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Joseph Hooker Takes a "Fixed Post": Transmutation and the "Present Unsatisfactory State of Systematic Botany", 1844–1860

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Abstract. Joseph Hooker first learned that Charles Darwin believed in the transmutation of species in 1844. For the next 14 years, Hooker remained a "nonconsenter" to Darwin's views, resolving to keep the question of species origin "subservient to Botany instead of Botany to it, as must be the true relation." Hooker placed particular emphasis on the need for any theory of species origin to support the broad taxonomic delimitation of species, a highly contentious issue. His always provisional support for special creation waned during the 1850s as he lost faith in its expediency for coordinating the study of plant geography, systematics and physiology. In 1858, Hooker embraced Darwin's "considerable revolution in natural history," but only after Darwin had carefully molded his transmutationism to meet Hooker's exacting specifications.

Keywords: biogeography, Charles Darwin, Joseph Hooker, principle of divergence, special creation, species delimitation, systematic botany, transmutation (evolution)

> *"And now for species. To begin, I do think it a most fair & most profitable subject for discussion, I have no formed opinion of my own on the subject, I argue for immutability, till I see cause to take a fixed post. A knowledge of Botany alone will never clear up the question & alas I can bring nothing else to bear upon it, my Geology is nil: & thus you see I am ever ready to make it subservient to Botany instead of Botany to it, as must be the true relation."*
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> *Joseph Hooker to Charles Darwin, 14 September 1845*[1]

When awarding the botanist Joseph Hooker the Royal Medal in 1854, the Earl of Rosse, the President of the Royal Society, praised him for "investigat[ing] critically one of the most difficult questions of natural science, which is now acquiring the prominence to which it is so well

[1] (*The Correspondence of Charles Darwin* is abbreviated *CCD* throughout.) *CCD* 3: 254.

entitled, – I mean the question of the origin and distribution of species." Unlike by implication most others who had addressed these questions, Hooker reassuringly possessed "moderation," "a cautious spirit of induction" and "a sagacious perception of the requirements of natural science."[2] No one at the time realized that Hooker also had been long engaged privately in a friendly but intense discussion with Charles Darwin on these charged questions.

The two men became correspondents in 1843 when John Stevens Henslow arranged to have the bulk of Darwin's HMS *Beagle* plants transferred to Hooker for description. Darwin was so impressed with Hooker that he revealed his belief in transmutation to his younger colleague in early 1844, famously admitting that "it is like confessing a murder."[3] To Darwin's delight and relief, Hooker responded serenely that he would be delighted to hear Darwin's ideas, "as no presently conceived opinions satisfy me on the subject."[4] This response sealed a relationship which, as Janet Browne notes, would draw the two men as close as brothers.[5] While debating species placed strain on many of Darwin's other friendships, his sparring with Hooker served to deepen mutual affection.

Hooker's two major methodological works from the 1850s – the Introductory Essays to *Flora Novæ-Zelandiæ* (1853) and *Flora Indica* (1855, written with Thomas Thomson) – argued firmly in favor of special creation. It would a mistake to interpret Hooker and Darwin's disagreement based on two competing theories of species origins, however. Hooker self-consciously charted a course between dogmatism and vagueness: endorsing special creation because he considered it best suited to the practical and philosophical requirements of botany, but refusing to accept it as settled doctrine.[6] As he put it to Darwin in 1858.

> My great desire was to put every possible objection [to your theory] as strongly as I could. I did not feel myself a dissenter from or opponent to your views, so much as a non consenter to them in the present state of my knowledge, nor till you had weighed my objections.[7]

[2] Parsons, 1854, pp. 261–262.

[3] CD to JDH, [11 January 1844], *CCD* 3: 2. For accounts of Darwin "confessing a murder," see Browne, 1995, pp. 451–453; Colp, 1986; Desmond and Moore, 1991, pp. 313–336; Porter, 1993.

[4] JDH to CD, 29 January 1844 and CD to JDH, 23 February [1844], *CCD*, 3: 7, 11.

[5] Browne, 2002, pp. 242–243.

[6] Hooker, 1853, p. ii.

[7] JDH to CD, 13–15 July 1858, *CCD* 7: 132.

For most of the 1850s, it was far from clear that transmutation could be made compatible with Hooker's "sagacious perception of the requirements of natural science." Darwin openly confessed that Hooker's "cautions on the species-question ought to overwhelm me in confusion & shame; it does make me feel deuced uncomfortable ... How awfully flat I shall feel, if when I get my notes together on species &c. &c, the whole thing explodes like an empty puff-ball."[8]

Quite the opposite happened, as we know. By the end of the 1850s, Hooker concluded that special creation had lost all utility as a tool of descriptive science. He took his "fixed post" in favor of transmutation and emerged as one of Darwinism's most effective champions. Darwin's triumph over special creation did not represent Hooker's defeat, however. On the contrary, Darwin aligned his species theory exactly with Hooker's conceptions of proper natural history practice. Both naturalists got from the other what they had wanted. They resolved their disagreement with a joint victory.

Orthodoxy and Heterodoxy

"A cautious spirit of induction" and "a sagacious perception of the requirements of natural science" were Hooker's birthrights.[9] He grew up in a household organized around science. His father, Sir William Hooker, held the botany chair at the University of Glasgow from 1820 to 1841, when he left to assume the directorship of the Royal Botanic Gardens, Kew. Sir William possessed an extensive botanical library and the largest and most valuable private herbarium in the world. He was also one of best connected botanists in Europe. Joseph spent his childhood around illustrious naturalists like George Bentham, Robert Brown, Robert Graham, John Stevens Henslow, John Lindley, Charles Lyell (senior) and Charles Lyell (junior).

Hooker worked hard to live up to the social, intellectual and material opportunities of this privileged upbringing.[10] He served four years as an assistant surgeon and botanist during Captain James Ross's Antarctic expedition (1839–1843), which provided an unprecedented chance for investigating the vegetation of the southern hemisphere.[11] After failure to win the botany chair at the University of Edinburgh in 1845, he

[8] CD to JDH, 26 March [1854], *CCD* 5: 186–187.

[9] For biographical details on Joseph Hooker, see Allan, 1967; Bellon, 2000, 2001 and 2005; Desmond, 1999; Endersby, 2002 and 2004; Huxley, 1918; Turrill, 1963.

[10] Bellon, 2001, pp. 68–73.

[11] Desmond, 1999, pp. 19–85; Huxley, 1918, vol. 1, pp. 37–167; Hooker, 1844–1860.

decided on further foreign travel.[12] From 1847 to 1851 he explored and collected in India, particularly in the Himalayas.[13] By his return, few botanists could claim comparable experience both in the field and herbarium. The Royal Medal in 1854 confirmed his place in the scientific elite. By 1856, not quite 40 years old, he considered himself one of botany's "old experienced fogies."[14]

Hooker instinctively accepted the consensus forged by Britain's elite scientific establishment. Unlike naturalists such as H.C. Watson and Alfred Russel Wallace, who were temperamentally or socially attracted to radical notions (including transmutation), Hooker invariably gave scientific orthodoxy the benefit of the doubt. Hooker's faith in the scientific establishment did not make him a reactionary, however. He accepted that sometimes theories and practices had to be turned upside down. Role models like Lyell and Brown highlighted the occasional need for rebellion – or, rather, for palace coups.

Hooker refused to abandon ideas which possessed scientific authority and utility when "no unassailable grounds" existed for doing so.[15]. He tartly informed Darwin in 1845 that "those who have had most species pass under their hands as Bentham, Brown, Linnæus, Decaisne & Miquel, all I believe argue for the validity of *species* in nature."[16] Hooker's faith in such authorities was not wholly matched by his personal experience, however. He admitted to the American botanist Asa Gray, a close confidant, that he accepted special creation "more upon principle than upon facts."

> Oh dear, oh dear, my mind is not fully, faithfully, implicitly given to species as created entities ab origine, but it is to the imperative necessity of sticking to one side or the other, and, without being bound by it, referring, arranging, and reasoning by it. I take that side which, though apparently the most narrow and prejudiced, is the only one which really keeps the mind open to investigate, which coordinates all the elements of geography, system and physiology, and which keeps the observer's attention alive to the importance of studying collateral phenomena.[17]

But whatever his misgivings, and however unwilling to assert any "fixed or unalterable opinion," he remained committed to the orthodox view

[12] Bellon, forthcoming.
[13] Desmond, 1999, pp. 96–199; Hooker, 1854; Huxley, 1918, vol. 1, pp. 223–342.
[14] JDH to AG, 15 October 1856, *JDH-AG*.
[15] Hooker, 1853, p. xxvi.
[16] JDH to CD, [4–9 September 1845], *CCD* 3: 250.
[17] JDH to AG, 26 January 1854 (copy), *From-JDH* 5: 10.

of species "for all practical purposes of progress in botanical science."[18]

But what constituted progress?

Descriptive Botany and Philosophical Botany

Even though Hooker had not, at first, considered himself "a good arranger of xtended [sic] views,"[19] he was no less influenced than Darwin by the unifying vision of naturalists like Charles Lyell and Alexander von Humboldt. Far from being a naïve empiricist, Hooker understood that all working naturalists inevitably made assumptions about fundamental questions, such as the origin of species. These assumptions influenced descriptive work, for better or worse, "however much [a naturalist] may try to avoid it."[20] Hooker recognized that it would not be desirable, even if it were possible, to banish theoretical considerations from botany. The problem, as he saw it, was botany's inability to break a vicious circle: sloppy and unreflective taxonomic practices resulted in a conceptual muddle which reinforced the poor practices.

In the 1850s Hooker lamented that botany was mired in a "unsatisfactory" and "confused state."[21] The inexcusable situation inspired him to elaborate publicly his progressive vision for botany in the Introductory Essays to *Flora Novæ-Zelandiæ* and *Flora Indica.* He insisted that his object was not to engage in "fruitless controversy" but rather "to draw attention to a few leading questions of great practical importance" and place the determination of species and genera "on a sound and philosophical basis."[22] The key word is "philosophical." The label was one of the highest accolades a Victorian scientist could bestow upon a colleague, his accomplishments or his method. The "philosophical" naturalist did not restrict himself to observing, collecting and describing but sought general causal theories to explain organic and geological patterns and processes.[23] Hooker insisted that his good friend, the Irish botanist William Harvey, had no claim to being philosophical, despite the excellence of his taxonomic work, because "he took up Botany as another takes up coins, or seals."[24] Philosophical botany, in particular, included establishing the principles

[18] Hooker, 1853, pp. viii, xxvi.
[19] JDH to CD, [12 December 1843–11 January 1844], *CCD* 2: 422.
[20] Hooker, 1853, pp. i–ii.
[21] Hooker and Thomson, 1855, p. 36.
[22] Hooker, 1853, pp. vii, xxvi; Hooker and Thomson, 1855, p. v.
[23] Hodge and Radick, 2003, pp. 10–11;Rehbock, 1983, pp. 3–12; Sloan, 2003.
[24] JDH to CD, 8 June 1860, *CCD* 8: 249.

of classification, developing biogeographical laws, investigating plant physiology, and pursuing economic botany. Further, it strove to integrate these various studies together and with the results of physics, chemistry and other scientific disciplines.

Botany's intellectual significance, social gentility and general usefulness depended upon its philosophical legitimacy.[25] Yet, if the abstract reasoning of philosophical botany transcended observation and description, it nonetheless could never abandon them. In an 1844 eulogium to the French naturalist Georges Cuvier, one of several he wrote, the Scottish natural philosopher David Brewster insisted that:

> Cuvier ... was familiarized from his youth with the drudgery of observation. He wrought with the microscope and the scalpel: he collected, he labelled, he delineated, and he arranged; but he was thus a minute and hard-working naturalist, because he had early seen that he could not otherwise become a great philosopher.[26]

The challenge for the would-be philosophical naturalist was to combine the patient toil, minute accuracy, and methodical habits of direct and particular observation with the vision to comprehend patterns, and ultimately causal laws, of universal validity – or, as Brewster put it, "to give unity and system to the fragments of desultory knowledge."[27]

For this reason, comprehensive collections of material specimens became the philosopher's stone of natural history. As James Edward Smith observed in his lecture inaugurating the Linnean Society in 1788, natural objects could not always be studied in their native soil so "botanic gardens and cabinets of natural history have been invented, in which the productions of the most distant climes are brought at once before us."[28] Cuvier built his exemplary career as a comparative anatomist on the unrivaled zoological collections of the Muséum d'Histoire Naturelle in Paris.[29] His stationary and scrupulously organized indoor work with preserved specimens allowed him, he believed, to acquire a deeper and truer understanding of nature than could ever be achieved on the hoof in the disorderly outdoors. "The [scientific] traveller can only travel one road," necessarily leaving his observations "broken and

[25] Jim Endersby (2002) provides a comprehensive and sophisticated study of philosophical botany, particularly as Hooker conceived, promoted and practiced it.
[26] [Brewster], 1844, p. 35.
[27] [Brewster], 1836, pp. 265, 277.
[28] Smith, 1791, pp. 51–52.
[29] Outram, 1996.

fleeting," Cuvier asserted; "it is only really in one's study [*cabinet*] that one can roam freely throughout the universe."[30]

Decades later, William Whewell, in his philosophical analysis of the Great Exhibition of 1851, expressed the identical sentiment. The philosophical observer could not hope to perceive the world in his mind's eye until he brought it under his bodily eye. "The accidental peculiarities of the traveller's personal narrative" were ill-suited for providing a "general view of the earth." Only by creating a material facsimile of the world through a representative collection of its material objects could one obtain "a *simultaneous* view of the condition of the whole globe" – a view which, by literally annihilating time and space, allowed one to reach universally valid conclusions.[31]

Hooker accepted unreservedly that truly philosophical conclusions in botany emerged only from the panoramic global study of vegetation, study which required a comprehensive herbarium like his father's. He centered his life and his aspirations on it.[32] He did place a high intellectual premium on fieldwork, but largely from his conviction that reliable study of dried plant specimens required experience collecting and observing living plants *in situ*.[33] As much as he valued his own fieldwork on the Indian subcontinent, he believed that his efforts would be "pretty well thrown into the sea" if he did not have the resources to arrange, name and label the thousands of resulting specimens at home in Kew Gardens, with his father's research library and herbarium at hand.[34] No one, he later concluded, could hope to undertake a broad and philosophical study of the India flora without basing themselves long-term at Kew Gardens.[35]

Hooker believed that the half century development of his father's herbarium was a microcosm of the progress of 19th century systematic botany generally.[36] When Hooker resolved to Darwin to keep the theoretical question of species "subservient to Botany instead of Botany to it," he meant that no species theory would be allowed to disrupt the practice established around this vital research tool. Otherwise, the theory would subvert the very global approach and research methods on

[30] Quoted from 1807 in Outram, 1996, pp. 260–261.

[31] Whewell, 1852–1853, pp. 10–15. Emphasis in the original.

[32] Bellon, 2001, pp. 69–70. Endersby, 2002, pp. 16, 58–59, 62, 161–166.

[33] JDH to Dawson Turner, 7 July 1847, *From-JDH* 13: 317–318. Hooker and Thomson, 1855, p. 20.

[34] JDH to W. Hooker, 8 August 1850, *IL* 296. His father agreed: see W. Hooker to Lord Seymour, *circa* March 1851 (copy), *IL* 323.

[35] JDH to AG, 15 October 1856, *JDH-AG* .

[36] Hooker and Thomson, 1855, p. 68.

which it must rest. "What I meant I still maintain," he told Darwin, "that to be able to handle the subject at all, one must have handled hundreds of species with a view to distinguishing them & that over a great part, – or brought from a great many parts, – of the globe."[37] While Hooker granted that Darwin's circumnavigation on the *Beagle* did equip him to treat species philosophically, Darwin acknowledged that this polite admission "does not alter one iota my long self-acknowledged presumption in accumulating facts & speculating on the subject of variation, without having worked out my due share of species."[38] From both practical and theoretical perspectives, reliable generalizations on species could only arise from, and conform to, painstaking work with comprehensive natural-history collections. Darwin soon began his exhaustive, eight-year taxonomic study of barnacles based on specimens drawn "from all quarters of the globe."[39]

The entire enterprise of philosophical natural history necessarily rested on the size, scope and quality of its collections. Bentham, a close friend and trusted advisor to both Hookers, noted to William that "a number of little herbaria spread all over the country are of little value – but the more large collections are established where there are Professors of Botany and Botanical libraries the better as it will tend to the encouragement of real botanists and the extension of their views."[40] As Jim Endersby emphasizes, the use of such "large collections" cannot be separated from the social relationships which made their creation, maintenance and expansion possible. At Kew Gardens, he notes, "Hooker was isolated, cut off from the plants he wanted to transform into a classification system and a career." For this reason, Hooker's budding career depended not only on physical objects – dried plant specimens and books – but, as importantly, on his father's extensive network of colleagues and collectors.[41]

Hooker could roam freely through the universe only if he could convince far-flung and diverse plant collectors to supply usable herbarium specimens. Specimens had little value unless they were standardized to fit his purposes – but his purposes did not always coincide with the purposes of his collectors, who had their own ideas, craft skills, local expertise and personal motivations. Coaxing compliance with his instructions required delicate negotiation. A specimen could not proxy

[37] JDH to CD, 14 September 1845, *CCD* 3: 254.
[38] CD to JDH, (18 September 1845), *CCD* 3: 256.
[39] Darwin, 1851–1854, vol. 2, p. 242. See Love, 2002; Southward, 1983; and Stott, 2003.
[40] GB to W. Hooker, 9 March 1856, *DC* 36: 70.
[41] Endersby, 2002, pp. 16, 45, 62.

for an extended natural population unless it was stripped of the individual plant's and the collector's idiosyncrasies. Hooker's collectors frequently disagreed with him on where "idiosyncrasy" ended and vital scientific data began.[42]

Remedying botany's "unsatisfactory" and "confused state" required Hooker to attain *oversight* of his discipline, both in the conceptual sense of acquiring a panoramic view of global plant distribution and affinity, and in the social sense of subordinating the collectors whose cooperation made such a view possible.[43] This oversight, in each sense, was imperiled by persistent controversies about the taxonomic demarcation between species and varieties.

Lumpers and Splitters

From outside natural history, describing and naming species might have appeared mundane and uncontentious. It was not. Hooker believed that systematic botany's "present unsatisfactory state" was rooted particularly in persistent controversies over the standards for the taxonomic delimitation of species. The long-standard definition of a species was deceptively straightforward in theory: a species encompassed all individuals descended from a common parent. In practice, botanists disagreed, rancorously, over how similar two forms had to be in order to assume common descent. Typical were the incessant and heated debates over the specific identity of primroses and cowslips.[44] No generally accepted standards existed for resolving whether to rank these plants as varieties of the same species or species of the same genus. In the case of an exceptionally variable plant like water crowfoot, some botanists would lump different forms under the same species name (*Ranunculus aquatilis*). Others would split them into three species, or six, or even more than a dozen.[45] Similar disagreements raged in hundreds of other cases.

Hooker considered it scandalous that confusion reigned in this most fundamental task of descriptive science. For systematic botanists, the delimitation of species was far from trivial. James Edward Smith, a close friend of Hooker's grandfather, Dawson Turner, argued that "species are perhaps the only distinctions which are indubitably natural,

[42] On the crafting and standardization of herbarium species, Endersby, 2002, pp. 79, 97, 102, 226–228, 255; on Hooker's collectors, Endersby, 2001 and 2002, chapters 2 and 3.

[43] Endersby, 2002, p. 256.

[44] Darwin, 1859, pp. 49–50; Darwin, 1975, pp. 128–133; Henslow, 1830.

[45] Watson, 1847–1859, vol. 4, p. 48.

and to stamp them clearly, as well as concisely, is the most important, perhaps the most difficult, office of the philosophical botanist." He further noted that the great Linnaeus himself insisted that distinguishing species represented "the strength or sinews" of botany.[46]

While a continuum of opinion on species limits existed, naturalists roughly divided into two warring camps, the "lumpers" and the "splitters." Lumpers – including both Hookers, Darwin, Henslow, Lyell, Gray, Watson and Bentham – took a broad view of species limits and reduced all doubtful cases to the rank of variety. Splitters had a much narrower view of species limits and more readily created species names. In a letter to Darwin, Watson used Hooker as a representative lumper, noting that he would recognize perhaps 60,000 species of vascular plants, while an extreme splitter (like the French botanist Alexis Jordan) would put the number nearer 300,000![47] As Bentham insisted, such differences did not arise from disagreements about the amount of biological diversity, "but is occasioned by a different appreciation of the value of species themselves."[48]

The startling differences between lumpers and splitters revealed the necessity of distinguishing between a *natural species* and *book species*. Most naturalists believed that natural species existed as distinct and indivisible assemblages of individuals. That belief did not resolve the problem of defining the limits of these natural units in thousands of particular cases as a technical matter of descriptive science. Theoretically, book species should correspond exactly to natural species; practically, this obviously was not happening. Given this problem, it is not surprising that, as Gordon McOuat has shown, establishment Victorian naturalists, including Darwin, were more concerned to discipline the practice of naming species than to define the species category.[49] A naturalist's theoretical concept of a species influenced his practical descriptive work, but the link was not straightforward. Watson noted that Charles Cardale Babington and William Arnold Bromfield differed widely in the number of book species they recognized in the well-studied British flora, "and yet it may be, that those two observant botanists would have closely concurred in their abstract ideas and definitions of the term *species*."[50]

[46] Smith, 1821, p. 53.

[47] H.C. Watson to CD, 13 August 1855, *CCD* 5: 405–406. He would repeat this comparison in Watson, 1847–1859, vol. 4, pp. 33–34. Disputes over delimitation existed not just at the level of the species, but at all levels of the taxonomic hierarchy.

[48] Bentham, 1858, p. xiii.

[49] McOuat, 2001. See also Stevens, 1997, pp. 357–363; and Winsor, 2003.

[50] Watson, 1847–1859, vol. 4, p. 34.

Generally, what separated lumpers and splitters was not so much abstract conceptions but the geographical scopes of their investigations. "It will generally be found that botanists who confine their attention to the vegetation of a circumscribed area, take a much more contracted view of the limits of species, than those who extend their investigation over the whole surface of the globe," Hooker and Thomson asserted in *Flora Indica*.[51] A global perspective required access to a large general herbarium; both Hookers insisted unequivocally that systematic botany could not be done properly without regular access to one.[52] By the 1850s, such herbaria had become too expensive for all but the wealthiest private individuals to maintain. Lumpers were invariably associated with large metropolitan collections, such as those housed at Kew Gardens, Harvard University, Geneva's Jardin Botanique and Paris's Muséum d'Histoire Naturelle.

The most vocal lumpers did not characterize their debate with the splitters as legitimate disagreement, but a fundamental contest between truth and error, expertise and incompetence. Hooker followed suit, with zeal. "A wider range of knowledge and a greater depth of study are required, to prove those dissimilar forms to be [specifically] identical, which any superficial observer can separate by words and a name," he asserted in *Flora Novæ-Zelandiæ*. (This rhetoric was put to practice in his work on New Zealand species. He "perseveres in the destruction of so-called species of name-makers, and with constant success," the *Gardeners' Chronicle* cheered).[53] In *Flora Indica*, he and Thomson charged that the unjustified multiplication of book species was "clearly traceable to the want of early training in accurate observation, and of proper instruction in the objects and the aims of natural science."[54] Privately, he raged even more intemperately against "species mongers," as lumpers contemptuously called their opponents.

Both David Allen and Jim Endersby show that the persistent assertions of superior competence by Hooker and his fellow lumpers has much more to do with establishing and protecting intellectual and social authority than with any straightforward issue of knowledge and skill.[55] As nakedly self-serving and often unfair as the denigration of splitters

[51] Hooker, 1853, p. xiii. Hooker and Thomson, 1855, p. 13. See also JDH to CD, 30 December 1844, *CCD* 3: 101; Watson, 1847–1859, vol. 4, p. 34; Allen, 2003, pp. 225–226.
[52] W. Hooker, 1846–1864, vol. 1, pp. viii–ix. Hooker and Thomson, 1855, pp. 11–12. Hooker and Thomson, 1858, pp. 2–3.
[53] [Anonymous], 1854, p. 287.
[54] Hooker, 1853, p. xiv. Hooker and Thomson, 1855, p. 12.
[55] Allen, 2003. Endersby, 2002.

might have been, it was not disingenuous: Hooker and his allies could not have pursued their vision of philosophical natural history if they could not dictate the terms of descriptive natural history. Hooker stressed that the need to avoid multiplying names had a solid practical rationale. If the botanist misjudged a true species as a variety, this error could be rectified. But the same was not true for a variety mistakenly elevated to the rank of species. Once a bad species name insinuated itself into the botanical literature, it became extremely difficult to expunge. He estimated gloomily that somewhere between a third and a half of all book species were spurious, and the problem worsened daily.[56] This profusion of names was not only a severe inconvenience, but, the lumpers insisted, a grave impediment to the global study of plant classification and distribution.[57]

Hooker and Thomson abandoned their ambitious goal of a twelve-volume *Flora Indica* after the first volume. Even with their extremely broad conception of species limits, the task of describing every Indian species proved too gruelingly time-consuming (especially since Thomson's ill-health prevented him from carrying his share of the burden).[58] A botanist who focused on the flora of a small geographical area – say, an English county or a province in the colonies – could afford to concentrate on variant local plant forms. Botanists like Hooker and Thomson had to ignore such variants if they hoped to pursue a panoramic understanding of the vegetation of vast and ecologically diverse territories like the Indian subcontinent. The philosophical botanist could not recognize universal patterns if his time and attention were overwhelmed by "accidental forms [separated] by trifling characters."[59] Not coincidentally, Hooker rejected field characters – characteristics visible in living plants, but not in dried specimens – as "trifling"; a character was sufficiently clear and strong for a diagnosis of a species only if he could see it the herbarium.[60]

Hooker never believed that the practice of delimiting book species was separate from abstract definitions of natural species. But he did

[56] Hooker and Thomson, 1855, p. 38. [Hooker], 1856, p. 255. Hooker, 1853, pp. xvii–xix .

[57] Hooker, 1853, p. xiv; Watson, 1847–1859, pp. 44–52; Allen, 2003, p. 225.

[58] For the original conception of the *Flora Indica*, see JDH to WJH, 30 January 1850 and 25 August 1850, *IL* 255, 300. For the rationale behind the decision to abandon the project, see JDH to AG, 17 March 1855 and 15 October 1856, *JDH-AG*; [Anonymous], 1855; and Hooker and Thomson, 1858.

[59] Hooker and Thomson, 1855, p. 12. See also Stevens, 1997, p. 354.

[60] Stevens, 1994, pp. 475–476. Stevens, 1997, pp. 349–352. Endersby, 2002, pp. 19–20, 154–159, 163–165.

subordinate theory to the needs of technical descriptive natural history. "As a Botanist I must be content to take species as they *appear to be* not as *they are*, & **still less** as **they were** or ought to be," he informed Darwin tartly in 1845.[61] Darwin agreed unreservedly with Hooker's adherence to the broad species concept. He declared himself "foolish & rabid against species mongers" and cheered on the "slaps" Hooker dealt to them. [62] Both naturalists were convinced that philosophical practice demanded a broad conception of species limits. They differed on whether these standards were compatible with a belief in transmutation.

Mutability and Variability

If there was nothing permanent about a species, on what theoretical basis did the botanist draw the stark line between the variety and the "good and true" species? Charles Lyell expressed this precise fear to Hooker:

> The species mongers will be delighted with a theory which sanctions to a great extent the conclusion that the boundaries of species are in the nature of things artificial, or mere human inventions, and therefore gives them a kind of right to affix their own arbitrary bounds. So long as they feared that a species might turn out to be a separate and independent creation, they might feel checked; but once abandon this article of faith, and every man becomes his own infallible Pope.[63]

Hooker agreed. In *Flora Indica* he and Thomson charged that the "superficial naturalists" who created so many spurious book species were the same ones who gravitated towards the theory of transmutation: a botanist who regarded all species as arbitrary creations delimited by arbitrary characters would consider it "of no moment how many or how few [species] he makes." Botanists who hold such an (unfortunate) opinion should avow it openly, they charged. Those who believe otherwise then will not "waste time in seeking for nature's laws in the works of naturalists who seek to bind nature by arbitrary laws."[64] Privately, Hooker admitted to Gray that the need to discourage "species mongers" kept him from accepting Darwin's hypothesis: "You certainly

[61] JDH to CD, [mid-July 1845], *CCD* 3: 223.
[62] CD to Hugh Strickland, [4 January 1849] and CD to JDH, [9 October 1853], *CCD* 4: 208, 5: 159.
[63] CL to JDH, 25 July 1856, Lyell, 1881, vol. 2, pp. 214–215.
[64] Hooker and Thomson, 1855, pp. 19–21. See also Hooker, 1853, pp. i–ii.

are more cautious in species reducing than I am, & had best be: not that I see any reason to recant: I have no choice but to go further or believe in universal variation as the origin of species – that's a fact."[65]

In order to place the determination of species on a firmer foundation, Hooker defended four axioms in *Flora Novæ-Zelandiæ* – all having "the advantage of being simple, intelligible, and as little exposed to the charge of being speculative, as any of that nature can be":

§1. That all the individuals of a species (as I attempt to confine the term) have proceeded from one parent (or pair), and that they retain their distinctive (specific) characters.
§2. That species vary more than is generally admitted to be the case.
§3. That they are also much more widely distributed than is usually supposed.
§4. That their distribution has been effected by natural causes; but that these are not necessarily the same as those to which they are now exposed.

He devoted the rest of the lengthy Introductory Essay to the defense and elaboration of these pillars.[66] *Flora Indica* repeated the lesson in a slightly different form. The first two axioms – species fixity and ubiquitous variation – appeared at variance. Hooker reconciled them by making a sharp distinction between "variability" and "mutability." Variability was the insignificant fluctuation of taxonomically unimportant characters; mutability was the fundamental alteration of a species' defining characteristics.[67] This distinction followed conventional scientific wisdom. Naturalists generally, if not universally, viewed variation as transient response to environmental change.[68] Authoritative sources like Augustin-Pyramus de Candolle's *Théorie élémentaire de la botanique*, Lyell's *Principles of Geology* and Gray's *Botanical Text-Book* insisted that variation was produced by external stimuli, such as changes in soil, exposure, temperature and moisture. Remove the stimuli and the corresponding variation inevitably vanishes as the plant

[65] JDH to AG, 28 July 1855, *JDH-AG* .
[66] Hooker, 1853, pp. vii–viii .
[67] Hooker and Thomson, 1855, pp. 20–21. See also Hooker, 1853, p. x.
[68] Louis Agassiz was one of the most prominent dissenters from the dominant environmentalism of the time; see Browne, 1983, pp. 138–144. Hooker considered Agassiz's theories on biogeography anathema and was deeply suspicious of any position taken by the Swiss naturalist.

reverts to type. The ability to vary did not undermine the stability of a species; it was in fact a vital part of specific character.[69]

This view of variation held particular appeal for Hooker. It offered a coherent program for handling variation taxonomically. The number of supposed species would diminish by a third if botanists stopped "confounding variations with specific differences."[70] The variability/mutability dichotomy enabled Hooker to argue that the characters which "species mongers" used to split species were ephemeral and thus illegitimate, while the characters he used were permanent and meaningful. But if *no* characters were stable then the botanist "can never hope to give that precision to his characters of organs and functions which is necessary to render his descriptions useful to others."[71]

Yet how to differentiate between an ephemeral "variation" and permanent "specific difference"? That was the challenge. Antoine-Laurent de Jussieu, the founder of the natural system of plant classification, had hoped that the success of chemists in uncovering the laws which regulated the combination of elements and the formation of compounds would be replicated in botany with the discovery of laws governing connections in plants between essential characters and non-essential differentiae.[72] A.-P. de Candolle, who refined Jussieu's natural system, turned to an even more promising analogy, mineralogy. At the turn of the century, René-Just Haüy rescued the classification of minerals from chaos by discovering the laws of crystallography.[73] These laws allowed the mineralogist to determine *à priori* all possible forms under which a mineral species may occur. This reduction of diverse individual peculiarities to fixed and universal laws was quintessential philosophical natural history.

Candolle believed that botany, like mineralogy, would uncover the laws governing deviation caused by "action of life" from a basic and unchanging type.[74] Candolle's analogy between botany and crystallography had implications for forming groups at all ranks of the classificatory hierarchy, but it held out particular appeal for solving controversies over species delimitation. Botanists would be able to determine with mathematical precision whether or not two groups of

[69] A.-P. Candolle, 1813, pp. 160–163. Gray, 1845, pp. 303–304. Lyell, 1830–1833, vol. 2, pp. 25–37, 64.
[70] Hooker and Thomson, 1855, pp. 30, 38.
[71] Hooker, 1853, p. viii.
[72] Stevens, 1994, pp. 38–39.
[73] Stevens, 1984 and Lorch, 1974.
[74] Stevens, 1984, p. 62.

plants shared the same underlying "symmetry" and so belonged to the same species. Candolle's analogy deeply and directly influenced both Whewell and Henslow.[75] Henslow insisted that until botany uncovered its own laws analogous to those of crystallography, "we cannot expect precision in the details of systematic botany."[76] Candolle himself sharply advised botanists to eschew transmutation (a "dangerous pyrrhonism") and instead concentrate on the search for the exact physical causes of organic variation.[77]

Most prominent naturalists believed that the intimate connection which existed between living beings and the physical influences of the environment explained not only the production of variation but also the global distribution of vegetation. Hooker's intense interest in plant geography was connected intimately, then, with his desire to reform botanical systematics along more "philosophical" lines. One of Hooker's major research objectives in India was accumulating data towards establishing the interrelated laws regulating the variation and distribution of species. As he told Darwin before setting out, "what I have aimed at is, to trace the connection between climate & the present state of vegetation."[78] Alexander von Humboldt's careful correlation of environmental factors with plant distribution provided both inspiration and method. "If the laws which nature has followed in the distribution of vegetable forms are more complicated than they first appeared, we are no less obligated to submit them to exact investigations," the celebrated German naturalist challenged in 1820.[79] In good Humboldtian fashion, Hooker systematically recorded a range of environmental variables in order to link them together as integrated components of a dynamic global system of vegetation.[80] As imperfect as special creation might have been, Candolle, Haüy, Humboldt, Jussieu and Lyell offered methods and models for its reformation.

Hooker never denied the possibility of transmutation in his Introductory Essays; in fact, he admitted candidly that "in the present imperfect state of our knowledge of the botany of any large area, we

[75] For A.-P. de Candolle's influence on Whewell, see Stevens, 1984, pp. 69–71; for his influence on Henslow, see Walters and Stow, 2002, pp. 60, 65–66.

[76] Henslow, 1836, pp. 116–118.

[77] A.-P. de Candolle, 1813, p. 160.

[78] JDH to CD, 28 October 1844, *CCD* 3: 70. See also Hooker and Thomson, 1855, p. 24.

[79] Quoted in Dettelbach, 1996, p. 297.

[80] Hooker, 1854, vol. 2, pp. 357-473. See also JDH to Alexander von Humboldt, 4 December 1847, *Humboldt* K. 12, Nr. 113; and 21 September 1854, *Humboldt* K. 11, Nr. 10.

have not the materials for solving the great questions as to the origin and permanence of species, upon general principles." The foundation for arriving at solutions "upon general principles" appeared well established, however. Naturalists from Linnaeus to Lyell insisted that the environment induced variation. Darwin himself accepted this. Candolle's sophisticated and critical approach to comparative plant morphology inspired the belief that botany would soon have its own laws to disentangle modifying circumstances from underlying regularity. Humboldt suggested a complementary methodology for combining these observations with laws regulating the global distribution of vegetation. Despite the current confusion over species limits, Hooker felt that the increasing knowledge of plant affinities demonstrated that botany was making "progress in the right direction."[81]

Hooker's defense of species fixity appeared outwardly impervious, precisely because of its ostentatious lack of dogmatism. Lyell warmly admired the two introductory essays (particularly their anti-transmutationism) and advised Hooker to rework them into a general treatise so that their principles could serve "as canons in natural history."[82] Thomas Bell, the president of the Linnean Society, pronounced Hooker's arguments against transmutation "conclusive."[83] Even Darwin predicted that, once he began writing his book on species, "I shall gnash my teeth & abuse you for having put so many hostile facts so confoundedly well."[84]

In actuality, the concept of species fixity teetered on the brink of collapse. Hooker's adherence to it rested on its authority and its usefulness. Both were being progressively undermined.

Wandering Thoughts and Practical Hands

The variability/mutability dichotomy allowed Hooker to insist that species were highly variable without abandoning species fixity. This served to underpin theoretically the case for broad taxonomic delimitation. Unfortunately, the logical and empirical foundation of this dichotomy was insufficient to bear the theoretical weight placed upon it. As Darwin pointed out in the *Origin*, naturalists frequently reasoned in a circle: they ranked a character as "important" as a specific mark because it did not vary and then argued it did not vary because it was

[81] Hooker, 1853, pp. viii, xiv.
[82] CL to JDH, 25 July 1856, Lyell, 1881, vol. 2, p. 214.
[83] [Bell], 1857, p. 243.
[84] CD to JDH, [9 October 1853], *CCD* 5: 159.

important.[85] This objection would not have been fatal if naturalists could break the circularity by uncovering a principle for determining the basic organizing principle of a living species, as Haüy accomplished with mineral species.

Yet the orthodox explanation of variation was proving more venerable than credible. The fact that domesticated plants and animals retained their unique characteristics over generations and geographic ranges presented the most obvious counter-argument against variation being inherently ephemeral and externally produced. William Hooker and G.A.W. Arnott, in the 1850 fifth edition of the *British Flora*, even suggested that varieties obtained from primitive, divinely-created species could become permanently fixed in their differences.[86] (A reviewer for the *Botanical Gazette*, probably Arthur Henfrey, rebuked this opinion, reiterating the orthodox position that variation lasted only as long as its external causes continued in action).[87] Naturalists who wanted to retain the belief in the environmental origin and inherently transient nature of variation stressed the unique circumstances of domestication. Gray, for example, argued that humans created domesticated races only by working assiduously to promote them against "that stronger natural tendency to reversion to the primitive type." He knew of no examples of such change happening in a perfectly wild species. And even domestic races "manifest an unequivocal disposition to return to their aboriginal state."[88]

In *Flora Novæ-Zelandiæ*, Hooker followed Gray's lead on the exceptional nature of domestication and quoted his claim that variation was caused by "varying physical influences, accidental circumstances, or from cultivation."[89] Hooker confessed privately to his friend, however, that "I believed in *you* in short, quite as much as in what you wrote."[90] Almost everyone agreed that external conditions influenced a plant's deviation from type, but no one could explain how. In *Flora Indica*, he and Thomson outlined the difficulties investigating "those laws that regulate the development of varieties."[91] Climate involved a bewildering number of interacting variables. Temperature and humidity react so

[85] Darwin, 1859, p. 46.
[86] W. Hooker and Arnott, 1850, pp. ix–xi.
[87] [Anonymous], 1850, p. 302.
[88] Gray, 1845, pp. 303–304.
[89] Hooker, 1853, pp. x–xii.
[90] JDH to AG, 24 March 1854 (copy), *From-JDH* 5: 14–21.
[91] Hooker and Thomson, 1855, p. 26.

naturally upon one another that it was not easy to determine cause and effect.[92] Untangling the influence of all these factors posed a daunting problem with no obvious solution.

The naturalist also had to account for frustrating inconsistencies. Very similar climates had very different floras, indicating that the environment did not simply imprint itself on plant life. "It is difficult," Hooker and Thomson admitted, "on the one hand, to reconcile the acknowledged tendency of varieties and hybrids to revert to their original state, with the fact that the floras of remote areas, possessing similar climates, are permanently and prominently different in their main elements."[93] If the environment did produce variation, it did not appear to do so in any predictable way. The plants of climatically uniform New Zealand produced proportionally *more* varieties than those of climatically diverse India.[94] New conditions sometimes produced variation and sometimes did not. In *Flora Novæ-Zelandæ* Hooker acknowledged that varieties would reproduce true even when transplanted into different climates, at least for a certain period.[95]

In 1854 Hooker had conceded to Gray that the Introductory Essay to *Flora Novæ-Zelandiæ* was "more as a résumé of general impressions than a specimen of *close reasoning*, for of the latter, in truth, the subject *does not admit*. There is not a single argument that will not cut both ways, and may not be turned pro and con species, specific centres, &c., &c."[96] The following year he confided to Darwin that "after all it is very easy to talk of the creation of a species in the Lyellian view of creation, but the *idea* is no more tangible than that of the Trinity & to be really firmly & implicitly believed is neither more nor less than a superstition – a believing in what the human mind cannot grasp."[97] Meanwhile, his study of plants continued to "shake species to their foundation."[98] He groaned to Gray in early 1856 that "the broad fact stares us [Thomson and himself] in the face that we are not advancing to unity in our ideas of species of any large genus, but diverging more & more every day. I do not say this because *my views* diverge from others, but because I see others as divergent from one another as I am from them."[99]

[92] Hooker and Thomson, 1855, pp. 74–75.
[93] Hooker and Thomson, 1855, p. 25.
[94] Hooker and Thomson, 1855, pp. 25–26, 74–75.
[95] Hooker, 1853, p. xviii.
[96] JDH to AG, 26 January 1854 (copy), *From-JDH* 5: 7.
[97] JDH to CD, [6–9 June 1855], *CCD* 5: 345.
[98] JDH to CD, [8 July 1855], *CCD* 5: 372.
[99] JDH to AG, 16 February 1856, *JDH-AG*.

Frustratingly, despite his earlier optimism, the increasing knowledge of plant form and affinity was fanning rather than smothering these disagreements.

Hooker's vague but growing doubts about the conventional understanding of species and varieties began to crystallize in the summer of 1855 with a close reading of *Géographie botanique*, a comprehensive and dense treatise of nearly 1,400 pages in two volumes by Alphonse de Candolle, the accomplished son of Augustin-Pyramus de Candolle. *Géographie botanique* paid particular attention to variation and its origin. While accepting the standard environmentalist interpretation of variation, the younger Candolle carefully acknowledged its uncertainties and inconsistencies. He estimated that it took a century to create or obliterate a plant variety, but could discover no fixed laws which regulated the process. Like William Hooker and Arnott five years earlier, Candolle concluded that in rare cases, the distinctiveness of a population became so ingrained that its peculiarities remained fixed over innumerable generations, a category he labeled as a *race*.[100]

These facts had profound implications for the species question. Candolle listed numerous definitions of species, and pessimistically concluded that, "unfortunately, naturalists will never agree on the degree of resemblance which permits a belief in common origin. In other words, they will never agree on the possible extent of variation between individuals of common ancestry." Observation might indicate that species remained fixed in human experience, but because of our ignorance of plants under different external conditions "perhaps this fact lacks great value."[101] The question of species origin was dangerous. A faulty theory could irreparably damage a naturalist's reputation and throw his descriptive work into disrepute.[102]

Hooker reacted favorably to *Géographie botanique*.[103] His admiration for its meticulous presentation of data and encyclopedic scope did not prevent him from assessing it critically, which he did privately to Gray and publicly in a long anonymous review published over several issues of *Hooker's Journal of Botany and Kew Garden Miscellany* in 1856.[104] Hooker did not blame his dissatisfaction on Candolle personally. The real problem was the vague principles, inexact methods, puzzling

[100] Alp. de Candolle, 1855, pp. 1081–1087.
[101] Alp. de Candolle, 1855, pp. 1070–1071.
[102] Alp. de Candolle, 1855, pp. 1068–1069.
[103] Hooker, 1856. Hooker, 1882, pp. 737–738. JDH to AG, 28 July 1855, *JDH-AG*. CD to AG, 24 August [1855], *CCD* 5: 419. JDH to GB, [late July 1855], *From-JDH* 2: 109.
[104] JDH to AG, 26 June 1856 and 10 December 1856, *JDH-AG*. [Hooker], 1856.

phenomena and speculative character of botanical geography as a subject.[105] An implicit strain of self-criticism runs through the entire review since his complaints applied as strongly to his own positions as to those of Candolle. (Bentham noted in the *Edinburgh Review* "how nearly De Candolle and Hooker agree in the conclusions they have drawn.")[106]

Hooker conceded that differentiating between *race* and *variety* had a valid theoretical rationale. From a practical point of view, however, it added another layer of confusion. It could take years to prove that a variety was nothing more than a variety. Proving a race was only a more permanent type of variety was "obviously hopeless."[107] Candolle further raised the possibility that geological change might break an island containing eight varieties into an archipelago of eight islets, each of which may retain but one or a few of the varieties. Further geological change might destroy all the islets except the ones containing two varieties. This hypothetical process bore remarkable similarity to the actual situation of plant distribution in the Galápagos Archipelago which Hooker described in an essay based on Darwin's collections. Hooker noted, but did not explain, the peculiar fact that the overwhelming majority of plant species unique to the archipelago were confined to a solitary islet.[108] In his discussion of Candolle's hypothetical archipelago, he noted that geographic separation and elapsed time might suffice to "render these permanent races, which hence may become undistinguishable from species." Eventually these two varieties might become so confirmed in their difference and sundered in their location to make any assumption of common specific origin impossible. Such a plausible possibility "does appear to render the attempt to arrive at any definite conclusion as to the limits of many species a desperate one."[109]

Such a process of geologically driven diversification did not, by itself, confirm Darwin's belief in limitless gradual modification. Forms (whether ranked as races, varieties or species) might still remain bounded to their aboriginal type. But, by breaking down the conceptual barriers between creation of varieties and the creation of species, Hooker made a significant move towards a transmutationist understanding of species origin. The creation of new species through geographic isolation and subsequent gradual differentiation provided a rational explanation for at least some observed phenomena.

[105] [Hooker], 1856, pp. 248–249.
[106] [Bentham], 1856, p. 253.
[107] [Hooker], 1856, p. 121.
[108] [Hooker], 1851. For a discussion of this essay, see Browne, 1983, pp. 157–159.
[109] [Hooker], 1856, p. 152.

Hooker's review displayed a sharp skepticism of special creation absent in earlier writing. "We are told that the majority of species were created such as they now exist, but there is not a shadow of proof for this."[110] He found the case for the transmutationist alternative no stronger, however. Neither theory, in his view, fully explained the available evidence. The review reiterated the opinion, stated privately to Darwin in 1845, that any position on this muddled question must remain subservient to the practical needs of descriptive botany. He sneered at the preference of "the general inquirer" for transmutation on misapprehended "philosophical" grounds, remarking that such a dilettante "has nothing to do with facts, and knows nothing of species or varieties." For those who dealt in natural-history facts, the question of "whether [species] are all suffering transmutation or not, appears to be immaterial as regards the progress of botanical science."[111]

Darwin's barnacle classification helped dispel Hooker's previous doubts about the compatibility of transmutation with good systematic practice. Darwin lumped barnacle forms aggressively and explicitly.[112] This demonstrated that transmutationism was entirely consistent with a practical commitment to the broad delimitation of both species and genera. He made sure that Hooker recognized this.[113] Hooker took the point, and subsequently acknowledged that the difference between special creationists and transmutationists "is very wide perhaps, but not so wide as to allow of their employing different methods towards the advancement of Botany in any one of its departments."[114]

Hooker's persistent methodological conservatism existed alongside his continued impatience with the perceived theoretical inadequacies of his discipline. He lamented the lack of originality in Candolle's treatment of botanical geography and concluded that "some perfectly original course of study must be adopted or some new ideas must be conceived, in relation to the subject ... before further advance can be attained in the direction now being followed."[115] The challenge was identifying ideas which would not undermine practices which *did* work. As he acknowledged to Gray, "the subject [of species] does not admit of

[110] [Hooker], 1856, p. 250.
[111] [Hooker], 1856, pp. 252–255.
[112] Darwin, 1851, pp. 27–32. Darwin, 1851–1854, vol. 1, pp. 98, 150–1; vol. 2, pp. 196–197, 242–246, 251–252. See also Southward, 1983.
[113] CD to JDH, 25 September [1853], *CCD* 5: 155–156.
[114] [Hooker], 1856, p. 255.
[115] Hooker, 1856, pp. 248–249.

fixed ideas, confound it altogether. Thus my thoughts wander when I begin to speculate upon the subject; but when I take the plants in hand, I find that the subject of species practically admits of being handled very well."[116] Any "bold original idea" needed to anchor his thoughts without tying his hands.

In May of 1856, Darwin gloated to Lyell that the change in Hooker's opinion about species "is really striking (but almost laughable to me)."[117] Darwin pressed his case by sending Hooker an extract from his "big species book" on arctic–alpine floras. Hooker was impressed enough to conceded that "your case is a most strong one & gives me a much higher idea of *change* than I had previously entertained; & though, as you know, never very stubborn about unalterability of specific type, I never felt so shaky about species before."[118]

Shaky, but not toppled. Undermining special creation did not prove transmutation. Hooker felt that both transmutation and special creation floundered against two paramount facts which "lead to diametrically opposite conclusions." The first fact, that a great number of species appeared hereditarily stable and distinct from allied species, argued for fixity. The second fact, that numerous other species were highly variable and "races with characters as strongly marked as species are constantly being produced under our eyes," pointed towards transmutation.[119] Hooker could not see how any theory of species origin could navigate around one fact without smashing fatally against the other.

By 1856, only two years after winning the Royal Medal in part for his "cautious" attention to the origin and distribution of species, Hooker no longer felt inclined to mount a defense of special creation, even conditionally. His investigation of species origin and distribution, praised so warmly by Lord Rosse, had relied on orthodox explanations of variation and species limits elaborated by the likes of Lyell, Henslow, Humboldt, Gray and the Candolles, and he had lost confidence in those explanations. Hooker's disagreement with Darwin over species did not end, however. He still indulged "vague hopes" of addressing the species question in print.[120] The way forward remained unclear, however. "We see no more means of forming an opinion on the subject of the origin of

[116] JDH to AG, 26 June 1856, *JDH-AG*.
[117] CD to CL, 3 May [1856], *CCD* 6: 100.
[118] JDH to CD, 9 November 1856, *CCD* 6: 259. The manuscript is transcribed with commentary in Darwin, 1975, pp. 528–566. See Browne, 1983, pp. 131–134.
[119] [Hooker], 1856, p. 251. See also Hooker, 1860, p. viii.
[120] JDH to CD, [11 April 1857], *CCD* 6: 369.

species, than we do of the origin of time," he declared despairingly in his Candolle review.[121]

Hooker approached species pragmatically – taking them "as they *appear to be* not as *they are*, & **still less** as **they were** or ought to be"[122] – but this forced him to face the uncomfortable problem that species appeared to be something different than either special creation or transmutation said they ought to be. Darwin's principle of divergence would break this impasse.

Varieties and Species

Darwin's principle of divergence was the most significant addition to his theory after 1838.[123] As Dov Ospovat pointed out, it also "appears as an instance in which the direction of Darwin's speculations was determined by the theories and perspectives of his fellow naturalists."[124] No perspective carried more weight with Darwin than Hooker's. Descent implied that all the descendants from an original species were linked by an unbroken series of minute gradations. Systematics demonstrated the "universal law" that taxa break up into progressively smaller groups, families into diverging genera, genera into diverging species, species into diverging varieties.[125] However the rarity of extant transitional forms confounded Darwin for a long time.[126] The history of life was best represented as a taxonomic tree, but *why*?[127] Genealogy by itself offered no explanation. Darwin's original assumption that accidents of transport, geographical isolation and extinction poked gaps between related forms seemed too dependent on random events to account for the universal pattern of branching diversification. Why, for example, would allied species become distinct when they shared the same contiguous territory? Divergence was a general phenomenon which demanded a general explanation.[128]

Darwin began to recognize that his relatively traditional understanding of variation represented the largest obstacle to providing such

[121] [Hooker], 1856, p. 255.
[122] JDH to CD, [mid-July 1845], *CCD* 3: 223.
[123] For discussions of Darwin's principle of divergence, see Browne, 1978, 1980 and 1983, pp. 206–220; Kohn, 1985; and Mayr, 1992; Ospovat, 1981, pp. 170–209.
[124] Ospovat, 1981, p. 190.
[125] Ospovat, 1981, pp. 175–176.
[126] Darwin, 1859, p. 174; Darwin, 1958, pp. 120–121.
[127] CD to THH, 5 July [1857] and 9 July [1857], *CCD* 6: 420, 427–428.
[128] Ospovat, 1981, p. 175.

a general explanation. While he rejected the notion of variation as inherently transient, he accepted that a form would differentiate from its parental stock only after a geographical or geological change, or the direct human intervention, "unsettle[d]" its constitution.[129] Under this view of variation, speciation could only take place if isolating mechanisms separated populations into distinct environments. He needed new views of variation and the economy of nature to explain divergence, but found it difficult to make that radical break.[130] Darwin finally realized that, if he decoupled the production of variation from the environment, natural selection could explain not only modification but also divergence. Variation appeared spontaneously as a natural consequence of sexual reproduction (even if the exact mechanism of the process remained mysterious), and did not rely absolutely upon external unsettling agents.

Darwin concluded that large genera, those with numerous species in a geographic region, should on average vary more and give rise to more new forms than small genera. A large genus became large because of its superior adaptation to the organic and inorganic conditions prevailing in its region. Superior adaptation resulted in the production of more individuals, and consequently more variation. More variation meant more diversity (behavioral, physiological and morphological). Greater diversity brought increased opportunities to seize on new and different places in the economy of nature. Divergence arose directly from the struggle for survival, as those forms which could radiate outward to occupy new places in the economy of nature would be more successful than those which could not. As divergent forms insinuated themselves into new places, they displaced and thus exterminated the less specialized parental form. As a result, groups continually branched into subordinate groups.

Natural selection favored divergent modification. According to this logic, then, the size and the diversification of a genus should correlate as consequences of success in the struggle for survival. If species were discrete creations, however, there was no reason why a large cosmopolitan genus would produce on average proportionally more variation than a small local one. Darwin decided to test whether his proposition was borne out in the botanical literature. He laboriously compiled statistics (culled from books lent by Hooker) and, despite some early computational missteps, demonstrated that he was right.[131] The

[129] Browne, 1983, pp. 192–202.
[130] CD to CL, 11 October [1859], *CCD* 7: 346.
[131] Browne, 1980, pp. 62–89. Ospovat, 1981, pp. 184–190.

principle of divergence provides the "origin of all classification," he crowed to both Hooker and Gray.[132]

Hooker knocked the legs out from Darwin's proof. He suggested that the greater number of recorded varieties in large genera resulted from the tendency of botanists to *identify* them more readily. If large genera did produce more variation than small ones, it would not have escaped notice. "The long & short of the matter is," Hooker explained, "that Botanists do not attach that *definite* importance to varieties that you suppose, they do not treat large & small genera equally & similarly, & the sum of inequalities thus produced tends to make the species of small genera look more invariable than of big."[133] In other words, Darwin's painstaking calculations of the frequency of varieties in differently sized genera uncovered the psychological peculiarities of botanists rather than realities of nature.[134] Darwin reacted to this bombshell with a combination of annoyance and admiration for Hooker, that "terrible worrier of poor theorists."[135]

Darwin doggedly created new statistical tables to answer Hooker's objections, groaning "oh my God how I do hate species & varieties."[136] This flurry of number crunching confirmed his original results. No matter which botanist he used – ranging from the splitter Babington to the lumper Bentham – the tabulations came out the same. He found it implausible that so many naturalists should have all "unconsciously & unintentionally" produced so uniform a result. While conceding it might be presumptuous to assume that "botanists have worked more philosophically than they themselves think they have," the statistics provided unambiguous corroboration.[137]

The principle of divergence, Darwin argued, gave legitimacy to the practice of classification. Even if the naturalist had at his fingertips every form that ever existed, the genealogical branching of life forms would still remain, allowing for "a natural classification, or at least a natural arrangement."[138] Clearly, while the groupings which naturalists relied upon were more-or-less artificial, the *system* they devised was not.[139]

[132] CD to JDH, 22 August [1857], CD to AG, 5 September [1857] and 29 November (1857), *CCD* 6: 443–444, 448–449, 492–493.
[133] JDH to CD, [14 March 1858], *CCD* 7: 49.
[134] Darwin, 1975, p. 161.
[135] CD to JDH, 28 February [1858], *CCD* 7: 40 .
[136] CD to JDH, 11 March [1858], *CCD* 7: 48.
[137] CD to JDH, 8 [June 1858], *CCD* 7: 102.
[138] Darwin, 1859, pp. 52, 432.
[139] Ghiselin, 1969, p. 85.

This principle also reconciled the apparently contradictory facts of species stability and variability. Darwin explained for the first time why some groups were limitable and others were not.

This conceptual advantage was not, moreover, purchased at the cost of undermining the broad species concept. The systematist could still treat species as functionally permanent because the checks to indiscriminate variation were so strong and the time necessary for a variety to depart significantly from its original specific type was so great. The difference between a variety and a "good and true species" was incapable of rigorous definition, but it was not pure taxonomic fiction. A variety had not *yet* diverged sufficiently from its parental form to deserve a specific name. Darwin presented the general rule that "species may be looked at as the result of variation at a former period; & varieties, as the result of contemporaneous variation."[140]

Darwin moreover provided criteria for adjudicating the degree of difference necessary to distinguish a species from a variety. As a general rule, species in large genera were more highly variable and closely related than those in small genera, so it followed that species of larger genera resemble varieties more than the species of smaller genera. This observation was crucial for taxonomy. "Undoubtedly there is one most important point of difference between varieties and species; namely, that the amount of difference between varieties, when compared with each other or with their parent-species, is much less than that between species of the same genus," he noted.[141] To make use of this advice, a naturalist needed access to a broad suite of specimens to determine the standard degree of variation in a given genus. He also needed to approach the question globally, since a geographically restricted study would provide insufficient data. In botany, this reinforced the authority of large institution (like Kew Gardens) with comprehensive general herbaria.

John Beatty pointed out that "Darwin did not tailor his use of the term 'species' to suit pre-Darwinian, nonevolutionary *definitions* of the term. Instead, he used the term in accordance with *examples* of its referential use by members of the naturalist community."[142] This analysis is correct, but incomplete. As the incessant debates between lumpers and splitters demonstrated, the "referential use" of the term species varied dramatically from naturalist to naturalist. Darwin's principle of divergence is important because it explained patterns of

[140] Darwin, 1975, p. 165.
[141] Darwin, 1859, pp. 47, 56–58. This validated Hooker's taxonomic practice of handling variation differently in large and small genera. See Stevens, 1997, p. 357.
[142] Beatty, 1985, p. 277.

biological affinity and distribution while supporting a particular approach to systematics – the broad species concept so dear to Hooker – within a transmutationist framework. Darwin was not attempting to create a unique species concept,[143] nor was he addressing an undifferentiated community of naturalists. He was bringing his theory into alignment with the practices and beliefs of lumpers like Lyell, Watson, Bentham, Gray, Henslow, Hooker and (of course) himself. Darwin carefully tied the abstract rationale for divergence to both the lumpers' global perspective and their routine use of comprehensive collections in taxonomic practice. This placed the considerable authority of Darwin's theory at the disposal of one side in a protracted and bitter dispute over the delimitation of species, just as Darwin intended and Hooker demanded.

In early May of 1858 Darwin sent Hooker the manuscript from his "big species book" which expounded the principle of divergence in its mature form.[144] To Darwin's immense relief, Hooker withdrew his objections.[145] Once Hooker accepted the principle of divergence, there was nothing standing in the way of taking a "fixed post" in favor of transmutation. He just needed a push.

One came unexpectedly in mid-June.

Barren Facts and Rational Explanation

In July of 1855, about the time he received *Géographie botanique*, Hooker admitted to Darwin that "if there were a possibility of bringing your & my opinions to *book*, it might prove that we were not so far divided."[146] The next three years brought Hooker and Darwin's views ever closer, but nothing brought them "to book" until Darwin received a package from Alfred Russel Wallace on 18 June 1858. Wallace enclosed a short essay setting out a theory of transmutation nearly identical to Darwin's own. Darwin, horrified at losing his priority, turned desperately to Lyell and Hooker for advice. They decided that the most evenhanded solution was joint-publication. Consequently, Darwin and Wallace's theories were presented at the Linnean Society on July 1st.[147] The arrival of Wallace's essay marked a watershed in Hooker's life. As long as Darwin's theory remained a secret work-in-progress, Hooker

[143] For a contrary position, see Stamos, 1999, esp. pp. 174–175.
[144] CD to JDH, 6 May [1858], *CCD* 7: 89.
[145] CD to JDH, 8 [June 1858], *CCD* 7: 102.
[146] JDH to CD, [8 July 1855], *CCD* 5: 372.
[147] Browne, 2002, pp. 33–42; Desmond and Moore, 1991, pp. 466–467.

was honor-bound not to discuss it publicly, pro or con. Once it entered the public domain, however, he could no longer remain mute. Silence would have been tantamount to disapproval. Wallace not only forced Darwin public, but finally pushed Hooker to stop waffling and acknowledge explicitly a preference for transmutationism.

Darwin gratefully acknowledged his friend's conversion (in a metaphor forgivable only from a man suffering a chronically rebellious digestive tract): "You cannot imagine how pleased I am that the notion of Natural Selection has acted as a purgative on your bowels of immutability."[148] It had been a slow-acting purgative. Hooker's accommodation to Darwin's theory had been almost painfully gradual. As he explained in *Flora Tasmaniæ*, "matured conclusions on these subjects are very slowly developed."[149] Hooker confirmed his acceptance of transmutation in a letter to Gray in September of 1858: "my faith is shaken to the foundation, & that the sum of all evidence I have encountered since I studied the subject is in favor of the origin of species by ~~transmutation~~ variation."[150] The striking out of "transmutation" was momentous. With a deceptively innocent scratch of the pen, Hooker signaled the abandonment of his position in *Flora Indica* that transmutation represented a fundamentally different phenomenon from the law-bound production of varieties. Quibbles with Darwin remained – such as the question about progressive development in plants over geological time – but these lacked the weight to counterbalance the evidence and arguments on Darwin's side of the scale.[151]

Hooker's conversion to Darwin's doctrine rendered many of his pronouncements in *Flora Novæ-Zelandiæ* and *Flora Indica* obsolete or inaccurate. He had started an Introductory Essay to *Flora Tasmaniæ* in 1857 with the intention of providing a succinct discussion of the main features of the Australian flora which would leave "all conclusion drawing to others."[152] He abandoned this unambitious plan in order to announce and defend his new acceptance of transmutation.[153] A stream of manuscripts, proof sheets and letters full of complaints, questions and moral support flowed freely between Kew Gardens and Down

[148] CD to JDH, 13 [July 1858], *CCD* 7: 130.
[149] Hooker, 1860, p. xxvi .
[150] JDH to AG, 23 September 1858, quoted in Porter, 1993, p. 31.
[151] Porter, 1993, pp. 31–32.
[152] JDH to CD, [17–23 December 1857], *CCD* 6: 508.
[153] JDH to AG, 21 October 1858, quoted and discussed in Porter, 1993, pp. 32–33.

House as Hooker and Darwin worked on their manifestos.[154] Both men relied on this camaraderie to dissipate "our mutual muddle."[155] Their work covered much the same ground, "but yet somehow everything is taken up from such different points of view," Darwin believed, "that I do not think we shall injure the originality of our respective Books."[156] The *Origin* was published in November while Hooker's "luckless" essay languished with the "lazy printers" until a few days after Christmas. Hooker declared that "beside your book [my essay] will look like a ragged handkerchief beside a Royal Standard."[157] Darwin, never one to leave his friend's self-deprecation unchallenged, countered that the essay "is by far the grandest & most interesting Essay on subjects of the nature discussed I have ever read."[158]

The *Origin* codified Darwin's views on systematics. These views were meticulously calibrated to validate Hooker's beliefs, practices and instincts. Darwin scrupulously framed his argument to avoid inadvertently encouraging "species mongers." A variety might be an "incipient species," but he insisted that, as a practical taxonomic matter, the species category should be interpreted broadly, particularly when a form blended insensibly into its parental form. In cases which invite doubt and conjecture, "the opinion of naturalists having sound judgment and wide experience seems the only guide to follow." Darwin followed the lumper playbook and explicitly cautioned the "young naturalist" to avoid the tendency borne of inexperience to make many book species.[159]

In the conclusion of the *Origin*, Darwin famously remarked that "when the views entertained in this volume on the origin of species, or when analogous views are generally admitted, we can dimly foresee that there will be a considerable revolution in natural history." The following passage, however, stresses that "systematists will be able to pursue their labours as at present; but they will not be incessantly haunted by the shadowy doubt whether this or that form be a true species."[160] This pragmatic approach to species – grounded as it was on deference to the "sound judgment and wide experience" of elite natu-

[154] JDH to CD, 22 December 1858, CD to JDH, 24–5 November [1858], 7 April [1859], 11 May [1859], 1 September [1859], 15 October [1859], and [23 October 1859], *CCD*: 7, 207, 218, 280–281, 297, 328, 349, and 355–356.
[155] CD to JDH, 11 May [1859], *CCD* 7: 297.
[156] CD to JDH, 7 April [1859], *CCD* 7: 280.
[157] JDH to CD, [21 December 1859], *CCD* 7: 383.
[158] CD to JDH, 3 January [1860], *CCD* 8: 6.
[159] Darwin, 1859, pp. 44–54.
[160] Darwin, 1859, pp. 484–485.

ralists – found a willing audience, even among naturalists otherwise skeptical of Darwin's theory.

As justifiably proud as Darwin was of the *Origin*, he recognized that successful applications of natural selection would be the "real engine to compel people to reflect on the modification of species."[161] Hooker's Introductory Essay to *Flora Tasmaniæ* provided one such application – or, in Gray's words, "a trial of the Theory."[162] Hooker stressed that his judgment emerged from "20 years' study of plants."[163] Where he explored the great diversity of opinions on species in *Flora Novæ-Zelandiæ, Flora Tasmaniæ* reduced the issue to a bipolar debate between transmutation and special creation. He presents a tightly-focused précis of the case for and against Darwin's theory.

Like Darwin, Hooker turned to domesticated forms to reinforce his case. He explicitly repudiated his former position that cultivated varieties reverted to their aboriginal form when freed from humanity's guiding hand. The phrase "reversion to wild type" seriously muddled what in fact happened. The majority of cultivated cereals and vegetables showed no disposition to assume the character of their wild state when neglected. This was why it was so difficult and often impossible to determine the wild parent species of most cultivated plants. Cultivars certainly degenerated in the wild, but resembled their progenitors only in so far that all stunted plants resemble wild plants of similar habit. It was the loss of luxuriance that gave the superficial appearance of reversion. When domesticated stocks of fine apple trees grew feral from seed, they might become crab states of their variety, but did not regress to the original wild crab-apple.[164] Hooker also rejected the notion that true reversion would take place if the domesticated plants were reintroduced into the original environmental conditions of their parental stock. Cultivated forms of cabbage escaped into the wild along the seashore but remained distinct from their wild cousins in the area.[165] Hooker's greater precision in treating reversion illustrates how much more sophisticated and nuanced his understanding of variation had become.

Hooker never mentioned the principle of divergence by name. But he did provide a catalogue of details and arguments to confirm it: the

[161] CD to JDH, 25 February [1862], *CCD* 10: 93.
[162] Gray, 1860, p. 153.
[163] Hooker, 1860, p. iv .
[164] This repudiates the position he took in JDH to CD, [4–9 September 1845], *CCD* 3: 251.
[165] Hooker, 1860, pp. ix, xii. This directly contradicted Lyell, 1830–1833, vol. 2, p. 33.

plausible assumption that a genus with a multitude of illimitable forms is reproducing at a proportionally greater rate than a genus with limitable species; the tendency of varieties to depart from and exterminate their parental form; the fact that the same spot can support more life when occupied by more diverse forms; and the contention that organic relationships are paramount in the formation of floras since plants grow where they can find a place in the economy of nature and not where they might be best suited to the physical environment.[166]

Hooker masterfully assaulted the logical and evidentiary foundations of special creation. He scoffed at the idea of some shadowy, boundless power intermittently conjuring the most complex and bulky organic forms whole from inorganic elements. Special creation also rendered the remarkable affinities between forms meaningless except as markers in a sterile act of bookkeeping. For transmutationists, on the other hand, affinities illuminated descent and allowed for the rational unification of classification with studies of distribution, morphology, and physiology.[167]

The failure of special creation to explain such "collateral biological phenomena" placed it at a serious conceptual disadvantage.[168] Hooker abandoned his Humboldtian aspiration to correlate plant distribution directly with environmental variables, but he still believed that botany had no claim to rigor unless it recognized and explicated the reciprocal action of the inorganic and organic worlds. Darwin provided guidance for teasing out the intricate ways in which the environment both encourages and limits variation, and this wove the study of botany more closely into the overall fabric of science. The "psychological philosophers" persuaded Hooker that a tendency to specialize pervades every attribute of organic life. The physicists taught him that there are limits to the amount and duration of heat, light, and every other manifestation of physical force. He lamented that "the reflecting botanist, knowing that his ultimate results must accord with these facts, is perplexed at feeling that he has failed to establish on independent evidence the doctrines of variation and progressive specialization, or to co-ordinate his attempts to do so with the successive discoveries in physical science."[169] Darwinism offered a way to ease this chagrin.

[166] Hooker, 1860, pp. vii–xiii. Hooker, 1867.
[167] Hooker, 1860, p. xxvi.
[168] Hooker, 1860, pp. xxv–xxvi.
[169] Hooker, 1860, p. xxiv.

Hooker now defined species as groups which "tend to transmit their characters unchanged through many generations."[170] He denied, however, that this definition would change the day-to-day work of descriptive botany. As a practical matter of descriptive science, the transmutationist differed from his creationist colleagues only in the expectation that an updated description of a contemporary species might be necessary in the far-flung future.[171] Even though diatribes against "species mongers" featured less conspicuously in this work, he nonetheless served notice that objections to the improvident creation of book species remained in full force.[172]

The most notable aspect of Hooker's essay is not the change but the continuity it represents in his thought.

> If, as I have endeavoured to show, all those attributes of organic life which are involved in the study of classification, representation, and distribution, and which are barren facts under the theory of special creations, may receive a rational explanation under another theory, it is to this latter that the naturalist should look for the means of penetrating the mystery which envelopes the history of species, holding himself ready to lay it down when it shall prove as useless for the further advance of science, as the long serviceable theory of special creations, founded on genetic resemblance, now appears to me.[173]

Hooker's conclusion that naturalists might need one day to jettison Darwin's ideas is not as extraordinary as it might seem. Even after taking his "fixed post," he never abandoned his resolution to subordinate the species question to the development of sound principles of classification and plant distribution.

Conclusion: The Master and the Stern Judge

Hooker depended upon Darwin's constant barrage of questions to keep up his interest in "philosophical botany" and to provide a respite from the "drudgery" of technical systematic botany. "All I do" on broad theoretical questions, he told Darwin in 1857, "is done in

[170] Hooker, 1860, p. iv.
[171] Hooker, 1860, p. iv.
[172] Hooker, 1860, p. xxvii.
[173] Hooker, 1860, p. xxvi. Privately, Hooker expressed the same sentiments more piquantly. See especially JDH to William Henry Harvey, 1 January 1859, Huxley, 1918, vol. 1, pp. 481-482.

correspondence to you, but for which I should soon loose sight of the whole matter."[174] Hooker clearly relied, intellectually and emotionally, on their freewheeling and open-ended philosophical discussions to avoid drowning in the minutiae of his daily work. Darwin marveled that Hooker could spend a whole day at his microscope and yet still be a fresh and pleasant companion in the evening.[175] As well he might, at least in Darwin's company. Darwin – always brimming with probing questions, perplexing problems and thought-provoking theories – reminded Hooker why he hunched day after day over microscopes and herbarium sheets. He cheerfully identified Darwin (along with Lyell) as one of his "masters."[176]

Benefits, intellectual and emotional, flowed as freely in the other direction. Darwin drew heavily on Hooker's tremendous store of recondite botanical knowledge, but these facts were never dispensed passively. Hooker, above all, presided over Darwin's developing ideas as a "stern & awful judge & sceptic."[177] He actively and persistently challenged Darwin, pushing him (willingly) to die-cast his transmutationist theory to the perspectives and practices of elite metropolitan philosophical natural history. "You may say what you like," Darwin declared to his friend, "but you will never convince me that I do not owe you *ten* times as much as you owe me."[178]

Darwin's success in convincing Hooker to accept transmutation as an explanation for systematic and biogeographical patterns is thus only part of the story, and not the most crucial part. The rest of the story involves Hooker's success in ensuring that Darwin's explanations aligned at every relevant point with the needs of descriptive and philosophical natural history as Hooker conceived them. The *Origin*, of course, touched on numerous zoological issues which fell well outside of Hooker's area of expertise and research tradition.[179] The overall tenor of the *Origin*, however, shows the ubiquitous influence of the man who Darwin considered "**by far** the most capable judge in Europe."[180] Given that Darwin discussed transmutation far more extensively with Hooker than with anyone else, it would have been remarkable had it been otherwise.

[174] JDH to CD, [11 April 1857], *CCD* 6: 369.
[175] Darwin, 1958, p. 105.
[176] JDH to B.H. Hodgson, 19 May 1849, *Hodgson*.
[177] CD to JDH, 6 May [1858], *CCD* 7: 89.
[178] CD to JDH, 31 December [1858], *CCD* 7: 231.
[179] Of these, embryology is the most important. See Richards, 1992.
[180] CD to Alfred Russel Wallace, 25 January [1859], *CCD* 7: 241.

Darwin's fingerprints on Hooker's technical botany are much fainter. An enduring monument to that work is *Genera Plantarum*, an account of the genera of all seed plants, written in collaboration with George Bentham over more than 20 years.[181] A reviewer noted that, though Hooker "is notoriously tinged with Darwinism, no evidence of it is allowed to transpire here." The defining characteristic of the work, the review found, was its familiar "'lumping' propensities."[182] The reviewer is accurate only up to a point, however. *Genera Plantarum* did not remain largely indistinguishable from pre-*Origin* systematics in spite of Hooker's (and, later, Bentham's) "notorious" Darwinism, but *because* of it. As Bentham remarked in a speech at the 1874 meeting of the British Association for the Advancement of Science, Darwin's theory gave "stability to the results of our labours" at a time when seemingly insurmountable theoretical difficulties threatened to ground progress in botany to a halt.[183] Darwin vanquished the nightmarish prospect that a conceptual revolution in biology would render decades of painstaking taxonomic work obsolete. It is the deepest mark of Darwin's influence that naturalists like Hooker and Bentham continued "to pursue their labours as at present."

The fourteen years of amicable disagreement between Darwin and Hooker on species resulted in the triumph of Darwin's theory – but a triumph won according to Hooker's rules.

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[181] Bentham and Hooker, 1862–1883.

[182] [Anonymous], 1862, p. 1103.

[183] Bentham, 1875, p. 33. For Bentham's response to Darwin's theory, see Bellon, 2003.

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