CHAPTER 2

GRAY, HOOKER, AND DARWIN ON THE "SPECIES PROBLEM," 1853-1859

By the mid-1850s various lines of natural history research were converging on major questions dealing with the meaning, origin, variation, and distribution of species.[1] These questions are collectively known as the "species problem."[2] It was becoming increasingly difficult by the late eighteenth century to accept the view, apparently supported by the biblical story of Noah's Ark and Flood (Genesis 6-9), that all plants, animals, and humans had originated in a single location in Armenia and subsequently migrated to the far corners of the world. Eighteenth-century naturalists and explorers were fascinated by the stunning varieties of new plants and animals

[1]Janet Browne has written an indispensable history of biogeography, *The Secular Ark: Studies in the History of Biogeography* (New Haven: Yale University Press, 1983). I have leaned heavily on this work in the following paragraphs. She ties the emergence of British interest in and interpretation of biogeography in the early nineteenth century to the global expansion of the British empire in "A Science of Empire: British Biogeography before Darwin," *Revue d'Histoire des Sciences* 43 (1992): 453-75.

John A De Jong provides a perceptive discussion of three episodes of the "species problem" prior to 1860 that raised many of the same issues that would later be argued in the Darwinian debate: the controversy over the origins of the human race, whether unified or plural; the debate between Tayler Lewis, the staunch upholder of the Bible's priority over science, who contended that the biblical language made ample room for a developmental approach to life, and James Dwight Dana, the prominent Congregationalist geologist at Yale, who argued that there was no scientific evidence to support and much to deny developmentalism; and Gray's studies of geographical distribution that were spurred by Darwin's numerous questions. "American Attitudes Toward Evolution Before Darwin," (Ph.D. diss., University of Iowa, 1962), 269-345.

[2]Darwin did not "solve" the "problem" with understanding the nature of "species"; it is still present in philosophical biology. Ernst Mayr, one of the architects of the modern neoevolutionary synthesis, has elucidated the historical and philosophical contours of the "species problem" in numerous works. His latest book is *This is Biology: The Science of the Living World* (Cambridge: Harvard University Press, 1997).

being discovered throughout the world; European collecting cabinets bulged with the overflowing wealth of specimens. Linnaeus lent his genius to solving the immediate problem of bringing order to this massive confusion with his binomial classification of all organisms into the logical categories of genus and species.

As European naturalists inspected these specimens and gained greater knowledge of diverse geological structures and geographical contours of the earth, they discovered curious anomalies and embarrassing difficulties for continuing to assume that all species were created in a single location for which they were best suited. Gradually they came to replace the view of a single center of creation with the view that distinctive groups of plants and animals had originated in unique regions around the world. These regions, which came to be called "faunas" and "floras," gradually came to be seen as comprising a unique web of relationships among those creatures that were "designed" for each particular habitat.

Once Ararat had been abandoned as the single origin of plant and animal life, it was fairly easy, even obvious, to transform these distinctive biological provinces into centers of creation. A. P. De Candolle, the famous Swiss botanist, lent his authority and prestige to this move.[3] Now, rather than migrating long distances from a single center, species were assumed to have been created and best adapted for living

[3]The following articles add depth to our understanding of the crucial role that developments in biogeography played in shaping the discussion of the "species problem" among Hooker, Darwin, and Gray. Gareth Nelson, "From Candolle to Croizat: Comments on the History of Biogeography," *Jour. Hist. Bio.* 11 (Fall 1978): 269-305; Michael Paul Kinch, "Geographical Distribution and the Origin of Life: The Development of Early Nineteenth-Century British Explanations," *Jour. Hist. Bio.* 13 (Spring 1980): 91-119; R. Alan Richardson, "Biogeography and the Genesis of Darwin's Ideas on Transmutation," *Jour. Hist. Bio.* 14 (Spring 1981): 1-41.

in the region where they were found. Although this move may have solved the problem of caribou migrating across deserts to reach their home in the Arctic or the jaguar swimming the Atlantic to reach its home in South America, it raised many other questions about how and where plants and animals had originated. Had species been created from single individuals, pairs, or entire groups? How many provinces were there in which creative activity took place? How many species had been created in each province? How many places within the province had species originated? Had all species appeared at once, successively, progressively, or sporadically in each of the provinces? How were identical species located in widely separated regions to be explained? What insight into answering these questions could be provided by geology, paleontology, and botanical geography? These questions were the immediate source of the profound interest in the "origin of species" in the early nineteenth century that captured the imagination of Charles Darwin and Joseph Dalton Hooker.[4] Gray had the unique opportunity to engage each of these men on these issues between 1853 and the publication of the *Origin* in the fall of 1859.

Gray and Hooker on the Origin, Variation, and Geographical Distribution of Species

Questions revolving around the "species problem" were most vigorously considered and showed the greatest promise of providing credible answers in the

[4]M. J. S. Hodge, *Origins and Species: A Study of the Historical Sources of Darwinism and the Contexts of Some Other Accounts of Organic Diversity from Plato to Aristotle On* (New York: Garland Publishing, Inc., 1991) is an exhaustive analysis of the historical and intellectual context of Darwin's theorizing on the origin of species.

emerging sub-field of botanical geography. The unquestioned leader in that field was Joseph Dalton Hooker. His father, Sir William Jackson Hooker, was an internationally renowned botanist who transformed the Royal Botanic Gardens at Kew into an herbarium without international peer. Taking advantage of his father's high-placed government connections, young Joseph, just out of medical school at the University of Glasgow, secured a position as botanist aboard the *Erebus* for a four-year exploration of the southern hemisphere, 1839-1843. Under the command of Captain James Ross, already famous for his explorations of Antarctica, the *Erebus* was commissioned by the English Admiralty to establish accurate magnetic polar readings throughout the southern oceans, a critical necessity for guiding British commercial shipping and naval fleets. The *Erebus* voyage gave Hooker the unparalleled opportunity to botanize four great land-masses of the southern hemisphere, Antarctica, Australia, New Zealand, and Tasmania. The botanical riches amassed in these virginal botanical regions laid the foundation for Hooker's major contributions, both systematic and philosophical, to the field of botanical geography.[5]

[5]Leonard Huxley, ed., *Life and Letters of Sir Joseph Dalton Hooker* (New York: D. Appleton & Co., 1918), 1: 1-56. Joseph Hooker is long overdue for a major modern biography befitting his front-rank position as a botanist in the nineteenth century and influential role in shaping Darwin's perspective. W. B. Turrill has provided liberal extracts from Hooker's most prominent essays in *Pioneer Plant Geography: The Phytogeographical Researches of Sir Joseph Dalton Hooker* (The Hague: Martinus Nijhoff, 1953) and a concise introduction to his life in *Joseph Dalton Hooker: Botanist, Explorer, and Administrator* (London: Thomas Nelson, 1963). The ongoing publication of the Darwin correspondence makes it abundantly clear that Joseph Hooker played a far more central role in shaping Darwin's thinking than has traditionally been thought. Janet Browne first called attention to Hooker's pervasive, yet anonymous, influence on Darwin in "The Charles Darwin-Joseph Hooker Correspondence: An Analysis of Manuscript Resources and Their Use in Biography," *Journal of the Society for the Bibliography of Natural History* 8 (1978): 351-366. The Hooker-Darwin letters, many of them unpublished, represent the largest volume of correspondence with a single person in the Darwin archive. Hooker was perhaps one of the very few persons who could boldly challenge

Following another extensive botanical exploration through the Indian subcontinent that ended in spring 1851, Hooker renewed his friendship with Gray at Kew during Gray's second trip to England. Hooker arranged a propitious meeting with Darwin, by then a well-known and respected naturalist, and one of his confidants. Shortly after Hooker returned to England in 1843 from his southern voyage, Darwin had given him his plant collection from the *Beagle* voyage for classification, a shrewd investment that paid rich dividends. Darwin subsequently drew on Hooker's profound knowledge of the patterns and implications of geographical distribution for resolving the "species problem" through countless visits, extensive correspondence, and sharing of his "heretical" views to confirm his theory of the transmutation of species. Gray, also gaining an international botanical reputation, was welcomed into the elite circle of Hooker and Darwin and introduced to the fundamental questions of botanical geography that both men were then actively considering.

Soon thereafter Hooker established contact with Gray, whom he now considered his only worthy botanical peer. Their subsequent voluminous correspondence, much of it surprisingly unpublished, provides an intimate portrait of two professional friends wrestling with the weightiest issues and each other, concerning the meaning, origin, variation, and distribution of species. Hooker's

Darwin's ideas and still retain Darwin's respect. Similarly, the Hooker-Gray letters, many of them also unpublished, dwarf Gray's considerable correspondence with numerous other persons. Duncan M. Porter has published extracts from a number of these letters in "On the Road to the *Origin* with Darwin, Hooker, and Gray," *Jour. Hist. Bio.* 26 (Spring 1993): 1-38.

breadth of botanical experience, probing questions, and biting criticisms were critical in shaping Gray's growing sympathy for a derivative hypothesis concerning the origin of species.

Joseph Hooker, with his vast botanical field knowledge, and Asa Gray, with his extensive taxonomic understanding of plants, formed an ideal debating team for exploring the intricacies and ambiguities of the "species problem." In the parlance of the day, Hooker was a "field" botanist, one who gained his experience primarily through extensive explorations, and Gray was a "closet" botanist, one who worked on specimens that others sent to his laboratory for classification. Hooker's broad field experience played an important role in preparing him much earlier than Gray to accept the derivative hypothesis as a solution to the "species problem."

Hooker and Gray were both committed to the natural system of classification that Antoine de Jussieu introduced and A. P. De Candolle elaborated in the early nineteenth century to replace the artificial system of Linneaus. The hallmark of the natural system was its attempt to classify plants according to their multiple "natural" relationships or affinities rather than the increasingly unworkable Linnean system of counting the male and female parts of a plant. As liberating as it was in throwing over the artificial system of Linnaeus and as simple as it sounded, the "natural" system, based on establishing a more complex web of morphological relationships within the vegetable kingdom, soon ran aground in determining exactly what those "natural" relationships were. They were not empirically self-evident, as became

painfully obvious to Hooker.[6] Confronted with the rich diversity of unknown plant life around the globe, how could or should a botanist squeeze these plants into the appropriate logical classes of family, order, genus, species, and variety? How could one be certain that the specimen before him was a "representative type" rather than a variation? What range of differences, and what kinds of differences, qualified to establish a new species, genus, order, or even family? How should one explain, let alone classify, those "disjunct" species that, though appearing to be identical, were found in widely separated locations? These, and countless other questions, haunted the work of systematic botany for Hooker and Gray. There being no international botanical standards committee to enforce its rules, every botanist and enthusiastic amateur collector chose whatever they regarded as the "natural" marks of classification

[6]In light of this later development, Gray's early enthusiastic endorsement of the natural system ironically hints at its subsequent dead-end. In a highly complimentary review of John Lindley's *A Natural System of Botany* (London: Longman, Rees, &c., 1836) Gray extolls the virtues of the new "Natural System of Botany" in his concluding remarks: "The grand natural divisions of the vegetable kingdom are, therefore, perfectly obvious, and may be very clearly defined. With our present knowledge of vegetable structure no great difficulty is experienced in characterizing the orders or natural families, and all subordinate groups. The great desideratum has ever been to effect such an arrangement of the orders under the primary classes, that each family should be placed next to those which it most nearly resembles. This might easily be accomplished, if the idea once so strongly insisted upon by poets and metaphysicians, of a chain of beings, a regular gradation, by a single series, from the most perfect and complicated to the most simple forms of existence, had any foundation in truth. On the contrary, nothing is more evident, than that almost every order, or other group, is allied not merely to one or two, but often to several others, which are sometimes widely separate from each other; and, indeed, these several points of resemblance or affinity, are occasionally of about equal importance. A truly natural lineal arrangement is therefore impracticable, since by it only one or two out of several points of agreement can be indicated. As this method is, however, the only one that can be followed in books, all that can be done is to arrange the orders in such a manner as to offer the least possible interruption to their natural affinities. The number of orders is so large that practical convenience seems to require their arrangement into groups subordinate to the primary classes; and when manifestly natural assemblages cannot be recognized, we are obliged to employ those which, being less strongly marked, and distinguished by a smaller number of characters, are apparently of a more artificial nature." "Dr. Lindley's Natural System of Botany," *Am. Jour. Sci.* 32 (March 1837): 297.

for making new species. The result was a mass of uncoordinated descriptions and overlapping criteria for determining what was and was not a "species." The "natural" system was, ironically, obscuring rather than illuminating botanical understanding with the discovery of every new form of plant life. The gradual breakdown of the "natural" system of classification, coupled with the many questions on the origin of species, formed the basis for the "species problem" into which Hooker's botanical explorations sank him in the 1850s.

As a field botanist Hooker was, above all, a practical botanist; he was immediately concerned with establishing sound, workable guidelines for the systematic classification of plants. All of his questions about species arose from his immersion in the abundance and surprising distribution of plants, both known and unknown, that he discovered on his Antarctic and Indian voyages, rather than from any philosophical or theological commitments. His broad experience in coming to grips with the reigning weaknesses of contemporary botany played an important role in shaping the thinking of the less experienced Gray. Throughout their relationship, despite being Hooker's senior by seven years, Gray was the passive student reacting to Hooker's more theoretically invigorating studies.

Hooker was continually exasperated and overwhelmed by his inability to determine and create new genera and species from the maze of specimens he had collected in his Antarctic and Indian voyages. As he was working over the New Zealand flora, Hooker moaned that it must

> be the most difficult in the world, hosts of obscure Nat. ords. genera & species,

> dirty dioecious green black brown fleshy flowers; & nat Ords of single or few rare genera & that are not to be found out without both flower & fruit. Upwards of 90 Nat. Ords. to be known to make out every 8 plants! & nothing natural about any of them.[7]

This lament, reflecting the breakdown of the natural system of classification, was a constant refrain in virtually all of his correspondence with Gray in subsequent years. By April 1859, while finishing up his introduction to the Tasmanian Flora, Hooker moaned that he felt "as helpless as a live bug with a pin through it." He was so "heartily sick of the whole abortion" that he expected "nothing but deserved sneers" when it was published.[8]

His own difficulty in determining what was a species was compounded by the wide-spread confusion on what "specific" marks were most critical in determining what was a new species. Amateur, ill-trained botanists jumped at the chance to create a new species with their discovery of every trifling difference. This had led to the wild proliferation of genera and species and a chaotic maze of descriptions in which the same plant was given several different names by different botanists and in different parts of the world. "If every noodle that knows a cabbage from a Cabbage Palm is to set up as a describer or new species from every corner of [the] globe, because he finds a difference in his specimens then [this] is an end of Systematic Botany."[9]

[7]Hooker to Gray, 21 September 1853, GHA.

[8]Hooker to Gray, 30 April 1859, GHA; cf. Hooker to Gray, 24 March 1854, GHA; Hooker to Gray, 16 February 1856; Hooker to Gray, 20 February 1857, GHA.

[9]Hooker to Gray, 21 September 1853, GHA; Hooker to Gray, 27 October 1855, GHA; Hooker to Gray, 26 June 1856, GHA; Hooker to Gray, 15 October 1856, GHA; Hooker to Gray, 20 February 1857, GHA; Hooker to Gray, 2 January 1858, GHA.

Not only did botany suffer from these weaknesses, which would be bad enough, but it also suffered from debates by ill-informed persons on the permanence and variability of species. Any well-schooled botanist knew, Hooker groaned, that this question could not be answered without detailed, discriminating study of numerous specimens. Creating some sense of clarity out of this confusing "species problem" became Hooker's lifelong mission.

In December 1853 Hooker published his "Introductory Essay" to the New Zealand flora that offered his considered judgments on many "obscure subjects."[10] Although Hooker downplayed his philosophical interests, his essay makes it clear that he had thought long and deeply about every permutation of the "species problem." He was particularly compelled to challenge those "superficial observers" who "have never been trained to habits of observation, or of reasoning upon what they read in the book of nature, nor have they been grounded in the elements of natural science; they are hence prone to rely for information on these speculative subjects (which they seek with avidity) upon a class of works that are, with very few exceptions, by authors who have no practical acquaintance with the science they write about, or with the facts they so often distort."[11] Though not mentioned, Hooker's target most likely were those countless works of natural theology, written by amateur naturalists, that dressed up their shallow and distorted understanding of the natural world with a biblical gloss.

[10]Joseph Dalton Hooker, *The Botany of the Antarctic Voyage*, vol. 2, *Flora Novae-Zelandiae*, Part 1, *Flowering Plants* (London: Lovell Reeve, 1853).

[11]*Flora N.Z.*, xxvi.

The important arguments set forth in this essay demonstrate Hooker's sure grasp of the fundamental issues that bedeviled botany in the mid-1850s. His answers form a base-line for tracing his gradual shift of opinion, and Gray's response, over the next six years.

Hooker outlined four deceptively straight-forward, but pregnant, propositions that emerged from his study of the New Zealand flora: "1. That all individuals of a species . . . 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."[12]

Hooker warned his readers that the present state of botanical knowledge of any large area was so imperfect that there was no current possibility of providing any definitive answers to "the great questions as to the origin and permanence of species" until much wider studies of global floras were done. Nevertheless, he had assumed, for practical purposes, "that species, however they originated or were created, have been handed down to us as such, and that all the individuals of a unisexual plant have proceeded from one individual, and all of a bisexual from a single pair."[13] The progress of botanical science required that species

be regarded as permanently distinct creations, which have survived great

[12]*Flora N.Z.*, viii.

[13]*Flora N.Z.*, vii.

> geological changes, and which will either die out, or be destroyed, with their distinctive marks unchanged. We have direct evidence of the impoverishment of the flora of the globe, in the extinction of many most peculiar insular species within the last century; but whether the balance of nature is kept up by the consequent increase of the remainder in individuals, or by the sudden creation of new ones, does not appear, nor have we any way of knowing.[14]

As far as Hooker was concerned, "whether or not such a theory be consonant with that great mystery, the origin of organic beings, animate and inanimate, is not the point I would dwell upon." Hooker recognized there were other points of view, from those who required a plurality of origins to account for present diversity, to those who believed "in a progressive character of all organized nature, from the cell . . . towards which man is the last step reached." Hooker's views on the origination and permanence of species were merely provisional guidelines for providing order in systematic botany and facilitating communication among botanists. Any final pronouncement on the subject was currently premature; it could only be finally determined by careful observation and rigorous study of existing flora.[15]

Hooker contended that views on the variability of species were likewise compromised by arbitrary limitations based upon no discernible principles. He sharply criticized those naturalists who multiply species without end on the basis of "every minute character," assume that plants from widely separated areas are distinct

[14]*Flora N.Z.*, xxvi.

[15]*Flora N.Z.*, ix. Hooker commended the view of Gray, "the most able of transatlantic botanists," who argued that "all classification and system in Natural History rests 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."

species, fail to consider the important role of climate and soil in modifying species, and ignore the contents of the world's herbaria and what has already been described in books. [16]

As a result of his own meticulous study of species, Hooker's practice was to "regard dubious species as varieties, to take enlarged views of the range and variation of species, and to weigh characters not only *per se*, but with reference to those which prevail in the order to which the species under consideration belong." Increased study had deepened his view, Hooker commented, that there "were many causes which render it extremely difficult to determine the limits of species," in both their variability and their geographical distribution. Ultimately, botanists would be compelled to see species as far more variable than they did at present and thereby greatly reduce the number of described species to a more manageable number.[17]

Botanists who split species unnecessarily were also prone to assume that species were distributed over a fairly narrow and well-defined area. Hooker's extensive experience cast serious doubt on this notion, especially when he discovered that New Zealand shared its flora with many other, often remote, locations. "The too prevalent idea that the plants of newly discovered, isolated, or little visited localities must necessarily be new, has been a fertile source of the undue multiplication of species. There are very many cases of naturalists having been so impressed with this idea, that they have not thought it worth while to consult either books or herbaria

[16]*Flora N.Z.*, xii.

[17]*Flora N.Z.*, xiii, xiv.

before describing the plants from such spots." He further discovered that numerous plants in New Zealand went by different names in many other countries in which it grew, thus obscuring the reality that all of these so-called distinct species were really broadly distributed varieties of a single species.[18] Gray pressed home the conclusion: "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 circle* [italics mine]."[19] The chief target of these strictures was surely Louis Agassiz, the most articulate proponent of multiple centers for the origin of species.

Even presuming that similar or identical plants in widely separated regions belonged to the same species, Hooker still faced the awkward problem of accounting for their presence by "natural" means alone. Those who argued for multiple centers of creation had a simple answer: God demonstrated his creative power anew in each place and time. Such an answer appealed to neither Hooker nor Gray. Even those who believed that these isolated plants belonged to identical species were not sufficiently aware of the enormous difficulties they had to overcome in order to explain how these plants migrated such great distances with no visible means of transport. It remained a central question for Hooker (and a goad to Darwin).

Hooker noted that "during my voyage amongst the Antarctic islands, I was led,

[18]*Flora N.Z.*, xviii.

[19]"Introductory Essay," 339.

by the constant recurrence of familiar plants in the most inaccessible spots, to reflect much on the subject of their possible transport; and the conviction was soon forced on me, that, putting aside the almost insuperable obstacles to trans-oceanic migration," many of these plants were the least likely candidates for such transportation as existed. In short, there were overwhelming barriers to continue assuming that all plants found in widely disbursed locations that seemed to be identical belonged to the same species. At the same time "there appeared nothing in the botany of the Antarctic region to support or even to favor the assumption of the double creation, and I hence dismissed it as a mere speculation which . . . could only be regarded as shelving a difficulty; *whilst the unstable doctrine, that would account for the creation of each species on each island by progressive development on the spot, was contradicted by every fact.*" [italics mine] Hooker was already aware that this was the direction in which Darwin was moving but refused to follow on purely empirical grounds.[20]

Lacking empirical support for either the hazardous migration of plants across large expanses of ocean or their double creation, Hooker constructed a speculative explanation grounded in the "invaluable labors of Lyell and Darwin."[21] Edward Forbes, an accomplished geologist and naturalist, used the geological studies of Lyell and Darwin to show that plants migrated into the British Isles at different periods in the remote past via a land bridge, subsequently submerged, between similar botanical provinces in Britain and the continent. Hooker felt that such a fruitful hypothesis

[20]*Flora N.Z.*, xx-xxi.

[21]Hooker had read Darwin's *Essay* outlining his theory in 1844.

suggested "the possibility of the plants of the Southern Ocean being the remains of a flora that had once spread over a larger and more continuous tract of land than now exists in that ocean; and that the peculiar Antarctic genera and species may be the vestiges of a flora characterized by the predominance of plants which are now scattered throughout the southern islands." Hooker's provisional solution was that plants neither migrated vast distances against overwhelming odds nor were created in multiple locations; they were stranded in isolated locations throughout the southern hemisphere as a consequence of powerful geological, climatic, and numerous other changes that took place across vast stretches of time.[22]

If existing species were remnants of an ancient flora once extended throughout the southern oceans, that meant that vast numbers of species had already gone extinct. That would explain, Hooker surmised, how "the destruction of a species must hence create a hiatus in our systems, and I believe that it is mainly through such losses that natural orders, genera, and species become isolated, that is, peculiar, in a naturalist's eyes."[23]

A continuous land connection, at some remote period, between South America and New Zealand, Australia, Antarctica, and the Pacific Islands still left unexplained the presence of Antarctic plants on the mountains of New Zealand. This could be explained, Hooker postulated, by the southern hemisphere experiencing a cold as severe as had the northern hemisphere during its glacial period such that the Antarctic

[22]*Flora N.Z.*, xxi-xxv.

[23]*Flora N.Z.*, xxi.

flora extended throughout New Zealand. As the climate warmed and the Antarctic continent subsided, the Antarctic plants escaped to the mountain tops in New Zealand. Thus, Hooker noted, "the Alpine plants of New Zealand, having survived the greatest changes, are its most ancient colonists" and seemed to have been the most widely distributed throughout the globe. This pregnant observation played an important role in Darwin's theorizing.

Hooker concluded by defending himself against critics who would dismiss his ideas as wild and groundless speculations. "No speculation," he countered, "is idle or fruitless, that is not opposed to the truth or to probability, and which, whilst it co-ordinates a body of well established facts, does so without violence to nature, and with a regard to the possible results of future discoveries."[24]

Soon after receiving his copy of Hooker's "Introductory Essay" Gray praised its virtues to Sir William Hooker, declaring how "charmed [he was] with Dr. Hooker's Introductory Essay to Fl. N. Zealand."[25] Gray wasted no time in reprinting liberal extracts, accompanied by his own commentary, of "some topics of high interest to the philosophical naturalist" in the March number of the *American Journal of Science*.[26] Gray was impressed with the breadth and learning Hooker displayed in his

[24]*Flora N.Z.*, xxvi-xxvii. This important statement showed that Hooker was moving outside the classic Newtonian philosophical framework so recently enshrined in the work of John Herschel, *A Preliminary Discourse on the Study of Natural Philosophy* (1830). This was the same standard that Darwin later invoked and defended in his own work.

[25]Gray to William Jackson Hooker, 23 January 1854, GHA.

[26]"Introductory Essay, in Dr. Hooker's Flora of New Zealand: Vol. 1," *Amer. Jour. Sci.* 17 (March 1854): 241-252, (May 1854): 335-350.

essay. He raised one quibble with Hooker's first proposition, claiming that it would be advisable for botanists to assume that all individuals of a species had descended from a "common stock" rather than squabbling over whether that "common stock" was a single individual, pair, or large group. Personally, he favored species originating in large groups rather than pairs. He heartily endorsed Hooker's other three propositions. Even Hooker's speculative hypothesis on the origin of insular floras Gray commended as "a very important one, and worthy of the most extended and critical examination." If established it would undermine the strongest arguments for the multiple origin of species.[27]

Having chastised the amateurs and laid down sound general guidelines, Hooker and Gray retired to their correspondence to wrestle the fine points raised in Hooker's essay. Their long initial exchange explored the complexities involved in understanding disjunct species at considerable depth. Hooker moaned that botany was such an incomplete science that "there is not a single argument [presented in his essay] that will not cut both ways, and may not be turned pro and con species, specific centers, &c., &c." Yes, it was hypothetically possible, he allowed, that "two originally created distinct species so similar as to be almost indistinguishable, may exist in two widely sundered localities," yet "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 by geographical segregation,

[27]"Introductory Essay," 243-244, 348.

of specific value." Accept that possibility and you would be forced to swallow all of Agassiz and geographical distribution would become a "vain study." No, he was not fully committed to treating "species as created ab origine," but it was the "only one which really keeps the mind open to investigate, which co-ordinates all the elements of geography, system and physiology, and which keeps the observer's attention alive to the importance of studying collateral phenomena." Agassiz, though clever, holds "heresies" that "are too extreme for respect" and "contradicted by facts." Better to ignore such people than encourage them to spread their prejudices even farther.[28]

Gray, always ready for a good argument with a strong mind, was not as ready as Hooker to abandon "representative species," the term then given to identical or closely resembling species found in separated areas.[29] Gray was not ready to side with Agassiz's solution, but he felt that a strong argument could nevertheless be made for their existence based on several key assumptions he believed the two shared.

> Now, it seems to me, that if species were originally given each to a circumscribed and [illegible word crossed out] local area (which you maintain, even in the

[28]Hooker to Gray, 26 January 1854, Leonard Huxley, ed., *Life and Letters of Sir Joseph Dalton Hooker* (New York: D. Appleton & Co., 1918), vol. 1: 473-476.

[29]Since terminology shapes thought, it is important to understand what botanists then meant by the terms Hooker and Gray were disputing. In the second installment of his article on the "Statistics of the Northern Flora of the United States," *Am. Jour. Sci.* 23 (January 1857), 80, Gray offered the following critical definitions: *geographical varieties* were those cases in which plants from two locations differed in such ways that even competent botanists might classify them as distinct species; *very close representative species* were recognized as distinct, but could conceivably be reclassified as geographical varieties; *strictly congeneric species* were those cases of species closely resembling each other, but yet were neither geographical varieties or closely representative species; *divergent congeneric species* were those species that belonged to different sub-genuses; and *strictly analogous or representative genera* were those genera that closely resembled each other. These were the important terms by which early nineteenth-century systematic botanists tried to capture the obvious gradations among groups of plants, yet without "seeing" any genetic relationship.

> strictest form); -- if species were created with almost infinitely various degrees of resemblance among each other (which is just what 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?[30]

Gray assumed that Hooker accepted each of his premises: species originally confined to local areas; species created with infinite degrees of resemblance with others; and "congeneric" species being found in widely separated areas. *If* Hooker accepted those premises, Gray insisted in good syllogistic fashion, *then* it followed that botanists should expect to find "two closely resembling, yet originally distinct species in widely separated places of similar climate," and Hooker need not go to such speculative lengths to account for disjunct species. The key to Gray's argument was the assumption that "congeneric forms are often found in two widely separated areas." Given the current definition of a "congeneric species," this made perfect sense. Hooker, however, without yet being able to break free from this assumption, was beginning to challenge it based on his field experience.

Gray clarified what he understood by the characteristics that identified a species. He agreed that naturalists were compelled to offer credible evidence, "specific marks," to show that two individuals were, in fact, different species. Such evidence could not be decided by speculations or rules; it had to be gained solely by

[30]Gray to Hooker, 21 February 1854, microfilm of the Royal Botanic Gardens, Kew, Asa Gray Correspondence with J. D. Hooker Correspondence and George Bentham, held by the American Philosophical Society; hereafter, Kew, APS.

"observation and experience alone." However, a good deal of confusion was still possible. Because "some species are *extremely polymorphous*," individuals of those species will often show considerable variations, while the same degree of variation in other individual plants, will be enough to declare them separate species. Thus, Gray concluded, the *marks* we use to define a species cannot be taken for the reality of the species themselves; "they are only the convenient 'outward and visible sign of an inward grace.'" Gray is hanging on to an idealized view of species. That is the point of his analogy between the reality of species and the reality of grace: just as Christian baptism is the "outward and visible sign of an inward [and invisible] grace," so also are the marks naturalists use to define species as the outward and visible signs of the inward reality of species in this individual plant, marks which escape ultimate detection by naturalists.

Now the question became for Gray how the botanist could determine whether "two such representative forms geographically connected by intermediate stations" were disjunct members of identical species that had "spread till their limits meet," or whether they were "varieties of one species of which perhaps the original type has disappeared." He allowed that this was a question that could only be decided "with more or less probability according to our best judgment on the case, but we cannot pretend to decide it with anything like certainty." However, "if, with the mingling or approximation of the areas we find a shading off of the differences, then it is far more likely that they all belong to one species, and practically we should so consider them," since it agreed with "the universal law that the offspring strongly tends to

resemble the parent in all respects. . . . On the other hand, the perpetuation of that form so long as subjected to only the same external conditions and isolated from its fellows, is no proof that this form is itself a species." We could be more certain that it was a species if it "held its own while growing commingled with the related form."

Gray was even sympathetic to the "exceptional, inexplicable . . . origin of races," as well as species, since, once established, races follow the "universal and fundamental law of genetic resemblance," unless they are overcome by intermixture or reversion. He agreed with those who maintained that "the origin of races is not to be explained by the prolonged activity of physical causes," but must be understood as being "mysterious" or "miraculous." We can determine that "certain races did not arise from a common stock" when we learn that "no natural external causes have a tendency to produce them," while other races "as marked have arisen within historical times." At this point, Gray allowed for both the "natural" and the "mysterious" origin of races, though he insisted that the naturalist could not clearly distinguish between them.

Hooker's essay was a "god-send," Gray enthused. It contained such a "great amount of sagacious & practical observations" that could be used to good effect in challenging Agassiz. Since Agassiz missed the last Cambridge Scientific Club meeting where he presented the substance of Hooker's essay, Gray planned to give him an extra copy of Hooker's essay. He needed to be shown "that his own data do

not at all necessitate the conclusions he sometimes draws from them."[31]

Hooker thanked Gray profusely for his "capital long letter" that displayed such "close, clear, and accurate reasoning." Perhaps Gray should have written the essay instead of him. He admitted that he was "one of those cross-grained fellows who, after building up a tall tottering castle" cannot bear to have either a friend prop it up or an enemy knock it down." He quite agreed with what Gray had said about representative species but "groaned over the hitch in deciding what we are to agree to call a species in such cases." Yes, "generic resemblance" should have more weight than he had given it, but it was so difficult to establish that he had focused on other facts that, "less valuable," yet "afford scope for reasoning and debate." All in all, Hooker concluded that he really did agree with Gray; that was why he included Gray's definition of species. "I believe in you in short, quite as much as in what you wrote."[32]

Gray concluded the first round of their debate by admonishing Hooker for his effusive praise. His whole purpose in going into such elaborate detail in challenging some of Hooker's points was to "blow you up a bit" in private. Since he placed Hooker's opinion of his own work in such high regard, he had to be careful in receiving it when it came. Gray confessed that he would "much rather receive now and then a little sharp criticism from so good a judge and so frank and plain-spoken a person as you are." Apparently Hooker took this admonition to heart in their

[31]Gray to Hooker, 21 February 1854, Kew, APS.

[32]Hooker to Gray, 3 March 1854, *LLJDH* 1: 476-77.

subsequent rounds.[33]

Over a year later, following Darwin's first request for information on the distribution of alpine plants in the northern U.S. from Gray, Hooker and Gray resumed their debate on species. On looking over Gray's response to Darwin, Hooker blurted out that "you are certainly more cautious in species reducing than I am, & had best be." As for himself, he felt that he had "no choice but to go farther or believe in universal variation as the origin of species -- that's a fact." He then chastised Gray for what he took to be Gray's inconsistent views on species. Hooker claimed that Gray had based his definition of species on his comparisons of American species with the limited specimens he had found in European herbaria and those covered only a limited range. Even as much as Gray retained species that Hooker would lump together, Gray "confessed" to Darwin that he would "unite 2 of every 3, that an average Europ. Flora would keep distinct" and "that of your retained species many will be doubtless yet be united, & if the work [?] you, you should not be surprised at intermediate states turning all any day."

Although Gray was reluctant to admit it, Hooker contended that the only conclusion that Darwin could draw from Gray's comments was "that nature[']s observations is [*sic*] hurrying you inevitable onward to illimitable reduction by actual observation, & leaving less & less to be opposed by those who would unite all of those that are lost. The whole process or rather progress of descriptive Botany seems

[33]Gray to Hooker, 29 May 1854, Kew, APS.

to tend to the reunion of heaps of species." His own work in determining "selected suites of varieties, from so wide an area [in India], "drive me so to despair, that I feel at times inclined to put the pistol of progression to my head & die specifically -- i.e. as a species maker."[34]

After working four long and agonizing years on the Indian flora, Hooker and his colleague, Thomas Thomson, finished their *Flora Indica*.[35] Hooker's introductory essay gave him the opportunity to update his views on the "species problem." He accepted the views of Charles Lyell that species were "definite creations," on the grounds that it was "more probable that species should have been created with a certain degree of variability, than that mutability should be a part of the scheme of nature." But whether species were widely variable or mutable, Hooker warned, could only be settled by closely observing numerous plants in the field and herbaria and attention to the actual range of variation common in many species. Superficial students, taught only "that species are definite creations, . . . without being cautioned as to their power of variation within certain limits, finds, when he begins to observe for himself, that he has constant difficulty in determining their limits." Such people were prime candidates to throw up their hands and declare that species were "the arbitrary creation of systematists." A sound systematist must balance his belief in the

[34]Hooker to Gray, 28 July 1855, GHA.

[35]Joseph Dalton Hooker and Thomas Thomson, *Flora Indica: Being a Systematic Account of the Plants of British India* (London: W. Pamplin, 1855).

"fixity" of species with a clear understanding of their variability.[36] Variability and mutability were slowly becoming ends of a continuum for Hooker.

Since it anticipates a theme that Darwin made the centerpiece of his theory, it is worthwhile noting what Hooker had to say about the war of nature and how it illuminated our understanding of species. Hooker noted that

> plants, in a state of nature, are always warring with one another, contending for the monopoly of the soil, -- the strong ejecting the weaker, -- the more vigorous overgrowing and killing the more delicate. Every modification of climate, every disturbance of the soil, every interference with the existing vegetation of an area, favours some species at the expense of others. The life of a plant is as much one of strife as that of an animal, with this difference, that the contention is not intermittent, but continuous, though unheeded by the common observer.

Thus, plants that were widely distributed tended to adapt themselves in different ways to the local climate, soil, and relationship to other species. Hooker argued that the power of plants to either adapt themselves to their conditions or to resist those changes was "very much underrated" by systematists who often confused what they took to be a distinctive habit of a species for its local adaptation to conditions during its migration. This practice unnecessarily multiplied the number of species.[37]

Gray was impressed with the *Flora Indica*.[38] At the same time he offered a caveat to Hooker's stricture against hybrids being the source of new species, a move that Darwin would also exploit. Gray noted that hybrids "may readily be fertilized by

[36]*Flora Indica*, 20-21.

[37]*Flora Indica, 42.*

[38]"Flora Indica," *Am. Jour. Sci.* 31 (January 1856): 134-137; Gray to George Engelmann, 18 October 1855, *LAG* 2: 417-419; Hooker to Gray, 27 October 1855, GHA.

the pollen of either of its parents; and that if hybrid plants are occasionally produced in nature, they would ordinarily stand a very good chance of being fertilized in this way." Gray wondered if it was not possible that where "two or more generally well-marked forms in nature are connected by certain occasional individuals of intermediate character, is it not very supposable that two species may have partially blended in this way? At any rate, here is a *vera causa* . . . which requires to be taken into account, as has not yet been done, as far as we know." As infrequent as this may be, it cannot be "overlooked in a thorough discussion of the general question of the limitation and permanence of species."

Though hybridization played an admittedly minor role in creating variations in species, Gray allowed that, for whatever reason, it was a fact that "legitimate offspring do occasionally possess a character foreign to those of its parents." Naturalists knew that all domesticated species had a "tendency to 'sport' into races, whether of ancient or recent introduction." Gray observed that this "natural tendency to perpetuation of individual differences" was greatly strengthened and encouraged when species were segregated, as in domestication.[39] While still committed to the "fixity" of species, Gray, even more than Hooker at this point, seemed willing to allow a wide range of variability within species and the inexplicable origin of unique characteristics that could then be perpetuated in isolation to form a new race.

Hooker's frustration with the chaos in descriptive botany compelled him to

[39]"Flora Indica," 136. *Vera causa* was the standard Latin phrase that physical scientists used to mean the true or physical cause. Transferring this meaning to biology created enormous conceptual difficulties for Darwin, Gray, and all others who debated these issues.

argue that the crying need of the field was for synopses to clarify what was already known. These synopses could then be used as a foundation to answer critical questions that botany needed in order to make any progress. These could only be done by the very few botanists, like Gray, who commanded a breadth of botanical knowledge. He regularly urged Gray to set aside his tedious work on classifying new species and work on his *American Flora*. Someone with Gray's knowledge should not waste his time on a task that could easily be left to the countless "mere describers." It was high-time that Gray finished his "magnum opera." This remained a sensitive issue for Gray the rest of his life. He felt caught in the vise of overwhelming administrative and teaching responsibilities, yet yearned for the freedom to finish his North American flora. Hooker, on the other hand, fled teaching and even lecturing, choosing to spend the remainder of his life working on the *Genera Plantarum*, which is still unsurpassed as a synopsis of the world's flora. [40]

The widening breach between Hooker and Gray on how to resolve the "species problems" came into clearer focus in their divergent reviews of Alphonse De Candolle's *Geographie Botanique Raisonnee* published in 1855. Alphonse was the son of A. P. De Candolle, one of the major architects of the natural system of classification and a renowned botanist. Together as father and son they made Geneva an international center of botany. Both Hooker and Gray had been shaped by the philosophical perspective of the elder De Candolle; they were in regular

[40]Hooker to Gray, 2 February 1856, GHA; Hooker to Gray, 15 October 1856, GHA; Hooker to Gray, 15 December 1856, GHA; Gray to Hooker, 5 January 1857, Kew, APS.

correspondence with the son. Alphonse De Candolle adopted the prevailing botanical opinion that "the majority of species were created such as they now exist." With even more experience to support his views, Hooker retorted that there was not a shred of empirical evidence to support such a view.

Hooker's final comment on De Candolle's position provide an accurate barometer of his shifting position on the key questions of the "species problem." He pointed out that there simply was no overwhelming evidence to help botanists decide if species, whether created originally or by transmutation, had appeared in one location or several. Existing patterns of distribution showed that, "as a general rule, species are grouped in more or less restricted areas, after a manner that is quite consistent with the hypothesis of their having spread from one spot." There are, however, numerous cases of disjunct species that could not have resulted from migration. How could they be best explained? The only two options available were double creation and transmutation, both of which were encumbered with grave difficulties. Double creation takes us "further in to the regions of pure hypothesis" and adds "miracle to miracle to assume the same species to be created not only at two or more spots, but at two or more times, and under two or more forms." Transmutation has the advantage of "allowing more latitude for variation" and "extending one law now in operation." It would also mean that "the conditions that induce the change, and hence the race, need not have occurred at the same time at two or more spots, nor when they did occur would they act with equal power or upon exactly similar individuals, when the individual races would not be altogether similar."

While transmutation appeared to make the strongest argument, Hooker saw "no more means of forming an opinion on the subject of the origin of species, than we do of the origin of time."[41]

Hooker contended that, in any event, it was irrelevant to the progress of science how a botanist explained the origin of species. "On the one hand we cannot treat practically of the species of plants, either systematically or physiologically, save under the assumption that more are hereditarily permanently distinct; and on the other, we cannot study any species or organ physiologically or morphologically without being strongly impressed with the fact that variability is an ever-cooperating law." To illustrate his point Hooker observed that he believed that "fully one-half of the registered species of plants are reducible to races and varieties; with regard to the other half, whatever their origin may be, they are, in comparison, permanently distinct species." In the final analysis, Hooker believed that transmutation was a credible concept. However, for "species so to change as to assume all the characters of another within the limits of our experience, is for Nature to break one of her best-established laws: *Natura nihil facit per saltus*." Ironically, this dictum would soon be used to support the very opposite conclusion.[42]

Gray, already in regular correspondence with De Candolle, praised his new book. "I cannot sufficiently express my profound admiration of this book, so thorough and conscientious, so capital in its methods, and embodying such a vast

[41]"Essay Review," *Kew Journal of Botany* 8 (1856): 254-255.

[42]"Essay Review," 256.

amount of facts well discussed; it might well be the work of a long life." Curiously, he noted that Hooker had written him, "eulogizing your work in the highest terms."[43] Hooker's review was hardly a "eulogy" of De Candolle's merits as a geographical botanist.

Gray's own review was in marked contrast to Hooker's disgruntled dismissal. He praised it as "one of the most important works of our day," soon to be recognized as a standard treatment of questions that concerned a wide range of specialists, from botanists to physicists. "Along with the admirable methodical spirit which is his by rightful inheritance, the younger De Candolle brings to these investigations a particular aptitude for numerical and exact forms, an intimate acquaintance with general physical science, and considerable ethnological and philological learning."[44]

Hooker groused that he "had no practice with the sugar-bread & butter system of reviews" and was compelled to give his frank opinions. The book offered little to advance geographical botany. His notion that the shape of the areas occupied by species "inclines to be circular or elliptical," Hooker sputtered, was "mere pretentious rubbish." He offered to "lay down a shilling for every plant whose area you trace & which approaches either one or the other." De Candolle's mind "appears to contract matters than to expand with his subject." The best he has done is provide us with "a careful first-rate summary" of what was already known about the subject. But this was faint praise since there have been few studies that had not been "pretentious &

[43]Gray to De Candolle, 27 October 1855, *LAG* 2: 419-421.

[44]"Botanical Notices," *Amer. Jour. Sci.* 22 (September 1856): 429-437.

feeble." Hooker was most disappointed that with De Candolle "the really great problems are almost overlooked, simply because no one has put them forward, & he has no originality to conceive them himself."[45] Gray had little response. He admitted that Hooker was "thoroughly correct . . . in saying that he has not advanced the science at all. That is not in him. He has none of your originality of mind, nor high intellectual activity."[46]

Throughout 1857 Hooker and Gray carried on an illuminating philosophical debate on materialism's threat to biology that was sparked by the first edition of Gray's *Botanical Text-Book*. The debate revolved around the relationship between physiology and the essence of life in living things. Gray understood that the province of "physiological botany" was "the kind of life with which [the plant] is endowed, the organization through which its life is manifested; -- in other words, how the plant lives and grows, and fulfills its function."[47] The essence of biology (or physiology), for Gray, was the study of how an "unknown something," which he termed vitalism or vital forces, controlled "physical forces and chemical actions in plants" and made them "work out ends they would not otherwise do."[48] "Vital forces use and direct the physical something as the will uses and directs the vital forces." In sharp contrast Hooker believed that physiology, following the lead of his friend Thomas Huxley, was

[45]Hooker to Gray, 26 June 1856, GHA; Hooker to Gray, 15 October 1856, GHA.

[46]Gray to Hooker, 5 January 1857, Kew, APS.

[47]*The Botanical Text-Book* (New York: Ivison, Blakeman, Taylor & Co., 1857), 13-14.

[48]Gray to Hooker, 5 May 1857, Kew, APS; cf. Gray to Hooker, 6 July 1857, Kew, APS.

the study of physical forces and chemical actions in living things; thus, physiology had and abandoned the study of the "essence" of life. By reducing physiology to physics, Gray feared that Hooker had fallen into the widespread Epicurean "heresy" of materialism led by Justus Liebig and John William Draper.[49]

Hooker did not take Gray's charge of materialism at all seriously.

> With regard to Physiology versus Physics, I suppose you think me hopeless heterodox: I hate this class of subjects but I must acknowledge to be one of those who cannot see that it. . . [is] at all derogatory to the most Exalted conception of the Deity to suppose vitality to be another manifestation of Physics, & correlateable with . . . Heat; Magnetism, Motion, Sound, & which are all different manifestations of one & the same force--You seem to think this materialism, I do not; any more than it is to correlate the fall of a stone with the motions of the planets which was once Blasphemy or next door to it. I do not think vitality is Physics or is chemistry or is both, but I doubt not it will prove to be a manifestation of force of which Physics & chemistry are the very Exponent & interpreters however imperfectly & all our Physiological or vital energies are nothing in the world but hammering away at vital phenomena with the laws of Physics & including chemistry.

As Hooker understood it, vitality was the name given to the unique way in which physical and chemical forces worked together in living things. "Vitality includes a manifestation of every phenomena of Physics in each of its own phenomena, & and our energies consist in attempts to isolate them." Hooker looked forward to the time when there would be a single law that would coordinate the way that force manifested itself in every phenomena, "that will Enable us to solve the complex web of physical phenomena that make it a vital one."[50]

[49]Gray to Hooker, 5 May 1857, Kew, APS; Gray to Hooker, 6 July 1857, Kew, APS; Gray to Hooker, 8 September 1857, Kew, APS; Gray to Hooker, 12 October 1857, Kew, APS.

[50]Hooker to Gray, 27 August 1857, GHA.

Hooker, apparently exasperated with Gray's persistent effort to maintain that vitality was a unique "something" distinct from physical force, called in his friend Thomas Huxley for re-enforcement. Huxley was at the time preparing a course of lectures on "The Principles of Biology" that touched directly on these questions. Huxley admonished Gray to consider "that vitality is not merely force, but a mode of operation of force. . . . All vital force is, I believe, physical force transformed: -- but for all that Vitality is something as inexplicable" in terms of "Physicality." Huxley suggested that the relationship between vitality and "vitalized matter & . . . physical force" was similar to the relationship between rider and horse, though he failed to clarify what exactly this relationship was. [51]

Though chastising Hooker for sending Huxley his rough notes on vital force, Gray allowed that Huxley had "perfectly clear conceptions." However, it still seemed to Gray that the directing or controlling force in both the horse and in plants was something distinct from the forces that propelled the horse. Gray again emphasized that vitality was that "something" in plants, analogous to the will in humans, that guided all subsequent plant activity.[52]

By October 1857 Gray's contact with Darwin was beginning to make its mark

[51]fragment of a letter from Huxley to Hooker enclosed with Hooker to Gray, 24 December 1857, GHA.

[52]Gray to Hooker, 1 February 1858, Kew, APS. Gray was apparently impressed enough by Hooker's raving about Huxley, as well as what he had read by Huxley, to extend an invitation to deliver the Lowell Institute lectures the following winter. He was confident that Lowell would accept his recommendation, or even better, that of Charles Lyell. Huxley never came, for whatever reason. It would have been most interesting to have seen what "natural theology" topic Huxley would have lectured on. One can only imagine what the dynamics would have been to have had Huxley in Boston during Gray's debates with Agassiz at the American Academy.

on his thinking about the "species problem." He observed to Hooker that he was gratified that someone of Darwin's stature as a naturalist was working on these critical problems. Darwin was on to something important, he felt.

> 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 whenever it is rendered the more probable hypothesis, I shall feel no hesitation in adopting it as such.

He added the important qualifier that he certainly did not think that it would be "derogatory to the Deity to originate the diversity of plants and animals in this way." It would be "very presumptuous . . . for any man to say that."[53] Gray would soon have the opportunity to consider this question first-hand.

Gray and Darwin on Geographical Distribution

Having completed his arduous eight-year study of barnacles, Charles Darwin was ready to start working on his "big book" on species. On 9 September 1854 he recorded in his pocket diary that he "began sorting notes for Species theory."[54] The next March, after accepting the help of his cousin William Fox in counting, sorting, weighing, comparing, breeding, and even killing, a wide variety of fowl for his many experiments, Darwin explained that he was "hard at work on my notes, collating & comparing them, in order in some 2 or 3 years to write a book with all the facts &

[53]Gray to Hooker, 12 October 1857, Kew, APS.

[54]Gavin de Beer, ed., "Darwin's Journal," *Bulletin of the British Museum (Natural History) Historical Series* 2 (1959): 13.

arguments, which I can collect, *for & versus* the immutability of species." His purpose was "to view all facts that I can master (eheu, eheu, how ignorant I find I am) in Nat. History (as on geograph. distribution, palaeontology, classification, Hybridism, domestic animals & plants &c &c &c) to see how far they favour or are opposed to the notion that wild species are mutable or immutable."[55] William Fox was now firmly enmeshed in Darwin's far-flung web of scientific aids from whom he solicited help on numerous questions he was considering in his "big book." He would soon enlist the aid of another correspondent from across the Atlantic, Asa Gray.

While fussing with pigeons and ducks, Darwin was deep into his chapter on geographical distribution. He clearly had the "species problem" in view.

> As I believe that all organic beings are produced by the ordinary laws of reproduction which includes, according to the theory under discussion, modification of specific forms, & as it is exceedingly improbable that the *same* species should ever have been generated in one place from one set of parents, & in another place . . . from another set of parents specifically dissimilar, the first & most obvious question is whether we can account on the ordinary notion of propagation for the existence of the same identical species in all quarters of the world. -- *This is the question, which has long agitated naturalists, namely whether the same species has been created once & therefore at a single point, or more than once at different points.*[56]

The most serious objection to this view, Darwin noted, was how to explain the existence of identical species of Arctic and alpine plants and animals on widely separated mountain tops. To make the answer even more difficult, Darwin abandoned

[55]Darwin to W. D. Fox, 19 March 1855, *CCD* 5: 288-289; Darwin to W. D. Fox, 26 March 1855, *CCD* 5: 293-295.

[56]R. C. Stauffer, ed., *Charles Darwin's Natural Selection: Being the Second Part of His Big Species Book Written from 1856 to 1858* (Cambridge: Cambridge University Press, 1975), chapter XI, "Geographical Distribution," 534. Italics mine.

migration across ancient land bridges that Edward Forbes, Joseph Hooker, and others had conveniently constructed, as needed, to facilitate the migration of every isolated species.

The answer to the "species problem," Darwin came to see, lay in the variability of species. His barnacle study had convinced Darwin that variation occurred far more widely and constantly in all parts of a species than he had formerly thought and expressed in his *Essay of 1844*. Although variation was the key, he knew that he could not answer the question with direct evidence; the question could only be approached obliquely by inference. It struck him that he would gain some insight into this problem by investigating the distribution patterns of large genera, those groups with the largest number of species and varieties. Presumably, these large genera would be most likely to contain a larger number of "close" species, those species that were intermediate between existing species and their varieties. These large genera would also be spread out over a larger geographical area where they would be subject to diverse conditions of living. This would lead to unique local adaptations. The overall result would be a finely graded series of closely related organisms in large genera. A consistent pattern of large genera comprising many "close" species would indirectly confirm the variability of species.[57]

These were the questions that were uppermost in Darwin's mind when he took pen in hand to solicit the help of Asa Gray, the authority on North American botany.

[57]Browne, *Secular Ark*, 202-206. Browne has devoted considerably more attention to this topic in "Darwin's Botanical Arithmetic and the 'Principle of Divergence,' 1854-1858," *Jour. Hist. Bio.* 13 (Spring 1980): 53-89.

In March 1854 Hooker had passed along Gray's long letter in which he responded to Hooker's introductory essay in *Flora N. Z.* Darwin was impressed with how well Gray wrote, his sharp criticisms of Agassiz, and his views on the possible crossing and obliteration of species. At the same time, "to see his & your caution on the species-question ought to overwhelm me in confusion & shame." In order to carry out his studies of the relationship between species and varieties in large genera he had to rely on existing floras of large geographical areas. Gray's *Manual of the Botany of the Northern United States* (1848) was ideally suited to provide important information on North American botany, then little understood in Europe and England. In March he requested Hooker's copy of Gray so that he could begin tabulating the "naturalised species" in North America. By April, "at cost of more trouble than worth," he had compiled a list of what he could make out were "naturalized plants," though Gray's terminology made their identity difficult to determine. [58] It was now time to contact Gray directly.

As was his custom, Darwin was most solicitous in seeking information from his network. He reminded Gray that he "had the pleasure of being introduced" to him during his visit to Kew in the spring of 1851. Stepping rather gingerly around his main interests, he told Gray that he had "been collecting facts on 'Variation', & when I find that any general remark seems to hold good amongst animals, I try to test it in Plants." To help him test some of his ideas, he had a "great favour" to ask that he

[58]Darwin to Hooker, 26 March 1854, *CCD* 5: 186-88; Darwin to Hooker, 7 March 1855, *CCD* 5: 279-280; Darwin to Hooker, 13 April 1855, *CCD* 5: 304-307.

hoped would not "cause you much trouble & will greatly oblige me." Perhaps with a sly wink as he wrote, Darwin was quick to add that Gray should answer these questions only from memory and not take extra time to consult any authorities.

Actually, he had a miscellany of questions that would cost Gray time and energy that he could ill-afford in the midst of his hectic schedule. Darwin was curious about the alpine flora of the U.S. On the enclosed list of indigenous species he had earlier tabulated from Gray's *Manual*, Darwin asked Gray to simply note, alongside each species, whether this species was confined to the U.S., or whether it was also found in the American Arctic, the European Arctic, any European mountains, or in the Asian Arctic. By the way, could Gray please give him the distance between the mountains of Vermont and New York. Finally, It would also be helpful to have a list of the flowering plants in North America that were common to Europe. Darwin was just testing the waters to see how much he could divulge about the true contours of his project.[59]

Gray was happy to provide Darwin with whatever information he had at hand, though he regretted that he could "do no more than to furnish some few data when asked for, that others, who happily have leisure for such inquiries, may work up." He had always hoped that he would have time after his *Flora of North America* was finished to take up the interesting questions Darwin had raised, but, alas, he doubted whether it would ever be finished. As he was in the midst of revising his *Manual* he

[59]Darwin to Gray, 25 April 1855, *CCD* 5: 322-323.

would be delighted to have Darwin tell him exactly what information he would find most useful and he would provide, and even publish, it. In the meantime, he complied with his request for information on the species common to both the U.S. and the other regions and gave him a thumbnail sketch of the alpine flora in the northeastern part of North America. Gray's sharp attention to botanical details, even though he was still unclear exactly how Darwin was going to use them, was already serving Darwin well.[60]

Gray had passed the test. It was now time to put him to serious work. Darwin wasted no time in following up Gray's timely and instructive response with more elaborate requests that explored the geographical distribution of North American flora in considerably more depth. He reminded Gray that "the great-goodnature of your letter to me, has been partly the cause, so that, as is too often the case in this world, you are punished for your good deeds." Darwin's supposition about the geographical range of species and large genera required a more sophisticated presentation of traditional floras in several different ways. It was now time to compel Gray to work out the "botanical arithmetic" of the North American flora.[61] He wanted Gray to show which plants America had in common with Europe, especially those in the northeastern quadrant of North America. Darwin also wanted Gray to

[60]Gray to Darwin, 22 May 1855, *CCD* 5: 334-336.

[61]cf. Browne, *Secular Ark*, chap. 3, "A Science of Pattern," is an essential history of "botanical arithmetic," following its introduction by Alexander von Humbolt in 1815 and subsequent widespread popularity in the early nineteenth century. Botanists were the first naturalists to recognize the value of statistics in analyzing masses of data.

indicate the range of plants found on the western side of the Rockies and those in eastern Asia. "The range of the plants, to the East & West, viz whether most found are in Greenland & Western Europe, or in E. Asia appears to me a very interesting point as tending to show whether migration has been Eastward or Westward." Gray was beginning to catch of a glimpse of where Darwin was heading.

Darwin then asked Gray for some statistical information to help him "speculate on means of transportal."[62] It would be most helpful if Gray could give "the proportions . . . to the whole of the great leading families," as well as "the proportion to the whole Flora of the European plants . . . *& of the separate great families.*" What was the proportion of species to each genera and what was the average number of species to genera? Darwin was working on the assumption, gained from Robert Brown and Joseph Hooker, "that near identity of proportional number of the great Families, in two countries, *shows probably that they were once continuously united* [italics mine]."

[62]Darwin carried out a host of curious experiments to demonstrate that seeds could have reached distant locations after many days in salt water and still germinate on arrival. He was determined to find some alternative means of dispersal to land bridges and submerged continents. Most importantly, such evidence would show how identical species could appear in widely separated locations and thereby undermine the Agassizian theory of multiple centers of creation. He sent along a report on one of those experiments, "Does Sea-Water Kill Seeds?," to the *Gardeners' Chronicle* in late May 1855. Gray had it reprinted in the *American Journal of Science*. Darwin concluded that, although all of his seeds sank in sea-water, it was far more likely that whole plants, rather than seeds, would have been washed out to sea. Everyone was familiar with how well the pods of the Compositae family closed when they got wet, as if preparing themselves for a long journey by water. After reaching a distant shore, the pods would dry, open, and disperse their seeds. This was most likely the way that "Nature sows her broad fields, and which have excited the admiration of every observer." Once dispersed the seeds would now face its "ordeal; will the old occupants in the great struggle for life allow the new and solitary immigrant room and sustenance?" Paul H. Barrett, ed., *The Collected Papers of Charles Darwin* (Chicago: University of Chicago Press, 1980), vol. 1: 255-258.

Darwin asked Gray to manipulate his data in his *Manual* in yet a third way. He would find it most helpful if Gray divided all of the species into four groups: 1) those common to both Europe and Asia; 2) indigenous species that were members of European genera; and 3) species wholly indigenous to the New World; and 4) genera common to Europe not found in America.

Finally, as if he had not already taxed Gray enough, Darwin pushed Gray for information that tied in more directly with his thinking on variation within large genera. He wanted Gray to mark "the '*close species*' in [his] Flora, so as to compare in *different* Floras whether the same genera have 'close species', & for other purposes too vague to enumerate. -- I have attempted by Hooker's help to ascertain in a similar way whether the different species of same genera in distant quarters of the Globe are variable or present varieties." By a "close species" Darwin meant those species that different botanists could, in good faith, identify as either a variety or a race. Darwin was not "vague" at all on his reason for requesting this information; it related directly to confirming his belief that botanists had a much greater appreciation for the variability of "species" than they either admitted or understood. His vagueness was only a strategic cover for the moment. Ever alert to incidentals, Darwin finally asked Gray to double-check the distance between the Green and White Mountains and the distance between the Green mountains and the mountains in New York; the distances given did not jibe with his rough calculations.[63]

[63]Darwin to Gray, 8 June 1855, *CCD* 5: 346-349. These distances figured prominently in Darwin's desire to show that disjunct alpine plants, inhabiting mountain tops widely separated from each other, could not have arrived at these locations by migration; they could only have been marooned

Gray promptly responded to Darwin's request for "close species," all the while lamenting his inability to contribute at any greater depth. "I rejoice in furnishing facts to others to work up in their bearing on *general questions*, and feel it the more my duty to do so in as much as, from preoccupation of mind & time & want of experience, I am unable to contribute direct original investigations of the sort to the advancement of science [.]" He sent Darwin a four-page listing of "close species," noting that European botanists would undoubtedly create two or more species where he and John Torrey created only one. Even though he and Torrey lumped more species into one than the Europeans did, Gray worried that Hooker's tendency "in boldly reducing nominal species . . . exposes him to temptations and dangers."

Already deep into correspondence with Hooker on this question, Gray outlined the error in logic Darwin and Hooker were making.

> [B]ecause you have shown that *a* and *b*. are so connected by intermediate forms that we cannot do otherwise than regard them as variations of one species, we may not conclude that *c & d*. differing much in the same way and to the same degree, are of one species before an equal amount of evidence is actually obtained. That is, when two sets of individuals exhibit any grave differences, the burden of proof of their common origin lies with the person who takes that view: and *each case must be decided on its own evidence, and not on analogy*, [italics mine] if our conclusions in this way are to be of real value. Of *course* we most often *jump* at conclusions from imperfect evidence.

But, of course, this was exactly the methodology that Darwin and Hooker were following. Hooker, who delivered Gray's letter to Darwin, objected to Gray precisely at this point: what good was Bacon's philosophy, Hooker asked, if we could not

in these distant mountain locations during the post-glacial warming period. cf. *Natural Selection*, 537.

proceed according to analogy when appropriate evidence was lacking?[64] Gray, as a taxonomist, may have been formally correct in arguing that analogy could not replace demonstration in determining specific differences, but to Darwin and Hooker, who were primarily botanical geographers, working with a mass of data, analogies and statistical inferences were equally feasible methods.

Gray mused that he "should like to write an essay on *species* some day; 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." He anticipated that Darwin would, in a few years, "settle the question as to whether Agassiz's -- or Hooker's views are correct: they are certainly widely different."[65]

Darwin thanked Gray profusely "for the *very* great trouble which the list of 'close species' must have caused you." He was sure that if he had done it himself it would have caused him "ten times the labour which it must have caused you." Both he and Hooker were "struck with [the list of close species] . . . as showing in a most striking manner the geographical affinity of species, & the difficulty of ascertaining what are species."[66] Gray's survey had also confirmed his hypothesis "that when *many* organic forms are allied, making what is called a genus, some of them are apt to be more closely allied than are the species in the smaller genera." This same result

[64]Hooker to Gray, 28 July 1855, GHA.

[65]Gray to Darwin, 30 June 1855, *CCD* 5: 362-363.

[66]Darwin to Gray, 21 July 1855, *CCD* 5: 384-85; Darwin to Gray, 24 August 1855, *CCD* 5: 418-20.

has been confirmed for the British flora. He had also discovered that in some animals, "when the species of a genus *differed* in some organ or part, which is usually constant in the species of the same genus, then that one or more of the species individually varied in some degree in this same organ or character." This would surely confirm his supposition that such species were lineal descendants.[67]

Darwin had an ulterior motive in requesting all of this information on geographical distribution from Gray. Perhaps, Darwin admonished Gray, he should use his keen philosophical mind to "write on the geographical distribution of the U.S. plants."[68] As he confided to Hooker, "I shall have done a good turn if I make him write a paper on Geograph. Distrib. of plants in the U. States."[69] Of course, such a study would strengthen his own cause. Darwin's requests had the desired effect.

As earlier promised, Gray immediately set to work on his "Statistics of the Flora of the Northern United States." It was published in three long installments in the *American Journal of Science.*[70] Gray's biographer, A. Hunter Dupree, argues that this work, ignored in Gray's collected works and infrequently cited by contemporary botanists, "is a landmark in the history of American botany and one of the foundation

[67]Darwin to Gray, 24 August 1855, *CCD* 5: 418-420.

[68]Darwin to Gray, 21 July 1855, *CCD* 5: 384-85.

[69]Darwin to Hooker, 10 June 1855, *CCD* 5: 349-351.

[70]"Statistics of the Flora of the Northern United States," *Amer. Jour. Sci.* 22 (September 1856): 204-232; 23 (January 1857): 62-84; 23 (May 1857): 367-403.

papers in the science of plant geography."[71] Both Hooker and Darwin would have agreed. Hooker believe it was "exceedingly articulate & one of the first fait accompli of this kind." "Good Heavens," Darwin exclaimed, "if I had written a paper half as good as yours, how conceited I should have been!" Too often "the very best workman sees blemishes" that the "poorer workman cannot even perceive, so that you cannot appreciate your own work in the generalising line."[72]

Throughout the spring and into the summer of 1856 Darwin worked steadily at completing his chapter on geographical distribution, all the while peppering Gray with more questions to answer and themes to pursue in his study. Could he distinguish between cultivated and naturally growing plants? While no doubt impossible, it would be nice to show "any especial affinity in genera . . . between America & *Western* Europe. America might be related to Eastern Asia (always excluding Arctic forms) by

[71]Dupree, *Gray*, 241. It was an immeasurable loss to botanical geography that Gray was unable to join his enormous intellectual gifts with those of Hooker and Darwin to "write his essay on species." He lacked both the financial means and the larger vision to heed the advice of Hooker to abandon his mind-numbing taxonomic work and use his acute understanding of the North American flora to explore the "big questions" that only he, Hooker, and Darwin were prepared to address. The scientific community would have benefited enormously if Gray would have been able to command even half of the patronage that flowed in to Agassiz. He was a man with all of the intellectual gifts necessary to make a major contribution to understanding the "species problem." He chose rather to commit himself to an earlier scientific ideal of making exquisite descriptions of botanical specimens. His North American flora, still uncompleted at his death, was a life-long chain around his neck, yet taxonomy, sorting out affinities, was his life-long passion. He yearned to spend two or three years side-by-side with Hooker doing taxonomic work. The sad consequence was that the work he spent the great bulk of his time doing is locked away in inaccessible railroad survey reports, dusty Smithsonian archives, numerous descriptions in society proceedings, and his name little known today, except as a "friend of Darwin." Even this essay would most likely not have been written without Darwin's prodding.

[72]Darwin to Gray, 24 November 1856, *CCD* 6: 284-285; Hooker to Gray, 15 October 1856, GHA; cf. Darwin to Gray, 1 January 1857, *CCD* 6: 314-316; Darwin to Gray, after 15 March 1857, *CCD* 6: 359-362.

a genus having the *same* species, the genus itself not found elsewhere. The relation of the genera (excluding identical species) seems to me a most important element in geographical distribution often ignored." Do the trees with a southern range living in the Cambridge area show signs of stunted growth? Do species seem to be more variable the further south one travels? He wondered "whether the Alleghenies are sufficiently continuous so that the plants could travel from the north in the course of the ages thus far south." Are agrarian plants more variable than naturalized plants? He was particularly curious about how many species ranged into the Arctic. And the questions kept coming.[73]

By September, the first installment of his essay already in press and on to Darwin, Gray's patience with Darwin's requests had worn thin. No, he could not give Darwin any precise information on the differences between the social plants of Europe and America. He still had lots of tabulation to do on disjoined species. Only then could he consider some of Darwin's "curious questions" on

> 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 So is it worth while to go on at this length of detail? -- There is no knowing how much space it may cover. Yet after all facts in all their fullness is what is wanted -- and those not gathered to support (or even to test) any foregone conclusions. It will be prosy; but it may be useful.

It seemed to Gray that he answered five of Darwin's questions only to receive ten more in return. When would he ever have time to answer all of them? How important were they anyway? Gray consoled himself that fulfilling the steady stream

[73]Darwin to Gray, 2 May, 1856, *CCD* 6: 92; Darwin to Gray, 14 July 1856, *CCD* 6: 182; Darwin to Gray, 28 August 1856, *CCD* 6: 208-209.

of Darwin's request could prove useful, in some vague way.[74]

Gray used Darwin's suggested topics as an outline for compiling his rather primitive, yet useful, statistical analysis of the North American flora east of the Mississippi and north of the Mason-Dixon line. His analysis showed a number of interesting features, most of which confirmed Darwin's suppositions. At its simplest, Darwin pictured a once continuous and ancient flora living in a warm climate throughout the circumpolar region of the North. The onset of the glacial period drove the flora south where it was divided between the two great land masses of East and West. During the subsequent warming period, plants either ascended mountain tops, went extinct, were modified, or slowly remigrated to the north. Thus, it was important for him to gain a broad understanding of how the present flora was distributed to confirm this reconstruction of geological and floral history. Gray seemed to have only the vaguest idea of why Darwin was asking the questions he was or how the statistical approach could be used to address more sophisticated questions concerning the "species problem."

Gray's analysis showed that there was "a curious analogy in the vegetation of the eastern sides of the two great continental land masses in the northern hemisphere, which is also borne out, though not so strikingly, in a comparison of the genera." Fully eighteen of the nineteen species not common with Europe were found in eastern Asia, while only three or four were found west of the Rockies. Darwin was

[74]Gray to Darwin, 23 September 1856, *CCD* 6: 228-230.

pleasantly surprised by this. "Nothing has surprised me more than the greater generic & specific affinity with E. Asia than with W. America. Can you tell me (& I will promise to inflict no other question) whether climate explains the affinity?" Is enough known about both western America and eastern Asia to allow for a fair comparison of their floras? "If we had the number of genera strictly or nearly strictly European, one could compare better with Asia & southern America &c. But I daresay this is a Utopian wish." Yes, the northern range of European species would be a "most important aspect" to work out.[75]

A second major theme that Darwin believed would sustain his hypothesis was the continuity of the alpine and Arctic flora between North America and Europe through the polar region. He believed there were few European species that did not extend into the Arctic.[76] In fulfilling Darwin's request for information on alpine plants, Gray sensed that this information bore directly on "the mooted question of the single or multiple origin of the species: -- upon whether they may have been diffused each from a common centre, or were originally given to two or more widely separated parts of the world." He noted that "Arctic regions form one botanical province," with the majority of its species found in both northern hemispheres in roughly the same ratio of proximity to one another. Since the European and American floras are connected through the Arctic region, Gray observed, an interesting problem was to

[75]Darwin to Gray, 12 October 1856, *CCD* 6: 244-46. Gray followed up this "curious analogy" in his essay on the Japanese flora.

[76]Darwin to Gray, 14 July 1856, *CCD* 6: 182; Darwin to Gray, 28 August 1856, *CCD* 6: 208-209.

determine the northern range of those species in America that were identical with species in Europe. The results showed what could be anticipated: 32 percent of the flowering plants common to Europe and America ranged into the Arctic.[77]

Gray, however, contended that "it seems almost certain 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."[78] This conclusion "riled" Darwin "dreadfully," since he believed in a strictly north-south pattern of migration over the pole. He wanted Gray's detailed reasons for this view.[79] Gray was surprised by Darwin's strong reaction to this conclusion. After all, Gray pointed out, he based this on the fact that geologists believed that the northern expanses had been much colder than earlier thought. In that case, "the northern limit of vegetation therefore [must have been] much lower than now, about the epoch when it would seem probable that the existing species of our plants were created." Of course, none of our present flowering plants could have grown anywhere near the Arctic during the glacial period. If Darwin insisted on the origin and diffusion of our present flora prior to the glacial period, then Gray had 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

[77]"Statistics," 63-72.

[78]"Statistics," 73.

[79]Darwin to Gray, 1 January 1857, *CCD* 6: 314-316.

single birth place of species." That is exactly what Darwin needed to assume.[80]

At the same time Darwin was delighted with Gray's conclusion that the largest genera had the largest range and an above-average number of species. "What a difference in regard to Europe your remarks in relation to the genera makes! I have been eminently glad to see your conclusion in regard to species of large genera widely ranging: it is in strict conformity with the results I have worked out in several ways. It is of great importance to my notions."[81]

Perhaps Darwin was less pleased with the fact that Gray still clung to the traditional terminology of "representative species" and "congeneric species" that made it difficult to recognize the existence of disjunct species and particularly disjunct genera. Gray set a high standard in determining what were "identical" species, as his earlier comments to Hooker and Darwin demonstrate. He observed that comparing two closely related and well-known floras, as the American and European, was "much more difficult, owing to the impossibility of estimating the degrees of resemblance among species, or at least of expressing them in any precise or definite way, or of bringing shades of difference to any common standard." Thus, whether a species could be classed as a "representative species" was crucial for establishing identity. "The degrees of affinity should be classified as strictly as the subject admits of, under several heads." In short, Gray was not about to take Darwin's, Hooker's, or anyone else's word that disjunct species were identical until they had passed his scrupulous

[80]Gray to Darwin, 16 February 1857, *CCD* 6: 338-342.

[81]Darwin to Gray, 1 January 1857, *CCD* 6: 314-316.

tests of "affinity." He left room, if only slight, for nearly identical species and, especially closely related genera, to have originated in separate locations.[82]

Gray confirmed this suspicion in correspondence with James Dwight Dana and George Bentham. He told Dana that while he believed in "a centre of radiation for each separate species," he did not "believe in centres of radiation for groups of species," though he sensed this was what Darwin believed.[83] Gray expanded on this distinction in correspondence with Bentham on the interchange of species between Europe and America through Asia. It would seem, Gray allowed, that "as respects identical species, interchange is the only thing that, on our views of what a species is, will explain the occurrence of the same species here and there." The same thing could not be maintained about identical genera. He failed to see why interchange must be assumed to explain the existence of identical genera or closely related species in widely separated locations.

> I see no reason why cognate species may not have been originally given to most widely separated stations; and, as to the facts of association, can we say more than this, that the species of a genus are apt to be confined to one part of the world? Are there not too many cases to the contrary to warrant our suspecting former continuity of two remote districts on account of common genera? Peculiar genera . . . divided between Japan and the United States of America, indicate some peculiar relation, and are most noteworthy, but I do not see why it points to connection.[84]

As these reservations clarify, Gray was still working within an idealistic understanding

[82]"Statistics," 78, 79.

[83]Gray to Dana, 13 December 1857, *LAG* 2: 424-426.

[84]Gray to Bentham, 4 May 1857, *LAG* 2: 427-428.

of the relationship between genera and species rather than the phyletic understanding that Darwin was constructing.

Darwin, as we have seen, believed that his hypothesis couid explain disjoined species better than either Forbes' land bridges or Agassiz's theory of multiple creations. It was an important question he asked Gray to explore. Gray was surprised to discover over 75 indigenous American species that were also found in the most curious and remote locations, e.g. North America north of 42 parallel and South America from Peru south; eastern Texas and eastern Himalayas; New Hampshire to Kentucky and New Zealand; Canada to Louisiana and Himalayas; colder parts of northern hemisphere and Tasmania.[85]

These data were interesting, but not quite precise enough for Darwin. He had a "VERY STRONG expectation that species having disjoined ranges would belong to *small* genera," on the grounds that small genera were the remnants of large genera that, over the course of the ages, had lost a significant number of species through extinction. "I look at Extinction as common cause of *small* genera & *disjoined* ranges & therefore they ought, *if they behaved properly* & as nature does not lie go together!"[86] Gray was surprised that Darwin "suspected *disjoined species* to belong to small genera & small orders, as a general thing." Unfortunately, Gray noted, "my 76 disjoined species belong to 34 families, -- and I cannot see that they incline to belong

[85]"Statistics," 381-386.

[86]Darwin to Gray, 9 May 1857, *CCD* 6: 391-92; 18 June 1857, *CCD* 6: 412-414.

to small families."[87]

Gray found Darwin's assumed relationship between extinction and small genera unpersuasive. Yes, he could understand how extinction could explain disjoined species and why "it *may* have reduced large genera to small, and so families." But perhaps Darwin had not thought through what other reasons there may be for small genera. How did Darwin know that

> there were as many small genera (nearly) at first as now, and as great a disproportion in the number of their species? . . . Is it philosophical, is it quite allowable, to assume (without evidence from fossil plants) that the family or any of the genera was once larger and wide spread? and occupied a continuous area? . . . But, granting you lots of species which have been greatly reduced by extinction, must you not admit that there are and were many more that never succeeded in getting a wide range? Would it not be pretty sure to be so of the late-comers? . . . What chance would a new species, of average powers, have had of getting a foot-hold, still less of ever getting a wide range, if introduced into the forest of N. America while yet untouched by man?" . . . Of any given local species, it seems to me *a priori* quite as likely . . . that it never had a wide dominion, as that it had, and is on the road to extinction.[88]

Of course, Gray admitted, Darwin had studied this problem far more than he had, but these were just the sorts of questions he would have to address with persuasive answers. Darwin subsequently toned down his emphasis on this relationship.[89]

Gray acknowledged, in closing his final installment, that disjoined species "are much relied upon by its advocates in proof of the doctrine of the double or multiple origin of species." Such a doctrine could be maintained, Gray allowed,

[87]Gray to Darwin, 1 June 1857, *CCD* 6: 401-403.

[88]Gray to Darwin, 7 July 1857, *CCD* 6: 422-24.

[89]Darwin to Gray, 20 July 1857, *CCD* 6:431. "What you say about extinction, in regard to *small* genera & local disjunction, being hypothetical seems very just."

> If the dispersion of other plants generally could be accounted for by existing agencies, acting under the present state of things, and if there were really any marked line of difference to be drawn between these and other widely dispersed but less isolated species, the supposition of a double birth-place for the exceptional species would be the most natural; although one would then be inclined to regard them as mostly cases of closely related species whose points of difference are still unascertained or undervalued. For we no more know how nearly alike two species may appear and yet be specifically distinct, than we know how widely they may differ and yet own a common origin.

Proponents of multiple creation of species, Gray summed, could make a credible case for their position on the grounds that "the botanist's best conclusions regarding the limitation of species are seldom more than judgments on imperfect data, constantly liable to be questioned and revised."[90]

It was, however, becoming apparent, Gray warned, that "these most striking cases . . . "are becoming too numerous to bear this exceptional mode of explanation. . . . We should therefore look in one and the same direction for the explanation of these extraordinary no less than of the more ordinary cases of distribution." The most reasonable explanation seemed to be that "plants must have been created at different epochs, and that the greater part of the existing species are older than the present configuration of our continents" and that we "should refer such anomalous distribution to very ancient dispersion; and all the more confidently as the known examples of the kind increase in number. "[91]

Joseph Hooker and Louis Agassiz represent the "two antagonistic positions" in resolving this issue. Hooker's position assumes that each species originated in a single

[90]"Statistics," 388.

[91]Ibid.

individual or pair and subsequently spread during a long period of time. Agassiz's position assumes virtually the opposite view, 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 is tested by its ability to "explain all the facts of distribution." Agassiz's theory faces no such test since it assumed that species were created where they are currently found; what are called disjoined species were merely nearly identical species.

> The first theory is based upon the natural idea 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. The second theory, being incompatible with this conception, leaves species no objective basis in nature, and seems to make even the ground of their limitation a matter of individual opinion.

These were the options. Without saying so, it was clear that Gray stood with Hooker and Darwin against Agassiz, although he had some equally stern tests for them, as his retorts to Darwin demonstrated.[92]

Having worked through the major outline of his theory for his "big book," Darwin saw that the time was right to share it with Gray. Fearing that Gray would "despise me & my crotchets," Darwin divulged that he had been considering "any sort of facts bearing on the question of the origin of species." There were only two possible choices: "either species have been independently created, or they have descended from other species, like varieties of one species."

To be brief I *assume* that species arise like our domestic varieties with *much*

[92]"Statistics," 389.

> extinction; & then test this hypothesis by comparison with as many general & pretty well established propositions as I can find made out, --in geograph. distribution, geological history--affinities &c. &c. &c. And it seems to me, that **supposing** that such hypothesis were to explain such general propositions, we ought, in accordance with common way of following all sciences, to admit it, till some better hypothesis be found out. For to my mind to say that species were created so & so is no scientific explanation, only a reverent way of saying it is so & so. But it is nonsensical trying to show how I proceed in compass of a note. But as an honest man I must tell you that have come to the heterodox conclusion that there are no such things as independently created species--that species are only strongly defined varieties. I know that this will make you despise me. I do not much underrate the many *huge* difficulties on this view, but yet it seems to me to explain too much, otherwise inexplicable, to be false. 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 the same grounds) all the individuals of the same species shd. have a continuous distribution. On this latter branch of the subject I have put a chapter together, & Hooker kindly read it over. I thought the exceptions & difficulties were so great that on the whole the balance weighed against my notions, but I was much pleased to find that it seemed to have considerable weight with Hooker, who said he had never been so much staggered about the permanence of species.

Just so that Gray would not think that he had come to these conclusions in haste, Darwin assured him that "all my notion about *how* species change are derived from long-continued study of the works of . . . agriculturalists; & I believe I see my way pretty clearly on the means used by nature to change her species & *adapt* them to the wondrous & exquisitely beautiful contingencies to which every living being is exposed."[93] Gray could have surmised everything that Darwin had revealed to him, except the means of adaptation. That would come in a few months.

Gray's initial response needs to be carefully interpreted. He was reading

[93]Darwin to Gray, 20 July 1857, *CCD* 6: 431-33.

Darwin's letter in the context of his long correspondence with Hooker on the elusive character and wide variability of species, as well as his own considerable experience as a taxonomist. From that perspective Gray agreed with Darwin's "misgivings about the definiteness of species." There was nothing controversial about this. Gray further supported what Darwin had said about varieties, noting that he had a strong belief that "every constitutional variety has a strong tendency to be perpetuated by seed, and the 2d. & 3d generations a stronger still tendency to transmit their inherited peculiarities." Gray strongly believed that when we witness the ease with which cultivated plants form varieties, "we cannot avoid suspecting they may . . . sport in some way in the wild state." It was most likely, Gray hypothesized, "that there is some law, some power inherent in plants generally prompting them to originate varieties. -- which is just what you want to come to, and I suppose is your starting point." He had "good, tangible facts," Gray nodded, "and I am greatly interested to see what is to be made out of them. First, can you get at the *law* of variation?" Here was the first sign of a parting of the ways. Darwin was not only unable to "get at the law of variation," he rather believed that variation occurred at random. As much as Gray agreed with Darwin to this point, he interpreted Darwin's statements within the context of his well-established traditional paradigm of the variability, not mutability, of species.[94] He did not sense its revolutionary impact.

Darwin was immensely relieved to receive Gray's strong endorsement for his

[94]Gray to Darwin, August 1857, *CCD* 6: 437.

views, especially since he "always expects my views to be received with contempt" by those closest to him in spite of the fact that he has been most conscientious in arriving at his views. Since Gray has shown an interest in his views, Darwin has sent along "the briefest abstract of my notions on the **means** by which nature makes her species. Why I think that species have really changed depends on general facts in the affinities, embryology, rudimentary organs, geological history & geographical distribution of organic beings." Gray would simply have to trust him for the supporting materials since each paragraph is fully discussed in one or two chapters. Whatever he thought of his ideas, Darwin asked Gray "not to mention my doctrine to anyone" for fear that "the Author of the Vestiges" may hear about and easily incorporate it into a new edition of his work. Since this work was "despised by naturalists," having to quote from it would seriously prejudice his views in the eyes of those he most wanted to persuade.[95]

Darwin continued to revise, with Hooker's help, his statistical comparison of the average number of variations in large genera and small genera. His earlier hypothesis that large genera had the largest number of variations has turned out to be false; the reverse, which a tally of Gray's list of "close" species, confirmed that. Would Gray please double-check his numbers on the grounds that he paid more attention to small than to large genera, but he did not want Gray to "'cook' the results for me." Perhaps, Darwin wondered, the "close species [were] *generally*

[95]Darwin to Gray, 5 September 1857, *CCD* 6: 445-450.

geographically representative species." If so, that "might make some difference."[96]

Darwin summarized his argument in six succinct paragraphs.

I. Man has produced "astonishing" results with the "principle of Selection. . . . I am convinced that intentional and occasional selection has been the main agent in making our domestic races. . . . Selection acts only by accumulating very slight or greater variations, caused by external conditions, or by the mere fact that in generation the child is not absolutely similar to its parents. Man by this power of accumulating variations adapts living beings to his wants -- he *may be said* to make the wool of one sheep good for carpets and another for cloth &c." Darwin stressed that "the accumulative power of natural selection" was "by far the most important element in the production of new forms."

II. "Now suppose there was a being, who did not judge by mere external appearance, but could study the whole internal organization -- who never was capricious, -- who should go on selecting for one end during millions of generations, who will say what he might not effect!" Since change in the conditions of existence is the main cause of the constantly occurring slight variations, and since we have had an "unlimited time" for those changes to have taken place, just imagine how many "millions on millions of generations" have been available for this being to exercise its power of selection.

III. "I think it can be shown that there is such an unerring power at work, or *Natural Selection* (the title of my Book), which selects exclusively for the good of each

[96]Browne has discussed the important role that Darwin's recalculation of larger and smaller genera played in arriving at his principle of divergence, "Darwin's Botanical Arithmetic, 1854-1858," 77-89, and *Secular Ark*, 210-220.

organic being." A. P. De Candolle, William Herbert, and Charles Lyell have each noted the struggle for life among individuals. Even the slow-breeding elephant, if all of its offspring survived, would soon over-run the entire globe in, at the most, a few thousand years. "I have found it hard constantly to bear in mind that the increase of every single species is checked during some part of its life, or during some shortly recurrent generation. Only a few of those annually born can live to propagate their kind. What a trifling difference must often determine which shall survive and which shall perish--"

IV. "Considering the infinitely various ways, beings have to obtain food by struggling with other beings, to escape danger at various times of life, to have their eggs or seeds disseminated &c. &c, I cannot doubt that during millions of generations individuals of a species will be born with some slight variation profitable to some part of its economy; such will have a better chance of surviving, propagating, this variation, which again will be slowly increased by the accumulative action of Natural selection; and the variety thus formed will either coexist with, or more commonly will exterminate its parent form."

V. Darwin acknowledged "multiform difficulties" with his theory, though believed that most of them could be answered. He cited the principle of "natura non facit saltum" [nature makes no leaps], slow change in only a few organisms over great lengths of time, and the "extreme imperfections" of the fossil record.

VI. Darwin believed that his newest addition to his theory, what he called "the principle of divergence,". . . "plays an important part in the origin of species. The

same spot will support more life if occupied by very diverse forms." The offspring of every species, when they have diverged into "true species," races, or varieties "will try (only a 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. 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."

Darwin apologized to Gray for his "*most* imperfect" sketch. "Your imagination must fill up many wide blanks. -- Without some reflexion it will appear all rubbish; perhaps it will appear so after reflexion." Darwin would soon discover that Gray lacked his own considerable imagination.[97]

Unfortunately, since Gray's response to Darwin's sketch has not been found, we can only surmise what his criticisms were from Darwin's answer. Darwin acknowledged that his work "was grievously hypothetical & large parts by no means worthy of being called inductive; my commonest error being probably induction from too few facts." He was quite surprised that Gray criticized him for "using the term 'Natural Selection' as an agent," explaining that he used it the same way that geologists used "the word 'Denudation,' for an agent, expressing the result of several

[97]Darwin to Gray, 5 September 1857, *CCD* 6: 445-450.

combined actions." While he would be careful to explain how he intended to use the term, he pleaded that there was no good substitute without a lengthy explanation of what he meant. Darwin never fully escaped from this central ambiguity in his theory.

Gray was apparently uneasy that, on Darwin's understanding, species lacked a "material base to rest on." Darwin countered that this was no "greater hardship than deciding what deserves to be called a variety. . . . When I was at systematic work [on his barnacles], I know I longed to have no other difficulty (great enough) than deciding whether the form was distinct enough to deserve a name; & not to be haunted with undefined & unanswerable question whether it was a true species. *What a jump it is from a well marked variety, produced by natural cause, to a species produced by the separate act of the Hand of God* [italics mine]."

The "jump" that Darwin seemed to have in mind was the difference between the two views on the origin of species shaping the contours of the "species problem" that he had spent nineteen years resolving. The fundamental question, as Darwin outlined in the opening of his chapter in *Natural Selection* on "geographical distribution," was whether species were the offspring of other species by the normal process of reproduction or were created multiple times in multiple locations. Gray had also accentuated the sharp contrast between the views of Hooker and Agassiz on the origin of species concluding his "Statistics" article. At the same time, Gray was hesitant to adopt fully Hooker's solution and reluctant to go all the way with Darwin's notion of species as incipient varieties. Darwin had abandoned the assumption shared by Gray and virtually all others that species had an "essence" that it was the task of

systematists to discover. The distinctions that Gray the taxonomist was so at pains to make between species, races, and varieties had become for Darwin a continuum in which varieties were but "incipient species."

In the interim since his last letter to Gray, Darwin had finally constructed his principle of divergence, which he had inferred from the fact that large genera produced the most varieties. "This rule, as I must consider it of the large genera varying most, I look at as most important for my work & I believe it to be the foundation of the manner in which all beings are grouped in classes &c, together with what I vaguely call my principle of divergence ie the tendency to the preservation from extinction of the most different members of each group."[98] Over the next seven months Darwin continued to seek confirmation of this principle from Hooker, Gray, and several other naturalists. Only then would his "big book" be complete.[99]

Apparently Gray asked Darwin about his views on post-glacial period patterns of migration to get a larger sense of how Darwin had used all of the information on geographical distribution he had provided. There was nothing on distribution in his 4 July sketch. Darwin outlined the views he had already written out for *Natural Selection* and would again summarize in the *Origin*.[100] Of particular importance to Gray's current work on the relationship between the Japanese flora and eastern North

[98]Darwin to Gray, 29 November 1857, *CCD* 6: 491-493.

[99]Darwin to Gray, 18 February 1858, *CCD* 7: 27-8; Darwin to Gray, 4 April 1858, CDD 7: 62-3.

[100]*Natural Selection*, 534-66; *Origin*, 365-82.

America, Darwin believed "Japan to have been joined to mainland China within no remote period: & then the migration n. & S. before, during & after glacial epoch would act on Japan, as on corresponding latitude of China & U. States. I shd. beyond anything like to know whether you have any Alpine collections from Japan & what is their character." Darwin would soon learn, to his immense satisfaction, the conclusions that Gray had drawn from his study of the Japanese flora.[101]

Darwin's insistence on chasing down every possible confirmation of his theory cost him dearly. He was hit with a thunder bolt on June 18, 1858. That day he received a packet from Alfred Russel Wallace, with whom he had corresponded on mutual interests, that was "most curiously coincident [with my theory] even in expression," even though "he could never have heard a word of my views." Hiding his devastation, he asked Gray to confirm when he had received his "little sketch of my notions of 'natural selection.'" He confessed that Lyell and Hooker, who had read an essay written in 1844, had "urged me with much kindness not to let myself to be quite forestalled & to allow them to publish with Wallace's paper an abstract of my letter to you, as the only very brief thing which I had written out was a copy of my letter to you." It had just been read before the Linnean Society.[102]

The Gray-Hooker Debate on the "Species Problem"

[101]Darwin to Gray, 11 August 1858, *CCD* 7: 149-51.

[102]Darwin to Gray, 4 July 1858, *CCD* 7: 125-26. Gray's reply, dated 27 July, has not been found. *CCD* 7: 151, n. 2.

The publication of the Darwin-Wallace papers in the fall of 1858 provided the occasion for Hooker and Gray to summarize their views on the "species problem" that they had been discussing for the past five years.[103] Hooker confessed that his

> faith is shaken to the foundation, & that the sum of all the evidence I have encountered since I studied the subject is in favor of the origin of species by variation ["transmutation" crossed out]. I feel (& I should like to know your feelings) that had I been originally taught the transmutation doctrine, as a dogma (like as I was taught the creation doctrine) -- I should have stuck to it to this hour & been convinced of it -- if only from evidence accumulated since my teaching. The worst of it is, that I cannot believe progression & so am no nearer the mystery of the origin of created things -- which is however quite a different question.[104]

It was hardly surprising that Hooker would consent to the origin of species by variation; all of his considerable thoughtful reflection on his far-ranging field experience had pushed him in that direction. What he could not accept, at this point, was the "progression" theory, no doubt thinking of Lamarck and Chambers. He carefully distinguished between the origin of species and the "mystery of the origin of created things," a distinction Darwin also made.

Hooker was especially thankful to be released from his knowledge of Darwin's views that he had held in strictest confidence the past fourteen years. Now that Darwin's theory of natural selection was out in the open, Hooker was free to use it to good effect in writing his introductory essay to the flora of Tasmania. It had already significantly modified his views on hybrids, varieties, and many other important

[103]Bert James Loewenberg, ed., *Darwin, Wallace and the Theory of Natural Selection* (Cambridge: Arlington Books, 1959).

[104]Hooker to Gray, 23 September 1858, GHA.

subjects. It was, he believed, "not only useful in itself in explaining many facts in variation, but as the most fatal argument against 'special creation' & for 'Derivation' being the rule for all species."[105]

Gray, busy with finishing his work on the Japanese flora, had not yet read the Darwin and Wallace papers when Hooker's initial letter arrived. He knew about their views "only in a general way" from Darwin's letters, so did not want to express any opinion on them until he had a chance to

> get the main facts & arguments before me. But I see a very strong case can be made. I see that I can go some way with D[arwin]. But whether the whole way is doubtful. For some time before I knew Darwin's views, I wondered that somebody did not revise our notions of species by beginning with those we are supposed to know most of, viz. -- domesticated species, and shown that the strong tendency to form races was by no means at all peculiar to them, but the exemplification of a common law.
>
> I have no prejudice against the coming view, I believe. -- I believe we have lots of derived species, but still suppose it most likely that there are plenty of aboriginal ones -- that nobody will be successful in the attempt to trace all the congeneric plants to one common specific type, still less all coordinate species to one origin. Nous verrons.[106]

By 1858, despite his intensive discussions with Hooker and Darwin during the past four years, Gray had not noticeably changed his views on the "species problem." He sympathized with their efforts to find a "natural" rather than "exceptional" solution; he believed that there was at least a *prima facia* scientific case to support the local and single origination of species throughout the earth's long history and to oppose Agassiz's appeal to the multiple origin of identical species in different

[105]Hooker to Gray, 21 October 1858, GHA.

[106]Gray to Hooker, 11 October 1858, Kew, APS.

locations. He agreed with Hooker and Darwin that species were far more variable and widely distributed than had traditionally been thought. Even as early as his review of Hooker's Indian flora, Gray had suggested the strong possibility of a law for forming races, which was a variety that was capable of perpetuating itself. Yet he was still cautious in adopting a fully natural explanation of the origin of species by transmutation. It was a long step, for Gray, from species being variable to species being mutable; he needed demonstrative proof to accept that.

Gray's remarks to Hooker are especially surprising in light of his own considerable discussion with Darwin on patterns of geographical distribution that culminated in his "Statistics" essay and receipt of Darwin's "sketch" of his theory. Gray's vocabulary in this letter suggests that he had not yet accepted the "spirit" and thrust of Darwin's views, despite his own belief that he was close to them. Gray had been careful throughout his discussion with Hooker to maintain a distinction between "derived" species and "aboriginal" species, presuming that some unknown(able) number of existing species were the same identical species as were originally created. In this way he maintained some contact with the Agassizian view. Gray's skepticism about tracing "congeneric" species to a common "type," as Hooker and Darwin now believed possible, indicated that he still retained the traditional idealistic understanding that species were related by exemplifying a common "type" or plan rather than the Darwinian view that species were related genetically by descent. Gray's difficulty in translating these traditional classifying terms into Darwin's idiom of descent through modification was a major hurdle for most naturalists who read Darwin. As he worked

furiously on the Charles Wright collection of Japanese flora during the summer and into the fall of 1858, Gray read the Darwin-Wallace papers in a new light.