that to which he has come. The first feature in the aspect of the botany of the west, which he will be likely to notice, is the paucity of arborescent and the variety of annual plants.” Sure enough, he noted, such “paucity” and “variety” resulted from a multitude of physical factors (geological, topographical, climatic, and the like), but he suspected that the cause “which has controlled the radiation of species from their original centers of creation” might have played a role.

The operation of this latter cause, though perhaps not less real, is far more obscure, requiring for its analysis an array of facts much greater than has yet been collected. This has, therefore, been entirely neglected, except in the few instances where plants are common to both sides of the continent, and an effort

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<sup>4</sup> AG to SB, 12/20/1853, *AGP*.

has been made to connect their eastern and western habitats. When the botany of the west shall come to be known far better than at present, we may expect that the physiological laws which have controlled the distribution of plants may be studied with equal profit with the more material influences of which I have spoken.<sup>5</sup>

To be sure, the flora of western North America differed from its eastern counterpart not only because of its “paucity of arborescent and... variety of annual plants” but also because it embraced such magnificent creatures as the Californian redwoods, or the “Mammoth-trees.” Soon after California became part of the country’s territory in 1853, American society was captivated by what appeared to be the most long-lived residents of the country (Figure 9-1).<sup>6</sup> J. M. Bigelow,

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<sup>5</sup> John S. Newberry, “Botanical Report Routes in California and Oregon: Explored by Lieut. R. S. Williamson, Corps of Topographical Engineers, and Lieut. Henry Abbot, Corps of Topographical Engineers, in 1855,” in *Reports of Explorations and Surveys, to Ascertain the Most Practicable and Economic Route for a Railroad from the Mississippi River to the Pacific Ocean, Vol. VI* (Washington: A.O.P. Nicholson, 1857), 9-10. Before joining the Pacific Railroad Surveys as a physician-naturalist, Newberry spent enormous time examining those fossilized plant fragments discovered in coalmines across Ohio. In a letter to Baird in 1853, he sent his preliminary diagnosis of Ohio’s fossil flora. “You will see that it contains a large number of new species—and yet I have carefully endeavored to avoid the creation of fictitious species—giving to my predecessors in this branch of science the full benefit of doubtful cases—Indeed I am confident that when our fossil plants are more carefully studied, when we shall compare our specimens with European specimens, that as in other departments of natural history, many cases of presumed identity will be found to be errors.” Still, he maintained that the criteria by which to judge whether or not two specimens belonged to the same species always gave rise to “a very nice question—as to what are the limits of variations in a species—” JSN to SFB, 2/1/1853, Folder 12, Box 3, *CM*.

<sup>6</sup> Berthold Seemann, “On the Mammoth-tree of Upper California,” *The Annals and Magazine of Natural History (Third Series)* 3, no. 15 (1859), 161-175. In 1853, six eminent residents of San Francisco (Henry Gibbons, Albert Kellogg, T. J. Nevins, Andrew Randall, Lewis W. Sloat, and John B. Trasks) founded the California Academy of Natural Sciences, officially bringing this new territory into the networks of learned societies that had for the past half a century endeavored to discover and document the county’s diverse natural landscape. One of the Academy’s first scientific missions was to send specimens and seeds of the Californian redwoods to



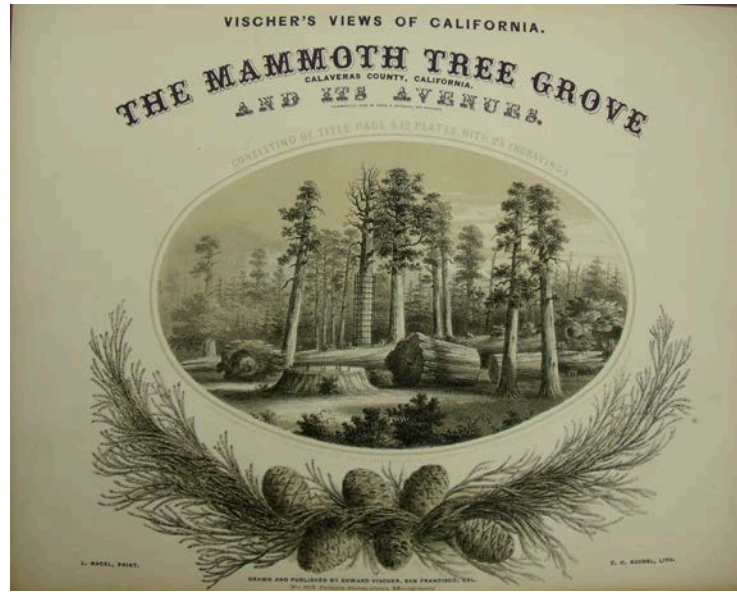


Figure 9-1. The cover page of Edward Vischer, *Vischer's Views of California: The Mammoth Tree Grove, Calaveras County, California, and Its Avenues* (San Francisco: Edward Vischer, 1862). (Courtesy of the Huntington Library, San Marino, CA, USA.)

physician- naturalist on the Pacific Railroad Surveys, made the following comment after a visit to a redwood forest: “Their extremely limited locality and number forcibly impressed me with the belief that the species is soon to become extinct, as is further evinced by its slow reproduction. Indeed these giants of the forest are so

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Torrey and Gray. The Academy’s chair, botanist Andrew Randall, suspected that an entirely new species and probably a new genus could be differentiated from the commonly known species *Sequoia sempervirens*. For want of botanical books and specimens, he decided to rely on Torrey and Gray’s judgment. And if his suspicion proved true, Randall wished his distinguished colleagues on the East Coast would give the new species a name that honored both the tree and the nation that bred the tree (something like *Washingtonia Californica*). Regarding the role of the California Academy of Natural Sciences in the history of botany, see Barbara Ertter, “People, Plants, and Politics: The Development of Institution-Based Botany in California,” in *Cultures and Institutions of Natural History: Essays in the History and Philosophy of Science*, edited by Michael T. Ghiselin and Alan E. Leviton (San Francisco: California Academy of Sciences, 2000), 203-248. Regarding the general history of the study of natural history in California, an essential reference is Michael L. Smith’s *Pacific Visions: California Scientists and the Environment, 1850-1915* (New Haven: Yale University Press, 1987); regarding Gray’s study of the California redwoods, see Asa Gray, “On the Age of the Large Tree Recently Felled in California,” *The American Journal of Science and Arts (2nd Series)* 17, no. 51 (1854), 440-443.

marked in their rusty habit from their present associates, that we can hardly view them in their present relations, except as links connecting us with ages so long past, that they seem but reminiscences of an eternal bygone.”<sup>7</sup> Like Newberry, Bigelow was amazed by how such factors as climate, soil, temperature, and “centers of creation” might have shaped geographical distribution of species. He went so far as to produce what he called the “Botanical Profile, Representing the Forest Trees” for the report of the Pacific Railroad Surveys (Figure 9-2).

It was against this agitated intellectual atmosphere that Gray’s “essay on species” appeared in *The American Journal of Science and Arts* under the title “Statistics of the Flora of the Northern United States” (1856-1857; in three parts).<sup>8</sup> In September 1856, when the first part of “Statistics” was in print, Gray promptly posted a copy to Darwin. “This is only a beginning,” he said proudly. “[T]he range of species in latitude must next be tabulated,—disjoined species catalogued... then some of the curious questions you have suggested.—the degree of consanguinity between the related species of our country & other countries, &c and the comparative range of species in large & small genera, &c &c &c.”<sup>9</sup> Gray was aware of his intellectual debt to Darwin. In the opening of “Statistics,” he acknowledged, “While engaged in

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<sup>7</sup> John M. Bigelow, “Description of Forest Trees,” in *Reports of Explorations and Surveys, to Ascertain the Most Practicable and Economic Route for a Railroad from the Mississippi River to the Pacific Ocean, Vol. VI* (Washington: Beverley Tucker, 1856), 23.

<sup>8</sup> Gray, “Statistics... [Part I]”; Gray, “Statistics of the Flora of the Northern United States [Part II & III],” *The American Journal of Science and Arts (2nd Series)* 23, no. 67 (1857), 62-84 and no. 69 (1857), 369-403.

<sup>9</sup> AG to CRD, 9/23/1856, *CCD, Vol. 6*, 229.

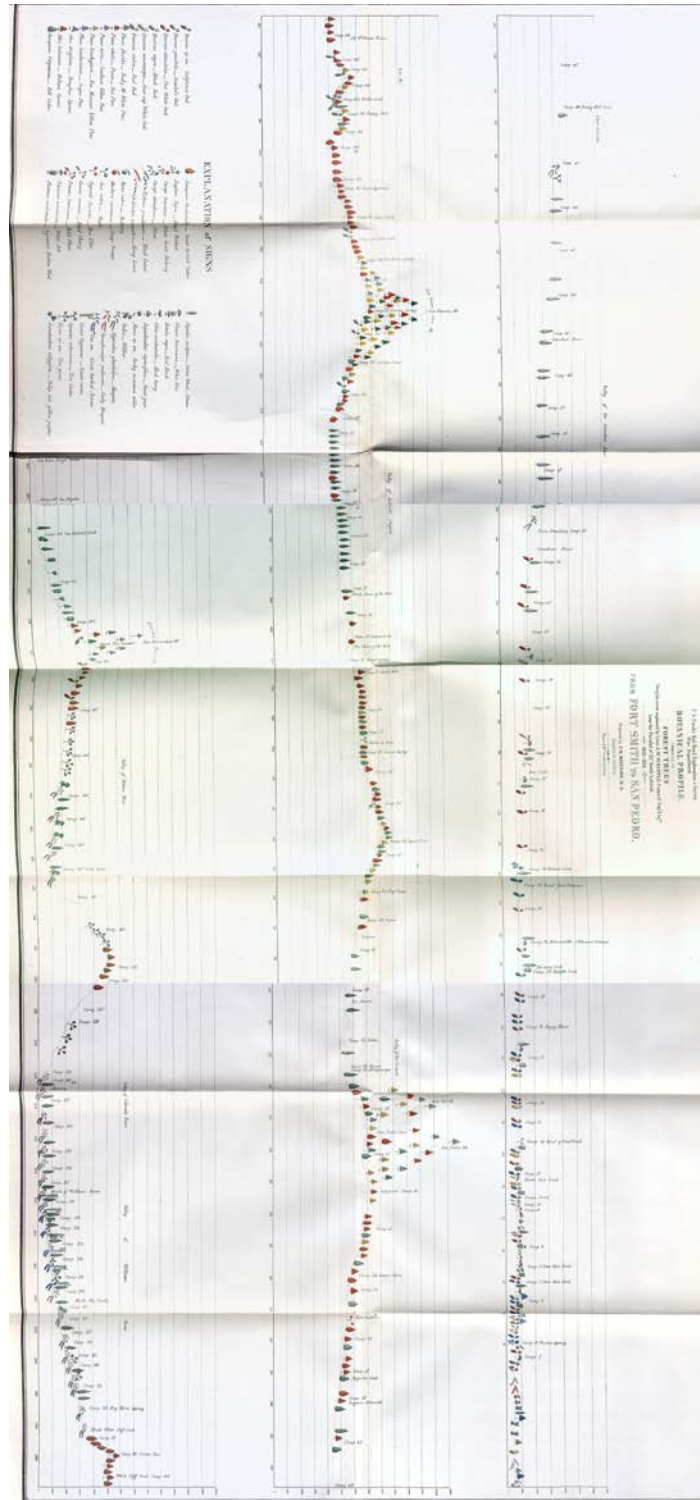


Figure 9-2. Bigelow’s “Botanical Profile” (From Bigelow, “Description of Forest Trees.” Courtesy of the Dibner Library of the History of Science and Technology, Smithsonian Libraries, Washington, DC, USA.)

the preparation of a second edition of the *Manual of the Botany of the Northern United States*, I was requested by an esteemed correspondent, upon whose judgment I place great reliance, to exhibit, in a compendious and convenient form, the elements of flora I was occupied with.”<sup>10</sup>

Darwin might have felt pride in his powers of persuasion when Gray’s “Statistics” arrived in his hands. “If my letter caused you to do this” he told Gray, “I shall congratulate myself in private, at having done good Botanical work.”<sup>11</sup> Indeed, given the vast array of facts that Gray had unearthed regarding the origin and distribution of species, Darwin deserved to congratulate himself. After integrating the flora of Europe into his comparisons, Gray found that the cases he had amassed over the past decades—namely, those similar or analogical Japanese and American plants—began connecting with each other, as it were, and a more sophisticated biogeographical pattern surfaced. In Gray’s own terms, these patterns suggested “(1) that a large percentage of our extra-European types [i.e., genera or families] are shared with eastern Asia; and (2) that no small part of these are unknown in western North America.”<sup>12</sup> Compared to Gray’s conclusion in the 1840s regarding analogies between the flora of Japan and that of the United States, Gray’s present thinking centered on whether or not the floristic relationship between the two sides of the Pacific indicated a pattern of disjunct distribution. He was now ready to confront the notorious question of disjoined species.

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<sup>10</sup> Gray, “Statistics... [Part I],” 204.

<sup>11</sup> CRD to AG, 7/21/1855, *CCD*, Vol. 5, 384-385.

<sup>12</sup> Gray, “Diagnostic...,” 442.

### **“Botanical arithmetic”**

A careful reader of “Statistics” will be startled by the flood of tables and lists that Gray wove into his arguments, a style he rarely adopted prior to the mid-1850s but would consciously maintain for the rest of his life. For example, in a section exhibiting “the principal elements of our flora, and some of its relations to the European, &c.,” Gray presented a “List of the Natural Orders of the Flora of the Northern United States,” “Extra-European Orders not Peculiar to America,” “List of the Principal Phaenogamous Natural Orders Represented in the Flora of [the] Northern United States,” and so forth.<sup>13</sup> Gray himself was proud of his ability to turn his “catalogue of species” into tables. As he put it in “Statistics,” he had accepted Darwin’s request to compare the North American flora with its counterparts in East Asia and Europe, for “I may be presumed to possess considerable facilities for collecting and correcting a portion of the required data.”<sup>14</sup> In the eyes of Gray’s colleagues in the United States, however, the Harvard professor’s “tabular view” seemed incomprehensible. “I have a good laugh every time when I see your tabular view,” Curtis once confessed to Gray.<sup>15</sup>

Laughable as it might have been, the technique that Gray deployed in “Statistics,” known as “botanical arithmetic,” had been popular among European botanists since Humboldt invented it and applied it in his classic *Essai sur la géographie des plantes*

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<sup>13</sup> Gray, “Statistics... [Part I],” 207-232.

<sup>14</sup> Gray, “Statistics... [Part I],” 204.

<sup>15</sup> MAC to AG, 10/3/1860, *HL*.

(1815). Humboldt's method was straightforward enough. To estimate the floristic relationship between two regions, he counted how many species were held in common, and calculated the ratio between these common species and the total number of species distributed in the two regions, respectively. Soon afterward, as Janet Browne puts it, Humboldt's method was given a methodological and theoretical turn, particularly in the hands of Robert Brown and Augustin Pyramus de Candolle. Both Brown and de Candolle suggested that botanists calculate the average number of species in a genus. In their opinion, the calculation should help researchers discern the "relative spread of species in any area," in turn hinting at the "distribution of creative power" in nature.<sup>16</sup> Decades later, as the debates regarding whether species had originated in single or in multiple birthplaces became prevalent, botanical arithmetic was hailed as a standard operation for the single-creation camp, as eloquently demonstrated in Joseph Hooker's *Flora Novae-Zelandiae* (1851-1853) and Alphonse de Candolle's *Géographie botanique raisonnée* (1855).

An American botanist who always kept a close eye on the progress of European botany, Gray could not have ignored the significance of botanical arithmetic. As early as the 1830s, he had tabulated those leading families to gain a quick and accurate diagnosis of the North American flora. But what concerned him in the

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<sup>16</sup> The quotations are all from Janet E. Browne, "Darwin's Botanical Arithmetic and the Principle of Divergence, 1854-1858," *Journal of the History of Biology* 13, no. 1 (1980), 55-56. On Humboldt's role in plant geography, see, among others, Malcolm Nicolson, "Alexander von Humboldt, Humboldtian Science and the Origins of the Study of Vegetation," *History of Science* 25 (1987), 167-194; Nicolson, "Humboldtian Plant Geography after Humboldt: The Link to Ecology." *The British Journal for the History of Science* 29, no. 3 (1996), 289-310.

1850s was rather the extent to which botanical arithmetic could provide proof for the single creation of species. Perhaps this is why Curtis thought Gray's tabular view laughable. As he declared to Gray in June 1859,

I have read only through your article on the "Botany of Japan," & find it very interesting. But I tell you, it is a knotty subject that you try to elucidate. It seems to me, that the antecedent probability is against the theory. Cosmopolite plants being all derived from a single individual. If derived from two or some individuals, then it does not matter, so far as I can see, whether those plants' stocks were seated within five yards of each other or 10,000 miles apart;—whether seated at the same moment, or at intervals of a century. In other words, I cannot see that time & place are essential to specific characters.<sup>17</sup>

What troubled Curtis was Gray's aggressive lumping of those similar Japanese and American species. When Gray did this, he assumed that species originated from a common stock or, as Hooker suggested, the dispersal of a single pair of individuals across as wide an area as the two sides of the Pacific, a proposition that Curtis found difficult to stomach. "It is plain enough," Gray announced in "Statistics," "that the numbers in this tabular view must be essentially influenced throughout by one's views as to the limitation of species and genera." For his part, he declared, "I can only say, on my own part, that an enlarged experience certainly inclines one to take broader views of species than those which prevail among the generality of European botanists."<sup>18</sup> Indeed, when Darwin asked whether Gray found any discrepancy between Gray's diagnosis of a species and other lumpers' diagnoses, he replied proudly, "our [American] species—thanks mostly to Dr. Torrey & myself—have been more thoroughly *castigated*. What stands as one species in the Manual would

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<sup>17</sup> MAC to AG, 6/3/1859, *HL*.

<sup>18</sup> Gray, "Statistics... [Part I]," 211-212.

figure in almost any European Flora as 2, 3, or more in a very considerable number of cases.”<sup>19</sup>

Adopting this broadened view of species, Gray knew that the matter at stake was the question de Candolle had left unsolved: the origin of disjoined species.

Unsatisfied with de Candolle’s compromise solution of multiple creations of species in this particular case, Gray presented a list comprising 69 American phaenogamous species that could be found either in Europe or in East Asia. “DeCandolle [*sic*] lays much stress upon the isolated occurrence of a single peculiarly United States species, *Phryma leptostachya*, in Nepal,” he commented. However, he went on, the cases of disjoined species were so numerous that these cases could not support de Candolle’s “exceptional mode of explanation.” More importantly, Gray told his readers that in comparison to de Candolle’s *Phryma leptostachya*, which might hint at some connections between North America and central Asia, there were far more “North American species which meet Himalayan ones in Japan,” thus suggesting a “line of connection” between Japan and North America.” The outstanding question was how to explain this connection.<sup>20</sup>

This was the point where Gray evaluated the applicability of Hooker’s and Agassiz’s respective theories on the origin and distribution of species, a task he previously thought either Darwin or Hooker might better perform:

The supporters of the first view regard each species as having spread from a single and local birth-place, or even, as the more thorough-going (like Dr. Hooker) maintain, from a single individual or pair. The opposing view finds its

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<sup>19</sup> AG to CRD, 6/30/1855, *CCD*, Vol. 5, 362.

<sup>20</sup> Gray, “Statistics... [Part II & III],” 388.



hadiest and most consistent advocate in Agassiz, who contends, if I rightly apprehend him, that each species probably originated in as many individuals, and covering from the first as large an area as it subsequently possessed.

Hooker's theory, he went on, was "based upon the natural ideas of species as consisting of kindred individuals descended from a common stock, which, whether demonstrable or not as a fact, gives us a clear and distinct conception of species, and the only one we possess." By contrast, Gray argued that Agassiz's theory led naturalists nowhere, because it left "species no objective basis," and seemed to "make even the ground of their limitation a matter of individual opinion."<sup>21</sup> Given the soundness of Hooker's theory (which was Gray's own theory, as well), Gray felt optimistic that one day the question of disjoined species would be answered—as long as naturalists allied themselves with the camp advocating the "single-creation-of-species" position.

### **Floristic relationships**

Coincidentally, when Gray was occupied by the writing of "Statistics," Darwin himself came to acknowledge the potential of botanical arithmetic to advance his species theory. After an eight-year study of barnacles, he became increasingly aware that he might have made a mistake in his theory on how species arise in nature. As far back as the 1840s, when Darwin first touched upon the issue of speciation, he believed that "very little variation was seen in a wild state." As a result, he hypothesized that geographical and geological changes must have served as the decisive factors that "unsettled" the reproductive systems of organisms, thereby

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<sup>21</sup> Gray, "Statistics... [Part II & III]," 389.

producing varieties of species. These varieties, he reasoned, must have served as the raw materials upon which natural selection could operate, so that new species could emerge through the eons of time. In the mid-1850s, however, after witnessing seemingly boundless varieties in the world of barnacles, he was no longer convinced that geological and geographical changes were crucial in speciation. He had to reconsider the correlation between varieties and species.<sup>22</sup>

The floristic data, documented and castigated by generations of botanists, furnished Darwin with rich information on the correlations among species, varieties, and genera. The years between 1855 and 1857 saw Darwin enthusiastically surveying floras from John Henslow's *Catalogue of British Plants* (second edition, 1835) to Hooker's *Flora Novae Zeladiae* (1853-1855) to Hooker and Thompson's *Flora Indica* (1855). Darwin used these references to test his recently formulated hypothesis: varieties were "little species" and appeared more frequently in "genera which had a large number of species," or "large genera." If this hypothesis proved true, Darwin believed, he could confidently argue that "where [there were] many species so there were correspondingly high frequencies of variation, and therefore the potential for further speciation."<sup>23</sup> It was against this backdrop that Darwin picked up Gray's *A Manual* to read and then asked Gray whether he had thought of publishing a list regarding those "[American] phanerogamic species common to Europe." He said that with such a list, a "non-botanist" like him might

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<sup>22</sup> Browne, "Darwin's botanical arithmetic...", 58-62.

<sup>23</sup> Browne, "Darwin's botanical arithmetic...", 62-63.

“judge a little on the relationship of the two floras.”<sup>24</sup>

“[I]f you will kindly give me hints as to what is needed, and how to do it,” Gray replied, “I will undertake the comparison.”<sup>25</sup> Darwin was delighted. In subsequent letters, he shared his findings with Gray regarding the floras compiled by European botanists and shared, as well, his expectations regarding what might be found among the North American flora. He suggested that Gray calculate “the proportions (leaving out introduced plants) to the whole of the great leading families,” “the proportion of the species to the genera,” and “on an average how many species each genus contains.” Regarding the floristic relationship between North America and Europe, he suggested that Gray divide the known American species into three categories: “(a) species common to the old world, stating numbers common to Europe & Asia (b) indigenous species, but belonging to genera found in the old world, & (c) species belonging to genera confined to America or the New World.”<sup>26</sup> In May 1856, when Gray said that he was ready to write an essay on species, Darwin offered a more sophisticated guideline. “I would give [a] list, of temperate plants (if any) found in Eastern Asia, China & Japan, & not elsewhere,” he noted. “America might be related to Eastern Asia (always excluding Arctic forms) by a genus having the *same* species confined to these two regions, or it might be related by the genus having different species, [and yet] the genus itself not being found elsewhere.” “The relation of the genera,” he assured Gray, “seems to me a most important element in

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<sup>24</sup> CBD to AG, 4/25/1855, *CCD*, Vol. 5, 322-323.

<sup>25</sup> AG to CRD, 5/22/1855, *CCD*, Vol. 5, 335.

<sup>26</sup> CRD to AG, 6/8/1855, *CCD*, Vol. 5, 347-348.

geographical distribution often ignored.”<sup>27</sup>

Gray faithfully followed Darwin’s suggestions in preparing “Statistics.” He immediately identified a new biogeographical pattern that connected East Asia and North America:

...of our 19 extra-European orders [families] not peculiarly American, only 3 or 4 are represented on the western or Pacific side of the United States, while all but one are represented in the corresponding parts of Eastern Asia;—indicating a curious analogy in the vegetation of the eastern sides of the two great continental masses in the northern hemisphere, which is also borne out, though not so strikingly, in a comparison of the genera.<sup>28</sup>

But what Gray found was not confined to those curious analogies. For the first time, he became aware that there might be two types of disjunct distribution. This awareness struck Gray as he tried to answer Darwin’s first question, pivoting on the floristic relationship between North America and Europe. Darwin’s series of instructions had made Gray aware that floristic relationships should be evaluated through multiple scales—for there would never be a universal measuring stick. In this light, if the relationships were evaluated by counting how many species were held in common, Gray found that the floristic relationship between North America and Europe would not be as close as commonly assumed. Gray noticed that de Candolle had cited the work of Hewett C. Watson, a renowned British botanist, and stated that there were “602 species out of 1428 phaenogamous British plants as common to Great Britain and America.” Gray disagreed with this enumeration, however. “I count only 321 out of 2091 Phaenogamous species,” he argued, which

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<sup>27</sup> CRD to AG, 5/2/1856, *CCD*, Vol. 6, 92.

<sup>28</sup> Gray, “Statistics... [Part I],” 215.

he believed to be “indigenous to the Northern United States as indigenous also to Europe.”<sup>29</sup>

The rather distant relationship between the flora of North America and that of Europe was also evident if naturalists judged the matter from the angle of the genus. At first glance, he argued, the floristic relationship between the two sides of the Atlantic appeared to be closer than if one drew one’s conclusions by counting species (326 genera of the 681 known genera in the North American flora could also be found in Europe). Yet Gray argued that the close generic relationship between North American and European floras was but an illusion. The fact was that if botanists subtracted those genera “wide-spread over the cooler parts of nearly the whole northern hemisphere,” or “cosmopolite genera,” Gray pointed out that they would find “an extraordinarily large proportion of the genera common to our flora and to Europe belong also to the cosmopolite genera.”<sup>30</sup>

Gray then argued that the rather distant relationship between the North American flora and its European counterpart would be more provocative if naturalists brought the East Asian flora into the comparison. He then identified a striking contrast: Whereas the species common to North America and Europe for the most part belonged to cosmopolitan genera, those American species to be found in East Asia fell majorly under the genera that were characteristic of and chiefly peculiar to the North American flora.<sup>31</sup>

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<sup>29</sup> Gray, “Statistics... [Part I],” 212.

<sup>30</sup> Gray, “Statistics... [Part II & III],” 78-79.

<sup>31</sup> Gray, “Statistics... [Part II & III],” 79.

Darwin was impressed (“The whole paper strikes me as quite exhausting the subject,” he told Gray), in particular the floristic relationship between East Asia and North America as compared to that of North America and Europe. “Here again Japan & Asia,” he wrote on the margin of his copy of “Statistics,” and asked Gray whether “climate explains this greater affinity” between East Asia and eastern North America.<sup>32</sup> Gray confirmed Darwin’s surmise and added a footnote in the second part of “Statistics”: “the remarkable number of eastern North American genera which are represented in an antipodal region of analogous extreme climate, but not in the less distant regions of Western Europe and Western North America, the greater part of which are endowed with a more equable climate.”<sup>33</sup>

Interestingly, as Gray devoted himself to writing the third part of “Statistics,” in which he paid particular attention to disjunct distribution, he became unsure as to whether climate was the categorical factor that had shaped the distribution of congeneric species. He was aware that he had ignored a crucial dimension in plant distribution: the migration and interchanges of species or even genera. This matter had become highly relevant for Gray as an American. In September 1856, Gray wrote to Darwin, “Engelmann and I have been noting the species truly indigenous here which... are increasing in the number of individuals instead of diminishing, as the country becomes more settled & forest removed.—The list of our wild plants which have become true weeds is larger than I had supposed, and these have

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<sup>32</sup> CRD to AG, 10/12/1856, *CCD*, Vol. 6, 245. Darwin’s copy of Gray’s “Statistics” is in *DAR* 135: 3.

<sup>33</sup> Gray, “Statistics... [Part II & III],” 62.

probably all of them increased their geographical range.”<sup>34</sup> In December of the same year, he told Yale geologist James D. Dana, “I cannot say that I believe in centres of radiation for groups of species [i.e., genera]. From Darwin’s questions to me I think I perceive some of the grounds on which he would maintain it.” Still, he admitted, “But as to a center of radiation for each separate species, I must say I have a bias that way.”<sup>35</sup>

### **Migration of species**

As early as the 1840s, Darwin had regarded the migration of species as a crucial dimension in biogeography. The chief trigger that drew Darwin’s attention to this connection was that identical alpine plants might be found in widely separated locations. Unable to accept any of the multiple creation hypotheses, Darwin relied on geological changes and plants’ mobility to explain this curious biogeographical pattern. He speculated that prior to the glacial period, the present alpine plants had belonged to a circumpolar flora. The subsequent glacial period induced a grand migration of plants. As the glaciers advanced, those polar plants were forced to migrate southward and spread all over Europe. Then, as the climate became warmer, and as the glaciers retreated to the north, those polar plants climbed up to the mountains and remained there, and a discontinuous distribution took shape. For Darwin, this explanation was more plausible than assuming that the Creator might have put species here and there. Plants’ capability to migrate, in conjunction with

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<sup>34</sup> AG to CRD, 9/23/1856, *CCD*, Vol. 6, 229.

<sup>35</sup> AG to JDD, 12/13/1856, *LAG*, 425.

the earth's geological changes, had been sufficient for a biogeographical explanation. There was no need for recourse to God's divine will in even the most curious pattern in nature.<sup>36</sup>

But Darwin then kept this viewpoint private for decades. Except to Hooker and Lyell, he rarely mentioned his theory on the distribution of alpine plants in print or in correspondence. He later regretted his delay. Edward Forbes, professor of botany at King's College London and palaeontologist to the Geological Survey of Great Britain, came up with nearly the same explanation and won the hearty support of renowned botanists of the day.<sup>37</sup> In August 1858, when Gray asked about the relationship between geological changes and plant distribution, he reiterated his view. "This is E. Forbes' theory," he added, "which however I may add I had written out 4 years before he published."<sup>38</sup>

The mid-1850s then witnessed Darwin's rekindled enthusiasm for the migration of plants and biogeography. He observed berry-pecking birds, tested whether fish enjoyed the onion seeds he fed them, immersed vegetable seeds in seawater, and engaged in an intensive debate with Hooker, who insisted that Darwin's means of dispersal—whether birds, fish, or ocean currents—were too accidental to explain why there had been biogeographical *patterns* instead of only irregularities. Darwin

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<sup>36</sup> Browne, *The Secular Ark*; James Moore, "Revolution of the Space Invaders: Darwin and Wallace on the Geography of Life," in *Geography and Revolution*, edited by David. N. Livingstone and Charles W. J. Withers (Chicago: University of Chicago Press, 2005), 106–132.

<sup>37</sup> Browne, *The Secular Ark*; Endersby, *Imperial Nature*, 238-243.

<sup>38</sup> CRD to AG, 08/11/1858, *CCD*, Vol. 7, 149.



often regretted that he could not recruit Hooker to stand with him in this particular case. He was perplexed when Hooker, following Forbes's later theories, suggested that there might have been some sort of landmass connecting different floras to one another, by which species could migrate and interchange systematically. "It shocks my philosophy to create land, without some other & independent evidence," he exclaimed to Hooker.<sup>3940</sup>

When Darwin began corresponding with Gray, his early thoughts on the distribution of alpine plants had blossomed into a systematic account of the floristic relationship between North America and Europe. His intensive studies in botanical arithmetic revealed that there must have been a number of species common to the two sides of the Atlantic. He speculated that those common species must have been distributed in a circumpolar area before the glacial period, migrated southward by any possible means, and settled down in North America and Europe, respectively. Thus, the migration of species and floristic relationships naturally surfaced as two interrelated themes in Darwin's correspondence with Gray. In April 1855, for instance, when suggesting that Gray evaluate the floristic relationships between North America and Europe, Darwin enclosed in the letter a list of alpine species based on his studies of Gray's *A Manual*. He wondered whether Gray could mark these species' habitats, for example, "mountains of the U.S," Arctic America, Arctic Europe, and so on, so that he (Darwin) could better calculate how many American

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<sup>39</sup> Browne, *The Secular Ark*; Browne, *Voyaging*; Endersby, *Imperial Nature*, 243-244.

<sup>40</sup> CRD to JDH, 6/5/1855, *CCD*, Vol. 5, 344.

alpine plants were held in common with Europe.<sup>41</sup> In June of the same year, when discussing the floristic relationship between North America and other temperate areas, Darwin pushed Gray to survey the “habitats of those plants found West of [the] Rocky mountains; & likewise those found in Eastern Asia.” “The ranges of the plants, to the East & West,” in Darwin’s opinion, should show “whether the migration has been Eastward or Westward.”<sup>42</sup> In mid-1856, when Darwin was deeply engaged correlating the sizes of genera and geographical distributions, he asked Gray about the routes by which plants might migrate from the arctic area to the American South. Darwin said he had read Gray’s early essay on his botanical excursion to North Carolina, and wondered if the Allegheny Mountains were “sufficiently continuous so that the plants could travel from the north in the course of ages thus far south.”<sup>43</sup>

It is against this backdrop that Gray learned from Darwin about Darwin’s essay on “the power of seeds to withstand seawater,” published in *The Gardeners’ Chronicle* in May 1855. Titled “Does Sea-Water Kill Seeds,” Darwin’s essay showed that even seeds of radishes, beets, lettuce, carrots, and onions could germinate after nearly three months’ immersion in seawater. Gray was intrigued. “Why has nobody thought of trying the experiment before!” he replied. “I shall have it nearly all reprinted in Silliman’s Journal, as a nut for Agassiz to crack.”<sup>44</sup> No doubt, if Agassiz’s

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<sup>41</sup> CRD to AG, 4/25/1855, *CCD*, Vol. 5, 322.

<sup>42</sup> CRD to AG, 6/8/1855, *CCD*, Vol. 5, 347.

<sup>43</sup> CRD to AG, 7/14/1856, *CCD*, Vol. 6, 182.

<sup>44</sup> AG to CRD, 6/30/1855, *CCD*, Vol. 5, 363.

theory was based on an assumption that each species should have retained a given number of individuals and be confined to certain geographical boundaries since it was created—a scheme that hardly allowed species to migrate—Darwin’s essay on plants’ capability to endure harsh environments constituted a potent contrast and antithesis.

When Darwin’s essay appeared in *The American Journal of Science and Arts* in September 1855, Gray added several words to situate Darwin’s arguments in his own rising disagreement with Agassiz’s species theory. He claimed that Darwin’s essay addressed “the problem whether the same organic being was created at one point, or at several or many widely separated points.” After Darwin’s articulation of the “great struggle for life,” he commented, “It would be well to submit to this experiment the seeds of a considerable number of those very species [which were capable of withstanding seawater] which naturally occur in two or more widely separated areas. And also to take the seeds of indigenous individuals, rather than of those of long-cultivated plants, which last certainly possess augmented vegetative power, though perhaps they may not offer increased resistance to the action of salt water.”<sup>45</sup> These remarks on Darwin’s experiments would be integrated into Gray’s 1859 essay on Japan’s flora. Gray would not only throw Agassiz “a nut” but also turn “some of Agassiz’s own guns” against him. If “the creation of beings” was endowed with “enormous multiplying power, and such means and facilities for dissemination,” “why then should we suppose the Creator to do that supernaturally

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<sup>45</sup> Asa Gray, “Does Sea-water Kill Seeds,” *The American Journal of Science and Arts (2nd Series)* 20, no. 59 (1855), 282-284.

which would be naturally effected by the very instrumentalities which he has set in operation?”<sup>46</sup>

In the second part of “Statistics,” Gray included a more sophisticated discussion of the migration of species between Europe and North America in a section titled “The Northward Range in This Country of the Phaenogamous Species Which Are Common to Us and to Europe.” “This is an interesting point of inquiry,” he claimed, because the northern range of those American species common to Europe should have offered some clues to the “mooted question of the single or multiple origin of the species.”<sup>47</sup> He soon arrived at the conclusion that “eleven, or one-third of our strictly alpine species common to Europe,—all but one of them arctic in the Old World,—are not known to cross the arctic circle on this continent.” Thus, Gray found it unavoidable to reason that “the interchange of alpine species between us and Europe must have taken place in the direction of Newfoundland, Labrador and Greenland, rather than through the polar regions.”<sup>48</sup>

Before Gray sent this part of “Statistics” to the press, Darwin knew Gray’s conclusion about the northern range of American species and the route by which American and European species might have mingled. He admitted that Gray’s conclusion “troubled me to a degree which would have made you laugh.” Helplessly, he tried to match Gray’s data with his assumption that the interchange of American and European species should occur in a circumpolar area. “But looking at *the Globe*

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<sup>46</sup> AG to JT, 1/7/1859, *LAG*, 450; Gray, “Diagnostic...,” 445.

<sup>47</sup> Gray, “Statistics... [Part II & III],” 63-64.

<sup>48</sup> Gray, “Statistics... [Part II & III],” 73.

is it not rather a forced expression to exclude Labrador from your 'Arctic continental regions?'" he asked Gray.<sup>49</sup> Afterward, he again made his disappointment felt to Gray: "One of your conclusions makes me groan, viz that the line of connection of the strictly Alpine plants is through Greenland: I shd. *extremely* like to see your reasons published in detail, for it 'riles' me."<sup>50</sup>

"Well," Gray replied, "I never meant to draw any conclusions at all, and am very sorry, that the only one I was beguiled into should 'rile' you." He claimed that his conclusion was mostly consistent with "what you geologists seem to have settled, viz. that the northern regions must have been a deal colder than they are now,—the northern limit of vegetation therefore much lower than now,—about the epoch when it would seem probable that the existing species of our plants were created." If, he reasoned, it was assumed that "during the glacial period, there could have been no phaenogamous plants on our continent anywhere near the polar regions," and "if there was any interchange of species between N. America & N. Europe in *those times*," "was not the communication more likely to be in lower latitudes than over the pole?" Still, he knew that his geological knowledge was incomparable to Darwin's: "If, however, you say... that the existing species got their present diffusion before the glacial epoch, I should have no answer. I suppose you must needs assume very great antiquity for species of plants in order to account for their present dispersion, so long as we cling—as one cannot but do, to the idea of the single birth

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<sup>49</sup> CRD to AG, 11/24/1856, *CCD*, Vol. 6, 285.

<sup>50</sup> CRD to AG, 1/1/1857, *CCD*, Vol. 6, 314-315.

place of species.”<sup>51</sup>

Gray’s above reflections on geology constitute a significant part of the third part of “Statistics.” After enumerating his 69 disjoined species, he sorted them into three groups:

1. Those which re-appear in high southern or Antarctic latitudes.
2. Those which re-appear in Japan, the Himalaya, or some part of Northern Asia, but are not European...
3. Species of Western Europe, and (chiefly) Eastern North America, not reaching here to high latitudes, and mostly of limited range on one or the other side of the Atlantic.

He then attempted to come up with a geological explanation for each group. He found his attempts failed, except for group 3, which he argued might be explained by “a former terrestrial connection between North America and Europe.” Gray then considered whether these 69 disjoined species were equipped with certain devices that could help them transgress geographical boundaries. He found that the hypothesis did not work, for nearly none of the 69 disjoined species “enjoy peculiar facilities for, or are endowed with any appliances favorable to adventitious transport.”<sup>52</sup>

Regardless, Gray insisted, naturalists should “look in one and the same direction for the explanation of these extraordinary no less than of the more ordinary cases of distribution.” He noticed that de Candolle himself had moved ahead with this line of explanation (but that he had made a drastic turn in the case of disjoined species): namely, “plants must have been created at different epochs, and that the greater

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<sup>51</sup> AG to CRD, 2/16/1857, *CCD*, Vol. 6, 340.

<sup>52</sup> Gray, “Statistics... [Part II & III],” 386-388.

part of the existing species are older than the present configuration of our continents,—should refer such anomalous distribution to very ancient dispersion.”<sup>53</sup>

Gray apparently had accepted Darwin’s opinion that a wave of dispersion might have occurred in ancient times. This “ancient dispersion,” in conjunction with geological changes, should have given rise to the disjunct distribution of the present day. Notably, Gray’s agreement with Darwin was followed by a statement that “plants must have been created at different epochs,” an opinion with which neither Darwin nor Hooker would have agreed. The fact was that by the time Gray finished “Statistics,” he had significantly revised his previous views on species and geographical distribution. And it turned out that Gray’s revised views were so different from Darwin’s that Darwin felt prompted to fully disclose his thoughts on evolution and the divergence of species late in 1857.

**“The creator established a definite number of species at the beginning”**

This quotation is from Gray’s new edition of the textbook *First Lessons in Botany and Vegetable Physiology* (1857), his major publication right *after* “Statistics.” In light of the extent to which Gray relied on Darwin’s judgment, as well as Gray’s disagreement with Agassiz’s idealist definition of species, it is startling that Gray could come up with such an un-Darwinian and somewhat Agassizian definition of species. *First Lessons*, together with its sister publication, *Introduction to Structural and Systematic Botany* (1858; the 5th and revised edition of Gray’s *Botanical Text-*

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<sup>53</sup> Gray, “Statistics... [Part II & III],” 388.

*Book series*), offers a window onto Gray's rising enthusiasm for synthesizing theology with the origin of species, geographical distribution, and even the philosophical foundation of systematic botany.<sup>54</sup>

What distinguished the two volumes from their predecessors was Gray's unambiguous application of theological terms to botanical studies. For example, he claimed that a thoughtful classification of species should reveal the "general scheme or plan of the Creator,"<sup>55</sup> an unprecedented statement. He then offered step-by-step instructions on how to use his new edition of *A Manual* to reveal the Creator's plan. "We naturally assume that the Creator established a definite, although a vast, number of types or sorts of plants, and animals, and endowed them with the faculty of propagation each after its kind." Along this line of thinking, he continued, "Species is... the type or original of each sort of plant, or animal, thus represented in time by a perennial succession of like individuals; and that all the descendants of the same stock, and of no other, compose one species." In the face of the increasingly heated question of how species originated, he noted, "We are led to conclude that the Creator established a definite number of species at the beginning, which have continued by propagation, each after its kind."<sup>56</sup>

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<sup>54</sup> Asa Gray, *First Lessons in Botany and Vegetable Physiology* (New York: Ivison & Phinney and G. P. Putnam & Co., 1857); Gray, *Introduction to Structural and Systematic Botany, and Vegetable Physiology (5th and Revised Edition)* (New York: Ivison and Phinney, 1858).

<sup>55</sup> Gray, *Introduction...*, 351.

<sup>56</sup> Gray, *First Lessons...*, 173.



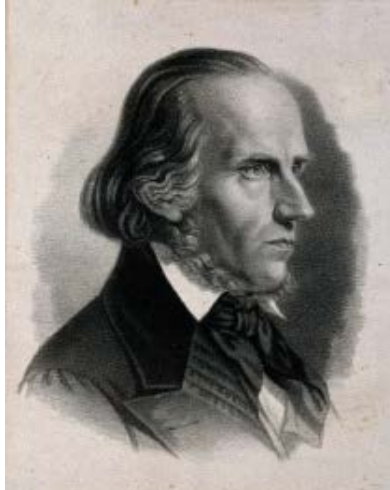


Figure 9-3. Alexander Carl Heinrich Braun in 1850 (The Wellcome Library, London, UK.)

Gray's divergence from Darwin in defining species and genera hints at how Gray's mind had been working to accommodate the mass of information he had unearthed and presented in "Statistics." Indeed, if we read closely Gray's botanical writings between 1856 and 1858, it is evident that besides Darwin's and Hooker's works in taxonomy and biogeography, Gray began entertaining such pertinent ideas as individuality, vegetable power, vitality, metamorphosis, and even "evolution," a set of ideas Gray mostly learned from an essay published in *Abhandlungen der königlichen Akademie der Wissenschaften zu Berlin*. Entitled "*Das Individuum der Pflanze in seinem Verhältniss zur Species*," this essay laid out the recent thoughts on species of Alexander Carl Heinrich Braun (1805-1877), a German botanist whom Gray deeply respected (Figure 9-3).<sup>57</sup> In 1853, when Braun's essay came out in Berlin, Gray admired it as much as he had Hooker's *Flora Novae-Zelandiae* and Darwin's essay on the power of seeds. He had his student Francis Stone translate

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<sup>57</sup> AG to GE, 5/6/1851, *LAG*, 383; Sargent, *Scientific Papers of Asa Gray, Vol. II*, 403-405.

Braun's essay into English and published the text in *The American Journal of Science and the Arts*.<sup>58</sup> He quoted Braun's arguments with admiration in *First Lessons* (1857) and *Introduction* (1858), as well.

What ultimately inspired Gray was Braun's attempt to diminish the theoretical gap between individuals and species. "In the whole realm of organic nature," Braun claimed, "we see each species adding generation to generation, by multiplying the individuals in time and space, until its day has ended, whether from internal or external causes." "In this particular, the species resembles the individual itself; having its allotted age, though measured by days of a higher order, and its pointed circle of life,—in which the individuals appear as members occupying a certain time and place,—resembling the successive relative forms through which the individual passes."<sup>59</sup> "In a word," Braun concluded, "the relation of the individual to the species is that of an inferior cycle of development to a superior: the individual is a member of the species" or, put differently, "a perfect representative of the character of the species, possessing all the functions necessary to the continuance of the species."<sup>60</sup>

Braun's argument furnished Gray with the perspective that species might be thought of as individuals. A similar understanding took hold of Darwin in the 1840s, when he wrote in one of his transmutation notebooks, "It is a generation of species

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<sup>58</sup> Alexander Carl Heinrich Braun's "The Vegetable Individual in Its Relation to Species" (*Das Individuum der Pflanze in seinem Verhältniss zur Species, Generationsfolge, Generationswechsel und Generationstheilung der Pflanze*) was published in three parts in *The American Journal of Science and Arts (2nd Series)* 19, no. 57 (1855), 297–317; 20, no. 59 (1855), 181–201; 21, no. 61 (1856), 58–79.

<sup>59</sup> Braun, "The Vegetable Individual..." 297-298.

<sup>60</sup> Braun, "The Vegetable Individual..." 299, 314.

like generation of individuals.”<sup>61</sup> Equipped with this refreshed mindset, when Gray read Darwin’s essay about how seeds could endure a harsh environment, drift across the sea, and thrive in spots far away from their birthplaces, what surfaced in his mind was how individuals strove to reach their “superior order as members of species.” In Braun’s terms,

Even the relations of geographical distribution, which are discovered by a comparison of all the stocks, depend upon the physiological character of the plants: plants of sensitive and inflexible constitutions are found only within narrow limits; Even the relations of geographical distribution, which are discovered by a comparison, and by degrees spread over almost all parts of the earth, if their seeds possess the necessary properties.<sup>62</sup>

The species was no longer a construct grounded in characteristics shared by a group of individuals; nor were species “thoughts of God,” as proposed by Agassiz. Instead, species were so real, so capable, and so full of “vegetable power” that they should be appreciated and regarded as “beings,” comprising all individuals and “forming one whole, according to one idea.”<sup>63</sup>

Inspired by Braun’s essay, Yale geologist James D. Dana published a short essay titled “Thoughts on Species,” originally a talk before the American Association for the Advancement of Science in August 1857. The best-known geologist in the United States, the chief editor of *The American Journal of Science and the Arts*, and Gray’s close ally against Agassiz’s increasing dominance in American natural history, Dana closely grasped the debates among different camps concerning the origin and

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<sup>61</sup> Browne, *Voyaging*, Chapter 16; Browne, *Darwin’s Origin of Species*, 40-41.

<sup>62</sup> Braun, “The Vegetable Individual...,” 315-316.

<sup>63</sup> Braun, “The Vegetable Individual...,” 312.

distribution of species. As a geologist, he regarded “Thoughts on Species” as his engagement in these thorny topics.

Dana arranged his essay under three themes posed as questions: “1. What is a species? 2. Are species permanent? 3. What is the basis of variations in species?” His foremost strategy was to establish an analogy between the inorganic and the organic worlds. From a geologist’s viewpoint, he claimed, he did not conceive of any essential difference between the mineral species (i.e., oxygen) and the biological species. These two kinds of “species,” Dana opined, similarly “correspond to a specific amount or condition of concentered force, defined in the act or law of creation.”<sup>64</sup>

But Dana was conscious that his readers might be unable to appreciate this analogy as well as he did. Hence he made an interpretative turn, in hopes that his fellow naturalists could comfortably regard biological species as though they were species in the inorganic world.

...when individuals multiply from generation to generation, it is but a repetition of the primordial type-idea; and the true notion of the species is not in the resulting group, but in the idea or potential element which is at the basis of every individual of the group; that is, the specific law of force, alike in all, upon which the power of each as an existence and agent in nature depends.<sup>65</sup>

Dana claimed that, by establishing an analogy between the inorganic and organic worlds, he had shown that species were permanent, regardless of how variable they might appear in the wild or in gardens or farmyards. “[I]n the organic, there is reproduction of like from like and no multiplication of kinds by combination,” he

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<sup>64</sup> Dana, “Thoughts on Species,” 305-307.

<sup>65</sup> Dana, “Thoughts on Species,” 307.

asserted.<sup>66</sup> Also, regarding the topic of “variations in species,” he argued, “liability to variation” was part of the “law of species”: that is to say, “Variations are not to be arranged under the head of accidents; for there is nothing accidental in nature.”<sup>67</sup>

Dana concluded that naturalists would eventually dispel the cloud surrounding the term species. “If we could write out in numbers the potential nature of an organic species, or of its germ, including the laws of its variables, this expression would be like any other term in the hands of a mathematician.” But, he reminded, “after all we have here a mere mathematical abstraction, a symbol for an amount or law of force, which can be turned into conceptions, only by imagining (supposing this possible) the force in the course of its evolution of concrete realities, according to the law of development and laws of variations embraced with it.”<sup>68</sup>

A contemporary reader could easily discern the continuity between Braun’s and Dana’s thoughts on species. Gray, however, found it difficult to accept Dana’s reasoning “from inorganic species to organic species.” In a lengthy letter to Dana in November 1857, he warned the geologist to be cautious of the “force of an analogy.” In some level, he wrote, Dana’s thoughts on species bore striking resemblance to Agassiz’s. “If we may do so even as far as you do, might not Agassiz (at least plausibly) say, that as the species Iron was created in a vast number of individuals over the whole earth, so the presumption is that any given species of plants or animals was originated in as many individuals as there are now, and over as wide an

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<sup>66</sup> Dana, “Thoughts on Species,” 311.

<sup>67</sup> Dana, “Thoughts on Species,” 313.

<sup>68</sup> Dana, “Thoughts on Species,” 316.

area?”<sup>69</sup>

Gray found an intellectual niche in these tidal waves of comments and thoughts on species. As he elaborated it in *First Lessons* (1857):

Between our largest and highest-organized trees, such as a Magnolia or an Oak, and the simplest of plants, reduced to a single cell or sphere, much too minute to be visible to the naked eye, how wide the difference! Yet the extremes are connected by intermediate grades of every sort, so as to leave no wide gap at any place; and not only so, but every grade, from the most complex to the most simple, is exhibited under a wide and most beautiful diversity of forms, all based upon the one plan of vegetation which we have been studying, and so connected and so answering to each other throughout as to convince the thoughtful botanist that all are parts of one system, works of one hand, realizations in nature of the conception of one Mind.<sup>70</sup>

Here we might compare Gray’s paragraph with Darwin’s famous portrayal of “an entangled bank” in *On the Origin of Species*:

It is interesting to contemplate an entangled bank, clothed with many plants of many kinds, with birds singing on the bushes, with various insects flitting about, and with worms crawling through the damp earth, and to reflect that these elaborately constructed forms, so different from each other, and dependent on each other in so complex a manner, have all been produced by laws acting around us... There is a grandeur in this view of life, with its several powers, having been originally breathed into a few forms or into one; and that whilst this planet has gone cycling on according to the fixed law of gravity, from so simple a beginning endless forms most beautiful and most wonderful have been, and are being, evolved.<sup>71</sup>

Admittedly, though both Gray and Darwin marveled at the diversity of nature, and were driven to come up with a systematic account of both the origin and the laws of nature’s grandeur, there was a vast gap between these two naturalists’ respective

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<sup>69</sup> AG to JDD, 11/7/1857, *LAG*, 431.

<sup>70</sup> Gray, *First Lessons...*, 175.

<sup>71</sup> Charles Robert Darwin, *On the Origin of Species by Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life* (London: John Murray, 1859), 490.

views of life.

When Gray wrapped the writing of *First Lessons* and *Introduction*, neither Gray nor Darwin recognized that they had differed and diverged from each other in many substantial aspects, such as on the origin and distribution of species. The catalyst that brought the matter to the surface was a “collection of small size” made during the U.S.-Japan Exploring Expedition (1852-1854) (see below). This remarkable event took place in February 1857, when Gray put the finishing touches on his two textbooks. Relieved, he wrote to Darwin, answering questions, gossiping about Agassiz, and sharing his progress on his studies of Japan’s flora. He told Darwin that he was now engaged with a collection of Japan’s plants. “As I expected,” he wrote with satisfaction, “the number of [Japanese] species common to N. America is considerably increased in this collection, as also the number of closely representative species in the two.” “The greater part of the identical species (of Japan & N. Amer.) are of those extending to or belonging to N.W. Coast of America; but there are several peculiar to Japan & E. U. States.”<sup>72</sup>

### **“Convince me”**

Darwin was infected by Gray’s enthusiasm. In May 1857, upon reading Gray’s letter about “a collection of small size from Japan,” in conjunction with Gray’s analysis of disjoined species in the third part of “Statistics,” he decided to test whether his theory worked in the case of disjunct distribution. He asked Gray whether he had found any correlation between disjoined species and the size of the genera to which

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<sup>72</sup> AG to CRD, 2/16/1857, *CCD*, Vol. 6, 339.

these species belonged. "Species having disjoined ranges would belong to small genera," he wrote.<sup>73</sup> Though having been accustomed to Darwin's sometimes eccentric inquires, this time Gray was stunned. "I did not know at all that you suspected disjoined species to belong to small genera & small orders, as a general thing," he replied.<sup>74</sup> Indeed, in "Statistics," he had hinted that there might be two types of disjoined species: those shared by North America and Europe for the most part belonged to cosmopolitan genera, whereas those common to East Asia and eastern North America were largely members of peculiar genera. Gray might have realized that even in the matter of the genus and its geographical distribution, a topic that had occupied so much of his and Darwin's attention, he and Darwin seemed not to be on the same page.

To Gray's challenge, Darwin replied that "disjoined species had suffered much local extinction," which was why he "inferred that genera & Families with very few species... would be apt (not necessarily always) to have narrow ranges & disjoined ranges."<sup>75</sup> Gray was taken aback. In his reply, he manifested his resolution to unearth Darwin's deepest thoughts about species and geographical distribution. "But why is it not just as likely that there were as many small genera (nearly) at first as now, and as great a disproportion in the number of their species?" he wrote. "I never yet saw any good reason for concluding that the several species of a genus must ever have had a common or continuous area." "Convince me," he declared

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<sup>73</sup> CRD to AG, 5/9/1857, *CCD*, Vol. 6, 391.

<sup>74</sup> AG to CRD, 6/1/1857, *CCD*, Vol. 6, 401.

<sup>75</sup> CRD to AG, 6/18/1857, *CCD*, Vol. 6, 413.



finally. “I am a very good subject for you to operate on.”<sup>76</sup>

Gray’s reaction is understandable. He had just published two textbooks in which—for the first time—he confirmed the Creator’s role in making and distributing species. He argued that the genus presented the plan or kind in nature, which should have nothing to do with geographical distribution of species. He knew Darwin must have been pursuing a certain agenda. Indeed, through his correspondence with Hooker, who at the time was engaged with another pattern in disjunct distribution (namely, the similarities between Arctic and Antarctic floras), Gray had been aware of Darwin’s influence on Hooker. He once received a letter from Hooker stating, “On the other hand may the said species not [“all” deleted] have originated in a polar Lat. under the one created form. & migrating South have diverged into others. I do incline to this latter doctrine [which was Darwin’s doctrine]...[and] assume that the species are not distinct at all anywhere.”<sup>77</sup> In his “convince me” letter, Gray added, “That is the way the question strikes an *outsider* like me,—a cautious and *slow* one,—just at first view.”<sup>78</sup> He would wait and see when Darwin would regard him as an “insider” belonging to a circle in which even heterodox ideas (for example, the idea that “the species are not distinct at all anywhere”) could be exchanged and discussed.

George Bentham suddenly surfaced at the juncture where Gray’s and Darwin’s

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<sup>76</sup> AG to CRD, 7/7/1857, *CCD*, Vol. 6, 423.

<sup>77</sup> JDH to AG, 07/16/1856, *HL*.

<sup>78</sup> AG to CRD, 7/7/1857, *CCD*, Vol. 6, 423.



Figure 9-4. George Bentham in 1861 (Courtesy of the Archives of the Gray Herbarium, Harvard University, Cambridge, MA. USA.)

views on disjoined species diverged (Figure 9-4). Oddly enough, though Gray had corresponded with Bentham earlier than either Darwin or Hooker, and the two botanists had enjoyed collaborating on various projects, Gray scarcely discussed his thoughts about species and geographical distribution with Bentham. Bentham might have felt uncomfortable being ignored. Early in 1857, upon receiving a parcel from Gray with an Asian plant and a note (“what is it?”), the British gentleman lost his temper. It was time to give the young botanist his first lesson in botanical geography, he might have thought. In April, he dispatched a letter to Gray. “I have been much interested in your letters to Joseph Hooker and in your paper on the distribution of your plants, etc.,” he wrote.

Since I reviewed De Candolle’s *Géographie Botanique* for the *Edinburgh Review* I have had my attention a good deal turned to the distribution of plants and cross connections of different theories & general facts from one who like yourself knows a little that what species is are very much wanting before any conclusion good for anything can be drawn and some day or another I think much light will be thrown upon the subject.

After this sharp comment, Bentham poured out his anger, targeting

“speciesmongers.” “I shall cut off a couple of hundreds species at least,” he exclaimed, as long as his project on British flora progressed. He then noted, “I find it necessary to state the general geographical area of each British species in order to show the Britishers how little they know of a species... and there are some curious points connected with the distribution of those that are common to Europe and the United States.” He went on to remark that among those “curious points” was the pattern that “many American genera extending into Asia and Europe diminish gradually in species as they go westward.” Such a remarkable pattern, he asserted, naturally led to a conclusion that “the interchange of the floras of the two sides of the Atlantic largely took place through Asia instead of the Atlantic.”<sup>79</sup>

Apparently, Bentham wanted to remind Gray that one could not unveil the origin of disjoined species without taking the interchange of floras into consideration. Bentham’s harsh criticism of Gray’s “Statistics” (“who like yourself knows a little that what species is”) probably stemmed from the conjecture Gray made in the section on disjoined species: that is, that the disjoined species common to the two sides of the Atlantic had something to do with “a former terrestrial connection between North America and Europe.”<sup>80</sup> It seemed to Bentham that Gray utterly confused the sequence and route through which the interchange of floras took place. It was Asia—instead of the Atlantic— that played the role in shaping disjunct distribution as Gray understood it.

The sequence and the route of floristic interchange were part of Bentham’s

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<sup>79</sup> GB to AG, 4/2/1857, *HL*.

<sup>80</sup> Gray, “Statistics... [Part II & III],” 387.

major arguments in his review of de Candolle's *Géographie botanique raisonnée*. Furthermore, Bentham extended these two issues to engage in the debates concerning the origin of species. He noticed de Candolle's compromise with the multiple-creation advocates and argued that it was unnecessary. If, he argued, de Candolle had paid more attention to geology, he would never have regarded disjoined species as exceptional, or the result of multiple creations of species: "We cannot deny that the [geological] effect must have been to destroy every existing species of plants over their whole area, with the exception of such islands, mountain-tops, or other isolated spots often far distant from each other, where they could still find a genial station."<sup>81</sup>

But Bentham was definitely aware of an exception: namely, "Climate and station... have little or no influence on that [the distribution] of genera," which depended much more on "circumstances of original creation, and must be well studied in speculating on the early history of vegetation." He then offered an example manifesting how curious the pattern might be regarding the distribution of genera: "When we see large American genera represented by a very few species in Eastern Asia, with one or two extending westwards even to Europe... we cannot avoid speculations on the ancient continuity of lands similar to those we are led into by the identity of certain species."<sup>82</sup>

Upon receiving Bentham's letter, Gray rushed to check Bentham's review of de

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<sup>81</sup> George Bentham, "Review of De Candolle's Geographical Botany and Other Works," *The Edinburgh Review or Critical Journal* 104, no. 212 (1856), 502-503.

<sup>82</sup> Bentham, "Review of De Candolle's Geographical Botany...," 517-518.

Candolle's *Géographie botanique raisonnée*. He wrote back to Bentham in May 1857, declaring, "I am quite prepared for what you say about interchange of species of United States and Europe taking place via Asia." "On our views of what a species is," he remarked, "interchange is the only thing that... will explain the occurrence of the same species here and there." Even so, he argued that the issue Bentham left unanswered—namely, the distribution of genera—was exactly the key to evaluating the extent to which the East Asian flora related to its North American counterpart. Considering, he wrote, how many "peculiar genera (i.e. *Torreya*, *Illicium*, *Philadelphus*, *Astilbe*) were divided between Japan and the United States," "I do not yet feel free to assume an interchange, or a former continuity of land, between two widely separated regions on account of their having identical genera or closely related species." In sum, he concluded, "I see no reason why cognate species may not have been originally given to most widely separated stations."<sup>83</sup>

In response, Bentham called Gray's attention to his essay titled the "Synopsis of the Genus *Clitoria*," published in the *Journal of the Proceedings of the Linnean Society* in 1857. In it Bentham listed a couple of disjointed species in addition to de Candolle's *Phyma leptostachya* (Bentham preferred to translate de Candolle's *espèces disjointes* as "discontinuous or dissevered species").<sup>84</sup> Bentham referred to *Clitoria macrophylla* as a species "only known by a small number of specimens, [and as one that] still remains a detached East Asiatic representative of a considerable

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<sup>83</sup> AG to GB, 5/4/1857, LAG, 428-429.

<sup>84</sup> George Bentham, "Synopsis of the Genus *Clitoria*," *Journal of the Proceedings of the Linnean Society: Botany* 2 (1857), 33-34.

American type.” He further reminded readers that

large American genera... are represented in Eastern Asia by a small number of species, which gradually diminish or disappear altogether as we proceed westward towards the Atlantic limits of Europe; whilst the types peculiar to the extreme west of Europe (excluding, of course, the Arctic flora) are wholly deficient in America. These are among the considerations which suggest an ancient continuity of territory between America and Asia under a latitude, or at any rate with a climate, more meridional than would be effected by a junction through the chains of the Aleutian and Kurile Islands.<sup>85</sup>

Here Bentham was to revise his previous thought on the interchange of the flora of East Asia and that of North America. In his *Labiatarum genera et species* (1832-1836), he noted that “The Aleutian, or Kurile region, comprising the islands between North China and America, and perhaps the southern extremity of Kamtschatka, is merely indicated here as probably connecting the Flora of the two continents.”<sup>86</sup>

Two decades later, Bentham had become unsure of whether the fragmented island chains of the Aleutian and Kurile regions could have served as a route for floristic interchange. In part inspired by Edward Forbes’s groundbreaking work on the geological effects that had shaped the current distribution of fauna and flora (1846), he came to assume the existence of “an ancient continuity of territory between America and Asia under [that] latitude.”<sup>87</sup> With respect to the systematic nature of a floristic pattern that covered Europe, Asia, and America, he had to retain this assumption despite little geological evidence. To him only a significant landmass, not the Aleutian and Kurile Islands, could give rise to such a remarkable

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<sup>85</sup> Bentham, “Synopsis...,” 34-35.

<sup>86</sup> George Bentham, *Labiatarum genera et species...*, xxxvi.

<sup>87</sup> Forbes’s influences on Bentham regarding disjunct distribution can be found in Bentham, “Synopsis...”; also see Endersby, *Imperial Nature*, 238-243.

biogeographical pattern.

Bentham's assumption of an ancient continuity of territory, obscure as it might be, was part of an intellectual tradition that won support from the most acclaimed naturalists in nineteenth-century Europe, including Lyell and Hooker (but not Darwin; see below). In the third part of "Statistics," Gray conjectured that the territorial connection might be why identical species could be found in widely separated "stations"; he nevertheless came to doubt Bentham's "ancient continuity" as soon as Bentham applied this geological assumption to the distribution of genera and congeneric species. Gray's statement that "cognate species" might have been "originally given to most widely separated stations" indicates his attempt to reconcile his theological definition of genera with Bentham's updated views on biogeography. As we shall see, the straw that broke Gray's rather durable conviction that genera presented nature's plan came soon after Bentham's criticism of "Statistics." It was a letter from Darwin that would "convince" Gray that the interchange of floras did not necessarily rely on "an ancient continuity of territory between America and Asia under [the] latitude" of the Aleutians and Kuriles, and that the distribution of genera could be explained without recourse to the Creator's will in nature.

### **"No such things as independently created species"**

Meanwhile, Darwin knew that it was time to discuss his species theory with Gray. He underlined the words "convince me" on Gray's letter and pondered what he should do to convince this new acquaintance, this faithful correspondent, and this ablest of

botanists on the other side of the Atlantic.<sup>88</sup> The “Statistics” exemplified Gray’s emphasis on facts and his reluctance to generalize on a large scale. Gray’s disagreement with Agassiz’s species theory indicated the scorn that Gray heaped on groundless speculation. Darwin knew he had to be careful. In July 1857, Darwin dispatched his reply to Gray: “What you say about extinction, in regard to small genera & local distinction, being hypothetical seems very just... but I shd. like to tell you, (& I do not *think* I have) how I view my work.” He then continued, “Nineteen years (!) ago, it occurred to me that... I might perhaps do good if I noted any sort of facts bearing on the question of the origin of species.” Throughout these years, Darwin admitted, he had arrived at a “heterodox conclusion”: namely, there were “no such things as independently created species—that species are only strongly defined varieties.” “I assume that species arise like our domestic varieties with *much* extinction,” Darwin claimed, and those congeneric species were “actual lineal descendants of one species.”<sup>89</sup> It was from this standpoint that he asked Gray whether or not most disjoined species belonged to small genera:

Just to allude to one point in your last note, viz about species of the same genus generally having a common or continuous area; if they are actually lineal descendants of one species, this of course would be the case; & the sadly too many exceptions (for me) have to be explained by climatal and geological changes. A fortiori on this view (but on exactly same grounds) all the individuals of the same species shd. have a continuous distribution.<sup>90</sup>

In September, Darwin sent Gray an abstract of his species theory. Step by step,

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<sup>88</sup> The original letter can be found at *Dar* 165: 98.

<sup>89</sup> CRD to AG, 7/20/1857, *CCD*, Vol. 6, 431-432.

<sup>90</sup> CRD to AG, 7/20/1857, *CCD*, Vol. 6, 432.



he outlined the points that had led him to conclude that “there were no such things as independently created species.” Gray eventually understood why Darwin was so eager for information about the size of genera, close species, varieties, and geographical distributions. Most of such discussions between him and Darwin surrounded what Darwin called “the principle of divergence”:

Now every single organic being, by propagating so rapidly, may be said to be striving its utmost to increase in numbers. So it will be with the offspring of any species after it has broken into varieties, or sub-species or true species. And it follows, I think, from the foregoing facts, that the varying offspring of each species will try (only few will succeed) to seize on as many and as diverse places in the economy of nature, as possible. Each new variety or species, when formed will generally take the place of and so exterminate its less well-fitted parent.

“This, I believe, to be the origin of the classification or arrangement of all organic beings at all times,” Darwin claimed. What followed was a statement that would be one of the most powerful metaphors Darwin ever developed—the tree of life:

“These always *seem* to branch and sub-branch like a tree from a common trunk; the flourishing twigs destroying the less vigorous,—the dead and lost branches rudely representing extinct genera and families.”<sup>91</sup>

It is difficult to conjecture about Gray’s immediate response to Darwin’s letter of September 1857. No replies that Gray might have made are in existence today.

However, in October 1857, Gray told Hooker, “Now I am much interested in Darwin’s endeavors... That vein has got to be worked: and we are much interested

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<sup>91</sup> CRD to AG, 9/5/1857, *CCD*, Vol. 6, 448-449. Regarding the making of Darwin’s principle of divergence, see Browne, “Darwin’s Botanical Arithmetic...”; Ernst Mayr, “Darwin’s Principle of Divergence,” *Journal of the History of Biology* 25, no. 3 (1992), 343–359; David Kohn, “Darwin’s Principle of Divergence as Internal Dialogue,” in *The Darwinian Heritage*, edited by David Kohn (Princeton: Princeton University Press, 1985), 245-257; Trevor Pearce, “‘A Great Complication of Circumstances’—Darwin and the Economy of Nature,” *Journal of the History of Biology* 43, no. 3 (2010), 493–528.

to have it done by a true naturalist & an honest & unprejudiced one. A better man to do it than D [Darwin] cannot be found.”

Following Gray’s acknowledgment of Darwin’s species theory was a lengthy and curious paragraph:

When we see how races are evolved, and how various are the degrees of resemblance between what we call species of the same genus,—& how impossible it is to do more than guess what are species & what are varieties, we cannot avoid asking whether there is not some law of development of species.—It is a true scientific question; and whether it is rendered the more probable hypothesis, I shall feel no hesitation in adopting it as such—certainly, with [i.e., which] you shall not, and do not think derogatory to the Deity to originate the diversity of plants and animals in this way. Very presumptuous it would be for any man to say that. And so of the relations of Vitality to physics. We are getting new light as to the relations, but I do not believe you are going to homologize them then.<sup>92</sup>

Gray’s letter to Hooker hints at the way in which Gray had incorporated Darwin’s species theory. The key terms—for example, the evolution of races, the development of species, the relation of vitality to physics—all appeared in Braun’s essay in 1855-1856 on individuals and species. According to Braun,

Now if we would conceive of a physiological individual, in the broadest meaning of the term, we should certainly be compelled to demand that our conception should be such as to exhibit not only single phases, but all the phases of the specific life during its entire development; that it should realize all the capabilities of the specific being, and thus present to us the whole plan, the whole destiny of species.<sup>93</sup>

Even Dana proposed a similar view in his “Thoughts on Species.” “Each species,” he argued, “has its own special mode of development as well as ultimate form or result.” “We comprehend the type-idea only when we understand the cycle of

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<sup>92</sup> AG to JDH, 10/12/1857, Porter, “On the Road to the *Origin...*,” 27-28.

<sup>93</sup> Braun, “The Vegetable Individual...,” 314-315.

evolution through all its laws of progress, both as regards the living structure under development within, and its successive relations to the external world.”<sup>94</sup>

In retrospect, Gray must have been impressed by Dana’s “special mode of development.” In his famous review of *On the Origin of Species*, Gray first argued that Dana’s “Thoughts on Species” were “so idealistic” that they could not “harmonize readily with a doctrine so thoroughly naturalistic as that of Mr. Darwin.” Yet, remarkably, he then noted,

... one who regards the kinds of elementary matter, such as oxygen and hydrogen, and the definite compounds of these elementary matters, and their compounds again, in the mineral kingdom, as constituting species, in the same sense, fundamentally, as that of animal and vegetable species, might admit an evolution of one species from another in the latter as well as the former case.<sup>95</sup>

It should be argued, however, that neither Braun’s nor Dana’s thoughts on evolution or development should have struck Gray as too sudden. In effect, since his early years, Gray had begun entertaining similar ideas, and had eagerly promoted them in his series of botanical textbooks. Needless to say, what interested Gray could not have been evolution in a Darwinian sense. The matter of interest was an issue commonly called “vegetable metamorphosis,” an approach to morphology, which—according to Gray—was first proposed by Linnaeus and which was substantially developed by Caspar Frederic Wolff, Goethe, and Augustin de

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<sup>94</sup> Dana, “Thoughts on Species,” 26-27.

<sup>95</sup> Asa Gray, “Review of Darwin’s Theory on the Origin of Species by Means of Natural Selection,” *The American Journal of Science and Arts (2nd Series)* 29, no. 86 (1860), 154.

Candolle.<sup>96</sup> Gray was intrigued by how many subdivisions in morphology could be unified by the idea of metamorphosis. From *Elements of Botany* (1836) to *Introduction to Structural and Systematic Botany* (1858), he not only admired the fact that the concept of metamorphosis had “placed the whole logic of Systematic Botany upon a new and philosophical basis,” but also offered rich examples supporting the assertion that flowers could be regarded as transformed branches, petals as changed stamens, sepals as modified leaves, petals as refined sepals, and so forth. In light of these sheer transformations among organs, Gray suggested, it became imperative to acknowledge a particular organ as the type, and view other organs as “modifications or metamorphoses of it.” Given the crucial role of the leaf in plant physiology, Gray argued, the leaf was the ultimate type or pattern, “to which all the others [organs] are to be referred,” and from which the individual organ developed toward its own destiny, during the “progressive evolution of the plant.”<sup>97</sup>

So what is the relationship between these careful identifications of forms and types and Gray’s personal reception of Darwin’s evolutionary theory? Here is involved a conceptual step in Gray’s intellectual development. Inspired by Braun’s essay on individual and species, Gray was no longer convinced that the seed was the only source from which individuals were propagated and that, from these propagated individuals, naturalists would accurately draw out the abstract concept

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<sup>96</sup> Regarding this approach in morphology, see, for example, Peter F. Stevens, “Haüy and A.-P. Candolle: Crystallography, Botanical Systematics, and Comparative Morphology, 1780-1840,” *Journal of the History of Biology* 17, no. 1 (1984), 49-82; Robert J. Richards, *The Tragic Sense of Life: Ernst Haeckel and the Struggle over Evolutionary Thought* (Chicago: University of Chicago Press, 2008), 455-488.

<sup>97</sup> Gray, *Introduction... (5th and Revised Edition)*, 231.

of species. Instead, he argued, buds, shoots, or any other part of the plant capable of developing “an indefinite number of similar parts,” producing “branches like the parent plants,” and so forth, were individuals, and that naturalists ought to examine them as individuals. “The individual plant is evidently not a simple and true individual in the proper sense of the words,—in the sense that an ordinary animal is,” he remarked in *Introduction*.<sup>98</sup>

Gray knew Hooker hardly agreed with this revised view of the individual. In 1855, after having his student translate Braun’s essay into English, Gray eagerly sent the text to Hooker, gauging this peer’s reaction to the ideas therein. To his disappointment, Gray found Hooker to be no fan of Braun. “I have some [more] patience with the ‘trouble’ of the first part than you seem to have,” Gray wrote, trying to persuade Hooker to pay more attention to Braun’s account of individuality. “I do not again [agree] with you at all in a scientific view [that] the individual must be viewed as the product of the seed. Pray why is not the product of a bud as much an individual as the product of a seed.”<sup>99</sup> Gray did not know that Hooker elsewhere despised Braun’s works as “German rubbish.” “I protest boldly against such work... being given to the British public without one word of explanation,” Hooker once exclaimed to a botanical colleague.<sup>100</sup>

Gray knew why Hooker had difficulty accepting Braun’s views. As he himself

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<sup>98</sup> Gray, *Introduction... (5th and Revised Edition)*, 352-353.

<sup>99</sup> AG to JDH, 7/2/1855, *GCK*.

<sup>100</sup> Quoted from Endersby, *Imperial Nature*, 217; for a detailed discussion, see pp. 216-219.

admitted, if naturalists came to regard buds, shoots, and such as individuals, a reasonable conclusion would be that “most of our Potato plants must belong to one multitudinous individual, while others wholly similar, but freshly grown from seed, are each individuals of themselves;—a view which apparently amounts to an absurdity in terms and in fact.”<sup>101</sup> But Gray found that such an absurdity was superficial. As he put it in *Introduction*, naturalists should familiarize themselves with another concept: “phytons” or “plant-elements.” To Gray the definition of phyton was evident enough. The first phyton was “the radicle of the embryo, with its cotyledon or pair of cotyledons.” This first phyton then gave birth to “the next phyton, or joint of stem and leaf, and so on, in lineal succession.” Besides seeds, the “joint of stem” was another place where phytons rested. When the joints of stems were “favorably situated,” the phytons there became capable of producing secondary roots and, thus, of completing “the vegetable individuality, having all the organs of vegetation.”<sup>102</sup> In this view, he argued, Braun’s view—to which he faithfully subscribed—placed its emphasis upon “simplicity and indivisibility, rather than upon tendency to separation... [and treated] the phytons in ordinary plants, and the cell in those of lowest grade, as on the whole best answering, in the vegetable kingdom.”<sup>103</sup>

It is from here that Gray devised his view on the relationship between

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<sup>101</sup> Gray, *Introduction... (5th and Revised Edition)*, 353.

<sup>102</sup> Gray, *Introduction... (5th and Revised Edition)*, 96.

<sup>103</sup> Gray, *Introduction... (5th and Revised Edition)*, 353. This viewpoint can be found as early as in Gray’s comment on Braun’s individuality in 1856. See Braun, “The Vegetable Individual...,” 79.

individuals and species. As he put it in *Introduction*, “it is evident that plants as well as animals occur in a continued succession of organisms or beings which stand in the relation of parent and offspring. Each particular sort is a chain, of which the individuals are the links. To this chain, or (as expressed by Linnaeus) this perennial succession of individuals, the natural-historian applies the name of species.”<sup>104</sup> He found Braun’s account of “self-existent beings” mattered here:

If what we call plants are nothing but complex chemical and physical *processes*, then we can no longer speak of their individuals and species in the sense the words usually bear; for the mere phenomena of the operations of the primary substance, which have no other efficient principle than the force of this substance, cannot be regarded as self-existent beings, or as peculiar (specific) kinds of these beings, or as single (individual) modifications of them.<sup>105</sup>

If, then, there was no essential gap between the individual and the species, then it became as legitimate to discuss the metamorphosis of species as it was to discuss the metamorphosis of the individual (which, in Braun’s and Gray’s schemes, included buds, shoots, and the like). Braun indeed had tried to undertake this task in his essay on individuality. Species, he reasoned, exhibited “a determinate circle of development” just like individuals. Though Braun admitted that a “species is not compared with the whole circle of the individual’s successive development,” this incomparability did not deny that species might proceed according to the “single step of the metamorphosis (which of course has its own subordinate members).” In this view, a species itself was “regarded as an inferior ‘momentum’ of a still more

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<sup>104</sup> Gray, *Introduction... (5th and Revised Edition)*, 354. Regarding Linnaeus’s definition of species that might have inspired Gray, see Staffan Müller-Wille, “Collection and Collation”; Mary P. Winsor, “Linnaeus’s Biology Was Not Essentialist,” *Annals of the Missouri Botanical Garden* 93, no. 1 (2006), 2-7.

<sup>105</sup> Braun, “The Vegetable Individual...,” 310.

comprehensive cycle of development.”<sup>106</sup>

Notably, however, Braun stopped his analysis here. It seems that Braun himself had no systematic account to offer, besides ascertaining the existence of the metamorphosis of species. Gray himself might have given considerable thought to this issue. He eventually turned to attribute to the Creator the role of the initiator of this great chain of beings, a chain of beings that naturalists identified as species and that consisted of individuals serving as links.

Against this backdrop, Darwin’s “heterodox conclusion” was not as heterodox for Gray as Darwin might have thought. Immersed in an atmosphere in which development was hailed as a guiding principle in nature, as well as his lasting engagement with the issue of metamorphosis, Gray found Darwin’s species theory relevant. At the very least, Darwin emphasized how new species emerged from individuals’ struggle for survival, and how natural selection privileged those species whose individual members could diverge from one another widely; the concept of this divergence, in Gray’s opinion, ultimately filled the void that Braun had left unaddressed. At some point, Gray might have thought that his own views on species and the individual were closer to Darwin’s than to Hooker’s. Hooker’s rather narrower views (in Gray’s opinion) on the individual, and Hooker’s subsequent refusal to accept that there was no essential boundary between species and the individual, should have hindered Hooker’s possible acceptance of the view that species might evolve over time from one into the other. “I do not believe you are going to homologize them [that is, Hooker’s own view on species and Darwin’s

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<sup>106</sup> Braun, “The Vegetable Individual...,” 299.



evolutionary theory],” he told Hooker. “When you do I will accept it like anything else proved. More likely, it may be shown how vitality directs the expenditure or transformation of physical force—i.e. makes physical force transform matter in peculiar ways.”<sup>107</sup>

Even though Gray might be able to “homologize” Darwin’s species theory with his own views on the matter, he had other theories to be homologized. As far as the origin of disjoined species was concerned, by the late 1850s, Gray had held two theories. The first, proposed by Darwin, emphasized transmutation and divergence of species. It suggested that disjoined species should belong to small genera, because small genera were supposed to suffer from more severe extinction, which in turn would result in discontinuous distribution of congeneric species. The second theory, suggested by Bentham, highlighted the interchange of floras across continents. Bentham supposed that disjoined species belonged to large genera. In his opinion, the disjunct distribution of congeneric species was a vestige left by a transcontinental exchange of floras from Europe to Asia to America. Migration on such a scale drove Bentham to assume the existence of a considerable landmass between Asia and North America, an assumption with which Darwin scarcely agreed.

For Gray, both theories proved relevant to his current purpose: to wrest authority from Agassiz in studies on the origin and distribution of species. Both Darwin and Bentham vehemently rejected any form of multiple creations of species—regardless of whether it was presented in as extreme a form as Agassiz’s,

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<sup>107</sup>AG to JDH, 10/12/1857, Porter, “On the Road to the *Origin...*,” 28.

or in as compromising a form as de Candolle's. Yet, despite this similarity, the gap between these two theories was too wide for there to be a compromise somewhere in the middle. Impressed by Darwin's groundbreaking reconstruction of the species theory, and Bentham's knowledge of plant geography, Gray understood that he was facing an either-or question. If he really needed a theory to explain why there was a floristic similarity between Japan and eastern North America, he had to make a choice.

This choice resurfaced as Gray incorporated geology into his thinking on the cause of disjunct distribution. The experience of writing "Statistics" had reminded Gray that disjunct distribution might stem from an "ancient dispersion" of plants and subsequent geological impacts. He was no longer convinced that climatic similarities in the present—for example, those characterizing eastern North America and Japan—could have explained the occurrence of congeneric species in mutually sundered locations. Yet, if there was an ancient dispersion of plants, when and where did it take place? What would be the connection between this ancient dispersion and the present distribution of floras?

Gray naturally consulted his geologist friend James D. Dana for advice. In reply, Dana sent Gray a three-page manuscript listing all geological epochs and their respective significance in shaping the geological history of North America. He called Gray's attention to two epochs following the glacier period: the fluvial and terrace epochs. In his opinion, these two epochs were the watershed during which the American continent formed its present geological features. Dana characterized the fluvial epoch based on its "milder temperature." He literally portrayed an America

that, probably due to the melting of the glaciers, was almost sunk under water. It was a time when “the region of the St. Lawrence and Lake Champlain was submerged, and the sea... rested 500 feet above its present level; and when the northern portions of our country generally as shown by the immense upper alluvial plains 50 to 300 feet above the present bed of the rivers.” Because of this milder climate, Dana remarked, the fluvial epoch was the “epoch of the *Elephas Americanus* & *E. primigenius*.” These two elephants of the temperate zone could populate the regions spanning from “Canada to New England South,” and *E. primigenius* could even find its habitat from the “Parallel of 40°N to the arctic coast of Siberia.”

What followed these dramatic changes was the terrace epoch. It was a time featuring a geological movement “towards the present condition.” “The continent was up with present level, and down with present cool temperature,” during which time the fauna and flora of the present North America took shape. Dana knew that not every geologist agreed with his scheme. Regardless, he insisted, “There is no doubt of the greater warmth than the present in the post-tertiary after the Glacier Epoch, and still greater in the Tertiary Epoch.”<sup>108</sup>

In August 1858, Darwin sent Gray a letter elaborating his theory of how the earth’s geological past might have given rise to the disjunct distribution of species. “In the older or perhaps newer pliocene age (a little *before* Glacial epoch),” wrote Darwin, “when the temperature was higher, I believe the northern part of Siberia & of N. America being almost *continuous* were peopled... by a nearly uniform Fauna & Flora.” The subsequent glacial period then drove “all living things far south,” to the

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<sup>108</sup> JDD to AG, 11/13/1857, *HL*.

extent that “middle or even southern Europe [was] being peopled with Arctic productions.” As a result, when the warmth returned, the “arctic productions slowly crawled up the mountains, as they became denuded of snow”—which was why “we now see on their summits the remnants of a once continuous flora & fauna.”

At some point Gray might have asked Darwin whether there was any evidence supporting Dana’s geological scheme. Darwin replied, “Some facts have made me vaguely **suspect** that between glacial & present temperature there was a period of *slightly* greater warmth.” “I look at many of the species of N. America which *closely* represent those of Europe, as having become modified since the pliocene period, when in northern part of world there was nearly free communication between old & new worlds.”<sup>109</sup>

Toward the end the 1850s, Gray found himself being exposed to multiple, mutually contradictory theories of species and their geographical distribution. Electrified, he nevertheless insisted that the legitimacy of any theory should be based on empirical data, in his case Wright’s rich collection of Japanese plants. “[I] will not till this autumn be able to touch his [Wright’s] Pacific collections, of which the best and principal were made in Hongkong, Bonin, and the Loo Choo Islands and Japan,” he told Sir William in June 1856.<sup>110</sup> “When that [Wright’s collection] comes to be studied, it will be worth while to compare the Japanese & N. American Floras rather critically,” he also informed Darwin.<sup>111</sup>

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<sup>109</sup> CRD to AG, 8/11/1858, *CCD*, Vol. 7, 149-150.

<sup>110</sup> AG to WJH, 6/30/1856, *LAG*, 422.

<sup>111</sup> AG to CRD, 2/16/1857, *CCD*, Vol. 6, 340.

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Gray also understood that when Wright's collection came to be studied, he would have to be careful about ownership, an issue that made a lot of trouble for him during Wright's service on the Boundary Commission. Sometime in May 1855, when he began research for "Statistics," he received an inquiry from James Morrow, agriculturalist on the U.S.-Japan Expedition. Morrow wondered whether Gray could help him identify the specimens procured during the expedition. He told Gray that Charles Mason of the U.S. Patent Office had instructed him (Morrow) to make the request. Months later, Gray received a letter from Williams, his old acquaintance who had sailed to East Asia in the 1830s. "It was a great pleasure to have the opportunity to ramble over the hills & valleys of Japan as Dr. Morrow & I did," Williams wrote. "Do you propose to draw up any description of them, or make a memoir on them, or put away drawings in any Periodical?" he asked. "It will be a good opportunity perhaps to get drawings of *Icones Plantarum* made at good expense."<sup>112</sup>

Gray replied that he was delighted to do Morrow this favor. To his dismay, however, he found himself drawn into a struggle between Morrow and both Matthew Calbraith Perry and the federal government. For various reasons, Morrow had not secured a salary when he joined the expedition. After his return, Morrow kept his records and collection on hold and begged Gray not to submit anything to either Perry or the government until he got paid. Gray agreed, and was soon

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<sup>112</sup> SWW to AG, 9/13/1855, *HL*.

bombarded by letters from Perry, who was furious about the conspiracy between his former crewman and a Harvard professor. He ordered Gray to submit a report on Morrow's collection immediately or to relinquish the collection to an abler hand. Annoyed, Gray put the collection aside, and refused to comply with Perry's order. He even disregarded Morrow's subsequent letters asking whether Gray could send him the report.<sup>113</sup> It wasn't until 1857 that Gray hastily submitted a brief paper to Perry to be included in the expedition's official report. He was then embarrassed to find that Perry remarked in the report that Morrow's collections had very unfortunately fallen in the hands of an irresponsible botanist, causing unnecessary delays. This henceforth prevented him from publishing a well-illustrated and formulated botanical report.

Despite the shameless struggle between the commodore and his former crewman, Gray did not find Morrow's collection particularly stimulating. Though it revealed some interesting aspects of Japan's flora, he thought that the collection merely confirmed the floristic similarities between Japan and that of eastern North America, which he had known about for many years.<sup>114</sup> Several factors had conspired to diminish the collection's scientific value. The first had to do with Morrow and Williams's proficiency in botanical collecting (neither Morrow nor

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<sup>113</sup> For a notable discussion on this debate, see Dupree, "Science vs. the Military."

<sup>114</sup> Asa Gray, "List of Dried Plants Collected in Japan, by S. Wells Williams, Esq., and Dr. James Morrow," in *Narrative of the Expeditions of an American Squadron to the China Seas and Japan, Performed in the Years, 1852, 1853, and 1854, Under the Command of Commodore M. C. Perry, United States Navy, by Order of the Government of the United States, Vol. 2*, edited by Matthew C. Perry (Washington, DC: A.O.P. Nicholson, 1856), 305-332.



Figure 9-5. *Tricercandra quadrifolia*, one of the new species Morrow and Williams collected on Perry’s Japan Expedition. (Courtesy of the Smithsonian National Museum of Natural History, Washington, DC, USA.)

Williams had extensive collecting experience before joining Perry’s expedition). A great number of Morrow and Williams’s specimens were but fragments, which multiplied the difficulty in classification (Figure 9-5).<sup>115</sup> Also, the “timing” was not right when Morrow and Williams botanized in Japan. As Williams put it in his journal, “Few flowers, I’m sorry to say, were found, most of those along the path

<sup>115</sup> Tetsuo Koyama, *Plant Collecting in Japan and Its Adjoining Territories by the Black Ships* (Fujisawa: Nihon University, 1994), 13.

having blossomed, and some berries already made their appearance.”<sup>116</sup> Finally, and probably most importantly, Commodore Perry did not even permit the two collectors to systematically explore Japan’s flora during the expedition. Though a natural history enthusiast and a navy officer who believed the expedition under his command would serve as a great stepping toward the United States’ becoming an “empire of science and commerce,” Perry was concerned that a pair of Americans wandering in the fields and backyards of the locals, plucking plants, would generate unnecessary anti-American reactions. The mission that Congress and the American people had entrusted to him, after all, was to persuade the Japanese government to sign a “treaty of amity and commerce” with the United States of America, not a scientific survey of Japan’s natural history.<sup>117</sup>

Gray might not have been aware of all the factors that had prevented Morrow and Williams from collecting effectively in Japan. Yet, while examining the two collectors’ rather small and fragmented collection of Japanese plants, he hoped that Charles Wright, then the botanist of the North Pacific Exploring Expedition, would have better luck “ravishing” Japan’s flora.

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<sup>116</sup> Frederick Wells Williams, ed., *The Life and Letters of Samuel Wells Williams, LL.D.: Missionary, Diplomatist, Sinologue* (G. P. Putnam’s Sons, New York, 1889), 222.

<sup>117</sup> In the words of Allan Burnett Cole, the editor of James Morrow’s journal, “Probably these two saw more of the land and its people than did any others in the expedition, for the frequency and extent of travel by American naval officers and men were strictly limited by orders of the Commodore and by the Tokugawa *bakufu*, or military government.” Cole, ed., *A Scientist with Perry in Japan, the Journal of Dr. James Morrow* (Chapel Hill: University of North Carolina Press, 1947), xix.



## CHAPTER TEN

### “A Passport to Observation”

Through the rain and fog of May 13, 1855, the *Vincennes* sailed into the harbor of Shimoda. Soon afterward, the *John Hancock* arrived and anchored nearby. The men onboard the two vessels examined the harbor with interest. They wondered what kind of reception the Japanese government would offer. After all, they thought, Rodgers’s expedition was the *first* visit paid by the U.S. government since Commodore Perry had concluded his historic Treaty of Kanagawa with the Japanese government. They hoped that the unpleasant experiences they had in Kagoshima Bay would not repeat themselves in Shimoda.

As for Wright, the months spent sailing from Hong Kong to Shimoda had made him a true naturalist appointed to a military expedition. Besides his regular working schedule—for example, checking relevant information in the library, compiling field notes and journals, and cataloging specimens—he became increasingly inclined to apply what Rodgers called his “the passport to observation” to further his collecting enterprise. Late in April 1855, when the *Vincennes* revisited the Ryukyu Islands and stayed there for about one week (from April 21 to 26), Wright again went to what he regarded as “the royal park without any game” to see if he could gather some specimens in flower. He climbed over the wall and prepared himself to enjoy this paradise in a perfect state of nature. But as Wright happily filled his portfolios with such flowers as *Lilium longiflorum* Thunberg, he found himself being hit by stones. Stunned, he looked for his attackers and saw a group of Ryukyu children raising a

ruckus on the other side of the wall. He was annoyed. He took out his pistol, exploding a cup before the children (who, in Wright's words, "were disposed to let out some of the mischief or the so called original sin abundantly attributed to all juveniles"). The display worked beautifully. As he told John, "they all scattered like a flock of scared sheep." Afterward, however, when Wright continued to botanize in other parts of the Ryukyu Islands and realized that exploding a cup could no longer keep these badly behaved juveniles away, he endeavored to show them something spectacular. "I threw a few crackers (bread) among a crowd of "sans-culottes" (breeches less) boys when a scrambling took place such as might occur on election day in a crowd of children among whom a handful of pennies had been thrown."<sup>1</sup>

But men onboard the *Vincennes* and the *John Hancock* soon discovered that they were neither the first Westerners nor the first Americans who had visited Shimoda since Perry's treaty with the Japanese government. In fact, only several hours after the two vessels arrived in Shimoda, a boat with three or four Western-looking men approached the *Vincennes*. When the boat was broadside to the *Vincennes*, the men on it said—with unmistakable New England accents—that they were citizens of the United States and had been residing in Shimoda since March 1855. They had heard of Commander Rodgers's mission to Japan, they said, and would like to deliver a letter to Rodgers in person. Curious, Rodgers invited these Americans to board the vessel. It turned out that they were representatives of a group of merchants, including W. C. Reed, T. T. Dougherty, E. A. Edgerton, W. E. Bidleman, and H. W. Peabody. Early in 1854, upon hearing of the signing of Commodore Perry's Treaty of

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<sup>1</sup> CW to JW, 6/10/1855, *CWC*.

Kanagawa (in March), they became aware of the great wealth that the treaty might generate. They thought that the treaty would bring many whalers to Japan's shores, and hence it would be lucrative to set up some sort of provision house for selling food, wine, tobacco, and other luxuries to whalers. With this prospect in mind, they had invested all they had, made contracts with merchants, paid a significant amount in deposit, and traveled halfway across the world to Shimoda. They brought their wives and children with them and prepared themselves to settle in this Japanese town.

But what they encountered in Shimoda was beyond their expectations, they told Rodgers. First, they found that Japanese officers in Shimoda had no interest in fulfilling the stipulations of the Treaty of Kanagawa. That is to say, instead of helping them get the provisioning business started, Shimoda officers had deployed every possible means to deter them. Moreover, local officials had dispatched spies to keep them under full surveillance. Since their arrival, said the American merchants, they had fought relentlessly for decent treatment, but to no avail. Literally and figuratively, they groaned, they were now prisoners in this remote corner of the world. Given the absence of the U.S. commissioner to Japan, they wanted Rodgers to stand up for his fellow citizens. After all, if the NPEE was a follow-up of Perry's expedition, enforcement of the Treaty of Kanagawa certainly also fell to Rodgers.<sup>2</sup>

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<sup>2</sup> Cole, *Yankee Surveyors...*, 104-106.



Figure 10-1. The Japanese officers. (From Heine, *Die Expedition... Vol. 2*, plate 3. (Courtesy of the Widener Library, Harvard University, Cambridge, MA, USA.)

While the men of the *Vincennes* were still listening sympathetically to their fellow Americans' grievances, a boat carrying several Shimoda officers arrived (Figure 10-1). They brought a Japanese translator who spoke tolerable English and said that they needed to interview Rodgers regarding the goals of the *Vincennes's* visit. Rodgers ordered his men to escort these officers onboard and treated them to glasses of wine. The Shimoda officers were pleased. For the next thirty minutes or so, they tasted the wine, examined the curiosities onboard, and occasionally chatted about Japan's relationship with the United States. Rodgers was pleased, too. With the help of the translator, he first outlined the origin and history of the NPEE. He said that the U.S. government and people had entrusted to him the duty of

conducting a thorough and systematic survey of the sea routes that might enhance traffic between the American and Japanese peoples. Considering the complexity of the matter, as well as his rank as commander of a national expedition, Rodgers said that he deemed it necessary to have an official meeting with the governor of Shimoda. He said that he had prepared a letter to the “Secretary of State of the Empire of Japan” and would like to ask the Governor to deliver the letter for him. This request astonished the Shimoda officers, however. They tried to dissuade Rodgers, but to no avail. Finally, they stood up and thanked Rodgers for his kindly reception. They said that they had to leave for another interview with the captain of the *John Hancock*. As for Rodgers’s request, they promised that they would try their best to arrange a meeting with the governor, but given his busy schedule, they asked Rodgers to be patient. They said that a notification would be officially addressed to Rodgers as soon as the meeting was set.<sup>3</sup>

While the officers and crewmen onboard the *Vincennes* were busy receiving their American and Japanese visitors, their squadmates onboard the *John Hancock* were thrilling to tales of the great success achieved by the Russian Navy in its scientific and diplomatic expedition to Japan, concluded only several months previously. They were chatting and having wine with a group of Russian Navy officers who paid the *John Hancock* a visit soon after the steamer’s arrival. The Russian guests told their American hosts that they were officers onboard the Russian frigate *Diana* under the command of Admiral Yevfimiy Vasilyevich Putyatin (1803-1883). In 1852, Emperor Nicholas I decided that Russia could not be left behind in the international race to

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<sup>3</sup> This paragraph is based on the *BGKB* 10, 193-200. 216-218, 245-248.

open Japan. He ordered Putyatin to organize the expedition with the goal of signing the first treaty of amity and commerce with the Japanese government. But the expedition had been replete with uncertainty ever since its departure from St. Petersburg in September 1852. First, Russia went to war with Britain and France, which significantly retarded the progress of the expedition. Second, in August 1853, when Putyatin led a squadron of four vessels to Nagasaki, the Japanese government exhibited the most reluctant attitude toward negotiations with the Admiral. For the next *year* or so, Putyatin steered the frigate *Diana* (the other three vessels had been detached from the expedition to join the war) to visit nearly every chief harbor in Japan, including Nagasaki, Hakodate, and Osaka. It was not until December 1854, when Putyatin arrived in Shimoda, that the Japanese government sent officers to negotiate the terms of the first Russo-Japanese Treaty of Commerce and Amity. But just when the negotiations had reached the critical stage, an unexpected incident occurred. On December 11, a tremendous earthquake nearly destroyed the entire town of Shimoda, and the *Diana* was seriously damaged. Putyatin was not discouraged. He asked the local officials to escort the *Diana* to some well-sheltered harbor nearby so that he could repair the vessel and avoid run-ins with British and French warships. The Shimoda officers assented. Early in January 1855, they dispatched a fleet to pull the *Diana* to a bay named Suruga. But just when the *Diana* was about to reach its destination, a sudden squall capsized her, and she sank instantly. Still not discouraged, Putyatin pressed the Shimoda officers for a harbor with abundant timber resources. He planned to build a launch from scratch and sail it back to Russia to ask for reinforcements. The Shimoda officers again assented.

They escorted the Russian officers to another port called Heda. There, Putyatin's negotiations with the Japanese government continued. In February 1855, he signed a treaty with representatives of the Japanese government. Two months later, the launch *Heda* (named after the port) was ready for the sea, and Putyatin and some 47 crewmen soon made their way to Petropavlovsk.<sup>4</sup> At the time the *John Hancock* arrived, some three hundred Russian officers and crewmen still remained in Shimoda. They had been daily waiting for the arrival of reinforcements from their mother country.

Then the Shimoda officers came and asked to board the *John Hancock*. Captain Stevens, following Commander Rodgers's example, escorted the officers to the wardroom and served them glasses of wine. Then, with the help of the interpreter and probably under the effect of alcohol, the interview went pleasantly. Finally, with the interview completed and the Shimoda officers relaxing and enjoying the wine, the crewmen of the *John Hancock* asked if they could take a close look at the officers' swords. The Shimoda officers shook their heads violently, as if nothing could induce them to draw their swords. Nevertheless, after emptying their glasses over and over again, their attitude softened. They gestured that they could hardly enjoy wine while wearing swords. They even implied that the Americans could take this rare chance to examine their swords. Then, after some reflection, the Shimoda officers pointed to the stateroom and gestured that the stateroom would be an ideal place where

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<sup>4</sup> Many studies have described this significant epoch in the Russo-Japan relations. My account here is based on E. Bretschneider, *History of European Botanical Discoveries in China* (London: Sampson Low, Marston and Co., 1898), 577-580.

they could show their swords in comfort. Then they got to their feet, moving to the stateroom, and a number of American officers followed suit.<sup>5</sup>

Alexander Wylly Habersham, the first lieutenant of the *John Hancock*, did not go to the stateroom with his messmates. He remained in the wardroom with the Russian officers and the Japanese interpreter. One of the Russians, while staring the Shimoda officers' movement, suddenly spoke out. "I'll tell you what it is!" he exclaimed. "These fellows talk very smoothly now, and promise you every thing you ask for; but wait until the time comes to fulfil their promises and see how they will act. The only way our admiral could get along with them was by getting the men under arms and threatening to march them upon Yeddo whenever they promised without acting; and, if you ever expect them to bring you half the provisions they have put on that list, your commodore will have to do the same thing."<sup>6</sup>

Sitting opposite this groaning Russian was the Japanese interpreter. When the Russian concluded his speech, Habersham heard the interpreter angrily whispering "Russe no good! Ameliken *very* good!" Then, while the three parties sat in silence, the Shimoda officers reentered the wardroom and gestured that it was time to leave. Satisfied with the reception, they said through the interpreter that the governor of Shimoda had given the crew of the *John Hancock* permission to go ashore. But the "Ameliken" could only stay onshore for a little while and were not allowed to ramble far, they emphasized. These words annoyed Captain Stevens. He called the interpreter back and demanded he translate the following words: "Commodore

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<sup>5</sup> Habersham, *My Last Cruise*, 200-201.

<sup>6</sup> Habersham, *My Last Cruise*, 201.



Perry had made a treaty with Japan, and we had a copy of it on board for our guidance. That treaty granted us the privilege of going on shore when it suited us; and though we fully appreciated the attention of the governor in noticing our arrival, still, we could not look to him to regulate our movements.” The Shimoda officers listened to the interpreter’s translation and replied that they understood the Americans’ desire, but it was no exception to the rules could be made. Then they bowed, boarded their boat, and sailed away.<sup>7</sup>

Thus ended the first round of negotiations between members of the expedition and the Shimoda officers. The commanding officers Rodgers and Stevens learned that their Japanese counterparts were not naïve and perhaps more strategic (or “cunning,” as Habersham put it in his journal) than they had anticipated.<sup>8</sup>

**“A considerable number of the plants remind me of home”**

Wright was truly amazed by the flora of Shimoda. “I found the flora so rich that I was on no excursion able to penetrate more than a mile or two into the country before my time was expired and my portfolio filled,” he exclaimed to John and Gray.<sup>9</sup> After witnessing how the Ryukyu people had dramatically disturbed their natural flora through farming and grazing, he was impressed by how much the flora of Shimoda had been preserved in a state of nature. “There [in Shimoda] the hills and uncultivable vallies are almost in a state of nature—no goats to eat the bushes nor

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<sup>7</sup> Habersham, *My Last Cruise*, 202.

<sup>8</sup> Habersham, *My Last Cruise*, 223 and 271.

<sup>9</sup> CW to JW, 6/10/1855, *CWC*; CW to AG, 6/8/1855, *HL*.

cattle the grass nor hogs to root up the ground,” he told Gray. In Wright’s opinion, the extraordinary botanical wealth of Shimoda had everything to do with the poverty of the local Japanese. “In truth the people [in Shimoda] are more abject slaves than any Negroes in our country.”<sup>10</sup>

Then, upon studying his collections with Endlicher’s and de Candolle’s reference books, Wright identified two aspects that characterized the flora of Shimoda. The first was the flora’s remarkable diversity. As he put it in a letter to John, “I found a greater variety of strange plants than I had ever seen any where on the same limited area.” The second was that he constantly experienced a strong sense of *déjà vu*, as he filled his portfolios with Shimoda’s “strange plants.” “A considerable number of the plants remind me of home,” he wrote home. “Here are pines and cedars ashes & maples, oaks & cherries roses & raspberries hydrangeas & saxifrage &c. A white rose grows wild all over the hills and a beautiful honeysuckle is about as common.”<sup>11</sup>

In Wright’s view, this resemblance between the flora of Shimoda and that of New England served his collecting enterprise well. For the first time, he felt truly confident grouping his specimens according to the natural orders that Endlicher and de Candolle had developed. Equally important, it was the first time that Wright felt

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<sup>10</sup> CW to AG, 6/8/1855, *HL*. Francis Asbury Roe, Navy officer serving onboard the *Vincennes*, was impressed by Shimoda’s botanical wealth, as well. “The vegetation which is very heavy and luxuriant, is mostly confined to the valleys and ravines,” he remarked in his journal. Furthermore, he added, “The pines about Simoda are extremely large trees. It is remarkable that they grow to so great a size, surpassing the general forest growth of America! they are very free from knots & would produce excellent lumber.” Roe, “Journal USS Vincennes 1855,” Folder 4, Box 1, *FARP*.

<sup>11</sup> CW to JW, 6/10/1855, *CWC*.

comfortable botanizing in a foreign land. He found the residents of Shimoda altogether hospitable. When he walked through the streets, no people screamed or ran off or threw stones at him. Moreover, when he located a botanical ground and moved to advance, he never needed to show his pistol or threw crackers to clear the way. By contrast, he found that the Japanese in Shimoda surrounded and greeted him with “Ohio.” At first, he had no clue why these Japanese were interested in the U.S. state, but soon realized that the sound “Ohio” meant “how do you do” in Japanese. Delighted by the hospitality of the local Japanese, Wright tried to recruit them to assist in collecting plants and other specimens of natural history. He afterward informed John of how much he had enjoyed conversing with the local Japanese:

They seemed to have but two opinions on the subject one that I collected for medicine the other that they were for food. The insects too which I collected in a bottle of alcohol they supposed were to be eaten & if I succeed in convincing any that they were for neither of these purpose they were probably lost in conjecture what I wanted with them.<sup>12</sup>

It is not difficult to sense a certain tinge of irony in Wright’s statement. It seems that Wright was telling his brother that collecting for science’s sake belonged to something beyond his Japanese collaborators’ comprehension. But a close inspection of Wright’s Shimoda collections suggests otherwise.<sup>13</sup> To be sure, besides

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<sup>12</sup> CW to JW, 6/10/1855, *CWC*.

<sup>13</sup> I should emphasize here that it is Japanese botanist Tetsuo Koyama’s work under the title *Plant Collecting in Japan and Its Adjoining Territories by the Black Ships* (1994) that drew my attention to those medicinal and food plants contained in Wright’s Shimoda collections. On the basis of Wright’s NPEE collections preserved in the New York Botanical Gardens, Koyama’s volume offers an excellent overview of what Wright actually collected in Japan from a Japanese botanist’s point of view. The

plenty of novelties unknown to botanists of the day, as well as representative species that reveal the floristic relationship between Japan and eastern North America, Wright's Shimoda collections contain some of the most celebrated edible and medicinal plants in Japan's tradition of *materia medica* (or *honzō*). For instance, there is a specimen of *Capsella bursa-pastoris* in Wright's Shimoda collections. Known in English as "shepherd's purse" and in Japanese as *nazuna* 薺 (meaning the plant that "does not have summer," for the species withers before summer), the species has played a significant role in Japanese people's daily lives for centuries. Indeed, *nazuna*, along with water dropwort, cudweed, chickweed, nipplewort, turnip, and radish, are seven critical ingredients used to prepare the special meal to celebrating *Nana kusa no sekku* 七草の節句 or the Festival of Seven Herbs (the seven-herb rice porridge or *nanakusa-gayu*). It has been a custom in Japanese society to have this special porridge on January 7 or *jinjitsu* 人日, "human's day." In Japan's folk religion, the seventh day of the New Year is the birthday of human beings (after the birthdays of a series of animals, for example: chickens, dogs, pigs, and sheep). And it appears that since the early eighteenth century, Japanese society has regarded *nazuna* as an essential herb for preparing the special *jinjitsu* meals (probably because the species's life cycle matches the transition of seasons from winter to spring). Kaibara Ekken 貝原益軒 (1630-1714), for example, composed an entry on *nazuna* in his influential *Yamato honzō* 大和本草 (1709). In it he not only remarks on *nazuna*'s role in *jinjitsu*, its characteristics, habitats, uses, and the like,

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volume helps me focus on the three species mentioned the text: *Capsella bursa-pastoris*, *Polygala japonica*, and *Berberis thunbergii*.

but also quotes a statement made by renowned Chinese writer Su Shi 蘇軾 (1037-1101) as authoritative. According to Su Shi, Ekken tells his readers, *nazuna* was a gift bestowed by Heaven for those who enjoy and prefer a reclusive life. What Ekken meant by this was that—if we return to Su Shi’s original text—there is no difficulty involved in making such an ordinary herb as *nazuna* into a gourmet meal. In fact, Su Shi’s admiration for *nazuna* (or *ji* in Chinese) goes so far as to claim that the herb was effective in treating various illnesses, in particular ulcers. As he put it in a letter to his friend (who then was suffering from ulcers), because the liver is the organ that houses blood, and because *ji* is capable of “harmonizing the *qi* of the liver,” this seemingly insignificant herb would help rid people blood-related illnesses like ulcers.

Another noteworthy specimen in Wright’s Shimoda collections belongs to the species *Polygala japonica* Houtt. Known in Japanese as *itohimehagi*, the plant’s root (*onji* 遠志) is widely used in Chinese and Japanese medicine for “sharpening one’s mental abilities” (which is why the root of *Polygala japonica* is called *onji* in Japanese and *yuanzhi* in Chinese, both of which mean “strengthening or stretching one’s will”). Finally, there is a specimen of *Berberis thunbergii* DC (or *megi* 目木 in Japanese, meaning “the tree of the eye”) in Wright’s Shimoda collection. Commonly known among horticulturalists nowadays as the Japanese berry, *megi* (bark in particular) has been popular in Chinese and Japanese medicine for centuries. Physicians often use the plant’s bark to make drugs to treat eye diseases and disorders.

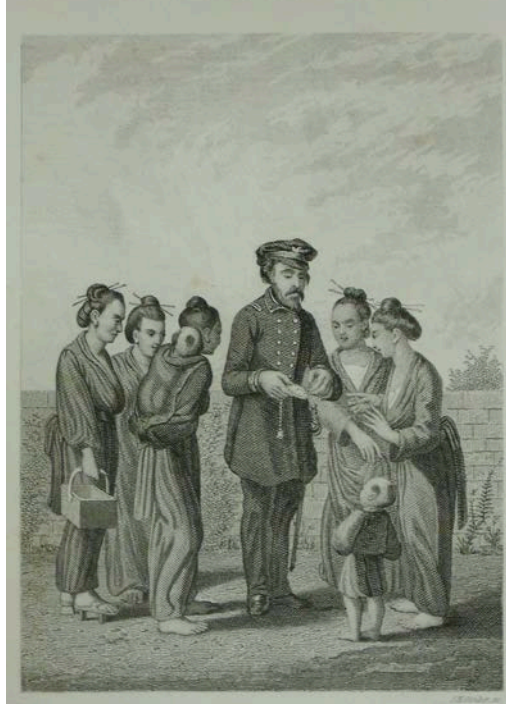


Figure 10-2. "Women admiring a watch in Japan." (From Habersham, *My Last Cruise*, inserted between title pages. Courtesy of the National Museum of Natural History Library, Smithsonian Libraries, Washington, DC, USA.)

In retrospect, whether it was Wright who gathered such useful plants as *nazuna*, *megi*, and *itohimehagi* or his Japanese collaborators, if these plants did appear in Wright's portfolios, and residents of Shimoda did see them, it seems unsurprising that the local Japanese came to regard Wright as a doctor of some sort. As Wright put it in a letter home:

One day as I was passing down the valley at the outlet of which Simodah lies an old woman started after me chattering along I struck across the valley in the direction of a house and supposing she was going there I paid no attention to her but when she manifested the intention of following me farther I turned to see what she wanted when she put her fingers to her eyes & drew down the lids when I could plainly perceive that she wanted to cure them for they were red & very much inflamed.<sup>14</sup>

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<sup>14</sup> CW to JW, 6/10/1855, *CWC*.

But what surprised Wright most was that he could face a Japanese woman and examine her inflamed eyelids. From Wright's perspective, this open attitude spoke profoundly of Japan's superior status in civilization compared to China and Okinawa (Figure 10-2). "I have elsewhere remarked that the Japanese appear to be a superior people to the Chinese," he wrote home. "They are more open frank and manly and manifest a greater degree of inquisitiveness." In fact, as Wright learned afterward, the openness of the Japanese residents of Shimoda could go far beyond his or perhaps any American's standard.

The people are nowadays fastidious about their personal appearance. When at work they through [threw] off all superfluous attire or what they consider such going about with a very narrow strap or strung about their loins as gaily as if totally covered & it is no uncommon thing to see the women at work in their houses or in front of them with their dress thrown entirely off one or both shoulders & supported only by a girdle around the waist. At the bath-houses as I was informed less restrained was practiced (you need not read this aloud unless particularly desired to do so), men & women & children mingled freely together and in the presence of strangers too without a particle of covering but such as nature supplies.<sup>15</sup>

Wright must have learned such fascinating information about Japanese bathers and bathhouses from officers of the *John Hancock*. Immediately after a short visit to Shimoda, officers of the *John Hancock* grew interested in local Japanese bathing customs. They were struck by the sight of male and female bathers mingled together, cleaning themselves and each other in open spaces like rivers and seashores. "I have seen as many as several hundred men, women, and children,—the entire population of villages, apparently,—rolling about in the surf in one promiscuous heap, and all the while yelling and screaming like so many savages,"

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<sup>15</sup> CW to JW, 6/10/1855, *CWC*.

Habersham commented in the journal. On the contrary, he went on, “The inhabitants of the cities of the interior... scarcely ever approach the beach, but patronize the bathing-houses twice, sometimes three times, a day.” After studying the practice of what Habersham called “surf-bath,” a group of NPEE officers decided to pay a visit to a bathhouse in Shimoda. A close inspection of the bathhouse might help them determine why the Japanese were so “fastidious about their personal appearance.”<sup>16</sup>

Arriving in the bathhouse, the officers found a Japanese woman there, apparently a gatekeeper responsible for collecting fees from bathers (a “hideous-looking old woman,” Habersham commented). The officers expressed their interest in studying the Japanese bathhouse. The woman nodded in agreement and gestured for the Americans to sit and make themselves comfortable. Bathers would soon arrive, she remarked. No, the officers replied, they were not here to see the bathers but the *bathhouse*. The woman then pointed out a path leading to the interior of the house, leaving the Americans to their business. Relaxed, the officers studied the architectural style, the heating system, the water pipes, and other aspects. But when they were finished with their study and were ready to leave, they ran into a group of male and female bathers. Now the American officers felt truly embarrassed. They found that they were surrounded by a group of Japanese men and women, curious and chatty as ever, but all naked. Habersham was among the officers who participated in this bathhouse expedition. “Our appearance did not seem to cause them [the Japanese bathers] either surprise or confusion,” he recorded in his journal. “[B]ut, on the contrary, as we tried to prevail on the woman of the house to

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<sup>16</sup> Habersham, *My Last Cruise*, 246.



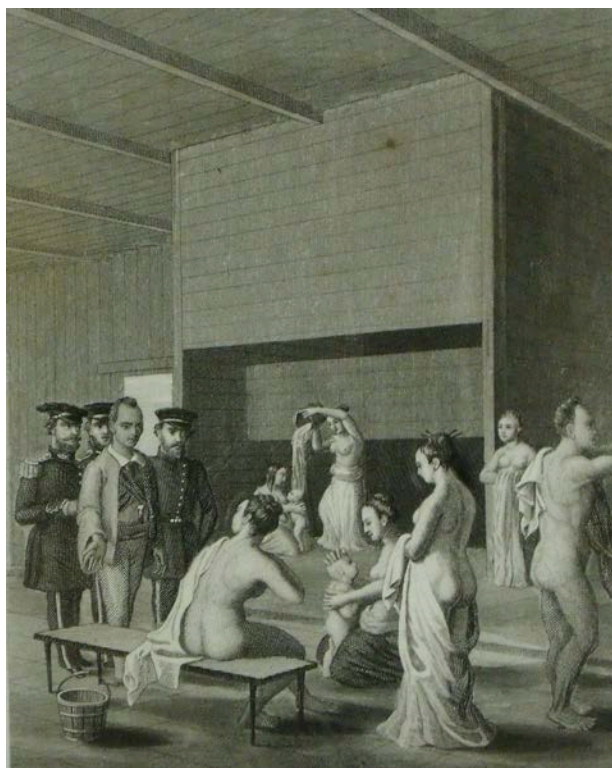


Figure 10-3. “They crowded around in their primitive costume.” (From Habersham, *My Last Cruise*, between p. 242 and p. 243. Courtesy of the National Museum of Natural History Library, Smithsonian Libraries, Washington, DC, USA.)

accept a dime in return for what we had seen, they crowded around in their primitive costume to see what the dispute was about.”<sup>17</sup> Later on, when Habersham published his journal, he added an illustration to demonstrate this exotic (if not erotic) encounter (Figure 10-3).

Besides the bathhouse in Shimoda, another place that captivated the American officers was the “bazaar” that the Shimoda officers set up for their foreign visitors. It was a thatched house that covered about three-fourths of an acre of ground. The shoppers, when entering the bazaar, would immediately encounter a grand array of stalls displaying samples of Japanese goods with price tags on them. The merchants

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<sup>17</sup> Habersham, *My Last Cruise*, 248.

who supplied the goods would stand by the stalls and answer customers' inquiries. Members of the expedition soon detected some remarkable differences between this thatched "shopping center" and similar establishments in cities of the United States. As Wright put it in a letter to John, "All the trading is a Government monopoly and there are officers appointed to superintend the sales to whom all the purchased articles are brought who receive the money and apparently keep an account of the sale & price of every thing sold." As for the ways in which Japanese merchants did their business, continued Wright, "It is exceedingly difficult to make them abate anything from the fixed price which on a slip of paper is appended to each article in Japanese & to many in Arabic characters or our figures as we call them."<sup>18</sup>

Nearly all members of the expedition found these delicate Japanese goods irresistible. "From what we then and have since observed," Habersham noted in his journal, "we all came to the conclusion that they are infinitely superior to all other nations as far as regards the quality of their porcelain, lacquer-ware, and swords."<sup>19</sup> Wright was fascinated, as well.

There is a great number of splendid articles some worth more than \$100. The most elaborate are tea-trays dressing cases writing desks &c. then they have all sorts of dishes cups & all lacquered and beautifully ornamented with gilt or silvered or painted figures of birds beasts insects & plants the figures splendidly colored. There are moreover baskets umbrellas shoves and wet weather dresses made apparently of oiled paper.<sup>20</sup>

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<sup>18</sup> CW to JW, 6/10/1855, *CWC*.

<sup>19</sup> Habersham, *My Last Cruise*, 225-226.

<sup>20</sup> CW to JW, 6/10/1855, *CWC*.

Wright eventually spent some \$80 purchasing a variety of Japanese goods (including a sewing box for Mrs. Gray<sup>21</sup>). “[Y]ou mustn’t tell mother of it,” he confided to John, or “she will say I am monstrous extravagant and that I will die in the poor-house.” Still, Wright thought that he had good reason to purchase the goods. “They are exceedingly neat and pretty articles and I believe you will agree with me when you see them that they are worth the money. The piece which cost the most (\$22.00) is a writing desk which I selfishly laid in for myself but if I should come to poverty in consequence I think I can get \$50 for it in our country.”<sup>22</sup>

### **“A couple of school-girls”**

But while Wright and other members of the expedition enjoyed the natural and cultural curiosities that Shimoda offered, their relationship with the local authorities deteriorated. The catalyst of this change was the seeming omnipresence of the Japanese “spies.” Almost immediately after their arrival, the Shimoda officers erected a small bamboo building near the landing spot of the *Vincennes* and the *John Hancock*. At first, members of the expedition could scarcely conjecture what this bamboo thing was used for, but they soon realized that it was some sort of “government spy-house.” Habersham, after a short conversation with American merchants residing in Shimoda, noted in his journal how intensively and extensively the Shimoda officers kept their visitors under control. “From the moment you enter their ports, they station boats to watch your ships. When you leave your ships, you

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<sup>21</sup> CW to JW, 1/1/1856, *CWC*.

<sup>22</sup> CW to JW, 6/10/1855, *CWC*.

are counted and watched. When you land, you are followed; and when you return on board, you are again counted to see that there are none left on shore. If a less number return than landed, a search is at once got under way and the missing ones are *always found*." Habersham found the whole matter disagreeable. "We were much annoyed during our first walks by the Japanese officials, who, after drinking all of our French punch on board ship, would encounter us on shore and dog our steps to prevent our communicating too freely with the country-people."<sup>23</sup>

So one day Habersham and some officers onboard the *John Hancock* rushed to the "government spy-house" and made their dissatisfaction felt to the officers stationed there. The officers replied that the Americans had made a mistake. The Japanese who followed and recorded their movements were not spies at all but guards committed to protect Shimoda's noble visitors. In fact, they emphasized, the Americans could "send away" these guards whenever desired.<sup>24</sup> Later, when Habersham decided to take an excursion to the edge of Shimoda with a merchant named Bridleman and an officer named Lawton, they decided to test the officers' words. Lawton, upon discovering that they were being followed, approached one of the spies and kicked his ass (quite literally). The kicking worked well. The spies dispersed in confusion, leaving Lawton and his companions to enjoy their excursion about Shimoda. Habersham was much impressed by how the effect a straightforward kicking could generate. "I never saw such a kick in my previous life,"

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<sup>23</sup> Habersham, *My Last Cruise*, 207, 211-213.

<sup>24</sup> Habersham, *My Last Cruise*, 210-213.

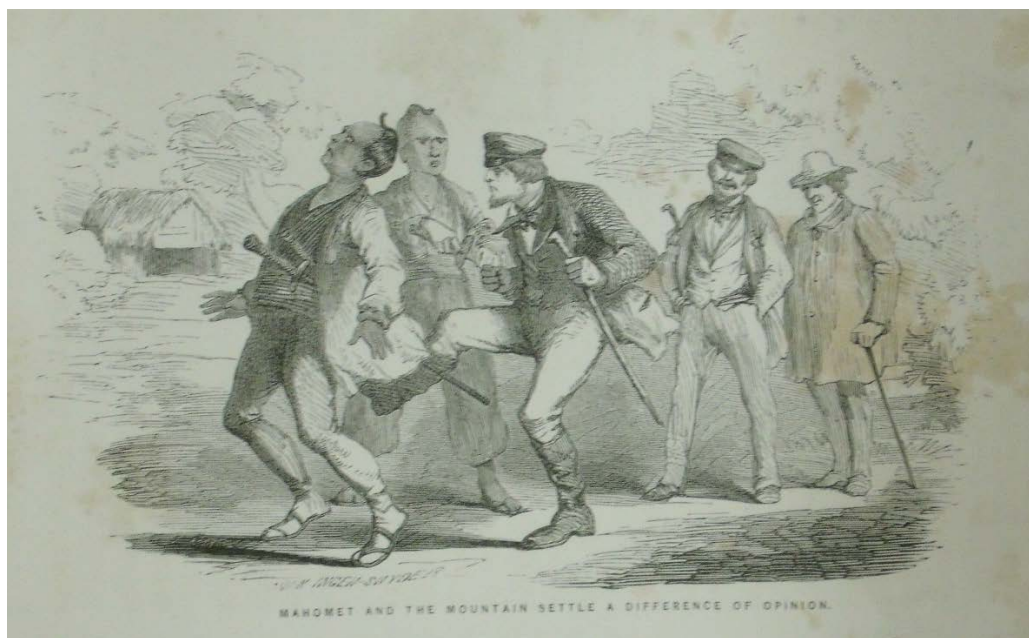


Figure 10-4. “Settle a difference of opinion.” (From Habersham, *My Last Cruise*, between p. 212 and p. 213. Courtesy of National Museum of Natural History Library, Smithsonian Libraries, Washington, DC, USA.)

he wrote admiringly in his journal. Afterward, he added an illustration in the published version of his journal showing how Lawton sent the spies away (Figure 10-4). He also informed his readers that the NPEE officers actually received a stream of thank-you notes from local residents for Lawton’s kicking. Although none of them understood a Japanese word, he noted, he could not help but conclude that “the *people* of Japan are as ripe for revolt as the most violent filibustier could desire, while, at the same time, we were forced to acknowledge that they were *kept under* more effectually than any nation under the sun.”<sup>25</sup>

While Habersham and his colleagues heroically fought against the despotic Shimoda officers on behalf of the “people of Japan,” Commander Rodgers engaged in two issues that he thought would decide the extent to which he could discharge his

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<sup>25</sup> Habersham, *My Last Cruise*, 219.

duties. First, he found that his fellow citizens now residing in Shimoda demanded his particular attention. Given the fact that the U.S. government had not dispatched any man of diplomacy to Japan, he thought that it was his responsibility to negotiate with the Japanese government on behalf of these merchants. Second, though he had prepared the letter to the “Honourable Secretary of State of the Empire of Japan,” he was still waiting for the chance to submit it and begin his survey.<sup>26</sup>

Rodgers brought up these two issues in his second interview with the Shimoda officers. He said that he needed to meet the governor of Shimoda in person to settle the issues. The Shimoda officers shook their heads wearily. They said that the governor was too busy and had neither the time nor the spirit for a personal meeting. This reply annoyed Rodgers. He exclaimed that he would consider it an insult if he could not meet the governor in person. “Please don't think that way,” the officers replied, and asked Rodgers to hand over the letter. They promised that they would submit it to the governor and do their best to arrange a meeting. Rodgers yielded, handing his letter over to the officers.<sup>27</sup>

As promised, the Shimoda officers arranged a meeting with the governor for Rodgers. The meeting would take place on May 13 at the bazaar, they informed the commander. Rodgers was delighted. He regarded the meeting as a monumental occasion at which he could show his “passport to observation.” As the date of the

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<sup>26</sup> “The enterprise of my countrymen in thus colonizing Japan, so soon after the treaty, has forced upon my attention some subjects which I beg to discuss.” See JR to JDD, 6/11/1855, in Cole, *Yankee Surveyors...*, 57.

<sup>27</sup> *BGKB* 10, 222-227, 289-294; on the Japanese translation of Rodgers's letter, see pp. 227-231.

meeting approached, he ordered his men to ready their uniforms, polish their boots, keep their hats in excellent shape, check the condition of their pistols, and so on. On the day of the meeting, he assembled a well-armed group of sailors and officers and ordered some sailors to play instruments (two drums and one fife) as they were marching to the bazaar. He was satisfied to see the local Japanese crowding the sides of streets and gazing at his men in awe. “[T]o judge from the great numbers of the fair sex that crowded the streets through which we passed,” Habersham commented in his journal, “there were weak heads in Japan also, who, like two out of three similar heads in other parts of the world, were too apt to judge birds by their feathers.”<sup>28</sup>

But Rodgers’s display of power was not impressive enough for him to gain the upper hand during his negotiations with the governor. The governor said that he had delivered Rodgers’s message to his superiors but had not received a reply. Thus, he demanded Rodgers not pursue any surveying project, or the friendship between the two countries would be undermined. As for the American merchants now residing in Shimoda, the governor said that no exception could be made for them. These merchants had to get out of Shimoda immediately.<sup>29</sup>

Rodgers was disappointed by how little he had gained after showing his “passport to observation.” Returning to the *Vincennes*, he promptly produced a long letter to “His Excellency the Governor of Simoda.” He first called the governor’s attention to the fifth article of the Treaty of Kanagawa, in which the United States

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<sup>28</sup> Habersham, *My Last Cruise*, 223.

<sup>29</sup> *BGKB* 10, 312-325.

and Japan had agreed, "Shipwrecked mariners, and other citizens of the United States temporarily being at Simoda, and Hakodadi [*sic*], shall not be subject to such restrictions, and confinement, as the Dutch and Chinese are at Nangasaki [*sic*], but should be free &c. &c." According to this particular article, he continued, it was apparent that Shimoda officers had violated the treaty. He then hinted at how the U.S. government would deal with those countries who broke their treaties. About a decade ago, he wrote, the U.S. government had troubles with the Mexican government regarding U.S. citizens residing in the Mexican territory in North America. "At last after fruitless attempts to obtain justice," he wrote, "we made war, took all their fortresses which opposed us, took their capital the city of Mexico, and overran their country." In light of this, he remarked, "I think the wise way would be for the Governors of Simoda, and Hakodadi, to be patient, and to avoid rash deeds. The American government is just, and peaceable, and honorable, as well as strong." "Let these Americans leave quietly here, or go, to Hakodadi, let them do every thing which Americans think is in the treaty, remonstrate with the American government and it will be time enough to get angry, and do things by force, after the remonstrance fails to receive attention."<sup>30</sup>

The letter did not produce any effect. In the weeks to come, Rodgers produced more letters and arranged more meetings with Shimoda officers.<sup>31</sup> He ordered his men to be careful about their interactions with Shimoda officers and common people alike. He wanted to ensure his negotiations went smoothly. For some men

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<sup>30</sup> JR to "His Excellency The Governor of Simoda," 5/20/1855, F87-90, R6, *REENP*; also Cole, *Yankee Surveyors...*, 106- 111.

<sup>31</sup> *BGKB* 10, 390-398, 422-432.



under Rodgers's command, however, the commander's interest in diplomatic matters had severely impeded the progress of the expedition. "I had not here [in Shimoda] the very best facilities," Wright complained to Gray. "I try to give as little trouble as possible but I shall have to complain if gentlemen who do nothing can at any time get a boat to go on a visit or a frolic & I can't get one to go ashore on duty." In Wright's opinion, Rodgers's reluctance to adopt a staunch attitude toward the Shimoda officers had made these "men of war" act like "a couple of school-girls."

We found here some Yankee adventurers come to seek a fortune accompanied by their wives & children. Boats were employed in affording facilities for giving & returning visits between the ship & these people onshore. Stimpson was deprived of the use of the boat which had served him to dredge in and of the services of his assistant who was set to watch the tide-grunge and in supreme disgust our Zoologist hitherto indefatigable lounged about most of the time and did little but go to the bazaar & purchase Japanese curiosities.<sup>32</sup>

Rodgers also grew worried about the delay of the expedition. Toward the end of May, he decided to make a strategic move. He communicated to the Shimoda officers that he intended to send the *John Hancock* to Heda to visit the Russian officers residing there. As a matter of courtesy, he wrote, he felt obliged to reciprocate the Russian officers' previous visit to the *John Hancock*. Rodgers then received a stream of replies insisting that a visit was not permitted ("Should it have become necessary to communicate with the Russians, they shall be called here to this place").<sup>33</sup> Now Rodgers became excessively tired of dealing with these "petty officers." "The American Commodore has power to ask permission to survey where he wishes," he

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<sup>32</sup> CW to AG, 6/8/1855, *HL*.

<sup>33</sup> Cole, *Yankee Surveyors...*, 64; also see *BGKB* 10, 325-340, 361-362, 365-367.

replied, “but has the Governor of Simoda power to refuse him permission, or to grant it to him for other parts of the Empire of Japan?”<sup>34</sup>

On the morning of May 24, the *John Hancock* set sail for Heda, paid a visit to the Russian officers, made a brief survey of the harbor, and returned to Shimoda the next day. Then, with Rodgers and his men breathlessly awaiting a response from the Shimoda officers—nothing happened.<sup>35</sup> Encouraged, Rodgers began devising a more ambitious scheme. He deemed it necessary to survey the coastline from Shimoda to Hakodate. In fact, in April 1855, when the *Vincennes* and the *John Hancock* had started for Japan, he had ordered Captain Gibson to command the *Fenimore Cooper* to survey the route that might connect the Ryukyu Islands with Hakodate along with the *western* coastline of the Japanese Archipelago.<sup>36</sup> In other words, if Rodgers could overwhelm the Shimoda officers and successfully launch a survey of the *eastern* coastline between Shimoda and Hakodate, the expedition would gather sufficient information about the *whole* coastline of Japan. It would be an unprecedented contribution to U.S.-Japan relations.

With such an agenda in mind, Rodgers facetiously asked Shimoda officers for permission. He was unsurprised that the officers once again bombarded him with letters declaring that the proposed survey could not take place. (“Should a vessel wantonly run into any harbor it cannot be sure that there may not occur unpleasant

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<sup>34</sup> Cole, *Yankee Surveyors...*, 64-65; the Japanese version of this paragraph can be found in *BGKB* 10, 366-367.

<sup>35</sup> Habersham, *My Last Cruise*, 256.

<sup>36</sup> Cole, *Yankee Surveyors...*, 53-56; regarding Gibson’s survey, see pp. 71-87.

difficulties, and our friendship might be really weakened.”<sup>37</sup>) But Rodgers decided he did not care about these petty officers’ opinions anymore. He ordered his men to cut down some trees near the harbor of Shimoda to outfit a launch for the *Vincennes*. Toward the end of May, the launch—now named the *Vincennes Jr.*—with one Dahlgren gun, twelve pounder howitzers, and forty rounds of ammunition—was ready for the sea. Rodgers ordered the astronomer John M. Brooke to command the launch. He estimated that the whole survey would take a month or so from Shimoda to Hakodate. Given the limited size of the *Vincennes Jr.*, he decided that Brooke should only take a crew of fifteen men (“all armed to the teeth and ready to go *anywhere* in spite of *any thing* the Japanese might say or do,” Habersham commented), with provisions for fifteen days—meaning that Brooke would have to ask for provisions from some villages or towns located between Shimoda and Hakodate.<sup>38</sup>

Rodgers felt that it was time to leave Shimoda for Hakodate. On May 28, the three vessels, the *Vincennes*, the *Vincennes Jr.*, and the *John Hancock*, put out to sea. Looking back, Rodgers thought that he knew how to deal with the Japanese authorities. “Words without the authority of many cannon will avail little,” he noted in a letter to Secretary Dobbin.<sup>39</sup> “[W]e knew well enough that the Japanese were like a large, savage, but quiescent dog,” Habersham remarked in his journal. “[W]alk by him in a quiet, cool, unconcerned manner, and he will probably content himself

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<sup>37</sup> Cole, *Yankee Surveyors...*, 66; BGKB 10, 477-478.

<sup>38</sup> Habersham, *My Last Cruise*, 257.

<sup>39</sup> JR to JDD, 6/11/1855, Cole, *Yankee Surveyors...*, 61.

with keeping his eye upon you; whereas, if you hesitate in your advance, he will more than probably spring at your throat.”<sup>40</sup>

### **“A national crisis”**

The one Rodgers considered the “governor” (or *bugyō* in Japanese) was actually three persons: Izawa Masayoshi 伊沢政義 (?-1864), Tsuzuki Mineshige 都筑峯重 (1803-1858), and Inoue Kiyonao 井上清直 (1809-1868). All of them were experienced officers and had learned a great deal about how to handle various requests made by foreign powers during their previous negotiations, first with the United States and then with Russia and Britain. Still, they found it difficult to stomach Rodgers’s request to open Japan’s coast and harbors for a through scientific survey. It was the first time that they had received an application made in the name of science. They knew they had to be careful.

So from the outset the three men of diplomacy were careful about making a complete record of their intercourse with Rodgers and the other NPEE members. For example, after their first meeting with Rodgers, they promptly composed a report to the elder of the bakufu, along with a translation of the dialogue between them and Rodgers. They let the elder know that Rodgers was extremely serious about his request and probably would not hesitate to deploy certain military means to achieve his aims. In any case, they urged the bakufu to send out concrete

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<sup>40</sup> Habersham, *My Last Cruise*, 257.

instructions for the local officers' reference.<sup>41</sup> Such strong messages got the elder's attention. In April 1855, he issued an order clarifying the grounds upon which any interaction with the NPEE officers was to be based. Given the fact that no precedent could be found in Japan's past interactions with foreign countries, the elder acknowledged, Rodgers's application could not be permitted.<sup>42</sup> However, aware of Rodgers's resolve, the elder was unsure whether the officers in Shimoda and Hakodate could enforce the order. Concerned, he decided to take a step rarely seen in the policy-making process of the day: He distributed Rodgers's message among the *daimyo*, or local lords, and asked for their opinions. By doing so, he elevated Rodgers's expedition to the level of a national crisis.

It was against this context that the *Vincennes Jr.* undertook its surveying cruise from Shimoda to Hakodate. Brooke, understanding that the survey had no official permission, did his best to avoid interacting with local people and officers alike. But he still had to draw the *Vincennes Jr.* close to the shore, either to get provisions or to make observations. And whenever he did so, he found that the *Vincennes Jr.* provoked great excitement. For instance, when the *Vincennes Jr.* anchored in a bay close to a village called "Sino Hama" (today's Shirahama), Brooke found that their movements attracted some five or six hundred Japanese people. As he put it in a report to Rodgers, these curious fellows so crowded the beach that the crewmen of the *Vincennes Jr.* could only "with difficulty find a footing upon the shore." Another remarkable encounter took place in the town "Isokona." Brooke informed

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<sup>41</sup> BGKB 10, 312-325, 340-342, 443-451.

<sup>42</sup> BGKB 11, 1-2.

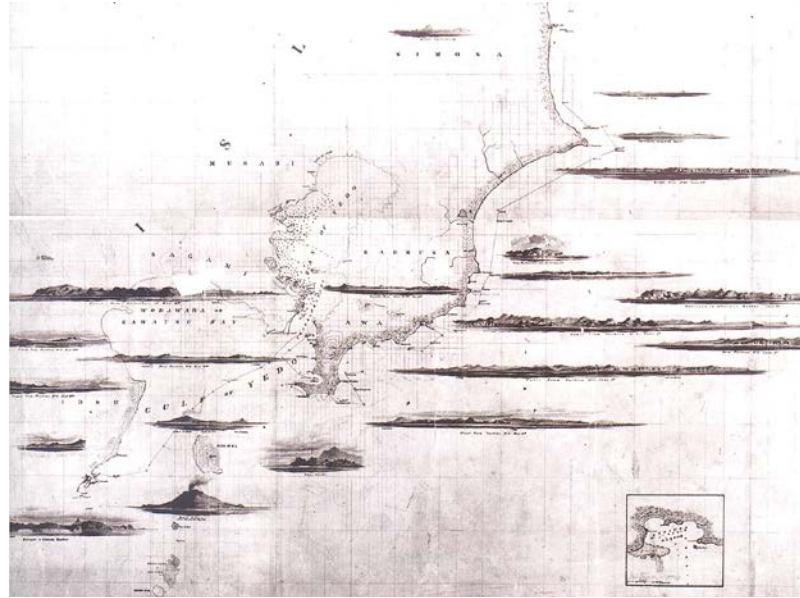


Figure 10-5. The map produced by Brooke based on his reconnaissance of the coastline from Shimoda to Hakodate (From Hayes, *Historical Atlas of the North Pacific Ocean*, 147).

Rodgers, “Between four and five thousand persons of both sexes assembled upon the shore” and “intensely gazed” at their movements. His attempt to be invisible had failed.<sup>43</sup>

Regardless, Brooke found his surveying cruise from Shimoda to Hakodate enjoyable and successful (Figure 10-5). He had no doubt that the survey could have been otherwise, had Japanese officers and common people not offered their assistance. What particularly impressed him was the reception he received from the officers of “Isokona.”

At daylight on the 11th we weighed and accompanied by numerous boats stood out to sea. The Isokona officials came after us in hot pursuit, and we luffed to the wind for them. The chief by word and gesture pressed us to return, he seemed to entertain a real affection for us, told us that if we would delay our departure the Governor or prince would visit us, that he was coming, and that we should have all the luxuries that the land afforded; and when at last I pointed the open sea and gently put his hand aside, he stepped back into his boat and perfectly

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<sup>43</sup> JMB to JR, 6/22/1855, Cole, *Yankee Surveyors...*, 89, 93.

dejected regarded us with a longing look as we sailed away; and until beyond the range of vision we could see the motionless figure of Captain Rice. The kindness of this man made a deep impression upon our crew, and he was often the subject of conversation.<sup>44</sup>

Brooke certainly could not have fathomed the scope of the excitement that the cruise of the *Vincennes Jr.* had provoked. In fact, when the *Vincennes Jr.* anchored in Shirahama and his men landed and asked for provisions, local officers trailed them, documented their activities, made a report, and circulated it to various levels of the government. The so-called Captain Rice, too, had followed the same steps to keep his superiors fully informed.<sup>45</sup>

Rodgers's negligence irritated a great portion of the Japanese officers. Even those who had been advocating an open and friendly attitude toward foreigners found the American commander's behavior despicable. For example, Iwase Tadanari 岩瀬忠震 (1818-1861), a Confucian scholar and a man of diplomacy who had played a critical role in negotiating with Commodore Perry, suggested to his superiors that if Rodgers and his men continued their unpermitted surveys, the bakufu should arrest them.<sup>46</sup> This rising annoyance with Rodgers and his men was made explicit in a memoir submitted to the elder by the *Ōmetsuke* 大目付, an official post responsible for inspecting officers' activities at various levels of the government. The *Ōmetsuke* first outlined Rodgers's misbehaviors and misdemeanors committed in Shimoda: for example, his interest in getting

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<sup>44</sup> JMB to JR, 6/22/1855, Cole, *Yankee Surveyors...*, 96.

<sup>45</sup> BGKB 11, 5-13, 96-97, 105-106.

<sup>46</sup> BGKB 11, 107-109, 183-190; also see Makabe, *Tokugawa kōki no gakumon to seiji*, 479-481.

permission for American merchants who had illegally set foot in Japan, his intentional negligence of local rules, his frequent threats that he would use military power to conduct the survey, and so on. Such instances, he commented, had sufficiently shown the true colors of this so-called scientific expedition. Instead of contributing to current knowledge about Japan, the *Ōmetsuke* argued that Commander Rodgers must have a very different agenda in mind. Although he could scarcely fathom what that agenda might be, he suggested that the elder should deploy every possible means to stop Rodgers's survey of Japan. If the elder failed to do so, he warned, Japan's current relationships with foreign powers would deteriorate, not to mention that greedy barbarians in Russia, Britain, and elsewhere might follow Rodgers's example. The *Ōmetsuke* pointed out that activists might ally themselves with foreign powers, or use the elder's incapability of handling Rodgers's expedition as an excuse to provoke anti-government sentiment. In either case, he noted, the governing regime of Japan that had lasted for two hundred years would be shaken to its very foundation.<sup>47</sup>

### **"The treaty cost us several millions"**

The *John Hancock* and the *Vincennes* sailed into the harbor of Hakodate on June 4 and on June 7, respectively. Takeuchi Yasunori 竹内保徳, "governor" of Hakodate, was disappointed to find Rodgers a belligerent and impatient man. What particularly annoyed him were the American commander's frequent threats that we would attack Hakodate or sail the squadron to Edo Bay if the Japanese government

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<sup>47</sup> *BGKB* 11, 15-23.



did not fulfill his wishes. Takeuchi did not hesitate to make his disagreement felt to Rodgers. In a letter to Rodgers in June 1855, for example, he claimed that he could hardly understand why Rodgers had made the following statement only because Japanese officers, in accordance with the Treaty of Kanagawa, had moved to expel American merchants who intended “not to stay temporarily” on Japan’s shore: “Your Excellency states that a written answer is necessary to be sent to the United States, and that in consequence a fleet of war would arrive.” After receiving a reply, in which Rodgers asked for a clear definition of the term “temporarily,” he wrote back that that common sense suggested that “to stay temporarily” meant to stay for a period of “five or seven days to the utmost a few months.” He told Rodgers that those American merchants had brought their wives and children, which meant that they had planned to “stay not temporarily, but longer.”<sup>48</sup>

Rodgers thought that it was time to give up. On June 18, he wrote to the governor that he would leave the matter of the American merchants in the hands of the Japanese government. “You will probably be surprised, and annoyed, at the interest I take in their affairs—My government requires it of me, and it is by the protection, and care, which the United States extends over its citizens wherever they may be, that while they observe the laws they go [in] safety to all lands.”<sup>49</sup> On the same day, he told W. C. Reed, T. T. Dougherty, and H. W. Peabody, the representatives of the American merchants in Japan, that he could no longer get

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<sup>48</sup> Cole, *Yankee Surveyors...*, 112-113. The Japanese translation of Rodgers’s letters can be found in *BGKB* 11, 275-279, 298-304, 315-320, 388-406.

<sup>49</sup> JR to “His Excellency, The Governor of Hakodadi,” 6/18/1855, F97, R6, *REENP*. The Japanese translation of this particular letter can be found in *BGKB* 11, 327-330.

involved in any diplomatic matters. No matter what losses they might have to bear, the American merchants had to leave.

The recipients of Rodgers's letter were disappointed. During their stay in Japan, they replied, they had suffered tremendously from all sorts of injustices at the hands of the Japanese government. Upon their return, they declared, they would "hold our Government responsible for all damages arising from this refusal, and claim this to be to all intents, and purposes, our protest against the Japanese Government, reserving to ourselves, the right of Extending this Protest, at any future time, and place."<sup>50</sup>

On June 26, the merchants and their families left Hakodate for the United States.<sup>51</sup> Habersham reported what happened to them in his journal:

Upon arriving at San Francisco and applying at Washington for indemnification for the losses they had sustained through the palpable treachery of the Japanese, they were informed that the phrase "temporary residence" did not mean temporary residence; that they had nothing to complain of; that the phrase meant, as the Japanese said, "a day's walk into the country," or "a few days on shore," or something equally absurd. I wonder when any more Americans will risk their capital upon the treaty, which cost us several millions?<sup>52</sup>

### **"Everything seems reversed"**

Indeed, the extent to which Perry's treaty had advanced the commercial and diplomatic relationships between Japan and the United States became an issue for the NPEE's members. First, they quickly came to believe that Hakodate officials had

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<sup>50</sup> WCR, TTD, and HWP to JR, 6/19/1855, Cole, *Yankee Surveyors...*, 117-118.

<sup>51</sup> The departure date of the *Foote* is based on a letter from Wright to Gray, dated June 8, 1855, in *HL* collection.

<sup>52</sup> Habersham, *My Last Cruise*, 294.

no interest in fulfilling the most basic article of the Treaty of Kanagawa: supplying American vessels with necessary provisions. As Wright put it in a letter home, “We have no fresh meat but between the authorities and our own nets we are able to obtain a tolerable quantity of fresh fish yet not so much as we could consume.”<sup>53</sup> The officers of the *John Hancock* were the first made their dissatisfaction felt to local authorities. In a meeting held on board the vessel, they said that they had noticed dozens of the “finest bullocks” ranging the neighboring hills and wondered if they could purchase them for fresh meat. The Hakodate officers replied that these bullocks were used by local people as “beasts of burden” and hence Americans could not purchase them. However, they added, if Americans were desperate for fresh meat, horsemeat would be an available option.<sup>54</sup> The Hakodate officers’ reply amazed the members of the NPEE. “Everything seems reversed,” Wright commented in a letter home.<sup>55</sup>

The members of the expedition declined the offer of fresh horsemeat. They declared that they would “do some fishery” in Hakodate Bay. The Hakodate officers were concerned by this declaration. They immediately returned to the *John Hancock*, agreeing to supply fresh fish at a reasonable price. The next day, they delivered three baskets of salmon and asked for \$4 in total. However, upon inspecting the

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<sup>53</sup> CW to JW, 6/10/1855, *CWC*.

<sup>54</sup> Habersham, *My Last Cruise*, 209-210.

<sup>55</sup> Wright suspected that it was because of the Japanese people’s belief in Buddhism that rendered bullocks as carriers and horses as food—a perfect contrast to what he had been familiar with in the United States. CW to JW, 6/10/1855, *CWC*.

salmons, officers of the *John Hancock* found that the gills had turned green. They threw the so-called fresh fish overboard.<sup>56</sup>

The affair did not end there. Afterward, when officers of the *John Hancock* noticed Hakodate officers brutally driving off some poor people who were picking up the salmon with the green gills, they were furious. It was imperative to intervene, they thought. As Habersham put it in his journal, having been considerably “emboldened” by what they had experienced in Shimoda, officers of the *John Hancock* gave Hakodate officers “several hearty kicks” and called the poor people back. Then, with “extreme disgust,” they witnessed one of the poor people “at once commenced eating his fish raw.”<sup>57</sup>

Even the bazaar gradually lost its attraction. “We have now had a pretty fair opportunity to test the value of Com. Perry’s treaty,” Wright wrote home. He said that he had examined the curiosities being displayed at the bazaar in Hakodate and decided not to purchase anything. He had become aware of a fatal mistake inherent in the Treaty of Kanagawa.

By a strange oversight with regard to the relative value of Chinese & Japanese cash (small copper coin) the value of our dollar was depreciated to that of the Japanese Itzebu which is really worth but about thirty cents—so that for any article the price of which is an Itzebu we have to pay a dollar—but we get no Itzebu in return for the dollars are immediately taken to the mint and coined into Japanese money. Thus in all our purchase we pay really three times the Japanese price of the article bought.<sup>58</sup>

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<sup>56</sup> Habersham, *My Last Cruise*, 271-272.

<sup>57</sup> Habersham, *My Last Cruise*, 272-273.

<sup>58</sup> CW to JW, 6/10/1855, *CWC*.

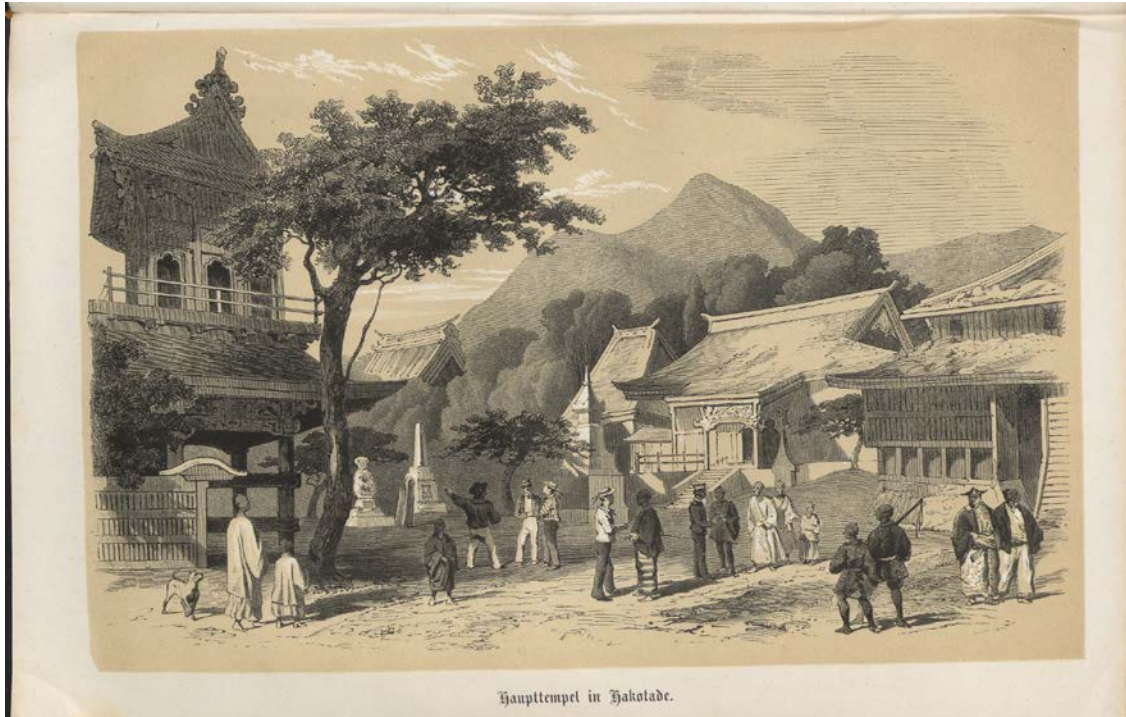


Figure 10-6. Exploring the “backstage” of Hakodate. (From Heine, *Die Expedition... Vol. 3*, plate 2. Courtesy of the Widener Library, Harvard University, Cambridge, MA, USA.)

“How was so great a mistake committed by Commodore Perry?” Habersham exclaimed in his journal. To Habersham, the mistake must have had something to do with the “cunning” of the Japanese government. He reasoned that Commodore Perry must have had been “cheated” when signing the treaty.<sup>59</sup>

Resented, members of the NPEE no longer regarded the treaty as the basis by which to interact with Japanese officers and common people. A direct consequence of this understanding was that they ignored those exotics on display at the bazaar but aspired to explore the “backstage” of Hakodate to collect curiosities—natural and cultural alike (Figure 10-6). One day, when Habersham and other NPEE members were wandering about Hakodate, they ran into a Japanese farmer with

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<sup>59</sup> Habersham, *My Last Cruise*, 223 and 271.

baskets of chickens. They felt their mouths watering. Since their arrival in Japan, they had hardly tasted any animal other than fish. So they approached the farmer and gestured that they wanted to purchase his chickens with American dollars. The farmer shook his head. Then one of Habersham's companions came up with a novel idea. He cut off one of his buttons and showed it to the farmer. The farmer nodded in agreement. The Americans then happily amassed some buttons to exchange for the chickens. Afterward, Habersham applied the same strategy to collect paintings of what he understood as Japan's "household gods."

The next day a Hakodate officer came onboard the *John Hancock* and asked for a meeting with Captain Stevens. Habersham recorded the officer's words as follows: "some Americans had been many miles back in the country and bought some poor people's household gods, and that another party had forced a poor man to sell them all of his singing-birds." The officer brought all the buttons that Habersham and his friends had used, asking the Americans to return the "household gods" and "singing-birds." To his disappointment, he found that the farmer's chickens had been entirely consumed. Exasperated, he insisted that Habersham should relinquish the paintings at least.<sup>60</sup>

Wright exploited the chance that this seemingly anarchical circumstance had offered as well. He threw himself into exploring the flora of Hakodate with refreshed enthusiasm. On June 22, when he tried to conquer a mountain adjoining Hakodate Bay, he shot a bear cub and a hawk. He understood that by acquiring the two zoological specimens he had violated the treaty, which stipulated that Americans

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<sup>60</sup> Habersham, *My Last Cruise*, 270-271.

were not allowed to fire any firearms onshore for any reason. Wright felt compelled to pull the trigger as he spotted these two prizes nevertheless. He considered the dead cub and hawk “a trophy” of his shooting skill, he told Gray proudly.<sup>61</sup>

Wright’s pride in his shooting was so overwhelming that he even dared to show his game to the Hakodate officers. He was not surprised that the same officers soon visited the *Vincennes* declaring that Wright had violated the Treaty of Kanagawa. Wright felt indifferent about the charge. To him the regulation of firearms made no sense. “It is my belief that this course is taken to prevent the knowledge of the efficacy of foreign firearms being communicated to the people,” he wrote home.<sup>62</sup>

### **“As one pea is like another”**

As in Shimoda, Wright was greatly impressed by the striking similarities between the flora of Hakodate and that of eastern North America. In his letters to John and Gray, he noted here and there that he had collected plants that reminded him of home: a horse-chestnut, a magnolia, a beech, a chestnut, a Geranium, a Solomon’s seal, a Hydrangea, a honeysuckle, buttercaps, alders, a ladies’ shipper, “a curious broad-leafed onion,” violets, lilies, a huckleberry, and so on. As this list became longer and longer, Wright could not help wondering why the Japanese in Hakodate did not make use of their land as New Englanders had done. As he told John, “June 23rd Yesterday I succeeded in reaching the top of the mountains toward which I started the other day I found all the way the same rich soil the same luxuriant

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<sup>61</sup> CW to AG, 6/8/1855, *HL*.

<sup>62</sup> CW to JW, 6/10/1855, *CWC*.

vegetation and the same evidence of a miserable people to whom such a fertile heritage is of little or no value.”<sup>63</sup>

Wright did not keep this discovery to himself. He immediately reported it to Rodgers and afterward made a collection to prove his point. (“I made a choice collection of plants.”<sup>64</sup>) Delighted, Rodgers promptly reported it to Secretary Dobbin and revealed the discovery’s significance from the viewpoint of U.S.-Japan relations:

The botanist, Mr. Wright, who has roamed over Texas and other very fruitful soils, does not know where he could find better land than the waste plains here. We should hire the fields at a price fixed by ourselves, with liberty to cultivate them in our own manner, with Americans, or Japanese, or Chinese. Thus only can supplies for ships be ensured.<sup>65</sup>

In November 1857, when Gray came to examine Wright’s Hakodate collections, he was surprised to find that nearly every specimen therein indicated some sort of floristic relationship between northern Japan and eastern North America. For example, as he put it in his 1859 essay on Japan’s flora, Wright had collected the famous *Wisteria sinensis* in Hakodate, which had “a strict representative” east of the Mississippi under the name *W. frutescens*. Also, he noticed that Wright’s Hakodate collections indicated that a great number of Japanese species bore close generic relationships to American species. For example, he noted in his 1859 paper that a

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<sup>63</sup> CW to JW, 6/10/1855, *CWC*.

<sup>64</sup> Wright made this statement after finishing his expedition to the mountains of Hakodate on June 23; see CW to JW, 6/10/1855, *CWC*.

<sup>65</sup> JR to JDD, 6/19/1855, Cole, *Yankee Surveyors...*, 101.



“congener” of the famous skunk cabbage of North America (*Symplocarpus foetidus*) could be found in Hakodate as well.<sup>66</sup>

Yet, as far as the issue of disjoined species was concerned, Gray understood that he needed more definite cases to show that identical—not merely similar or congeneric—species could be found both in Japan and eastern North America. To this end he found Wright’s Hakodate collections particularly revealing. In November 1857, he exclaimed to George Bentham, “I have only peeped into one or two parcels [of Wright’s Hakodate collections].... Imagine the two most characteristic possible eastern United States plants, *Caulophyllum* and *Diphylleia*, both, I believe, our very species. Tell this to Hooker!”<sup>67</sup> Afterward, when he made a closer examination on the Japanese *Caulophyllum* and *Diphylleia*, he became convinced that the two Japanese species were identical to their American counterparts (Figure 10-7). In his unpublished manuscript (written sometime between 1857 and 1858), he struggled to come up with terms to describe the striking resemblance between Japanese and American *Caulophyllum* and *Diphylleia*: “The Japanese specimens are as like American ones as one pea is like another.”<sup>68</sup> The extraordinary discovery strengthened Gray’s belief that there must have been an “ancient dispersion” of species across the temperate flora. He soon asked Dana and Darwin when and

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<sup>66</sup> Gray, “Diagnostic...,” 386, 408. Gray also found that Wright had collected in Hakodate two plants that had for a long time occupied botanists’ attention as regards the floristic relationship between East Asia and eastern North America: ginseng and the poison sumac. Gray, “Diagnostic...,” 384, 391.

<sup>67</sup> AG to GB, 11/ 16/1857, *LAG*, 435.

<sup>68</sup> The manuscript is part of the *CWP* collection. The statement appears on a page pencil-marked 16.



Figure 10-7. The *Caulophyllum* collected by Wright's assistant James Small in Hakodate and prepared by Wright for Gray's study. The species now known as *Caulophyllum robustum*. (Courtesy of the Gray Herbarium, Harvard University, Cambridge, MA, USA.)

where this dispersion might have happened (see Chapter 9).

It should be noted that Wright was not the only one in Rodgers's expedition who stimulated the likes of Gray to delve into the issue of disjointed species. Stimpson, too, upon examining the zoological specimens gathered in the middle of Hakodate Bay, became convinced that certain faunal relationships must have existed between Hakodate and eastern North America. "On a general view of the marine fauna, of this part of Japan," he noted in his journal, "the naturalist cannot fail to be struck with its resemblance to that of the East Coast of North America in a corresponding latitude,—that of New England. The boreal greatly predominates here, and but few species of Southern Japan,—even those of Simoda [*sic*], only degrees South, occur."<sup>69</sup> Still Stimpson echoed what Agassiz had eagerly advocated in the early 1850s: Naturalists should consider those similar Asian and American species distinct, for in so doing they would better capture the Creator's plan in distributing species.

At some point, Stimpson must have had made his observation known to Dana, in response to Dana's view that naturalists should consider similar Asian and American species identical, and it was such disjunct distribution of species that revealed God's plan. Dana must have had mulled this information over. In his talk before the American Academy of Arts and Sciences in August 1857, he somehow *twisted* Stimpson's observation to match his theory. Dana told his audience that "a zoologist" who had gathered "shells or mollusks from the coast of eastern America and that of Japan" made out "his list of identical species," with the "full assurance" that the listed species were "definite and stable existences." With the list

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<sup>69</sup> Vasile *et al.*, *William Stimpson's Journal*, 90.

established, he continued, this zoologist was “even surprised with the identity of characters between the individuals of a species gathered from so remote localities.” “And as he sees zoological geography rising into one of the grandest of the sciences, his faith in species becomes identified with his faith in nature and all physical truth.”<sup>70</sup>

The so-called first Darwinian debate in the United States owed its very origin to a small town in northern Japan named Hakodate.

### **“A matter of life and death”**

On June 26, 1855, the *Vincennes* left Hakodate for Kamchatka. Three days later (on June 29), the *John Hancock* left the same harbor, and would spend the next month or so exploring the coasts of Hokkaido (another unpermitted cruise). Rodgers left a message for the Japanese officers that he would come back within a year or so and that he wanted the Japanese government to be fully prepared for his second visit. According to Rodgers’s plan, the rest of the year would be devoted to surveying the route that connected Hakodate with San Francisco through the Bering Straits. Then the squadron would remain in San Francisco Bay until the spring of 1856. The squadron would then set sail again for Japan—this time via the Sandwich Islands. During this second visit to Japan, Rodgers expected that the expedition would gather sufficient information about Japan’s topography, natural history, oceanography, meteorology, and the like. At a moment when the Chinese market seemed

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<sup>70</sup> Dana, “Thoughts on Species,” 313.

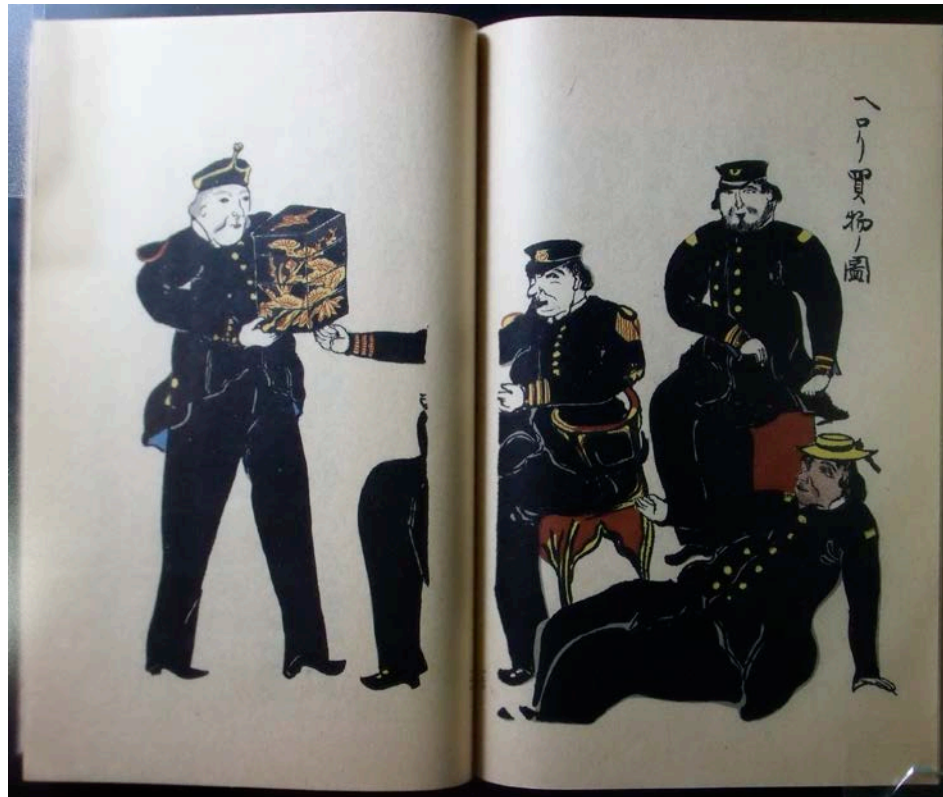


Figure 10-8. Foreigners' collecting activities from the Japanese point of view. It is very likely that the man carrying many plants (left) was Wright. (From *AIU* [above], *HIJD* [left] and *BSS* [right]. From the Sapporo City Archive Museum, Sapporo, Japan [above], the Hakodate Central Library, Hakodate, Japan [left], and Yokohama Archives of History, Yokohama, Japan [right].)

boundless, and many countries were rubbing their hands together greedily as a result, he was convinced that the more scientific information about Japan a country controlled, the greater share of China's market the country could grab. "[T]he routes to China [through Japan] must be surveyed by the United States," he told Secretary Dobbin.<sup>71</sup>

The controversy and confusion that Rodgers's expedition had excited in Japan hardly quieted down as the NPEE squadron left Japan's shores.<sup>72</sup> In fact, even the common Japanese were provoked by Rodgers's visit. They were aware that the gates of their country had been opened, and aspired to document their colorful encounters with foreigners through paintings and written works. The curiosity of their foreign visitors toward everything Japanese turned out to be their favorite subject (Figure 10-8). Still they were not naïve about the asymmetrical power relationships between Japan and her European and American counterparts. For instance, in 1858, an anonymous author produced a series of paintings documenting the reception prepared by Japanese officers for Elisha E. Rice, the first American consul in Hakodate. The climax of the reception, according to the author, was a girl bathing before the smoking American diplomat—a perfect sister image to what Habersham depicted in *My Last Cruise*. Then, as if to show his discontent with the course of the reception, the author produced a painting showing the scenario taking

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<sup>71</sup> JR to JCD, 1/29/1856, [frame number illegible,] R5, *REENP*, from San Francisco.

<sup>72</sup> A summary of what Rodgers and his men did and how they behaved in Shimoda and Hakodate, submitted by local officers to the elder, can be found in *BGKB* 11, 363-366.



Figure 10-9. A Japanese woman crying before her parents and then bathing before the American consul Elisha E. Rice. (From *AGNW*. Courtesy of the Hakodate Central Library, Hakodate, Japan.)

place right before the girl's performance: A crying girl kneeling down before her worried parents (Figure 10-9).

Koga Kin'ichirō 古賀謹一郎 (1816-1884), a distinguished Confucian scholar and a widely respected government consultant on foreign affairs, decided to make his opinion on the NPEE known to the bakufu. Over the preceding decades, Koga had immersed himself in various publications concerning Japan's place in the world. In the face of an increasing number of foreign countries that for many reasons demanded the bakufu cease its isolation, Koga had been advocating an open approach for Japan's foreign affairs. He thought two foreign concepts particularly distinguished Rodgers's expedition: science and exploration, and felt obliged to call the bakufu's attention to these particular points.

Sometime between June and July 1855, Koga submitted his opinion to the elder concerning the way in which the bakufu should properly handle the return of Rodgers's expedition. He first criticized the prevailing government view: that Rodgers should not be permitted to return. Indeed, he continued, when he tried to comprehend the rationale behind the view, he said it called to mind a herd of dogs barking at strangers.

Koga went on to say that he was particularly amused, but annoyed at the same time, by the statement that the United States would invade Japan if the elder permitted Rodgers to collect sufficient information about Japan's topography and natural history. As far as he knew, Koga argued, there was no precedent for a survey expedition to lead to an invasion. Just take a look at the history of the United States and Europe, he urged the bakufu. If invasion and surveys were simply two sides of



the same coin, these countries, all with vast experience conducting surveying expeditions, would have driven each other to extinction through constant war. Koga then put forth his understanding of what a surveying expedition meant in the Western context. Instead of invading a country, he said, the goal of a surveying expedition was to explore (*tansaku* 探索) such barrens as the Arctic and Antarctic waters and to acquire “unknown geographical knowledge.” In Koga’s opinion, it was this eagerness to survey and to explore the unknown world that gave rise to the “science of surveying” (*sokuryōgaku* 測量学) and in many ways made the United States and European countries highly civilized nations.

Koga then called the elder’s attention to the current status of Japanese surveying science. To be sure, he noted, though the eminent cartographer Ino Tadataka 伊能忠敬 (1745-1818) had surveyed the entire coastline of Japan according to longitude and latitude, Japanese people’s topographical knowledge about their country remained mostly rough. Considering Japanese’s society’s burgeoning enthusiasm for purchasing Western vessels and building vessels in the Western style, he continued, the acquisition of an accurate understanding of Japan’s coastlines, including sea depths, shoals, and other topographical information, was an urgent matter. In this light, he told the elder that Rodgers’s application to undertake a thorough scientific survey of Japan’s coastline was nothing but lucky for the bakufu. For a better future for Japan, he wrote, the bakufu should make use of Rodgers’s expedition. For example, he suggested that the bakufu send a group of Japanese students to join Rodgers’s expedition and have them familiarize themselves with the surveying techniques of the United States. If the bakufu could appreciate the value of surveying

science and collaborate with Rodgers in “a spirit of openness and justice,” he concluded, Japan’s status in contemporary international politics would be significantly elevated—otherwise Japan would retain its deplorable status as a toy or pet being teased and manipulated by foreign powers.

The elder did not accept any of Koga’s suggestions, however. About three months after Koga submitted his opinions (sometime between September and October 1855), the elder issued his formal order regarding Rodgers’s application. He instructed local officers to interact with Rodgers and his men with patience, but sternly stop any Americans from surveying Japan’s harbors and coastlines. To local officers in charge of foreign affairs, however, the order was quite ambiguous.<sup>73</sup> In October 1856, when Townsend Harris arrived in Shimoda and prepared himself to negotiate with the bakufu about revising the Treaty of Kanagawa, the Shimoda officers brought up the subject. They confessed to Harris that Rodgers’s application to survey Japan’s coasts had provoked great confusion, and their superiors had scarcely given them any concrete instructions. Harris was touched by the agony that the officers seemed to suffer. “[T]hey told me that it was a matter of life and death to them, as they must perform the hara-kiri or ‘happy dispatch’ (suicide) if the surveys went on,” he recorded in his journal.<sup>74</sup>

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<sup>73</sup> Tanabe, *Bakumatsu gaikō dan*, 28-29.

<sup>74</sup> William Elliot Griffis, *Townsend Harris: First American Envoy in Japan* (Boston, New York: Houghton, Mifflin and Company, 1895), 82. Notably, the great excitement and confusion that Rodgers’s visit to Japan stirred up did not end after Harris successfully signed the Treaty of Amity and Commerce with the bakufu in 1858. On the contrary, Harris’s treaty turned out to be the “template” for other foreign powers, thus forcing Japan to be exposed to a multitude of foreign influences. As a result, for those dissatisfied with the bakufu, Rodgers’s visit became the perfect

### **“A nobody”**

On October 13, 1855, the *Vincennes* sailed into San Francisco Bay, and was then escorted to the Navy Yard, Navy Island. Relentless as he was, Rodgers immediately devoted himself to preparing the next stage of the NPEE. He had promised the Japanese officers that he would return, and he resolved to keep his word.

Not all members of the expedition shared Rodgers’s enthusiasm, however. “We are in a state of betweenity or suspense,” Wright wrote home in mid-October. “Some want to go home others to go on with our survey; some believe or try to persuade themselves and others that the appropriation is exhausted.” Weeks later, he again wrote home about the chaotic status of the NPEE. With some surprise, he wrote that the belligerent Commander Rodgers had abruptly come up with an idea to make the NPEE more useful to the nation: “A day or two since all hands were called on deck and the Capt made them a little speech in which he made known his intention of fitting out temporarily the ship and to go to the assistance of our countrymen in

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example of its inability to protect Japan from foreign “invasions,” In the late 1850s and the early 1860s, a document said to be composed by Ii Naosuke (1815-1860), the powerful elder of the day, began circulating among officials and common people alike. Written in the form of a petition to the Emperor, the document first made a list of the mischief that Rodgers and his men had caused on Japan’s shores: insulting officers by stepping on their swords, demanding the compliance of the Japanese government through shows of force, rudeness in correspondence and meetings, intentional violations of the stipulations of the Treaty of Kanagawa, and so on. In light of the disturbances instigated by the rude American commander, and to prevent such unfortunate circumstances from coming up again and again, the document declared that it was time for the Emperor to take back the rulership of Japan from the bakufu and restore the ancient regime. In so doing, it concluded, Japan would continue to be a respected and respectable country in the world. Tanabe, *Bakumatsu gaikō dan*, 57-73.

Oregon against whom the Indians almost to a man are now wagging [*sic*] a cruel war." But Wright did not think that Rodgers's idea could be put in practice. He noticed Rodgers's difficulties getting money and shoring up support to hold the now collapsing NPEE together. "So there is the remotest possibility that I may have a little skrimmage [scrimmage] with the red skins and have an opportunity of performing the surgical operation of taking a scalp or submitting to that of losing my own."<sup>75</sup>

But the uncertain sense of "betweenity" soon grew to the extent that Wright found it difficult to bear. In December, he cried to John that he had been "growing so fat that my legs will hardly stay crossed." He would need some extensive exercise—preferably a botanizing expedition—to keep his body in shape, he wrote.<sup>76</sup> Wright was then glad to learn that a letter from Secretary Dobbin had arrived officially concluding the NPEE. "The fund appropriated by Congress for the Survey of Behring Straits, North Pacific Ocean and China Seas has long since been exhausted," Dobbin wrote.<sup>77</sup>

Now Rodgers had no option but to lead the squadron back to New York. Yet, as if he were unwilling to bring the NPEE to such an abrupt end, he made his mind up to explore the Sandwich Islands during the homeward voyage. Wright decided not to follow suit, however. He did not think that Rodgers would allow him enough time to explore the Sandwich flora ("The prospect of a long tedious voyage was before me in

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<sup>75</sup> CW to JW, 10/14/1855, *CWC*; CW to JW, 11/9/1855, *CWC*.

<sup>76</sup> CW to JW, 12/3/1855, *CWC*.

<sup>77</sup> JCD to JR, 12/5/1855, Folder 3, Box 8, *RFP*.

which a novelty in Botany was hardly to be expected”). He would rather act independently and return to New York via Nicaragua and collect plants along the way. To his disappointment, Rodgers refused to approve the plan. Anxiously, he assured Rodgers that he would make this homeward journey at his own expense. To this Rodgers consented.<sup>78</sup>

On February 1, 1856, Wright left the *Vincennes*, and three days later, he boarded the steamer *Cortes* sailing for San Juan del Sur. Arriving on February 17, he immediately joined a group of travelers organized by a transit company setting out for Virgin Bay, Nicaragua. When Wright arrived in Nicaragua, “Walker’s affair looked somewhat gloomy.”<sup>79</sup> Early in April 1856, when he felt satisfied with what he had collected, he found that the circumstances had grown harsh. Worryingly, he spent most of his money securing a spot on a boat sailing to New York (“a rumor was afloat that the lake and river steamers were just starting on their last trip and if I stayed any longer it was uncertain when another opportunity for leaving the country would be presented”).<sup>80</sup> Late in May, when Wright set foot again on the soil of the United States, he reported his arrival to Secretary Dobbin and to Rodgers. He soon received Dobbin’s reply that Wright’s connection with the NPEE “for the

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<sup>78</sup> CW to AG, 2/13/1856, *HL*; CW to JW, 2/7/1856, *CWC*.

<sup>79</sup> CW to JW, 2/24/1856, *CWC*; CW to AG, 5/18/1857, *HL*. When Wright had just arrived in Virgin Bay, he wrote home, “There is considerable military excitement—rumors of attacks threatened from the neighboring states and the little land of inhabitants are daily drilled to the use of arms—guards stationed at night &c.”

<sup>80</sup> “I was several days on the lake & river boats when a part of the time I actually had to beg for some thing to eat.” CW to AG, 5/17/1856, *HL*.

present” had ceased.<sup>81</sup> Wright was relieved. Given his current financial status, he needed to embark on another round of enterprises that might turn his botanical knowledge into cash.

At the suggestion of Gray, Wright chose Cuba as his next botanical ground (“If I go on that botanical Expedition to Cuba, how many sets can be sold and at what price?”).<sup>82</sup> Early in November, he struck out for Cuba. Nothing seemed to have changed—despite the fact that he had explored and collected plants in a scope wider than any botanical collector in the United States.

But Wright did manage to make some changes to his career course. In April 1857, when he heard that Congress had granted an appropriation for the publication of a “Report on the Natural History Collections of the Behring’s Straits Expedition,” he promptly wrote to Rodgers from Cuba: “If you see proper to instruct to me the preparation of the Report on Botany will you let me know how soon it will be necessary for me to report to Washington as I do not wish to return to the United States till about the last of June.”<sup>83</sup> Then, learning that it was likely that the Navy Department would appoint him as the botanist overseeing the NPEE’s botanical report, he replied that he would return to the United States as soon as he had completed his Cuba expedition. On August 22, 1857, Wright jumped on a steamer sailing back to New York. On September 9, he composed a letter to Gray proposing “a joint labor” in preparing the NPEE botanical report.

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<sup>81</sup> JCB to CW, 5/23/1856, *CWC*.

<sup>82</sup> AG to WJH, 6/30/1856, *LAG*, 422; CW to AG, 9/8/1856, *HL*.

<sup>83</sup> CW to JR, 4/7/1857, Folder 25, Box 1, *NPEEC*.

I suppose it will be necessary for me to go to Washington at an early day to attend to the botany of Behring Strait's Expedition. I have been thinking of proposing that we make a joint labor of it. I suppose I can do a good deal of the work of distribution, comparison & the manual labor generally as well as the description of many of the novelties if we find any. I don't know what the pay is. As that will be my only income, I shall necessarily [take] a considerable position of it. I would like your views on the subject. Don't hesitate to express your mind freely for it would be no great sacrifice for me to give up the job entirely though I think we could do it better together.<sup>84</sup>

Late in September, Wright rented a room close to Gray's house and began paying regular visits to Gray.<sup>85</sup> Gray was delighted and shared his views on how to bring out the NPEE botanical report in the most effective manner. First, he suggested that Wright discharge this duty at Gray's herbarium and library instead of the Smithsonian. Let the Smithsonian take care of all the manual labor, like packing and distributing specimens, he told Wright. Also, he suggested that Wright persuade Rodgers to cooperate more intensively with the botanical community at large. For example, he said that William S. Sullivant was *the* expert in the United States capable of making a report on the NPEE's moss collections, as were Moses Ashley Curtis on fungi and Edward Tuckerman on lichens. In fact, considering the wide geographical scope of Wright's NPEE collections, Gray strongly suggested that Rodgers recruit foreign botanists to collaborate with Wright.<sup>86</sup> Finally, regarding the format of the report, he suggested that Wright divide the report into several parts according to

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<sup>84</sup> CW to AG, 9/9/1857, *HL*.

<sup>85</sup> IT to JR, 11/27/1857, Folder 18, Box 1, *NPEEC*.

<sup>86</sup> For instance, Gray told Wright, since the great American botanist Jacob Whitman Bailey had just passed away (in 1857), he believed that the only botanist capable of enumerating the NPEE's seaweed collections was William H. Harvey of Dublin.

phytogeographical regions. In his view, the collections made at the Cape of Good Hope should form its own part; those of Hong Kong, the Bonin Islands, and the Ryukyu Islands the second part; Japan the third part; and the Okhotsk Sea and the Bering Strait the last part.<sup>87</sup>

Rodgers agreed to every proposal that Gray made. A grateful Gray, when examining Wright's collections made in Hakodate, named a new genus *Rodgersia* (under the order *Saxifragaceae*) and made a statement in his 1859 paper on Japan's flora: "I gave the expression to Mr. Wright's wishes, as well as to my own sentiments, in dedicating this genus to the commander of the expedition, in acknowledgment of the enlightened and generous interest he took in the naturalists of his squadron, and of his constant care to facilitate their explorations."<sup>88</sup>

With Rodgers's support in dealing with funding and political matters in Washington, DC, and with Gray's support in recruiting eminent men of science to work on the NPEE's botanical collections, Wright's work went smoothly. On the first day of October 1858, Wright reported his progress to Rodgers:

With the exception of the collection from Japan, on which Prof. Gray is now at work, all the phenogamous plants & ferns have been pretty well examined & the manuscript written out. A few doubtful plants, not exceeding one percent of the whole, await further examination & comparison. Mr. Tuckerman is at work on the Lichens, & Mr. Curtis is occupied with the Fungi. I do not know whether Mr. Harvey has made much progress with the seaweed, & from Mr. Sullivant, who should be employed in elaborating the mosses, I can get no answer to my letters. I believe however that by the end of the year all or nearly all the several parts of the report will be in readiness.

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<sup>87</sup> Wright made Gray's proposals known to Rodgers in three letters, dated 9/23/1857, 10/31/1857, and 11/7/1857, all in Folder 25, Box 1, *NPEEC*.

<sup>88</sup> Gray, "Diagnostic...", 59-60.



Wright told Rodgers that he had done all he could do. If Rodgers did not object, he went on, he planned to resume his expedition to Cuba and leave the work in the care of Gray (“They could hardly be in better or more careful hand”).<sup>89</sup> Wright left the United States for Cuba early in November. Late in January 1859, he wrote from Monte Verde, updating Gray on his whereabouts. Before once again thrusting himself into the unknown territory of botany, he wrote, he could not help but look back on some unpleasant experiences from the past year, in particular Sullivant’s negligence of his correspondence. How could Sullivant have done this to him? Wright cried. During his “solitary ramble” in the wilderness, he wrote that he perhaps reflected on “such treatment” too frequently. Although undeniably he had, at times, received money from Sullivant for specimens, Wright claimed that he did not care “a good deal” for such financial matters, “so long as I feel that he thinks me a “nobody.””<sup>90</sup>

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On November 12, 1858, Gray initiated his official correspondence with Rodgers.

I expect that, by the end of this month I shall have finished the account of Mr. Wright’s Japan collection, which is (at least to me) the most interesting part of the fine botanical collection made in the expedition under your command. As soon as that is done, I propose, in accordance with your suggestions and my own wishes, to publish, in the *Memoirs of Proceedings of the American Academy of Arts and Sciences*, the short descriptions or characters of the new species, along with the new genus bearing your name,—at the same time giving a cursory remarks upon the interesting things which though not absolutely new, are perhaps as interesting, an account of their completing our knowledge of the

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<sup>89</sup> CW to JR, 10/1/1858, Folder 26, Box 1, *NPEEC*.

<sup>90</sup> CW to AG, 1/24/1859, *HL*.

species or an account of their geographical distribution (as for example, enumerating the remarkable number of United States species which occur in Japan, or have close representations there).

He went on to explain why he had decided to take this course of action. First, he noted, given his influence in the American Academy of Arts and Sciences, he could readily “secure 100 extra copies [of the Memoirs] for distribution at little or no expense.” In this way, he continued, “we shall secure some interesting discoveries to your expedition, which might else be anticipated abroad; and also—as you observe, enlist attention in this country, and especially Washington, and show the importance of complete publication of the scientific results of the expedition.”

Second, he urged Rodgers to be more open-minded regarding the distribution of the NPEE specimens. In accord with “the spirit of science,” he told Rodgers, it was necessary to send any duplicates of specimens to prominent institutions such as the Jardin des Plantes and the Royal Botanical Gardens at Kew. He suspected that Rodgers’s superiors would agree to distribute the NPEE collections in a liberal manner, Gray said. But if Rodgers could help him make it happen, Gray asserted that he would publicly acknowledge Rodgers’s contribution to science:

In my abstract of the Japan Botany, which I shall soon lay before the American Academy, with an appropriate introduction, I wish, as a cultivator of science to make a public and grateful acknowledgement of the great advantage which has resulted from the thoughtful consideration of the Commander of the Expedition, in cheerfully affording every proper facility to the researches of the naturalists employed, and in securing their enterprise—a course which has not always been followed in similar cases.<sup>91</sup>

On December 10, Gray gave a talk on the “geographical distribution of Plants & existence of some species on widely separated continents” such as Japan and

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<sup>91</sup> AG to JR, 11/12/1858, Folder 3, Box 1, *NPEEC*.

eastern North America before the Cambridge Scientific Club. A number of eminent men came to the presentation, including the past, present, and future Harvard presidents Jared Sparks, James Walker, and Corneilius Conway Felton. Agassiz was present too, and seemed to take Gray's subject "very well."<sup>92</sup> To be sure, it was not the first time that members of the Cambridge Scientific Club had heard about the United States' effort to reach Japan and tease out its significance in science. Almost two years earlier, on December 19, 1856, Jared Sparks had made a presentation on "Commodore Perry's expedition to Japan." Gray's talk definitely continued on this line of inquiry.

On January 7, 1859, Gray sent out an invitation to Torrey. He wished Torrey could attend "a social meeting of the Academy at Mr. Loring's house" on January 11. He stated that he would read a paper on "Japan botany" and would "knock out the underpinning of Agassiz's theories about species and their origin." His weapons, he continued, were those "that tumbled de Candolle," ultimately turning some of "Agassiz's own guns" against Agassiz himself. "Now come on," the botanist urged, "stop there, 8 Ashburton Place, where I will be."<sup>93</sup>

On January 11, he stood before members of the American Academy of Arts and Sciences and prepared himself to present his "abstract of the Japan Botany." As he had promised Commander Rodgers, his presentation would show the NPEE's scientific contributions, and he decided to do this by challenging Agassiz's species theory, which was, in Gray's view, too idealistic—if not too slippery—to pass a

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<sup>92</sup> Dupree, *Asa Gray*, 252-253.

<sup>93</sup> AG to JT, 1/7/1859, *LAG*, 450.

naturalist's critical gaze. He began his talk by remarking on the striking similarities between the floras of Japan and eastern North America. Then he threw the following questions to the audience:

... whether each species originated in one local area, whence it has spread, as circumstances permitted, over more or less broad tracts, in some cases becoming discontinuous in area through changes in climate or other physical conditions operating during a long period of time; or, whether each species originated where it now occurs, probably in as great a number of individuals occupying as large an area, and generally the same area, or even the same discontinuous areas, as at the present time. The latter is understood to be the view of Professor of Agassiz.<sup>94</sup>

If the debate surrounding the origin and distribution of species was a war, Gray had his arsenal fully armed and aimed.

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<sup>94</sup> Anonymous, "Four Hundred and Fifty-Eighth Meeting," 132.

## CONCLUSION

In June 1858, Gray noted to Torrey: “Deep in Japan botany; interesting result.” Some six months later, he submitted these results to the American Academy of Arts and Sciences for publication. In February 1859, when the essay appeared in *Memoirs of the American Academy of Arts and Sciences* and was reprinted in numerous scientific journals across North America and Europe, it would make Gray known as a pioneer, the first to apply Darwin’s evolutionary theory to one of the thorniest issues in plant geography, and as a radical who refuted the view of nature advocated by the larger-than-life Harvard zoologist Louis Agassiz. “[I]t seems to me a most curious case of distribution & how very well you argue & put the case from analogy on the high probability of single centres of creation,” Darwin told Gray after reading the 1859 essay. “That great man Agassiz, when he comes to reason seems to me as great in taking a wrong view as he is great in observing & classifying.”<sup>1</sup>

Gray began his essay by enumerating Wright’s collections based on a broad view of species. Though Gray once believed that similar Japanese and American species were merely analogical, his views had now changed, moving the issue of the single creation of species, disjoined species, migration of species, and so on to the foreground. Understanding how risky this move was, he skillfully intertwined his diagnosis with an overview of contemporary debates on the origin and distribution of species. Over and again, he expressed surprise, describing one specimen as “the most interesting and most unexpected discovery of the expedition.” For example,

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<sup>1</sup> AD to JT, 7/27/1858, *LAG*, 445; CRD to AG, 1/7/1860, *CCD*, Vol. 8, 23.

when directing readers' attention to two species collected on the "northern end" and "the northeastern extremity" of Japan, he claimed that he had little doubt that "these two Japanese plants were identical with *Caulophyllum thalictroides* and *Diphylleia cymosa*, two strictly Eastern North American species." "Supposing these two plants to be satisfactorily identified as to species, are we to regard them as the descendants of a common stock, though now separated by one hundred and forty degrees of longitude? Or are we to suppose them independently originated in two such widely distant regions?"<sup>2</sup> Anyone who knew Agassiz's species theory would nod in understanding.

Besides Agassiz, Gray also targeted Alphonse de Candolle for his account of the origin of disjoined species. "Those (such as De Candolle the younger) who, although convinced that species in general have had a single, local origin, are yet constrained to adopt the hypothesis of a double origin in the special case of certain species known to occur only in two widely dissevered regions." Yet, he continued, the number of such disjoined species has already "so increased, that they can no longer be regarded as exceptional or casual."<sup>3</sup> By any means, he insisted, naturalists should affiliate themselves with the concept of a single origin of species—regardless of whether the species' distribution is geographically continuous or disjoined—without entertaining the thought of even a slight compromise with the concept of multiple origins of species.

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<sup>2</sup> Gray, "Diagnostic...", 380-381.

<sup>3</sup> Gray, "Diagnostic...", 399.

TABULAR VIEW OF THE DISTRIBUTION OF JAPANESE PLANTS AND THEIR NEAREST ALLIES IN THE NORTHERN TEMPERATE ZONE.

EUROPE.	CENTRAL & N. ASIA.	JAPAN.	W. N. AMERICA.	E. N. AMERICA.
<i>Clematis vitalba</i>	<i>C. Nepalensis</i> & <i>montana</i> <i>C. pinnata</i>	<i>Clematis florida</i> & <i>parsonsii</i> <i>Clematis Williamsii</i> & <i>Japanica</i> <i>Clematis epiphiila</i> , <i>Wiemana</i> , &c.	<i>C. ligusticifolia</i>	<i>C. Virginiana</i> .
<i>T. Romanica</i>	<i>T. Romanica</i>	<i>Thalictrum Thunbergii</i>		
<i>T. simplex</i>	<i>T. affine</i>	<i>Thalictrum affine</i>		
<i>P. pratensis</i>	<i>P. pratensis</i> <i>A. umbrosa</i> <i>A. Altaica</i> <i>A. Daikalensis</i>	<i>Pulsatilla cernua</i> <i>Anemone umbrosa</i> <i>Anemone Altaica</i> <i>Anemone Daikalensis</i>		<i>P. Nuttalliana</i>
<i>A. narcissiflora</i>	<i>A. narcissiflora</i> <i>A. Pennsylvanica</i>	<i>(Anemone narcissiflora)</i> <i>(Anemone Pennsylvanica)</i>	<i>A. spicata/flora</i> <i>A. Pennsylvanica</i>	<i>A. Pennsylvanica</i>
<i>H. tridoba</i>	<i>H. tridoba</i>	<i>(Hesperis tridoba)</i> <i>(Thunbergia Japonica, S. &amp; Z.)</i> <i>(Adonis Apennina, var. Sibirica)</i>	<i>H. tridoba</i> <i>T. palmata</i>	<i>H. tridoba</i> & <i>occidentalis</i> <i>T. palmata</i>
<i>A. Apennina</i>	<i>A. Apennina, Sibirica</i>	<i>Ranunculus repens, var.</i>	<i>R. repens, var.</i>	<i>R. repens, var.</i>
<i>R. repens</i>	<i>R. repens</i>	<i>Ranunculus sceleratus</i>	<i>R. sceleratus</i>	<i>R. sceleratus (exarce)</i>
<i>R. sceleratus</i>	<i>R. sceleratus</i>	<i>Ranunculus repens</i>		
<i>R. acris</i>	<i>R. prorepens</i>	<i>Ranunculus prorepens</i>		
<i>Calla palustris</i>	<i>Calla palustris</i>	<i>Calla palustris</i>	<i>Calla palustris</i>	<i>Calla palustris</i>
<i>T. Europaeus</i>	<i>T. yarusu</i>	<i>Trollius yarusu?</i>		<i>T. Americanus</i>
<i>C. trifolia</i>	<i>C. trifolia</i>  ( <i>C. Tieto</i> )	<i>Cypripis trifolia</i> <i>(Cypripis brachypoda, S. &amp; Z.)</i> <i>(Cypripis anomala/flora, S. &amp; Z.)</i>	<i>C. trifolia</i> <i>C. asplenifolia</i> <i>C. occidentalis</i>	<i>C. trifolia</i> <i>C. trifolia</i>
<i>I. thalictroides</i>		<i>Isopyrum subuloides</i>	<i>I. occidentale</i>	<i>I. biornatum</i>
<i>A. vulgare</i>	<i>A. vulgare</i> & <i>var.</i>	<i>(Aquilegia Hungarica)</i>	<i>A. Canadensis</i>	<i>A. Canadensis</i>
<i>A. Epistemon</i>	<i>A. Epistemon</i>	<i>(Anemone Japonica)</i>		<i>A. radicans</i>
<i>A. spicata</i>	<i>A. spicata, f. var.</i>	<i>Actea spicata</i>	<i>A. spicata, v. rubra</i>	<i>A. spicata, v. alba &amp; rubra</i>
<i>C. fetida</i> [H. Eu.]	<i>C. fetida</i>	<i>(Cimicifuga fetida?)</i> Phytosperma acerrimum, obtusilobum, & biornatum Glaucidium palmatum	<i>C. fetida</i>	<i>C. Americana</i> Cimicifuga (Macrotys) [occidentalis & racemosa] Hydrastis Canadensis
<i>P. officinalis</i>	<i>P. officinalis</i>	<i>Paeonia officinalis</i>	<i>P. Rossii</i>	
<i>I. Griffithii</i>		<i>Ilicium religiosum</i>		<i>I. Floribundum</i> & <i>parvif.</i>
<i>Magnolia &amp; Michelia</i> spp.		<i>Magnolia &amp; Eurygia</i> spp.		<i>Magnolia</i> spp.
<i>K. Rorschachiana</i>		<i>Kadsura Japonica</i>		<i>Schizandra coccinea</i>
<i>Sphaerostoma</i> spp.		<i>Sphaerostoma Japonica</i>		
<i>Parvatia &amp; Holboellia</i>		<i>Akebia &amp; Stuntonia</i> spp. [Larixabala spp. in Chili]		
<i>Cocculus</i> spp. & <i>Mentha</i> spp.		<i>Cocculus Thunbergii</i> , &c.		<i>Cocculus &amp; Mentha</i> spp.
<i>D. vulgaris</i>	<i>D. vulgaris</i>	<i>Berberis vulgaris</i>		<i>Berberis</i>
<i>B. vulgaris, Cretica</i>	<i>B. vulgaris, Cretica</i>	<i>Berberis Thunbergii</i>		<i>B. Canadensis</i>

Figure C-1. Gray's "tabular view of the distribution of species." (From Gray, "Diagnosis...", 424.)

Gray then summarized his enumeration in a table titled "Tabular View of the Distribution of Japanese Plants and Their Nearest Allies in the Northern Temperate Zone." In Gray's words, this table showed the "species, or at least the genera, of the Japanese plants... which have particular relatives in other and distant parts of the northern temperate zone." Notably, he centered the column "Japan," and placed the columns "W. N. America" and "E. N. America" to its right, and "Central & N. Asia" and "Europe" to its left. In doing so, he made his table a *map*, in which species were arranged according to their spatial relationships. With this format set out, he placed some 580 Japanese species in the column "Japan," and printed those Japanese species and genera with "particular relatives in other and distant parts of the

northern temperate zone” in italic type. He considered it the best way by which to evaluate floristic affinities among temperate regions (Figure C-1).<sup>4</sup>

Patterns surfaced as the above trifling steps were repeated. To his satisfaction, Gray found that a stunning number of American species were either identical with or closely allied to their Japanese counterparts. More importantly, he found that 157 Japanese species could also be found in Europe, 134 species in eastern North America, and finally 120 species in western North America. In January 1859, when he came to present his paper, he would begin his talk by claiming that “the Flora of Japan with that of the United States east of the Mississippi were peculiarly intimate, as evinced by the great number of congeneric, of closely representative, and of identical species in the two floras.” He would then argue that this remarkable intimacy would be made evident by juxtaposing it with two considerations: First, “most of the more striking points of similarity were presented in species or in types which are absent from the flora of Europe”; second, “although there is a considerable number of species common to the western side of the American continent and to Japan... the likeness was less strong between their floras than between those of Eastern North America and of Japan.” How could naturalists explain the remarkable intimacy between Japan and eastern North America, especially considering the fact that the two floras were “geographically separated by about one hundred and forty degrees of longitude?”<sup>5</sup>

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<sup>4</sup> Gray, “Diagnostic...,” 423-436.

<sup>5</sup> Anonymous, “Four Hundred and Fifty-Eighth Meeting,” 131.



As I argue, toward the end of 1858, when Gray came to elaborate his thoughts on species using Japan's flora as an example, a multitude of theories were racing through his mind. As to the nature of species, Darwin argued in favor of the transmutation and divergence of organisms; Bentham insisted on the contrary. As to the origin of disjoined species, Darwin regarded disjoined species as "remnants" of a grand migration of species occurring since the glacier period: that is, species of temperate floras must have originated in the polar area when the climate was warm, migrated southward during periods of chilling, and in some cases distributed discontinuously during local extinctions; Bentham argued that disjoined species resulted from the interchange of the flora of Europe with that of America, with Asia acting as a channel. Finally, as for the means by which species migrated from one place to another, Darwin regarded icebergs, birds, currents, and the like as the means of dispersion for plants; Bentham insisted on the existence of a landmass that bridged different continents—for only a "continental transmission" could form a biogeographical pattern on a transcontinental scale.

To Gray, the incompatibility—if not incommensurability—of these different theories was also evident in the matter of how the geological past of the earth might have shaped present biogeographical patterns. Dana told Gray that the epochs *after* the glacier period must have played a crucial role in shaping the present state of North America, while Darwin doubted whether there had been such recent, dramatic moments. Unlike Dana, Darwin regarded the present biogeographical patterns as having resulted from an enduring, ceaseless series of changes commencing in the epochs *before* the glacier period. In other words, like the

dilemma Gray encountered in the matter of the origin of disjoined species, the question left for Gray to answer here was how to choose among this great diversity of views on species and on geographical distribution.

Gray did not follow any theory in its entirety to develop his disjunction thesis. He dismantled those relevant views and reassembled them, so that he could cover as much data as possible. He first told his readers that the esteemed British botanist George Bentham had theorized that a grand migration must have occurred along the route from Europe to Asia to America. He had agreed with the theory then, he went on, but now felt that the theory required further deliberation. First, he argued, “Eastern North America has more in common with Japan than Western North America,” which showed that “geographical continuity” could not have been the only factor in shaping floristic relationships. Second, it was unnecessary to assume “an ancient continuity of territory between America and Asia,” he emphasized. “I shall presently state why connection in a more meridional latitude need not be supposed.”<sup>6</sup>

For Gray, all the above elaborations helped to smooth the path toward the “considerations which Mr. Darwin first brought to bear upon such questions, and which have been systematically developed and applied by the late Edward Forbes, by Dr. Hooker, and by Alphonse De Candolle.”<sup>7</sup> Such considerations, to be sure, had little to do with Darwin’s evolution by natural selection—after all, Edward Forbes could not have been an evolutionist, let alone a Darwinist. What Gray referred to

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<sup>6</sup> Gray, “Diagnostic...,” 437, 442.

<sup>7</sup> Gray, “Diagnostic...,” 445-446.

was a scheme Darwin suggested to him in August 1858. He found that Darwin's scheme regarding the relationship between geology and species migration could supply ground upon which Bentham's theory on the interchange of temperate floras could better operate.

At first sight, Gray noted, the result seemed unsurprising—considering the geographical proximity between Japan and the Asian continent, it seemed very likely that a great portion of Japanese species might have migrated to Europe through Asia. But was geographical proximity the only factor that shaped biogeographical patterns? Gray did not think so. In effect, he continued, if proximity played such an important role in shaping the distribution of species, how could naturalists explain the fact that a *greater* portion of Japanese species could be found in eastern North America than in western North America?

Here was the point at which Gray introduced Darwin's geological sequences. Prior to the glacial period, he began, "the temperate floras, which now in Western Europe touches the Arctic Circle, must then have reached equally high latitudes in Central or Western North America." In the meantime, he noted that "the temperate floras of America and Asia must then have been conterminous... and therefore have commingled as conterminous floras of similar climate everywhere do." The subsequent glacial period brought about "an extraordinary refrigeration of the northern hemisphere," he went on, which must have forced plants to migrate southward. A portion of them must have retreated up to the mountains as "climate ameliorated and the ice receded [afterwards]." In a Darwinian fashion, he then portrayed how plants were crawling, wriggling, and struggling for survival. Gray

pointed out that there were isothermal lines that stretched from the polar area to eastern North America, which must have offered a hospitable environment in which plants could dwell and thrive. Put differently, he elaborated, by the closing of the glacial period, or the post-Tertiary Period, the temperate floras had been separated, and the flora of eastern North America and that of East Asia bore a remarkable resemblance to each other, since the two sets of specimens were direct descendants from a common stock that once existed before the glacial period.<sup>8</sup>

Remarkably, Gray did not stop his analysis here but added two epochs subsequent to the tertiary period: Dana's fluvial and terrace epochs. He argued that what characterized the fluvial epoch was a "milder climate," which must have allowed those species that had retreated to the south to return north, and they "must have advanced much farther northward, and especially northwestward." It was necessary to take such northwestward movements seriously, he argued, for it must have given rise to "a second time coterminous... at Behring's Straits... by way of the Aleutian and Kurile Islands." "I cannot imagine a state of circumstances under which the Siberian Elephant could migrate, and temperate plants could not."

Following this second coterminous period was Dana's terrace epoch, Gray noted, during which the physical conditions of North America approached their present levels. As a consequence, he reasoned, plants must have returned southward, many of them to their old homes in East Asia and eastern North America, and then remained there to the present. Here Gray offered a revision of Bentham's view:

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<sup>8</sup> Gray, "Diagnostic...", 446-448.

...that the interchange has mainly taken place in high northern latitudes, and that the isothermal lines have in earlier times turned northward on our eastern, and southward on our northwest coast, as they now do, are points which go far towards explaining why Eastern North America, rather than Oregon and California, has been mainly concerned in this interchange, and why the temperate interchange, even with Europe, has principally taken place through Asia.<sup>9</sup>

Gray concluded his essay by placing his argument in opposition to Agassiz's species theory. "That the extent vegetable kingdom has a long and eventual history, and that the explanation of apparent anomalies in the geographical distribution of species may be found in the various and prolonged climatic or other physical vicissitudes to which they have been subject in earlier times." Hence, he emphasized, "The occurrence of certain species... in a remote or antipodal region affords of itself no presumption that they were originated there."<sup>10</sup>

In the years that followed, the flora of Northeast Asia (Japan in particular) and its relationship to that of North America turned out to be Gray's favorite subject. He sought correspondents, procured specimens, and recruited students and assistants who could help him bridge the gap between the United States and the "Far East." His fascination with Northeast Asia grew more fervent as his knowledge deepened. In his presidential talk for the American Association for the Advancement of Science in 1872, for example, he noted, "If we now compare, as to their flora generally, the Atlantic United States with Japan, Manchuria, and northern China,—i.e., eastern North America with eastern north Asia, half the earth's circumference apart,—we

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<sup>9</sup> Gray, "Diagnostic...", 447-449.

<sup>10</sup> Gray, "Diagnostic...", 449.

find an astonishing similarity.”<sup>11</sup> In “Forest Geography and Archaeology,” a lecture delivered before the Harvard Natural History Society in 1878, he burst out in surprise: “a narrow region in east Asia [Japan, eastern Manchuria, and the adjacent borders of China] contains twice as many genera and about twice as many species of indigenous trees as are possessed by all Europe.”<sup>12</sup> Though Gray never had seized the chance to visit Japan or China or Manchuria, not to mention the isles that bridged Northeast Asia with Northwest America, for Gray—and probably for those who were affiliated with him—Northeast Asia played a role in natural history and many other fields much like that of the Amazonian forest in contemporary studies on biodiversity.<sup>13</sup>

Toward the end of his life, Gray explained why he had been captivated by the floristic relationship between East Asia and North America. It came from a wild thought that had surfaced in his mind when he was “a very young botanist”:

It is as if Nature, when she had enough species of a genus to go round, dealt them fairly, one at least to each quarter of our zone; but when she had only two of some peculiar kind gave one to us and the other to Japan, Manchuria, or the Himalayas; when she had only one, divided these between the two partners on the opposite side of the table.<sup>14</sup>

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<sup>11</sup> Gray, “Sequoia and Its History,” 290.

<sup>12</sup> Gray, “Forest Geography and Archaeology,” 186.

<sup>13</sup> Two notable cases are Charles Sprague Sargent and Liberty Hyde Bailey. See Charles S. Sargent, *Forest Flora of Japan: Notes on the Forest Flora of Japan* (Houghton Mifflin and Company, Boston and New York, 1894); Liberty Hyde Bailey, *The Survival of the Unlike: A Collection of Evolution Essays Suggested By the Study of Domestic Plants* (New York: Macmillan Co., 1896). An excellent analysis of the “afterlife” of Gray’s disjunction thesis belongs to Stephen A Spongberg, *A Reunion of Trees: The Discovery of Exotic Plants and Their Introduction into North American and European Landscapes* (Cambridge: Harvard University Press, 1990).

<sup>14</sup> Gray, “Forest Geography and Archaeology,” 188.

As we have seen, until the publication of his paper on Japan's flora in 1859, Gray had spent the previous two decades proving this statement true. He was to spend the next three decades rejecting it. There were no such things as independently created species. Nor would Nature ever place species here and there.<sup>15</sup> Compared to the 1840s, when Gray enthusiastically protested against the evolutionary views of nature advocated by Rafinesque and the anonymous author of *Vestiges*, Gray in the late 1850s held a different view of the origin and distribution of species. The question that remains is to what extent historians of science can characterize Gray's shift as part of the Darwinian Revolution.

### **The First Darwinian Debate in the United States?**

This dissertation has traced the course through which Gray developed his disjunction thesis, the thesis that made Gray's name as a philosophical naturalist. Well known in botany and the history of science, Gray's thesis examined a curious pattern in nature: why there was a floristic similarity between Japan and eastern North America. As we have seen, before Gray published his thoughts on Japan's flora, he had spent decades on this topic. He cataloged species, compiled statistics,

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<sup>15</sup> Notably, in his revised edition of *Elements of Botany* (1887), the newest edition of *Botanical Text-Book*, published a year before his death, Gray removed all paragraphs about the Creator's role in making and distributing species, an argument that once occupied much space in his *First Lessons* of 1857. He defined species as strongly defined varieties. He substituted Darwin's account of plants' adaption for Braun's developmental plan of species. Given Gray's enthusiasm for bringing the Creator back into evolutionary thought in the 1860s and the 1870s, it appears that Gray was making another twist. See Gray, *The Elements of Botany for Beginners and for Schools (Revised Edition)* (New York: Ivison, Blakeman, and Co., 1887).

consulted correspondents, dispatched collectors, and incorporated the issue of disjoined species to argue against Agassiz. As historians of science have noted, insofar as nineteenth-century biogeography was concerned, no discovery could be considered self-evident. To identify a biogeographical pattern, to be sure, involves and revolves around a naturalist's views on species, plant migration, the origin of species, the geological past, and so forth, all of which conditioned the extent to which naturalists could connect the dots and draw out underlying patterns. The making of Gray's disjunction thesis is therefore a remarkable page in the history of biology. It captured the essence and dynamics of the debates surrounding the origin and distribution of species.

As far back as the 1840s, Gray had identified remarkable "analogies" between the flora of East Asia and that of North America. As I showed in Part I, this particular way of dealing with the floristic relationship between Japan and North America reflected Gray's scientific practices and view of nature between the 1840s and the early 1850s. By analyzing Gray and his contemporaries' "free and liberal exchange of specimens" and Gray's view of nature as laid out in *Genera Florae Americae Boreali-Orientalis Illustrata*, I argued that Gray in the 1840s and the early 1850s was convinced that "a particular plan" existed in nature, and he thought the floristic similarities between Japan and eastern North America manifested this plan. Based on this view of nature, I argued, Gray at the time sternly rejected any view that suggested the transmutation of species and regarded the origin and the distribution of species as falling upon the *terra incognita* of natural history. As he highlighted in



his review of *Vestiges*, any hasty move into this terrain would spoil his reputation and lower his rank to that of the anonymous author of *Vestiges*, if not of Agassiz.

Yet, as I showed in Part III, entering in the mid-1850s, when he plunged himself into the writing of “Statistics” on the one hand and vigorously corresponded with Darwin on the other, Gray no longer regarded the origin and distribution of species as an illegitimate subject in natural history studies. My analysis indicates that Gray’s writings throughout the mid- and late 1850s exhibited his resolution in confronting those issues that he had so carefully bracketed—as it were—during the past decade. Darwin did change him.

A historiographical and biographical question, therefore, is the extent to which Gray integrated Darwin’s theory of evolution by natural selection into his 1859 paper. To be sure, Gray mentioned Darwin’s evolutionary theory only in a footnote, after a paragraph noting that “related species of phaenogamous plants are commonly associated in the same region, or are found in comparatively approximate (however large) areas of similar climate.” He then noted that “the fundamental and most difficult question remaining in natural history is here presented,” namely, the issue of “whether this actual geographical association of congeneric or other nearly related species is primordial, and therefore beyond all scientific explanation, or whether even this may be to a certain extent a natural result.” This was the point where Gray introduced Darwin and Wallace’s joint paper of 1858. “I am already disposed... to admit that what are termed closely related species may in many cases be lineal descendants from a pristine stock, just as domesticated races are; or in other words, that the limits of occasional variation in

species (if by them we mean primordial forms) are wider than is generally supposed, and that derivative forms when segregated may be as constantly reproduced as their originals.”<sup>16</sup>

There is no disguising the fact that in 1859 Gray had been exposed to Darwin’s groundbreaking theory of species, and in some measure had been ready to accept the evolution-by-natural-selection concept as a new foundation for systematic botany. But it is worth noting that Gray never discussed evolution by natural selection in the *text* of his 1859 essay. Considering Gray’s attempt to weave different theories into a coherent theme, it seems unfair to characterize his 1859 essay as an outright application of Darwin’s species theory. In Gray’s case, Darwin’s fame after the publication of *On the Origin of Species* eclipsed the fame that other authors had derived from concepts that once inspired Gray: for example, Bentham’s plant geography, Dana’s scheme of geology, Braun’s individuality, and so forth. One hundred and fifty years or so after Gray published the essay that changed the texture of plant geography, perhaps it is time to examine the 1859 essay against its author’s intellectual development, instead of Darwin’s. Perhaps we should pay more attention to what Gray argues in the text rather than let a footnote monopolize our attention.

First, unlike current studies that emphasize how Darwin converted Gray to his camp, I suggest that historians of science look into Gray’s “conversion” more closely. Gray’s curious addition to Darwin’s geological scheme, the fluvial and terrace epochs, hints at Gray’s attitude toward evolution by natural selection. It should be

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<sup>16</sup> Gray, “Diagnostic...,” 443.

noted that Darwin was troubled by this part of Gray's analysis ("inextricable confusion," in Darwin's words). "He [Gray] has changed my doctrine," he complained to Hooker in May 1859, "apparently after consulting Dana [available documents however suggest that Gray consulted Dana *before* Darwin]." "I knew there was some slight evidence from range of the shell *Gnathodon* in U. States of warmer climate since Glacial epoch; but I do not believe there is any such evidence for Europe." "The argument from the woolly Elephant & woolly Rhinoceros I look at as false," he declared.<sup>17</sup>

Darwin had every reason to express his resentment for Gray's geological explanation. After all, as early as August 1858, he had expressed his doubts that a milder climate existed *after* the glacier period. So why did Gray insist on the existence of the fluvial and terrace epochs, though he knew he had to respect Darwin's geological knowledge?

The tension inherent in systematic taxonomy and evolutionary theory—recently explicated by Endersby with Hooker as one case—may explain why Gray turned his back on Darwin's geological scheme.<sup>18</sup> My analysis has shown that in the mid-1850s, Gray's self-identity as a lumper became evident. Stimulated by Hooker's eagerness to reduce those "nominal species," he began assuming that those species common to East Asia and eastern North America were identical—not merely analogical—so that he could readily confront the dilemma Agassiz had discarded and de Candolle

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<sup>17</sup> CRD to JDH, 5/11/1859, *CCD*, Vol. 7, 297.

<sup>18</sup> Endersby, *Imperial Nature*; Endersby, "Lumpers and Splitters: Darwin, Hooker, and the Search for Order," *Science* 326, no. 5959 (2009), 1496–1499.

had failed to resolve: the origin of disjointed species. As a result, when Darwin told him that species for the most part were dispersed and divergent, Gray must have found himself in a quandary. If he accepted Darwin's evolutionary theory, a reasonable conclusion was that those similar Japanese and American species could not have been identical—they might have descended from the same ancestor, or a common stock, but must have evolved into other forms since the glacial period depending on their adaptation to local habitats. Gray indeed managed to deploy Darwin's species theory against Agassiz. But he had to be careful. Darwin's theory was a double-edged sword. If used inappropriately, it might prove lethal to Gray's arguments, given that Agassiz had built up his views upon a conviction that those similar Japanese and American species were essentially different.

I argue that the concerns listed above eventually led Gray to embrace Dana's fluvial and terrace epochs and conceptualize the "second mingling of species" during these two epochs. Gray needed these two epochs to *offset* the influences of evolution by natural selection. If, according to Darwin, those species that had retreated to East Asia and North America would eventually evolve into other forms, the second commingling of species in the fluvial epoch could have shuffled—as it were—those evolved species and redistributed them to the two sides of the Pacific. Moreover, if, according to Dana, the fluvial and terrace epochs were so close to the present, those redistributed species should not have had enough time to evolve into other forms. Taken altogether, we can readily claim that the Gray of 1859 was an evolutionist, though not an evolutionist as historians of science have commonly understood it.

Gray's reception of Darwinism is by no means the only way to place Gray's disjunction thesis in a broader historiographical context. In light of recent discussions on the history of field sciences, this dissertation has offered a detailed analysis of how Gray acquired specimens from Japan—from the origin of the NPEE, to the arrangement the Navy Department made with the Smithsonian regarding natural history collecting and preservation, to Charles Wright's collecting practices in Japan, to the Japanese officers' and common people's responses to the NPEE. I have argued that a close examination of a biogeographical concept like Gray's disjunction thesis should include the institutional, cultural, and material aspects that connect the naturalist in the closet with the collectors in the field. As I discussed in Chapters 8 and 10, in part because of the active involvement of men of science like Gray, and in part because of the role played by the Smithsonian as the bridge between men of science and multiple governmental agencies, the NPEE evolved into an exploratory project that highlighted what Robert E. Kohler has termed the "exacting sciences." Equally important, the goal of making the NPEE's science exact was eagerly pursued by Commander Rodgers, with superior U.S. military power serving as its "passport to observation." In hindsight, it is fair to say that it was Rodgers's aggressive style of commanding the NPEE that empowered collectors like Wright and Stimpson to gather sufficient materials for men of science to elaborate their theories. Indeed, had the NPEE not created favorable circumstances for Wright to look for those plants "in the state of nature," Gray would not have had enough specimens to measure the degree of resemblance between the flora of Japan and that of eastern North America, nor would he have had been confident about his

application of Darwin's species theory to argue against Agassiz. In the history of American surveying expeditions, if Commodore Perry's Japan Expedition made Japan politically open, the NPEE aspired to make Japan *scientifically* open, and indeed, *exposed* to the gaze and scrutiny of modern sciences. Gray's successful application of Darwin's theory in biogeography was part of this aspiration, which would soon dominate natural history collecting in the United States during the second half of the nineteenth century.<sup>19</sup>

Still, as historians of colonial science have noted, the people and natural forms that scientists study are by no means passive objects but active agents who participate in the making of science in the presence of a colonial or other type of power imbalance.<sup>20</sup> I find this statement particularly true in the case of the NPEE. By using the records made by the Japanese government during the NPEE's visit, I have argued that the NPEE's scientific discoveries—Gray's disjunction thesis included—resulted from a series of negotiations among various parties regarding what a “surveying expedition” meant and what it was meant to be. For example, for Commander Rodgers, science and diplomacy were two sides of the same coin, whereas for local officials in Shimoda and Hakodate, science merely served as an excuse for a military man like Commander Rodgers to diminish Japan's status in the international pecking order. The issue at stake is that historians often assume that

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<sup>19</sup> Kohler, *All Creatures*; Laubacher, “Cultures of Collection...”

<sup>20</sup> Here I find Warwick Anderson's analysis on “postcolonial studies of science” particularly relevant to my discussion; Anderson, “From Subjugated Knowledge to Conjugated Subjects: Science and Globalisation, or Postcolonial Studies of Science?” *Postcolonial Studies* 12, no. 4 (2009), 389-400.

an expedition can be divided up and analyzed as part of military history, diplomatic history, or the history of science. My analysis shows that science, military matters, and diplomacy were so commingled in the course of a scientific survey like the NPEE that these aspects cannot be separated and rigorously studied alone. Likewise, Wright's correspondence indicates that he eagerly recruited local Japanese people to advance his collecting endeavors. In one way or another, local people's knowledge about plants as food and as drugs must have integrated itself into Wright's collections.

Furthermore, this dissertation has argued that shifting our focus to the collector in the field is important not just to provide background information about the making of a scientific theory. I argue that Wright's field practices struck to the core of Gray's disjunction thesis. The point at stake is this: Ever since Gray made his diagnosis of Japan's flora known to the world of science, botanists have been puzzled by how Gray could have detected such striking similarities between the flora of Japan and that of eastern North America. In an essay published in *New Phytologist* in 1913, for example, Japanese botanist Hisayoshi Takeda (1883-1972) challenges Gray's estimation that "more than 60% of Japanese plants grew on the eastern coast of North America, or if not, were represented by closely related species." Although Takeda greatly admires Gray's pioneering role in placing Japan's flora in an evolutionary scheme, he notes that botanists would be wrong to believe that Japan's flora bore such striking resemblance to its North American counterpart as Gray claimed. "[T]he vegetation of Japan, except that of the northern islands, has little actual resemblance to that of N. America," he remarks. "Although the same or

closely allied plants occur in both regions, they are not found in the same proportion.”<sup>21</sup>

I answer this puzzling question by closely reading Wright’s family correspondence, particularly focusing on Wright’s collecting strategy. My conclusion is that Gray detected what he understood as striking floristic similarities because Wright had consciously collected those Japanese plants that “reminded him of home.” But why did Wright practice such a seemingly unscientific collecting strategy? First, it should be noted that no natural-history collecting can be absolutely comprehensive and systematic. Collecting, after all, involves complex mental processes, including judging what is worth collecting according to the collector’s available resources (time, energy, knowledge about the flora, amount of drying paper, transportation, and so on). Wright’s family letters indicate that when he thrust himself into exploring the flora of Shimoda and Hakodate (as a collector appointed to a governmental surveying expedition under local Japanese officers’ surveillance according to the Treaty of Kanagawa), he aspired to collect those plants that reminded him of the New England flora. What drove him to amass “a choice collection of plants”—in Wright’s words—was that strange sense of *déjà vu* rather than curiosity about things exotic. Second, Wright consciously collected those plants that looked familiar to him because in so doing he could easily catalogue his Japan collections—a great motivation for a traveling collector with no easy access to herbaria and botanical libraries. Although surviving archival material cannot help us evaluate the extent to which Wright’s special collecting strategy might have

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<sup>21</sup> Hisayoshi Takeda, “The Vegetation of Japan,” *New Phytologist* 12, no. 2 (1913), 44.



influenced Gray's diagnosis of Japan's flora, it seems fair to say that Wright's collecting practices shaped the course of the Gray-Agassiz debate. Had Wright decided not to collect plants that reminded him of the New England flora but to focus on those plants peculiar to northern Japan, how could Gray have developed his disjunction thesis? And had Gray developed a thesis showing that the flora of Japan only bore remote resemblance to that of eastern North America, what would have become of his debate with Agassiz?

Finally, it is worth noting how *arbitrary* it was for Gray to declare the North Pacific the primary bridge through which the Japanese and American floras communicated with each other. In his presidential address before the Botanical Society of Edinburgh, British naturalist Andrew Murray first brought up this issue. First, Murray informed his audience that Gray had highlighted the role of the North Pacific on the basis that "the identical Japanese species which occur in Europe are 157; in Eastern America, 134; in Western America, 120." "The natural inference from this," he commented, "if no other facts come to derange our calculation, is that there plants have come from Japan to Western America *via* Europe—that is, first to Asia, then to Europe, then to Eastern America, and last to Western America." Why didn't Gray adopt this straightforward conclusion instead of relying on some obscure, still quite unsupported geological theory to make sense of the data? To Murray, Gray's disjunction thesis seemed to reflect the NPEE's ultimate goal: that the North Pacific should serve as the bridge between North America and East Asia.<sup>22</sup>

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<sup>22</sup> Andrew Murray, "The President's Address," *Transactions of the Botanical Society* 6 (1860), 313.

In a sense, my analysis concurs with Murray's critical opinion: that Gray's disjunction thesis did reflect the scientific interests of the NPEE. But instead of claiming that Gray's close association with the NPEE might have "distorted" his disjunction thesis, I emphasize the institutional and scientific basis of Gray's "arbitrariness" in making this bold biogeographical statement. As I showed in Part II, during the late 1840s and the early 1850s, Gray hardly made any biogeographical accounts of the collections he enumerated. His reluctance to do so, I argued, was largely because he did not receive topographical information compiled in a way that a closet naturalist like him could understand. The "exacting sciences" emphasized by the NPEE furnished Gray with rich geographical information which he could use to better figure how "ancient dispersions of species" might have given shape to the disjunct distribution of species (Figure C-2).

In historiographical terms, my analysis of the making of Gray's disjunction thesis goes beyond the well-developed account of the reception of Darwinism in the United States to a direct dialogue with the body of literature that arose surrounding Bruno Latour's innovative concepts of "immutable mobiles" and "centers of calculation." Here it is worth noting Neil Safier's essay on "Global Knowledge on the Move" (2010). In Safier's words, Latour's concepts help historians of science formulate an influential approach that places an emphasis "either on the omnipotence of the imperial center or the centrality of the colonial periphery," with keen focus on "the commensurability of knowledge." But he argues that the approach sees "only half the picture." By using Alexandre Rodrigues Ferreira (1756-1815)'s expedition (1783-1792) to Amazonia as an example, he suggests "three possible frames that

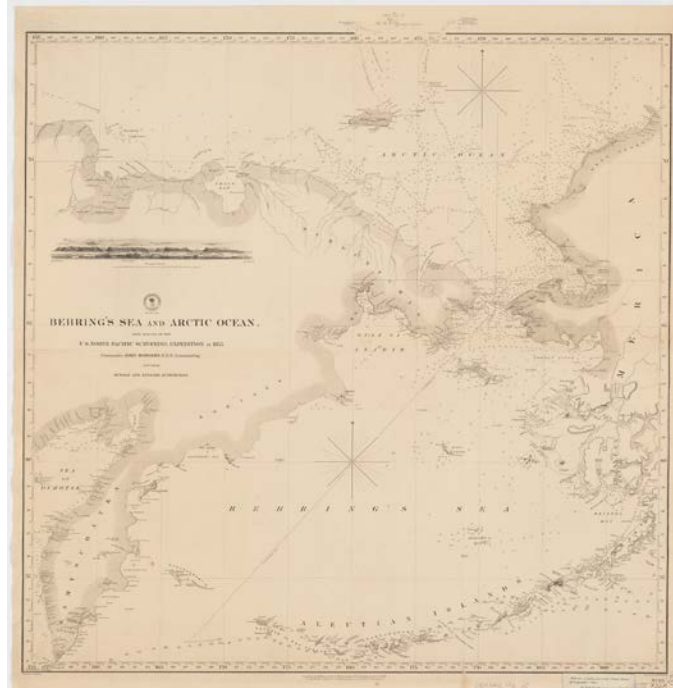


Figure C-2. The chart of the North Pacific produced on the basis of the NPEE (From United States Hydrographic Office. *Behring's Sea and Arctic Ocean: From Surveys of the U.S. North Pacific Surveying Expedition in 1855, Commander John Rodgers U.S.N. Commanding and from Russian and English Authorities* Washington, DC: U.S. Navy, Hydrographic Office, 1882. Courtesy of the Harvard Map Collection, Pusey Library, Harvard University, Cambridge, MA, USA.)

may help to create more seamless histories of science that include indigenous actors and categories: an emphasis on itineraries and connected histories; attempts to redefine traditional indigenous knowledge on its own terms; and... the use of deep history to rethink more remote strands of science and its histories in a global frame.”<sup>23</sup> I find Safier’s analysis relevant not only because of its modification of the approach based on Latour’s concepts but also because it reminds Darwinian scholars of how much current interest in “global Darwin” still grows along the East-West dichotomy. It is interesting to note that studies aiming to combine Darwinism

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<sup>23</sup> Neil Safier, “Global Knowledge on the Move: Itineraries, Amerindian Narratives, and Deep Histories of Science,” *Isis* 101, no. 1 (2010), 133, 136-137.

with East Asian studies are mostly confined to such issues as the reception of Darwinism in Japan or in China in the late nineteenth and early twentieth centuries. If we can accept that Gray's disjunction thesis in one way or another participated in the Darwinian Revolution, then my dissertation has shown that the natural history of East Asia was part and parcel of the Darwinian Revolution, or, to put it the other way around, that the Darwinian Revolution had significant roots in the intellectual and material exchanges between the East and the West, particularly during the mid-nineteenth century. I expect that by clarifying this point, this dissertation will offer a fresh baseline for ongoing studies on the reception of Darwinism in East Asia.

### **A Darwinian Revolution?**

How could historians of science connect Gray's disjunction thesis with the well-studied Gray-Agassiz debate or the "first Darwinian debate in the United States"? In historiographical terms, since Hunter Dupree made this characterization in 1959, historians of science have long acknowledged the role played by the Gray-Agassiz debate in initiating the Darwinian Revolution in American science and attributed historical significance to it as some American version of the famous Huxley-Wilberforce debate at Oxford in 1860. But it is worth noting that even Dupree himself admits that this "first Darwinian debate" was not as Darwinian as historians of science might expect. "Many of the most inflammatory controversies usually connected with Darwinism were startlingly absent from the first American debate," he points out.<sup>24</sup> In effect, as I have shown, Gray's thesis cannot be considered as

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<sup>24</sup> Dupree, "The First Darwinian Debate in America," 560.

Darwinian as historians of science have long understood the term, and its making was part and parcel with the United States' Western expansion and scientific imperialism. In what sense can historians of science still feature the Gray-Agassiz debate as the "first Darwinian debate in the United States"?

Here it is useful to refer to Jim Endersby's argument in his scientific biography of Joseph D. Hooker. Reviewing current literature on Hooker's role in the Darwinian Revolution in Victorian science, Endersby points out that historians of science appear to have been puzzled by what he considers a false question: "[W]as Hooker eagerly waiting to make use of his friend's theories, or was he a late convert, only reluctantly pinning his colors to the Darwinian mast?" He claims that his answer to this question is "neither." "[A]sking 'when was Hooker converted?' is a product of the popular myth of the Darwinian revolution," he argues. To better understand the extent to which Darwin's theory changed Victorian science, he suggests that historians of science ask questions like, "What made natural selection useful to Hooker?" By closely analyzing Hooker's scientific practices—for example, corresponding, associating, governing, and traveling—Endersby argues that Hooker's seemingly ambivalent attitude toward Darwinism stemmed from his peculiar status as a man of science who had to make a living by classifying plants. In Endersby's opinion, Hooker used Darwin's doctrine to "philosophize" his job so that he could gain status as a "philosophical botanist," but he also was concerned that Darwin's doctrine would encourage "species-mongers" and thus threaten the foundation of systematic botany. "The question of when Hooker became a Darwinist," he argues, "privileges the 'intellectual'—theories and concepts—over

the practical—collecting and classifying,” a “wrong question” that yields little historical insight. “If instead we ask what use Hooker made of Darwin, considering the question from the perspective of Hooker’s career and interests, it becomes clear that for him, as for many of his contemporaries, stabilizing species was the big question, not least because stable species were the key to transforming botanists from mere ‘species-mongers’ into respected members of the scientific community.”<sup>25</sup>

Endersby’s close analysis of Hooker’s career and its relationship to Darwinism offers this research a comparative case. First, like Endersby, I disagree with the view that Gray must have experienced some Eureka moment in 1857, when Darwin confided in Gray his theory of evolution by natural selection in response to Gray’s “convince me” letter. As I argued in Chapter 9, inspired by such novel ideas as individuality and nature’s seriality advocated by German botanist Alexander Braun, Gray in the 1850s had quite independently developed a view on species akin to Darwin’s. My analysis shows that Gray accepted Darwin’s doctrine almost without reluctance, to the extent that he even believed that his view on species could “homologize” Darwin’s more easily than Hooker could possibly have done. Here it is worthwhile to elaborate more about how *little* Darwin’s doctrine changed Gray’s view on species. In *First Lessons* (1857), Gray composed a footnote remarking on the limitations of any classification of species.

The best classification must fail to give more than an imperfect and considerably distorted reflection, not merely of the plan of creation, but even of our knowledge of it. It is often obliged to make arbitrary divisions where Nature

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<sup>25</sup> Endersby, *Imperial Nature*, 320, 327.

shows only transitions, and to consider genera &c. as equal units, or groups of equally related species, while in fact they may be very unequal,—to assume, on paper at least, a strictly definite limitation of genera, of tribes, and of course, although observation shows so much blending here and there of natural groups, sufficiently distinct on the whole, as to warrant us in assuming the likelihood that the Creator’s plan is *one of graduation, not of definite limitation*, except as to the species themselves.<sup>26</sup>

In 1868, after years of serving as a spokesman and defender of Darwin’s theory in the United States, Gray came to revise *First Lessons* for a generation of students in botany who had been exposed to the influences of Darwin’s *On the Origin of Species*. Gray must have pondered the extent to which Darwin’s species theory had changed his view of nature and of classification as he came to revise the paragraph quoted above. Finally he copied the entire paragraph but changed the last sentence from “except as to the species themselves” to “even perhaps to the species themselves.”<sup>27</sup> This revision, small as it is, speaks profoundly about what the Darwinian Revolution meant to Gray. In Gray’s view, Darwin’s theory erased what he had regarded as the last exception in nature. To him the principle that the “Creator’s plan is *one of graduation, not of definite limitation*” was now able to explain any issue involved in classification. Worth noting is that when Gray made this minor change, he was also trying relentlessly to *convince* Darwin that the whole subject of natural selection scarcely conflicted with natural theology and design.

Still, if we follow the approach developed by Latour, Desmond, Endersby, and McOuat, and shift our focus to Gray’s scientific practices, a much richer picture

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<sup>26</sup> Gray, *First Lessons...*, 195-196.

<sup>27</sup> Gray, *First Lessons...*, 195-196; Asa Gray, *First Lessons in Botany and Vegetable Physiology* (New York: Ivison, Phinney, Blakeman & Co., 1867), 196.

emerges. As I showed in Part II, Gray the 1850s grew increasingly inclined to regard the flora of North America as a catalogue of species. This transition stemmed from his early collaboration with George Engelmann in exploring the flora of Texas and New Mexico using the subscription system. It was the period when distinguished natural-history establishments in Europe began to standardize an array of critical issues in taxonomy and nomenclature (in the words of McOuat, its overall effect was to promote a definition of species that “species are merely what competent naturalists say they are”). Faced with rules that (to American naturalists) seemed to favor European naturalists, most of whom were equipped with unparalleled herbaria, museums, and libraries, American naturalists became deeply anxious that they would be deprived of their “right” to name and describe American species.

Gray and Engelmann’s use of the subscription system to explore the country’s new territory largely reflected this rising anxiety among American naturalists. As I detailed in Part II, throughout the late 1840s and the early 1850s, the two botanists recruited talented and intelligent collectors, sent them to the field, searched for institutions and individuals who could subscribe to incoming collections, compiled catalogues, collected subscription fees, and so on. I argued that it was by undertaking the manual labor of cataloging species and charging subscription fees according to the catalogued species that Gray established his status as a metropolitan botanist. A critical consequence of Gray’s rising profile was that he came by sufficient “social capital” to secure his status as an authoritative cataloguer of species and as a “mint” or “storehouse” that produced well-pruned lists of American species as a means of transaction for American and European



botanists. (Gray's reputation as a gentleman naturalist, his connections with learned societies, his large herbarium and library, his insistence on following European standards to study the North American flora, and his wife's savings that allowed him to spend time and money on botanical studies all contributed to this higher profile.)

Supporting Gray's transition from college professor to metropolitan botanist was a group of "professional collectors" who aspired to make a living by collecting plants. By tracing the lives and careers of Gray's three close collectors, Lindheimer, Fendler, and Wright, this dissertation captured the "right" character of "professional collectors" in the minds of metropolitan botanists. First and foremost, to be a professional collector meant to be capable of identifying botanical novelties in the field, making specimens of high quality, preparing field notes for subscribers' reference, and contriving every possible means to deliver collections to subscribers' hands. A collector under the subscription system also had a certain moral code to follow. He should never attempt to enumerate his own collections or name any new species. He should place his full confidence and trust in metropolitan botanists and let them take charge of the matter. Most importantly, no matter how much physical hardship and mental turmoil he might suffer in the field, a professional collector always had to remain committed to his subscribers, keeping them well-informed and ensuring that the collector's suffering would not damage the value of the subscription, and he had to keep faith with the metropolitan botanists in charge. A collector under the subscription system, in a word, needed to be a trustworthy gentleman of science. He should never behave like those "species mongers" who had a penchant for multiplying the number of species to extract more money from his

buyers. And it was metropolitan botanists' duty to draw a fine line between professional collectors and species mongers.<sup>28</sup>

These two trends accelerated when Gray began devoting himself to enumerating collections brought back by professional collectors appointed to governmental surveying expeditions. As I discussed in Chapters 5 and 6, thanks to the Smithsonian's serving as a bridge between men of science and governmental agencies, Gray and his close botanical associates were able to closely coordinate their research with the country's territorial and commercial ambitions. Toward the end of the 1850s, a community of what I call "practical naturalists" emerged. As I argued in Chapter 6, characterizing these practical naturalists was their

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<sup>28</sup> For example, in May 1855, Gray described French collector Jean-Louis Berlandier's misconduct in ticketing and distributing specimens for Charles Short's reference (Short had purchased a set of Berlandier's Texan and Mexican collections):

I see ample evidence that Berlandier was not only a lazy lout, but a great *scamp*, as you say. I find he used to collect a quantity of a plant all at one and the same time, and distribute only a part under one number—and afterward give the rest under another number—Sometimes the same stock was distributed and sold under 3 or 4 numbers! (AG to CWS, 5/21/1855, Coker, "Letters...", 144)

In a letter to Engelmann in December 1855, Gray again remarked on Berlandier's misconduct:

You know what a scamp Berlandier was.—He collected a great stock—distributed a portion one year—say no. 993—the rest next year, say 2423. &c.—&c... His original tickets enable me to ascertain that [he] proceeded systematically with this knavery year after year,—sometimes selling specimens gathered from the same tree 3 or 4 times over, under as many diff't. numbers. (AG to GE, 12/10/1855, *AGPG*).

Gray mentioned Berlandier's "dishonesty" in Short's obituary as well. Asa Gray, "Dr. Charles Wilkins Short," *The American Journal of Science and Arts (2nd Series)* 36, no. 106 (1863), 131. For a description and reevaluation of Berlandier's collecting career, see Cornelius Herman Muller's introduction to Berlandier's *Journey to Mexico...*

commitment to turning the North American flora and fauna into a series of catalogues of species. By faithfully following the rules of access Baird of the Smithsonian had established, these practical naturalists obtained ample information about species' variation and variability, published their results in journals and proceedings, secured priority for American natural history, and turned a limited number of scientific establishments in the United States into homes for as many "type specimens" as possible. Compared to the 1840s, during which American naturalists chiefly undertook their studies on the basis of the free and liberal exchange of specimens and information, the 1850s witnessed rapid centralization and standardization in American natural history.

I argued that it was against this backdrop that Darwin's theory of natural selection became appealing to Gray. Here it is worth emphasizing that Darwin recruited Gray to join his network of correspondents not by gushing about his theory of natural selection but by encouraging Gray to tabulate those catalogued species, estimate their geographical ranges, compile statistics, and so on. As I described in Chapter 9, Gray was amazed by Darwin's methodology, and was proud that the catalogue of species that he had spent more than a decade carefully calibrating could satisfy the distinguished naturalist's appetite for facts. And when Darwin finally disclosed his thoughts on species and their evolution, Gray embraced them with enthusiasm. He did not consider Darwin's theory airy or overblown, because he could clearly see how his "botanical statistics" found the way to the core of Darwin's species theory. Though it was a "hypothesis," Gray had no doubt that Darwin's hypothesis was verifiable, so long as naturalists could observe nature

closely and consistently. Here we may refer to Richard Bellon's "Inspiration in the Harness of Daily Labor: Darwin, Botany, and the Triumph of Evolution, 1859-1868" (2011). To prove his point that Darwin's botany did not provide ironclad evidence for either evolution or natural selection, nor did it revise the conceptual structure of the *Origin*, Bellon quotes Gray's comment made in 1873 arguing that Darwin's botany "transformed the debate by showing how an evolutionary theory could guide original investigation in addition to reorganizing existing knowledge." Concurring with Bellon's insightful analysis, I want to add that it was Darwin's "ordinary" and yet "original" way of doing botany that truly captivated Gray and "converted" the American botanist to become an evolutionist.<sup>29</sup>

Clarifying what made Darwin's theory appealing to Gray will help historians of science shed fresh light on the Gray-Agassiz "first Darwinian debate." First, we should debunk a common myth that depicts Agassiz's "unpreparedness" in the face of Gray's presentation concerning the floristic relationships between the flora of Japan and that of eastern North America. As I pointed out in Chapter 7, almost right after his arrival in the United States, Agassiz had independently arrived at an understanding similar to Gray's disjunction thesis, and in effect had extended it to cover various departments in natural history, ranging from zoology, botany and embryology to anthropology and paleontology. Hence, early in 1859, when Gray

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<sup>29</sup> Richard Bellon, "Inspiration in the Harness of Daily Labor: Darwin, Botany, and the Triumph of Evolution, 1859-1868," *Isis* 102, no. 3 (2011), 396. I am also inspired by Bellon's important essay on how Darwin changed Hooker's view on species: Bellon, "Joseph Hooker Takes a 'Fixed Post': Transmutation and the "Present Unsatisfactory State of Systematic Botany', 1844-1860," *Journal of the History of Biology* 39, no. 1 (2006), 1-39.

eagerly spread his discovery of the striking similarities between the flora of Japan and that of eastern North America, Agassiz would not have taken issue with the discovery *per se*. What he found difficult to stomach was Gray's insistence on the single creation of species and on the approach that highlighted the earth's geological past and the migration of species. I argue that any account that attributes Gray's getting the upper hand in the debate because of Agassiz's lack of knowledge about Japan's flora and fauna is misleading.

The common account considering the Gray-Agassiz debate only as a Darwinian debate also overshadows a relevant phenomenon: Agassiz's rising tension with "practical naturalists" like Gray and Baird right before and after the publication of *On the Origin of Species*. In effect, throughout the 1850s, when Gray and other practical naturalists occupied themselves with the "descriptive catalog business," Agassiz vehemently protested against it. Although Agassiz heavily relied on governmental surveys like the Pacific Railroad Surveys (PRS) to acquire specimens, he refused to comply with the rules of access Baird had set up for men of science (enumerating the collections and publishing the results in the form of journal articles). In his *Contributions to the Natural History of the United States of America, Vol. 1* (1857), for example, Agassiz wrote page after page about the progress he made developing a natural classification of turtles, but seemed unwilling to spare a few pages to catalogue the PRS's turtle collections. Afterward, after receiving frequent inquiries from Baird about when the agreed-upon catalogues would be ready, Agassiz grew annoyed. He replied that a philosophical naturalist was not supposed to engage in the catalogue-making business. Agassiz's disagreement with

Baird's scientific "taste" also prompted him to file a variety of complaints (plagiarism, for example) with Secretary Henry to stop Baird and his assistants' work (to the extent that Baird exclaimed to Henry, "If Agassiz does much more of this small business I shall lose all respect for him").<sup>30</sup>

As I showed in Chapters 5 and 6, throughout the 1850s, Gray firmly allied himself with Baird and vigorously participated in Baird's "descriptive catalog business." Moreover, in return for the Smithsonian's patronage of his collecting endeavors, Gray always made sure that the Smithsonian housed complete sets of "important and authentic" specimens that represented the North American flora. The tension between Agassiz and practical naturalists like Baird and Gray finally surfaced in 1864, when Agassiz devoted himself to establishing the National Academy of Sciences. Despite Baird's heroic efforts to compile a complete catalogue of the country's fauna, Agassiz adamantly objected to Baird's election to the Academy (in Agassiz's opinion, only "philosophical naturalists" could be elected as members). To his dismay, however, Gray, Dana, Henry, and other influential men of science sided with Baird over Agassiz's objection. Agassiz felt defeated. In a letter to

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<sup>30</sup> See *PJH*, Vol. 8, 376. Agassiz's words did worry Henry. In August 1860, he wrote to Gray concerning a recent conversation with Agassiz: "Prof. Agassiz complained to me," he wrote, "that we were doing injury to the course of the advance of this branch of knowledge by the encouragement we were giving to young men to devote themselves to descriptive natural history instead of applying themselves to the higher study of the laws of structure and the philosophy of life." Henry then threw a series of questions to Gray, for example: "Is it important to make collections of duplicates in natural history?" "Is it important that ~~they~~ objects in natural history should be described in order that they may be identified?" "Ought the Institution to encourage young naturalists to devote themselves to the description of species? or to think impediments in this way by refusing free access to the specimens?" JH to AG, 8/14/1860, *PJH*, Vol. 10, 163-164. No reply from Gray to Henry's inquiries has been found.

Agassiz, Henry tried to console the furious and humiliated naturalist: "He [Baird] was the choice of a large majority of the cultivators of natural history; and although your opposition was honest in intention, and your position correct in general principle yet I think that had you prevailed in your opposition, a majority of all the naturalists would have resigned, and a condition of affairs would have been produced deeply to be deplored." "I presume the philosophical world also concurs with you," he went on, "that as a class of investigations those which relate to Physiology and the mode of production and existence of organic forms are of higher order than those which belong to descriptive natural history." Gray, on the other hand, informed Baird that he had "fully expressed" his "sentiments" in that "very injudicious getting up of [the] National Academy." He could hardly allow "the bearing off of your name to please a spite of Agassiz" to happen, he explained. "These and other things which my judgment does not approve, have led me (as they have others) to refrain from all participation in the concern, so far, what course I may adopt for the future I cannot now say."<sup>31</sup>

It turned out that Gray refrained from "all participation" in the National Academy for the rest of his life. (Afterward he suggested to Henry that the word "Washington, DC," should be added to the name of the National Academy, thus transforming it into a local scientific association.<sup>32</sup>) But regarding Agassiz and other like-minded naturalists' criticism of practical naturalists' "descriptive catalog business," he ended up adopting a course similar to the one Hooker had taken in

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<sup>31</sup> AG to SFB, 2/3/1864, Folder 5, Box 20, *CM*.

<sup>32</sup> AG to JH, 4/11/1874, Reingold, *Science in Nineteenth-Century America*, 223.

Britain: that is, justifying his labor spent cataloguing species by proving Darwin's theory of natural selection a truly philosophical one. On the one hand, he argued fervently that there was no essential conflict between Darwin's theory and Christianity, and that Darwin's empirical studies on the variation and adaptation of species could be understood as an excellent piece of work in natural theology. On the other hand, with Darwin's more botanical works published (for example, *On the Various Contrivances by Which British and Foreign Orchids Are Fertilised by Insects, and on the Good Effects of Intercrossing* [1862]), Gray admiringly said that Darwin's botany, if read alongside *On the Origin of Species*, represented precisely the science natural philosophers highly valued: empirical observation, sound judgment, and keen appreciation of the plan in nature (though not necessarily a divine one). In Gray's view, since Darwin's theory had proved that the "Creator's plan is *one of graduation, not of definite limitation*," it was now the business of practical naturalists to differentiate one species from another on the basis of devoted studies in large libraries and herbaria, and to publish their judgments of species in catalogue form. The catalogued species played such an important role as a means of transaction in naturalists' daily practice; they were too important to be left in the hands of those naturalists who had all sorts of "quiddity" about the "philosophical" definition of species and knew nothing about species-cataloging.

Still, as Endersby points out, Darwin's theory of natural selection was a double-edged sword for systematists like Hooker and Gray. That is to say, although systematists needed Darwin's theory as a philosophical basis for their taxonomic work, they were worried that the view that species were forever in a state of change



would encourage species-splitters to multiply the number of species. In Endersby's opinion, Hooker's strategy in handling this dilemma was to highlight the view that the evolution of species could only take place in an almost inconceivably slow manner. Gray adopted a similar tack. In his famous "Darwin and His Reviewers" (1860), for example, he assured his readers that naturalists could readily adopt Darwin's theory *and* retain the species concept developed by founding figures in systematic botany like Linnaeus and de Jussieu (that is, "a species is the perennial succession of similar individuals in continued generations"), because Linnaeus's and de Jussieu's species concepts left "untouched the question as to how and when the 'perennial successions' were established."<sup>33</sup> Then, in his talk titled "Sequoia and Its History" (1872), he explained what he called "organic nature":

Organic nature,—by which I mean the system and totality of living things, and their adaptation to each other and to the world,—with all its apparent and indeed real stability, should be likened, not to the ocean, which varies only by tidal oscillations from a fixed level to which it is always returning, but rather to a river, so vast that we can neither discern its shores nor reach its sources, whose onward flow is not less actual because [it is] too slow to be observed by the ephemerae which hover over its surface, or are borne upon its bosom.<sup>34</sup>

In 1882, fifty years after he decided to pursue science as a vocation and forty years after his appointment as a professor in botany at Harvard, the aged Gray claimed that "species are judgments" at the annual meeting of the American Association for the Advancement of Science. Gray's audience might have immediately recalled the botanist's major criticism of Agassiz's view of species: namely, that species were not "ideas." So what was the difference between the view

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<sup>33</sup> Asa Gray, "Darwin and his Reviewers," *The Atlantic Monthly* 6, no. 36 (1860), 420.

<sup>34</sup> Gray, "Sequoia and Its History," 163.

that regarded species as judgments and the view that regarded species as ideas? In Gray's terms, when naturalists came to consider species ideas, they inevitably assumed that "the objects from which the idea is derived cannot vary or blend, and cannot have had a genealogical connection," that "species exist only 'as categories of thought'—that, having no material existence, they can have had no material variation, and no material community of origin," and that "the practised eye" could always "infallibly determine" species. But in his view, he told his audiences, species were "judgments of variable value, and often very fallible judgments, as we botanists well know." Interestingly, after putting forth this definition of species, Gray went on to assure his audience that he himself and his four close and competent collaborators at Harvard, Senero Watson, William Gilson Farlow, Charles Sprague Sargent, and George Lincoln Goodale, would continue to produce trustworthy species catalogues for botanists.<sup>35</sup> Once again, Gray's view of species was intertwined with botanists' identity and botany's status in natural philosophy; it was not only scientific but also social and institutional. No wonder he could never accept that species were ideas or categories of thought in the Creator's mind. In light of Gray's frequent appearances in the history of science, this dissertation argues that

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<sup>35</sup> Gray, "Darwin and His Reviewers," 419-420; Gray, "Remarks...", 330. It is worth noting that Gray's emphasis on the subject was part of an international effort to standardize late nineteenth-century botany, see, among others, McOuat, "From Cutting Nature at Its Joints to Measuring It"; Christophe Bonneuil, "The Manufacture of Species: Kew Gardens, the Empire and the Standardisation of Taxonomic Practices in Late 19th Century Botany," in *Instruments, Travel and Science: Itineraries of Precision the Seventeenth to the Twentieth Century*, edited by Marie-Noelle Bourguet, Christian Licoppe, and H. Otto Sibum (London: Routledge, 2002), 189-215.

there is no single Asa Gray when it comes to Gray's thoughts on natural history and its relationships to other realms of knowledge. Historians of science should trace the eventual history of Gray as an American botanist, and of the world where he was renowned as a friend of Darwin.

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