

## CHAPTER 3

### ASA GRAY AND WILLIAM ROGERS DEFEND DARWIN'S DERIVATIVE HYPOTHESIS, 1858-1860

America's expansion into the Far East in the mid-nineteenth century opened up not only foreign markets but foreign flora and fauna, to the everlasting delight of American naturalists. Commodore Matthew Perry, best known for brandishing American naval power to intimidate the Japanese emperor into signing a treaty opening Japan to American commerce in 1853, played an important role in Gray's attempt to resolve the "species problem." Although he was not keen on having scientists accompany his voyage, Perry invited James Morrow, a friend of Gray, to take charge of impressing the Japanese with American farm implements. Morrow, an amateur botanist, collected a small group of plants and sent them to Gray for classification. Gray eventually published the list in Perry's three-volume *Narrative of the expedition* in 1856.

More scientifically noteworthy was the North Pacific Exploring Expedition, under the command of Captain John Rodgers, that brought back rich natural history collections from throughout the western Pacific. The botanical collections from Japan were especially auspicious for advancing Gray's understanding of botanical geography. Charles Wright, the expedition's botanist and one of Gray's favorite botanical

collector, sent his abundant specimens to Gray for classification.<sup>1</sup> Though momentarily frustrated with yet another interruption to his yet-unfinished *Flora*, Gray soon realized that the Japanese flora was an important key to unlock the mystery of the origin and distribution of species that he had been vigorously discussing with Darwin and Hooker for the past three years. "I have only peeped into one or two parcels [of Wright's collection from Japan]; but in one I saw two things which will interest you as much as they did me. Imagine the two most characteristic possible eastern United States plants, . . . both I believe, our very species. Tell this to Dr. Hooker!"<sup>2</sup> Twenty years earlier he had briefly noticed the *Flora Japonica* in which he discovered "how many of our characteristic genera are reproduced in Japan, not to speak of striking analogous forms."<sup>3</sup> Combined with his intimate knowledge of the flora of eastern North America, Gray's work on the Japanese flora conclusively demonstrated that the two floras were not only analogically related; they were disjointed branches of a single ancient flora. He had been waiting patiently for the propitious moment to confront Agassiz. That time had now arrived.

### **The Japanese Flora and Gray's Showdown with Agassiz**

Gray was quick to act. Soon after completing his extensive taxonomic work

<sup>1</sup>Dupree, *Gray*, 208-210, 248-250.

<sup>2</sup>Gray to George Bentham, 16 November 1857, *LAG* 2: 434-435.

<sup>3</sup>Gray, "Analogy between the Flora of Japan and that of the United States," *Amer. Jour. Sci.* 2 (1846): 135-136.

on the Japanese flora in early December 1858 Gray presented a synopsis of his views on the genetic relationship between the Japanese flora and eastern North America to the Cambridge Scientific Club, a cozy gathering of Harvard colleagues. Agassiz, though present, apparently "took it very well indeed."<sup>4</sup> A few days later he sent off a comprehensive memoir to the American Academy of Arts and Sciences to be published a few months hence.<sup>5</sup> The conclusion took advantage of the lessons he had learned from Hooker, Darwin, and his own work to challenge Agassiz's idealistic conception of the origin and distribution of species.

Gray's primary strategy in his memoir was to marginalize Agassiz's views and paint them as idiosyncratic. The "natural" and "ordinary" understanding, Gray maintained, was that "each species originated in a local area, whence it has been diffused, according to circumstances, over more or less broad tracts, -- in some cases becoming widely discontinuous in area through climatic or other physical changes operating during a long period of time."<sup>6</sup> The most competent geographical botanists, including Joseph Hooker and Alphonse De Candolle, had confirmed and extended this

<sup>4</sup>Cambridge Scientific Club, Manuscript Minutes, 10 December 1858, Harvard University Archives.

<sup>5</sup>Gray, "Diagnostic Characters of New Species of Phaenogamous Plants, Collected in Japan by Charles Wright, Botanist of U.S. North Pacific Exploring Expedition. . . with Observations upon the Relations of Japanese Flora to that of North America, and of Other Parts of the Northern Temperate Zone." American Academy of Arts and Sciences. *Memoirs* 6 (1859): 377-452.

<sup>6</sup>"Japanese Flora," 445. It is important to understand that when Gray cited this understanding of the "ordinary" view of the *origin* of species he was not being disingenuous. Those who were most interested in this subject were primarily interested in whether the origin of species could be traced to specific regions. They were not so much interested in the primordial origin of organisms or life. It was only after Darwin had "foolishly" addressed the issue that the question of *origins* took on a distinctly new and more ominous meaning in public debate.

understanding. His own investigation of the Japanese flora had strengthened this claim.

So many species are now known to be common to eastern and northern Asia and eastern North America, -- some of them occurring also in northwestern America and some not, -- and so many genera are divided between these two regions, that the antecedent improbability of such occurrence is done away, and more cases of the kind may be confidently expected.<sup>7</sup>

Out of the 580 species that Gray described for Japan, 48 percent had representative species and 27 percent were identical species in Europe; 37 percent had representative species and 20 percent were identical species in Western North America; and 61 percent had representative species and 23 percent identical species in Eastern North America.<sup>8</sup> The solid empirical support for the "ordinary" view was flowing in from so many diverse locations that it was no longer necessary to fall back on J. F. Schouw's hypothesis of double or multiple creations to account for identical species in isolated locations.

The paleontological researches of Edward Forbes, botanical studies of Joseph Hooker and Alphonse De Candolle, and the numerous natural history investigations of Charles Darwin amply demonstrated, Gray contended, that our existing species of plants should be dated from the end of the tertiary period, "an immense number of years" ago. Citing extensive geological, paleontological, and zoological evidence

<sup>7</sup>"Japanese Flora," 444.

<sup>8</sup>"Japanese Flora," 437.

from Agassiz's own domain to corroborate this view, Gray concluded that

the extant vegetable kingdom has a long and eventful history, and that the explanation of apparent anomalies in the geographical distribution of species may be found in the various and prolonged climatic or other physical vicissitudes to which they have been subject in earlier times; that the occurrence of certain species, formerly supposed to be peculiar to North America, in a remote or antipodal region affords itself no presumption that they were originated there, and that the interchange of plants between eastern North America and eastern Asia is explicable upon the most natural and generally received hypothesis, . . . and is perhaps not more extensive than might be expected under the circumstances. That the interchange has mainly taken place in high northern latitudes, and that the isothermal lines have in earlier times turned northward on our eastern, and southward on our northwest coast, as they do now, are points that go far towards explaining why eastern North America, rather than Oregon and California, has been mainly concerned in this interchange, and why the temperate interchange, even with Europe, has principally taken place through Asia.<sup>9</sup>

Gray had adroitly identified his own breathtaking achievement in reconstructing the botanical history connecting eastern Asia and eastern North America with the direction of the "normal" understanding the origin and distribution of species.

The only remaining question, Gray commented, was whether the existing distribution of plants was "primordial, and therefore beyond all scientific explanation" or whether it was "to a certain extent a natural result" of physical causes. In a significant footnote Gray cited the joint Linnean Society papers of Darwin and Wallace, only recently published, as a "noteworthy attempt at a scientific solution to the problem" that would play a prominent role in all subsequent discussions of the origin and distribution of species. Declaring his own independence from them, Gray announced that he was

<sup>9</sup>"Japanese Flora," 449.

already disposed, on these and other grounds, to admit that what are termed closed related species may in many cases be lineal descendants from a pristine stock, just as domesticated races are; or, in other words that the limits of occasional variation in species (if by them we mean primordial forms) are wider than is generally supposed, and that derivative forms when segregated may be as constantly reproduced as their originals.<sup>10</sup>

Gray was the first significant naturalist to publicly declare his support for the Darwin-Wallace position. Furthermore, he had persuasively argued that the "normal" understanding of the origin and distribution of species, held by such eminent authorities as Forbes, Hooker, De Candolle, and even Darwin and Wallace, was moving irresistibly toward some form of a derivative hypothesis. He counted himself among them.

Gray threw Agassiz sharply on the defensive. While even the cautious Alphonse de Candolle had quietly abandoned Schouw's questionable hypothesis of multiple creations, Agassiz still stubbornly clung to it in the face of mounting difficulties, both scientific and theological. He continued to believe that the Divine Will created "each species . . . where it now occurs, probably in as great a number of individuals occupying as large an area, and generally the same area, or the same discontinuous areas as at the present time."<sup>11</sup> Not only did the growing body of

<sup>10</sup>"Japanese Flora," 443. Dupree dates this added footnote to not later than 25 April 1859 in "The First Darwinian Debate in America: Gray Versus Agassiz." *Daedalus* 88 (1959): 567. Dupree overstates the meaning of this statement by saying that it "reflects a clear understanding of the workings of evolution." *Gray*, 259. It does not. Although he was by this time aware of Darwin's transmutation hypothesis, Gray was most concerned at this stage to establish the local origin of species and that their variation was wider than formerly supposed, not their evolution. Such talk of "evolution" would have smacked of the despised doctrines of *The Vestiges* and would have needlessly side-tracked his debate with Agassiz. He was too smart a strategist for that to happen.

<sup>11</sup>"Japanese Flora," 445.

empirical studies contradict this view, but it "removed the whole question out of the field of inductive science" by referring the whole question to Divine Will.

Gray maintained that such a mock piety adopted an unnecessarily narrow view of the Deity. Stepping on slippery ground, Gray cited Maupertuis's principle "that it is inconsistent with our idea of Divine wisdom that the Creator should use more power than was necessary to accomplish a given end." Benjamin Peirce, the well-respected Harvard mathematician and friend of Agassiz, had already demonstrated the validity of this principle for all mechanical operations. There were thus good grounds for insisting that it could also be applied "to the creation of beings endowed with such enormous multiplying power, and such means and facilities for dissemination, as most plants and animals." This being true, Gray asked rhetorically, "why then should we suppose the Creator to do that supernaturally which would be naturally effected by the very instrumentalities which he had set in motion?" The answer was obvious. The "normal" understanding of the origin and distribution of species was fully consistent with inductive science and the most exalted concept of the Creator.<sup>12</sup>

Having carefully prepared his memoir, Gray was ready for a full-scale debate with Agassiz at the January meeting of the American Academy held at the home of his father-in-law, Charles Loring. Hoping for a dramatic confrontation with Agassiz, Gray asked his mentor and friend, John Torrey, to come up from New York to attend the meeting.

<sup>12</sup>"Japanese Flora," 445.

I am going to hold forth for nearly an hour, upon Japan Botany in its relation to our and the rest of the northern temp. zones, and knock out the underpinnings of Agassiz' theories about species and their origin -- show, from the very facts that stumbled De Candolle the high probability of single and local creation of species, -- turning some of Agassiz' own guns against him.<sup>13</sup>

Unfortunately Torrey was unable to attend. The anticipated drama never materialized.

Gray opened the debate by summarizing his views on "the remarkable interchange between the floras of Eastern North America and Eastern Asia," noting the remarkable "number of congeneric, of closely representative, and of identical species in the two floras." Such a "dispersion of species" could best be explained by "a former homogeneousness of the temperate American and East-Asian floras." The critical question in light of these findings was

whether each species originated in one local area, whence it has spread, as circumstances permitted, over more or less broad tracts, in some cases becoming discontinuous in area through changes in climate or other physical conditions operating during a long period of time; or, whether each species originated where it now occurs, probably in as great a number of individuals occupying as large an area, and generally the same area, or even the same discontinuous areas, as at the present time. The latter is understood to be the view of Professor Agassiz.<sup>14</sup>

As he had shown in his memoir, the former view was the "natural" and most widely held view among naturalists. The latter view was peculiar only to Agassiz.

Gray zeroed in on three grave weaknesses of Agassiz's approach to this issue

<sup>13</sup>Gray to Torrey, 7 January 1859, *LLAG* 2: 450.

<sup>14</sup>American Academy of Arts and Sciences. *Proc.* 4 (1857-1860): 132; Merrily Kodis Swoboda notes that Gray considerably expanded the comments he made at these meetings for the published proceedings, unlike Agassiz. "The American Rhetorical Career of Louis Agassiz: A Case Study of Transformations in American Science, 1846-1860" (Ph.D. diss., University of Pittsburgh, 1977), 128-130; cf. American Academy of Arts and Sciences, Manuscript Minutes, 11 January 1859, Harvard University Archives.



in the published proceedings, considerably amplifying the criticisms he offered in his memoir and at the meeting.

1. That it offers no *scientific* explanation of the present distribution of species over the globe; but simply supersedes explanation, by affirming, that as things now are, so they were at the beginning; whereas the facts of the case -- often very peculiar -- appear to demand from science something more than a direct reference of the phenomena as they are to the Divine will.
2. that the idea of the descent of all similar or conspecific individuals from a common stock is so natural, and so inevitably suggested by common observation, that it must needs be first tried upon the problem; and if the trial be satisfactory, its adoption would follow as a matter of course.
3. That, since it is conceded that the present era of the world is of extremely long duration, and since it is most probable, not to say certain, that the existing species of plants of the region in question, or a part of them, are of high antiquity, dating back to the post-tertiary, or even to the later tertiary epoch, -- and therefore must have been subject to great climatic changes, accompanied or caused by no inconsiderable changes in the relative extent and configuration of the land, -- the objections formerly raised against such wide dispersion of species lose most of their force. And the explanation of such anomalies in the actual distribution of species is to be sought in the vicissitudes to which the species must have been subject in their earlier days.<sup>15</sup>

Gray's long discussions with Hooker and Darwin on these same matters, seasoned by his own immense botanical knowledge and penchant for theorizing, was beginning to bear fruit.

Gray's comments caught Agassiz, normally not at a loss for words, completely off guard and unprepared to respond, even when given time to prepare his remarks for the published proceedings. He was at the time consumed with raising nearly \$250,000 for his Museum of Comparative Zoology at Harvard, overseeing its construction, and satisfying his numerous speaking engagements. Besides, he had already definitively

<sup>15</sup>AAAS. *Proc.* 4 (1857-1860): 132-133.

answered these questions in the first volume of his immensely popular *Contributions to the Natural History of the United States* (1857).<sup>16</sup>

Agassiz courteously "expressed great interest in the statements and opinions of Prof. Gray, and said he regarded them as of very great importance with reference to the question of the common origin of species." At the same time his views, based on his close observations of the animal kingdom, differed substantially from those of Gray.<sup>17</sup> He noted that he had already discussed a similar resemblance between the fauna of eastern North America and eastern Asia in his *Contributions*. He insisted that the similarity was due, not to climatic or other physical changes, but was "a primitive adaptation of organic types to similar corresponding physical features, which have remained respectively unchanged since the first introduction upon each of these organisms," even accepting "the immensely long duration" of the post-tertiary period. Echoing a position close to Gray's, Agassiz believed "that the warfare which so many species wage upon others was in itself an insuperable objection to the assumption that any one species could have originated in a single pair."<sup>18</sup> Agassiz was not backing down, but he could only weakly reiterate his own well-defined position.

<sup>16</sup>Lurie, *Agassiz*, 225-234.

<sup>17</sup>Swoboda, "Louis Agassiz," 129; AAAS. *Proc.* 4 (1857-1860): 133.

<sup>18</sup>AAAS. *Proc.* 4 (1857-1860): 133-134. The image of warfare and struggle among species was known before *The Origin* popularized it. As we will see below, Gray also doubted whether a single pair could account for the origination of species, but professed that settling that question was far less important than answering whether a *species* had originated, by whatever means, in a local area. This question was a major challenge that Darwin addressed with a variety of mechanisms, from isolation to divergence. Thus, Agassiz's comment should not be read as coming "within a hair's breadth of the principle of natural selection," as Dupree reads it in "The First Darwinian Debate in America," 564. The image of warfare connoted very different perceptions for them.

In February, at the first of three supplemental monthly meetings called at the urging of Agassiz, Gray discussed the "vicissitudes to which our extant vegetation must have been exposed in earlier times." He was fortunately able to draw on the work of another member of his "accurate" botanical collectors, Leo Lesquereux, to supplement the material on this point in his memoir. Lesquereux's recently concluded study of vegetable fossils on the banks of the Mississippi near Columbus, Kentucky had confirmed that "some of our species of plants were in existence anterior to the drift or glacial epoch, and even in the later tertiary period." Virtually all of the fossilized tree leaves he was able to identify belonged to existing species of trees. His exploration of an older deposit in Tennessee, perhaps as old as the lower or middle pliocene, turned up fossilized leaves that could be traced to existing genera in the area: 40 percent of the leaves could actually be identified as belonging to existing species. All of this paleontological evidence, Gray concluded, "conspired to render it in the highest degree probable . . . that at least a considerable portion of our temperate flora was in existence in the early post-tertiary, and even in the later tertiary times."<sup>19</sup>

Gray assured his audience that his discussion of the "vicissitudes" to which organisms had been subjected did not undermine "the idea of a plan in creation" and the adaptation of organisms to their physical conditions. The only way that it could was if one went "to the extreme of implying that the present state of things so strictly

<sup>19</sup>AAAS. *Proc.* 4 (1857-1860): 171-173.

represents the primitive condition as to exclude second causes, and to deny that physical influences, known to have been in operation, should have produced their natural effects in former times as well as now." Gray's thinly veiled reference to Agassiz's "extreme views" could not pass unnoticed.<sup>20</sup>

Gray acknowledged, in conclusion, that speculating on the origin of species took us "beyond the region of induction" where only analogies and probabilities could guide us. This being true, scientific progress was only possible when those with clashing perspectives cooperated in the search for truth. Agassiz was welcome in that quest. As for his own position, Gray held to the

local origination of species; not origination in single individuals or single pairs, -- which might or might not be the case in different species. The improbability of single origin appeared to him to be great in the lower grades of animals; the probability of it greater and greater as we rise in the scale of being. But the local origination of each species appeared to him not only the natural hypothesis to begin with, . . . but also the one which, on applying it to the case in hand, he thought best adapted to explain the actual distribution of plants.<sup>21</sup>

Agassiz, by now embroiled in the ugly Marcou affair with his fellow-editor at the *Journal of American Science*, James Dwight Dana, could only reiterate his claim that climate had nothing to do with the present distribution of animals.<sup>22</sup> How else

<sup>20</sup>AAAS. *Proc.* 4 (1857-1860): 175.

<sup>21</sup>AAAS. *Proc.* 4 (1857-1860): 176-177.

<sup>22</sup>In the 1850s Agassiz had a shameful and highhanded dispute with James Dwight Dana, the senior editor of *The American Journal of Science*, over the scientific accuracy of several books of geological maps of the United States that had been compiled by Jules Marcou, his young admirer and fellow Swiss. All of the reviewers panned Marcou for his ignorance and baseless criticisms of two prominent American geologists, James Hall and his friend collaborator, William Barton Rogers. Agassiz defended Marcou against the critics without even reading the books on the grounds of Marcou's excellent past scholarship, Lurie, *Agassiz*, 272-276.

explain the remarkably dissimilar fauna living in nearly identical climates in both hemispheres? The only explanation was that "animals were primarily distributed over the surface according to a plan hardly intelligible as yet to us, but independent of climatic influences. This plan . . . included the preparation for the earth's surface and the various external conditions of their existence for its inhabitants."<sup>23</sup>

As a good debater Agassiz tweaked Gray's botanical expertise to confirm this point. Gray had relied on the plant identifications taken by Des Hayes and Lyell to show that identical species existed in both the tertiary epoch and the present, thus confirming continuity. Of course, if true, this would count heavily against Agassiz's position. Agassiz, perhaps with a glint in his eye, cautioned that "if Professor Gray were to exercise the same critical judgment upon the fossil Flora which he does with reference to the existing Flora, he would find differences between the species of the two epochs similar to those found in the animal world." Agassiz concluded that "he believed that the present races of animals were originally created on the earth in about the same proportionate numbers as they are found to have at the present time, and in about the same localities as those they now occupy." In any case our botanical and zoological knowledge was too deficient at the present to accept Gray's confident claims about the continuity of the tertiary and the present floras.<sup>24</sup>

It was apparent by the March special meeting that, despite Agassiz's weak replies to Gray, the members did not understand Gray's argument for the local origin

<sup>23</sup>AAAS. *Proc.* 4 (1857-1860): 177-78.

<sup>24</sup>AAAS. *Proc.* 4 (1857-1860): 178-79.

and subsequent diffusion of species. The discussion at that meeting centered on Agassiz's presentation on "Classification of the Animal Kingdom" in the Paleozoic period, hardly a topic that could work to Gray's advantage. Members offered numerous examples of some species continuing unchanged over thousands of years and others inhabiting the same locations now as in the Paleozoic period. Charles Pickering, a veteran of the Wilkes Expedition, explained the presence of unique species on isolated mountain peaks and islands as an indication of a recent "exercise of creative power." Traditional assumptions remained unshaken.

Gray was reduced to quibbling with Agassiz's earlier claim that the present distribution pattern of animals had nothing to do with climate or other physical conditions. Drawing on his discussions with Hooker, he countered that the uniformity of species in the Arctic regions in both hemispheres and the dissimilarity of species in the tropics could be explained by contrasting geography of the two regions; where the land masses were contiguous, as in the two poles, the species were more likely to be uniform; where they were separated, as in the tropics, they were more likely to be dissimilar. Thus, "the actual distribution was *as if* species had spread over as much of the earth's surface as they had a chance to reach and occupy." Of course, this made no dent in Agassiz's assumptions since he believed that species had been created *in situ*.

Gray then tried to explain the unique presence of the western North American flora between Japan and eastern North American flora by hypothesizing a northern shift in isothermals that isolated the western flora of Oregon and California. Such an

explanation no doubt sounded rather esoteric to those unable to understand Gray's earlier explanations of continuity. Perfunctory meetings in April and May added nothing of substance and changed no minds. Gray's best efforts to communicate the dramatic implications of his study of the Japanese flora for understanding the origin and distribution of species fell on uncomprehending ears at the American Academy.

Gray was exasperated with Agassiz's lackluster performance in their debates. Agassiz spoke *ex tempore* at both the January and February meetings. After each meeting Gray gave Agassiz a manuscript of his own remarks so that he could write out his response for the official *Proceedings*. It was important, Gray felt, that Agassiz be compelled to put his comments down in "black and white, that he might stand fairly committed to the subject." Each time Agassiz failed to keep his promise to write out his remarks, claiming overwork and bad headaches. He simply returned the recording secretary's abstract of his remarks to be published in the *Proceedings* as his own. The only consolation Gray could muster was that he "had taken the measure" of Agassiz's views on the multiple creation of species during an extremely long "present era." It is doubtful whether any of the Academy members would have agreed with Gray's assessment.<sup>25</sup>

To make matters even worse, though lauding the essay, Hooker severely chastised him for several egregious mistakes and even seemed to defend Agassiz. The geological terms of "present era," "post-tertiary," and "later tertiary" were hopelessly

<sup>25</sup>Gray to Hooker, 30 May 1859, Kew, APS.

vague and useless for botanists, Hooker counseled. Botanists needed a "tangible & practical" point in time, like the glacial epoch. All of Gray's talk about climatic and geographical changes and "vicissitudes" were equally meaningless. Hooker felt that Gray had seriously over-reached in this regard. "All that you can say is that the explanations of anomalies [in distribution] are to be sought in the environment & physical conditions to which species have been exposed since their creation (whether by fiat or by variation)."<sup>26</sup>

Hooker, then laboring on the bewildering variety in the southern flora, "hated the word homogenous as applied to Flora."<sup>27</sup> Gray should have known better than to declare the identity of tertiary and modern plant species since "you cannot distinguish the . . . discolored cast in carbon of one surface of the fossil leaf from the leaf of the quickened living existing species." Any paleontologist who "united extinct species with existing, would be excommunicated & this applies to every case of *identification* of tertiary & post tertiary plants with which I am acquainted. Common sense & common experience of existing Botany contradicts with assumptions." This was not to say, Hooker added, that existing species were not lineal descendants of the fossilized species, it was just that their relationship tells equally for both special creation and creation by variation. And where did he get his geological information about two warm periods anyway? Agassiz should have "annihilated you with your own weapon" (Maupertuis' dictum of "least action") since Gray believed in both special

<sup>26</sup>Hooker to Gray, 30 April 1859, GHA.

<sup>27</sup>Ibid.



creation and even multiple creation of some species. Agassiz should have come back at him, Hooker insisted, with the question: "if multiple [creations] in numbers in space, why not in time?" Gray did not realize it, Hooker warned, but "a bite of fruit in your soft sweet cake of special creation at which the more you snap the more teeth you will break." These biting criticisms from his close friend were far more severe than any that Agassiz was able to muster. Hooker could not even be calmed with Gray's lame explanations or convinced by the authority of James Dwight Dana's geological hypotheses.<sup>28</sup>

The response of colleagues at the Academy of Natural Sciences and the American Philosophical Society in Philadelphia to his debate with Agassiz was equally disappointing to Gray. Elias Durand, a fellow botanist, wrote in April to say that he and others had been following his debate with Agassiz "on the two hypotheses of a single, and of a multiple original vegetable type," with great interest. He regretted to say that, "although I have the greatest respect for your scientific opinions, yet, in this instance, I have not been able to side with you." After carefully preparing a long paper on their debate for the American Philosophical Society and his objections to Gray's views, Durand demurred due to poor "powers of elocution and want of spunk." Perhaps most disappointing to Gray, Durand reported that Joseph Leidy, the

<sup>28</sup>Hooker to Gray, summer 1859, two undated fragments, dated by internal evidence, GHA. Dana also objected to the vague term "present era." Apparently Gray had asked Dana to look over his memoir before publication. Dana advised Gray to drop the term "because the present era means nothing special when restricted to the period since the creation of man; and with regard to an immense period since that time it is not the place to make any statements, or geology has nothing definite to say on the subject." Dana to Gray, undated fragment; internal evidence suggests it was written in the spring 1859, GHA.

well-known paleontologist then teaching anatomy at the University of Pennsylvania, shared his views. Despite his initial disagreement Durand wished Gray to keep him informed, especially if he had any additional information that would support his hypothesis of a single creation. In the end, echoing the sentiments of many naturalists on these questions, Durand counseled Gray that "we shall not see clear in those mysteries before our poor souls have past to the other world."<sup>29</sup>

James Dwight Dana, who supplied the geological information to Gray, believed that Gray had "made out a strong case" for his position. At the same time, he was undecided on the "main point -- whether the same species had been created in more than one country." Dana doubtless felt trapped in this circumstance. After all, he, a man of irenic disposition, was currently embroiled in a battle for his reputation with Agassiz in the Marcou affair. How could he risk estranging either Agassiz or Gray by taking sides in their debate? His answer was to say that Agassiz allowed too little attention to migration and Gray too much. That was fair enough. That Dana showed even this little openness to Gray's argument for local origin of species was a testimony to his scientific integrity since he was on record supporting the possibility of plural origins.

While he was undecided on the point at issue between Gray and Agassiz, Dana did have some definite ideas about Darwin's Linnean Society paper that bore on Gray's argument.

<sup>29</sup>Durand to Gray, 16 April 1859, GHA; Robert V. Bruce, *The Launching of Modern American Science, 1846-1876* (New York: Alfred A. Knopf, 1987), 48-49.

As an article on the variation of species it is good; but when it is turned into an argument on the origin of species it is in my view defective. When his variations have gone so far that mixture produces only infertile hybrids, then I would admit that he has touched the subject of the origin of species; but not till then.

Later that year Dana suffered a serious emotional and physical breakdown. It is unfortunate that he was unable to add his voice to the forthcoming debate on *The Origin*.<sup>30</sup>

Gray fared no better at the May meeting of the Cambridge Scientific Club in his own home. He wanted to introduce his Harvard colleagues to Darwin's views on the origin of species "to see how it would strike a dozen people of varied minds and habits of thought." Well, there was the other "malicious" motive, Gray confessed to Hooker, that his talk was designed to "vex the soul of Agassiz with views so diametrically opposed to all his pet notions." Apparently Agassiz was "worried a good deal" since he believed that even the geographical distribution of species was a supernatural act. That being true, Gray noted to Hooker, with tongue in cheek, "your new heretical school are [*sic*] undertaking to bring even the *origin* of species within the domain of cause and effect." At the same time Gray was not as brave in standing alone for Darwin as he put on in public. He anxiously asked Hooker to send his essay on the Tasmanian flora so that he could "see how far you carry the hypothesis and how you support it."<sup>31</sup>

<sup>30</sup>Dana to Gray, undated; internal evidence suggests late winter, early spring 1859, GHA.

<sup>31</sup>Gray to Hooker, 16 May 1859, Kew, APS. Gray had no idea that his tongue-in-cheek comment would actually become the title of Darwin's book, realizing a gnawing doubt in his own mind about the implications of this "heretical" view.

Judging by the response, Gray's talk sparked no fireworks and little comprehension of Darwin's theory. E. S. Dixwell scribbled that Gray read from some papers on "the sporting of species & varieties -- approaching Lamarck's theory." Benjamin Peirce, the mathematician, noted that Gray had discussed "a Mr. Darwin's and a Mr. Wallace's speculations as to the possible changes of a species under the influence of external agencies."<sup>32</sup> Even the best prepared minds of the Harvard faculty failed to appreciate the gravity of the moment.

R. W. Church, an Anglican priest whom Gray had met on his earlier trip to England, provided a glimmer of hope that summer that his debate with Agassiz had made some difference in people's minds. Gray had sent Church the extracts from the American Academy *Proceedings* covering his debate with Agassiz. Though he professed his own ignorance of the matters at hand, Church declared that

it certainly strikes me that your view, as a theory to be tried, is the one to take, instead of Agassiz's, which simply amounts to taking species as they are found, without any inquiry as to their previous possible history. With the indications of affinities and vicissitudes in the history of species which there are, it is more philosophical to see if they bear being traced out into a simple connection with each other.

He seconded Hooker's doubts about the appropriateness of Gray's quotation of Maupertuis against Agassiz, declaring that he was more impressed with the waste of nature than its economy. The multitude of "unimpregnated ova" seemed to serve no "direct purpose, though of course they may [have] some other [purpose]." Yet, Church exclaimed, "the strangeness of creation, which in many distant centres or one,

<sup>32</sup>Cambridge Scientific Club, Records, 12 May 1859, Harvard University Archives; Benjamin Peirce to Mrs. Benjamin Peirce, 13 May 1859; quoted in Dupree, *Gray*, 259.

whether by an individual or pair, or by a whole family at once, seems equally overwhelming to our present faculties and thoughts." Gray could at least take comfort in the fact that Church understood his argument more clearly than had the American Academy.<sup>33</sup>

Gray's declared intention of "knocking out the underpinnings of Agassiz's theories about species and their origin" had apparently come to naught in his initial showdown with Agassiz on the merits of Darwin's derivative hypothesis during the spring of 1859. It was not for want of effort, preparation, or eloquence. Yet it was clear from the dazed response of his audience that they were so deeply embedded in the traditional understanding of species, ably articulated by Agassiz, and so unfamiliar with the mounting empirical evidence then challenging it that they could not comprehend even Gray's conservative interpretation of Darwin's derivative hypothesis. His parallel correspondence with Hooker on these same issues during the spring was equally disappointing, if not alarming.

### **Gray's Sharp Divergence from Hooker on the Origin of Species**

By December 1858 Hooker declared that his work on the Australian botany had "prepared [him] for the admission of Darwin's views far more effectively than any arguments." He was particularly struck by the fact that southwestern Australia "teems with new species," and yet has no "new order or new group." The proliferation of new specific forms of well-known genera seemed to be

<sup>33</sup>Church to Gray, 5 July 1859, Mary C. Church, ed., *Life and Letters of Dean Church* (New York: Macmillan, 1894), 181-82.

outcreating creation! -- but this is a question of probability versus improbability, not of fact versus fiction. What strikes me is the rapid succession of specific peculiarity in advancing from N. E. to S. W. in Australia. Hitherto we have regarded change in species as the concomitant of physical change; & have assumed the species to have been created adapted to physical change. -- Here we have the specific change complete, without the Physical change -- this does not throw much light on the question, but it places the argument in a different point of view.<sup>34</sup>

Hooker had by this time gone further in Darwin's direction than made Gray comfortable. He observed that Hooker's "Essay on Fl. Tasmania is going to interest me very much, despite its very speculative character. You are gradually leading me to feel an interest in such matters, -- rather against my will. But I cannot help you, even criticize or carp at you yet."<sup>35</sup>

What Hooker was compelled to admit in his next letters staggered Gray even more. Hooker declared that "I can no longer believe that species in general are immutable -- some may be it is true for indefinite ages, &, (however created is another matter) I cannot but think that genera are only varieties of a higher order with the linking forms lost. But the subject of vegetable types or orders or classes I do not enter, because we have no facts whatever." For the same reason he would not discuss "progression." However, he had to consider the fact that so many genera "consist of two groups of species, one group of species all evidently & bona fide distinct. -- The other group of species all varying so greatly that the differences between the varieties, of many are often greater than between the species of group first." It appeared that the

<sup>34</sup>Hooker to Gray, 8 December 1858, GHA.

<sup>35</sup>Gray to Hooker, 17 January 1858, Kew, APS.

intermediate forms in this group had been destroyed. Since species and their varieties remained "more or less restricted in area," it was most conceivable that "the facilities for such an operation are great -- add the intensification of characters by selection and the forms of growth of the several varieties, & we have all the elements of species. All this compared with gardening operations."<sup>36</sup>

His complete conversion to the doctrine of creation by variation amazed Hooker. No sooner was he convinced that "creation by variation" was the soundest choice, than he felt as strongly about it as he had earlier felt about the "original creation dogma." He attributed this to the fact that creation by variation was capable of some empirical support "whereas the creation doctrine leaves you in a dead lock. . . . It is a preconceived idea & the worst logical choice of the two." It was most likely "adopted by every one before they can have means of judging for themselves."<sup>37</sup>

How discomfoting it must have been for Gray in May when, fresh from playing the radical challenger of traditional notions of species in Cambridge, he had now to ward off Hooker's even bolder strides in Darwin's direction. Gray agreed that Hooker's conclusions in his Tasmanian essay

appear to show -- what you have maintained very strongly on general grounds of observation -- that species vary more widely, and are dispersed more widely than was thought, that dispersed do, as they naturally should on these grounds, tend to extreme of type, -- and that derivative types may be (as I have long maintained) -- & under favoring circumstances would be, as true and as likely to continue so as the original stock. That many of our species are not original ancestors but derivative forms, and so, many fossil species thought to be distinct are the

<sup>36</sup>Hooker to Gray, 10 February 1859, GHA.

<sup>37</sup>Hooker to Gray, 15 April 1859, GHA.

ancestors of existing species they resemble.

This was as much as Gray had always been willing to accept; he did not have to swallow Darwin's derivative hypothesis to go this far. All of his traditional categories could easily accommodate these "very probable" inferences.

It was a far different matter when it came to what Gray perceived to be Hooker's illegitimate argument by analogy for the derivation of the higher groups.

So much for what may be inferred. But, on these grounds do you infer that because some species of a genus are derivative forms, therefore all are? . . . That all species of the same genus are branches of one stock? and so all genera of the same tribe? -- all tribes of the same order? That would be false reasoning. And as every step leads to a greater improbability, I want to see: 1: what you think the human facts go to prove. & 2d -- what may be probably inferred from them after freely granting every thing that may be directly inferred. I expect to find your published essay far more guarded and less bold than your letters.

As strongly as Gray had supported Hooker's claims about *species*, he never accepted the argument that, by analogy, genera and the higher orders were derived in the same way that species were. As he had earlier stated to Darwin and Hooker, such reasoning was using a false analogy and would lead to all sorts of improbabilities. Now he added the further barrier to a "progression" theory: the gulf between humans and animals.

Gray did allow that Hooker had some strong points in his favor of his development hypothesis.

On your side you have the important fact that in a general way, the distribution of genera follows that of species and you may ask those who stick at your hypothesis to explain in any way other way the actual grouping of congeneric and coordinate species on the earth's surface -- and so long as you offer a scientific solution of the problem, and your opponent cannot, you have a certain advantage. . . . I can



adopt your views when you establish them, and meanwhile can judge them without much prejudice.<sup>38</sup>

Perhaps Gray had a bit more prejudice against the "coming view" than he admitted here.

It did not take the feisty Hooker long to respond to Gray's challenge. Hooker wondered why Gray was seeking to have the development hypothesis *demonstrated*. After all neither of them ever "supposed the creation hypoth. to be demonstrated." That being the case it was simply a matter of fairness to hold the development hypothesis to the same test of its validity. He was amused that Darwin "compares my old attempts at stopping short at greater variation of species, with unitarianism -- i.e. a 'feather bed to catch a falling Christian.'" To Gray's charge that "because some species are derivative all must be, -- certainly not; though if I did that not be 'false reasoning,' only hasty & inconclusive reasoning, leading perhaps to false results." Hooker then got to the nub of the persuasive power of the development hypothesis and the grounds of its rational validity.<sup>39</sup>

I only say if some are demonstrated to be so, & you cannot distinguish these from any others by any logical process, it is then legitimate to speculate on the possibility of all being so -- & if you can further demonstrate as Darwin has done that you can correlate the life phenomena of all species with the facts of any few, . . . & further show that the effective causes of these phenomena are universally acting . . . why then I say you have a foundation for a hypothesis of creation by variation against which that of species creation has nothing whatever to urge but the naked fact of genetic resemblance & chap. 1 of Genesis.

Although the development hypothesis could not be demonstrated any more than could

<sup>38</sup>Gray to Hooker, 16 May 1859, Kew, APS.

<sup>39</sup>Hooker to Gray, 31 May 1859, GHA.

"species creation," its major advantage was that it could appeal to legitimate scientific facts while "species creation" could only appeal to Genesis 1 and the "naked fact of genetic resemblance." There was no doubt in Hooker's mind which one was the most consistent with his field experience.

Hooker was particularly irked that Gray should consider his position "hasty." While he appreciated the caution, he defended his right to make up his mind on a matter that he had known about for the past fifteen years.

I can truly say that the last 10 years of my studies has not added to or taken away one real effective acquaintance of my own for or against creation or development. -- I have waited long enough for Darwin to publish, & I am sure you must have seen & felt that I have been very shaky on my [views?] ever since I came home from India. As it is, all I want was to wipe my hands of the whole affair. -- I am sick of it -- & Darwin ill from it.<sup>40</sup>

If Gray would have paid careful attention to all of their previous correspondence, Hooker urged, he would have clearly seen that his views were moving steadily toward the development hypothesis rather than building any animus against the creation hypothesis.

Hooker allowed Gray's charge that his views were "hasty" to fester for a month and then exploded. "I protest vehemently against the tone in which you take up my views on representation, creation, & characterizing them as hasty, -- jumping at conclusions, novel, -- half considered & so forth." He then carefully rehearsed his very long history of twenty-four years given over to considering every facet of the "species problem." The origin of species had always intrigued him. During the first

<sup>40</sup>Hooker to Gray, 31 May 1859, GHA.

ten of those years, Hooker stressed, he was "a disciple & thorough believer in special creations -- & not a blind believer,

but as one convinced by the "facts bearing on the subject. . . . After 15 years I found that my accumulated facts did not give me any satisfaction & Darwin's theory of variation and selection was privately communicated to me: -- for 10 more I went on doubting as I [worked?], & expressing myself doubtfully as I wrote, & if after good 20 years of active exertion I profess myself a convert, -- to be doctrine neither less or more incapable of proof than the opposite with which I started, but demanding life miracle, & opening up a field for far wider generalization, it is surely not through haste, or want of full consideration. I defy any mind to hold such a balance honestly & to work at original materials at the same time.

It was on the basis of this long history that Hooker constructed his own theory of "centrifugal variation," the view that species had the inherent tendency to "depart further from type & never to return to it." The path of his arriving at this idea was so tortured that he could not even give Gray a satisfactory account of it. Hooker did, however, "think the idea is, if good, pregnant with results, & I therefore enunciate it - - not claiming for it assent without investigation, but courting investigation for it. -- I see it in the human race -- if true it appears to me to oppose the idea of the same species originating in separate localities, & to account for representative species both occurring & yet being same."<sup>41</sup>

Hooker laid down a bold challenge to Gray to further strengthen and justify his acceptance of the development hypothesis. What would Gray's position be if he started his botanical career all over again,

with nothing to reason from but your past life's experiences of the horrible

<sup>41</sup>Hooker was fairly obstinate in maintaining "centrifugal variation" against Darwin's protest.

variability of species, of the limitation of varieties, of the power of intensifying the characters by breeding of the great age of vegetable forms, of the parallels of attributes in varieties, species, genera: of the difficulty of limiting genera & & &. -- I say begin think, fresh in thought & ripe in experience, & which then would be the jumped-at conclusion? What sp. were creatures? or what they were modulated variations?

Gray had even admitted, Hooker pointed out, that he could make a better case for "creation by variation" than Gray could of "species made by creative effort." Hooker was adamant that Gray understand that he had slowly and painstakingly arrived at the development hypothesis on the basis of just these kinds of facts. He had not recklessly adopted them on the basis of wild speculations and shaky evidence. Perhaps, Hooker coyly concluded, Gray was, after all, a catastrophist and a believer in absolute certainties "in politics, religion & all else."<sup>42</sup>

Hooker's preparation of his "Essay on the Flora of Tasmania" cost him the "utmost pain & thought" in his attempt "to be impartially honest in stating my arguments & conclusions." But as he had publicly espoused the creation doctrine he was duty bound to publicly disavow it and adopt the view of Darwin as the most probable, no matter the desertion of friends and the "cold shoulder of the orthodox."

Gray had to understand, Hooker pleaded, that

I adopt no creed in the matter & accept no conclusion as ultimate. I am not an adherent to the doctrine of variation & I only give my conviction, that in the present state of view it is more consonant with all the facts & laws of biology, & with the laws of psychology & physics to assume that the development of plants has been in a continuous series, not in one composed of as many special developments as there are species. Be the facts as they may, I derive great comfort from the adoption of the alternative [to special creation]. I see or fancy I

<sup>42</sup>Hooker to Gray, 29 June 1859, GHA.

see clearer into botany I am less hampered with fears as to what will become of my species & genera.<sup>43</sup>

His patience with Gray's continued belief in special creation and concern that Darwin's theory was incompatible with belief in an intelligent Creator was wearing thin. How could Gray assert that the supernatural creation of species was a "simple postulate" that "explains the facts" most naturally? Hooker emphatically denied that: that was the fundamental question being examined. How was Gray's belief in special creation consistent with his rejection of spontaneous generation? How could Gray continue to maintain that creation by variation was incompatible with belief in an intelligent Creator? How did Gray know what methods were compatible with that belief?

Hooker finally laid out the fundamental area of disagreement between Gray and himself.

In all this discussion there is [one] matter of real importance that I would regret to think we differed on. . . . If you feel that even religious views are compromised by such a doctrine as what I prefer to the specific creationist: or that they would be by that of progressive development from the organic to amoeba & from amoeba to man--then we go to work with fundamentally differently constituted minds & can never agree. Between . . . the phenomena of nature . . . [and] God, there is a gulf fixed that man may never fathom or perhaps know more of than he does now. . . . My ideas of the attributes of the deity are not affected by my estimate of his modus operandi [sic] which must change in kind with every discovery of science. I for one should be prepared to receive a doctrine that correlated the organic & inorganic under one simple law of evolution without a disturbance of my religious ideas. Life is neither more nor less . . . a mystery than heat, light; true chemical affinity & why we should regard all biological phenomena in one relation to our attributes of the almighty [sic], & then of inorganic life in matter, I cannot see or feel. Pray do not allude

<sup>43</sup>Hooker to Gray, 2 August 1859, GHA.

to these subjects in any letter to me that may come to my Father during my absence, as these matters only disturb & vex him -- but I really should like to have your opinion on X.[Christianity] & this only to know whether you are hampered by it or not.-- perhaps all turns on your idea of the word 'Being' & on that I confess, that metaphysicians & theologians have given me no satisfaction whatever.<sup>44</sup>

Hooker was simply mystified that Darwin's hypothesis on the origin of species had the remotest bearing on whether one was a Christian or not. Hooker was finished with the discussion. He never discussed the theological bearing of Darwin's theory with Gray again.

Gray, on the other hand, became even more agitated by Hooker's position. He voiced the depth of his anxiety to Hooker less than a month before the *Origin* was published. He was quite prepared to "believe that any particular cognate species so called originated by variation, wherever you say so. But [I] still stick at the progression, and the development of the Vegetable kingdom by variation from a primordial something." He was, he admitted, "staggered" by Hooker's philosophy. Why would it not be conceivable, Gray wondered, for species to have been created "in time" at a "fitting time," whenever that happened to be? If species were not created "in time," that would mean that "the present series of causes & effects [were] conceived as eternal. . . . That in my mind must be pure atheism." There was already a strong tendency in physical science to push back the series of effects ad infinitum since it deals only with effects and not efficient causes. Gray was most concerned with how Hooker intended to "connect the philosophy of religion with the

<sup>44</sup>Hooker to Gray, 5 August 1859, GHA.

philosophy of your science" when he pushed his "progression theory . . . to its logical and legitimate results." He was not denying that Hooker had found a way to harmonize his religion with his science. Gray was simply saying that it seemed to him that "any scheme which evolves the animal kingdom out of non-animal naturally, and this out of the mineral, & so on and thinks it unphilosophical to conceive of the [Creator] acting in time, would, for all I can see, lead me directly to pantheism."<sup>45</sup> How was it possible to conceive of the Creator operating "in time," to guard against sliding into either atheism or pantheism, and yet act in perfect conformity to known physical laws?

This foreboding dilemma, bequeathed to him by the growing tensions within the Newtonian paradigm of natural theology, along with his own understanding of the "species problem," gnawed at Gray for the rest of his life. It became the focus of all of his subsequent discussion with Darwin on the implications of his derivative hypothesis for the traditional design argument's understanding of God's relationship to the world. His dilemma would take on concrete shape with the arrival of Darwin's "abstract" of his theory.

### ***The Origin of Species Comes to Cambridge and Boston***

Wallace's revelation of his remarkably parallel theory of natural selection forced Darwin to abruptly change course and dash off an "abstract" of *Natural Selection*. Amid constant bouts of vomiting and agonizing writing and revising, he

<sup>45</sup>Gray to Hooker, 18 October 1859, Kew, APS.

finished it by October 1, 1859 and shipped it off to John Murray, his publisher. It went on sale November 22, 1859 under the title, *The Origin of Species by Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life*.

Everyone, most of all Darwin himself, was surprised when the initial 1250 copies sold on the first day and a second printing of 3000 copies was immediately ordered.

Darwin astutely recognized that the next major step was to get copies of his book into the hands of prominent naturalists whose opinions would be critical in gaining support in the scientific community. He had corresponded with scores of naturalists, ranging across the entire spectrum of the natural sciences, for over two decades on a wide range of topics related to his theory. He would, of course, have to send copies to men whom he knew were hostile to his views, such as Louis Agassiz, Richard Owen, and Adam Sedgwick, but he also understood that the views of these men could be effectively countered by the opinions of those most sympathetic to his views. These men included his closest confidants, the men whose expertise and criticisms he had most relied on, men whom he had already trusted with the outline of his theory, Joseph Hooker, Charles Lyell, and Asa Gray.<sup>46</sup> Gray was a wiser and more strategic

<sup>46</sup>Darwin's complete list and biographical register of the 90 people to whom he sent presentation copies of the first edition of *The Origin* is given in Appendix III of *CCD* 8: 554-570. The only Americans to receive copies were Louis Agassiz, James Dwight Dana, and Asa Gray. Agassiz had sent Darwin a copy of his *Essay on Classification* in 1858. They had been in periodic correspondence on topics of mutual interest since 1841. They were, thus, clearly aware of each other's positions by fall 1859. Darwin had been following Gray's debates with Agassiz before the Academy throughout 1859 with great interest. Dana served as a mineralogist with the Wilkes Expedition, 1838-1842; he wrote several papers summarizing his findings on crustacea and mineralogy which Darwin admired. They began corresponding on these and related matters in 1849. Though Darwin disagreed with Dana's views that there had been a warmer period subsequent to the retreat of the glaciers, a view Gray adopted, he continued to value Dana's opinions. Unfortunately, Dana had suffered a severe physical, mental, and emotional breakdown in the fall of 1859 brought on by the crush of his heavy



choice than Darwin could have dared to hope. He would play the most significant role in gaining a sympathetic hearing for Darwin in America during the first decade of debate.

Darwin broke nearly a year's silence to inform Gray in mid-November 1859 that his "abstract" on the origin of species was soon to be published; Gray's copy would be on its way shortly. Darwin, always apologetic for his intrusions and solicitous of his correspondents' opinions, knew that Gray was

pressed for time; but if you can read it, I shall be infinitely gratified. From its condensed state it is *indispensable* to read it *all* straight through. If ever you do read it, & can screw out time to send me (as I value your opinion so highly) however short a note, telling me what you think its weakest & best parts, I should be extremely grateful.

Yes, he admitted, his theory had "very many difficulties not satisfactorily explained by my theory of descent with modification, but I cannot possibly believe that a false theory would explain so many classes of facts, as I think it certainly does." He was prepared for the abuse he knew would come from those entrenched in their belief in the fixity of species, but was confident that his theory explained a far greater range of facts than did their "repeated acts of Creation." Darwin declared that "on these grounds" he was willing to "drop . . . anchor & believe that the difficulties will slowly disappear." He was buoyed by the first wave of support already coming in.

I am rejoiced to say that Lyell is a complete convert; & is heroic & candid enough to be now writing a public change of opinion. -- Huxley, a first-rate zoologist, & Carpenter first-rate physiologist are converts; as is H. C. Watson.

duties. He confessed to Darwin three years later that he had still not found the energy to read his book. Darwin sent presentation copies of the third edition to Chauncey Wright and Jeffries Wyman; CCD 9: Appendix VII, 420-425.

So that I am more than contented. I am *sure* to be in error in many parts; but my general view, I conclude, must have some truth in it.

It was now time for Gray to work on his "short note" on the "weakest and best parts."<sup>47</sup>

Gray was quick to act. Gray immediately assumed the role of Darwin's American publishing agent. American publishers at the time did not respect foreign copyrights; that would mean Darwin would lose both editorial control and royalties. While negotiating with the Boston publisher Ticknor and Fields, Gray learned that two other publishers were already planning their own editions. He eventually negotiated an agreement with the New York firm of D. Appleton that would secure Darwin's editorial control and royalties. They brought out an American edition, with additions and corrections supplied by Darwin even before the second English edition appeared, and a preface by Gray, in mid-January 1860.<sup>48</sup> They sold 2250 copies of three separate printings by May 1. The "revised and augmented" American edition, ahead

<sup>47</sup>Darwin to Gray, 11 Nov. 1859, *CCD* 7: 369-370; Darwin to Gray, 21 Dec. 1859, *CCD* 7: 440; Darwin to Gray, 24 Dec. 1859, *CCD* 7: 445-447. He was perhaps too enthusiastic about Lyell's "conversion." Lyell would continue to waffle on the degree of his commitment to transmutation of species and Natural Selection till his death, all the while remaining cordial friends with Darwin and his family.

<sup>48</sup>Gray's "long and valuable letter" offering his services, written sometime around the beginning of December, has never been found; Darwin to Gray, 21 Dec. 1869, *CCD* 7: 440, n.2. Appleton was also spurred into publishing Darwin by its enthusiastic and energetic science advisor, the popular science lecturer, Edward L. Youmans. There is no record of any contact between Gray and Youmans, but it would be surprising if there were none. Youmans played a major role in establishing Appleton as the leading American publisher of *avant garde* English and European authors, including Buckle, Darwin, Huxley, Tyndall, and Haeckel. Youmans himself became an early and ardent disciple of Herbert Spencer and persuaded Appleton to become his American publisher. John Fiske, ed., *Life and Letters of Edward Livingston Youmans* (London: Chapman & Hall, 1894), 2 vols, 1:110-111; Grant Overton, *Portrait of a Publisher* (New York: D. Appleton, 1925), 50.

of its British counterpart in several important respects, was published, with a preface by Gray, on July 1.<sup>49</sup> Appleton remained Darwin's American publisher for all of his subsequent books.

Through an oversight by the publisher, Gray did not receive his copy of *The Origin* until near Christmas 1859. By then Hooker had already told him that it was a truly "marvelous work" and causing "a tremendous furor." Hooker was "staggered with the masterly manner of condensation . . . , the depth of thought, range of knowledge, power of concentrated reasoning & very agreeable . . . style." He was confident Gray would be "delighted and converted" by it.<sup>50</sup>

The Cambridge intellectual community was already buzzing over *The Origin*. Charles Eliot Norton, who later served as editor of the *North American Review* and professor of aesthetics at Harvard, professed to be the first to read it; he then passed it on to Jeffries Wyman, the prominent Harvard anatomist.<sup>51</sup> He joined James Russell Lowell, professor of modern languages at Harvard and editor of *The Atlantic Monthly*, and Henry Torrey, a Harvard historian, in the work room of Jeffries Wyman to discuss it the day after Christmas. They had all closely followed the debates between Agassiz and Gray on the merits of Darwin's theory before the American Academy for

<sup>49</sup>The publishing history of the American edition of *The Origin* is included in CCD 8, Appendix IV, 571-583.

<sup>50</sup>J. D. Hooker to Gray, 27 Nov 1859, GHA. Darwin to Gray, 21 Dec. 1859, CCD 7: 440.

<sup>51</sup>Wyman to Charles E. Norton, 11 January 1860; Houghton Library, Harvard University; cited in Toby A. Appel, "Jeffries Wyman, Philosophical Anatomy, and Darwin," *Jour. Hist. Bio.* 21 (Spring 1988), 84 n.40.

Arts and Sciences and the Cambridge Scientific Club for the past year. Apparently Gray had at least persuaded them through his Academy debates with Agassiz, and presumably more informal conversation, that there were significant scientific gaps in Agassiz's theories and that Darwin provided, at the least, more plausible explanations of organic phenomena. The known stout opponents of Darwin, notably Louis Agassiz and Francis Bowen, were absent.

Norton, for the moment, believed that it was "interesting and able. -- but it does not seem to me that the argument in it is complete, or that the reasoning is as good as the science. At any rate I wait to be convinced that I am nothing but a modified fish. . . . His book will help overthrow many old & cumbrous superstitions. even if it establish but few truths in their place."<sup>52</sup> Wyman declared to Gray that *The Origin* is 'thundering able", --"a thoroughly scientific & philosophical work."<sup>53</sup> The group clearly understood what was at stake: "if Darwin is right Agassiz is wrong." They fully expected that Agassiz would soon join battle with Darwin to defend his enormous ego and the validity of his immense scholarship.

Gray devoted the week between Christmas and New Year's to an attentive and incisive reading of Darwin's book. In principle, he accepted Darwin's major claims tempered with some qualifications. As he had mentioned in a previous letter to Darwin, he was skeptical of Darwin's tendency to personify his concepts, especially

<sup>52</sup>Charles Eliot Norton to Elizabeth C. Gaskell, 27 Dec. 1859, Jane Whitehill, ed., *Letters of Mrs. Gaskell and Charles Eliot Norton, 1855-1865* (London: Oxford University Press, 1932), 43.

<sup>53</sup> Gray to Darwin, 10 Jan. 1860, *CCD* 8: 26-27.

natural selection; several marginal notes chided Darwin for "too much personification" and his "overdone" and "too fanciful!" description of the concept. He agreed with Darwin's views on inheritance, though he questioned how natural selection explained variation, a major weakness he would concentrate on in the future. He was especially skeptical of Darwin's Lamarckian tendency to explain variation through use and disuse and external conditions. Perhaps it all "comes to *Lamarck* after all!" His most serious criticism was that Darwin explained the formation of the eye through the fortuity of natural selection; this became a major criticism for many other critics. Darwin had no doubt chosen this example with Paley in mind since the eye was his prime example of exquisite design. Darwin himself confessed that it seemed "absurd in the highest possible degree" to believe that natural selection had formed the "inimitable contrivances" of the eye. Gray was quick to note, "so it does."<sup>54</sup>

These reservations notwithstanding, Gray was impressed with Darwin's work. He exclaimed to Hooker that "It is crammed full of most interesting matter--thoroughly digested--well expressed--close, cogent-- and taken as a system it makes out a better case than I had supposed possible." His friends were also impressed. "A hard-headed friend, of a very impartial mind, familiar with physical science . . . is much impressed with it."<sup>55</sup> He was pleased to tell Darwin that "Wyman--the best of judges--& no convert, [is] much struck with it: -- says your

<sup>54</sup>Gray's marginal comments from his copy of *The Origin*, 186, GHA.

<sup>55</sup>Gray to Hooker, 5 January 1860, *CCD* 8: 15-17. His "hard-headed friend" was likely Chauncey Wright.

book is 'thundering able,' -- 'a thoroughly scientific & philosophical work.'"<sup>56</sup>

Agassiz, as expected, "growls over it, like a well-cudgelled dog," resents Darwin's "good physical or natural explanations of all his capital points," and denounces it as sheer atheism.<sup>57</sup>

He assured Darwin "that the *best* part, I think is the *whole . . . plan and treatment*. . . . Surely 20 years was not "too much time to produce such a book . . . ." It was clear to Gray that as soon as he understood Darwin's premises, he did "not see how he [could] stop short of [Darwin's] conclusions, as a probable hypothesis, at least." While Darwin had clearly deepened our understanding of hybridism, Gray felt that he still had not offered a satisfactory account of all the intricate issues of sterility, fertility, and reversion in crosses. Gray felt this would be a major attack point for his critics; it was. The weakest part of the book, Gray felt, was the Lamarckian "attempt to account for the formation of organs, -- the making of eyes, &c by natural selection." But, apart from these two or three "great gaps in the evidence," Gray concluded that he had "never learned so much from one book."<sup>58</sup>

As he read he knew immediately that, despite the enormous amount of work still undone on the Wilkes Expedition specimens and his distaste for political battles, he must use his international prestige as a botanist to "defend Darwin against illogical attacks and absurd propositions." He could best accomplish this, he assured Darwin,

<sup>56</sup>Gray to Darwin, 10 January 1860, *CCD* 8: 26-28.

<sup>57</sup>Gray to Hooker, 5 January 1860, *CCD* 8: 15-17.

<sup>58</sup>Gray to Darwin, 23 January 1860, *CCD* 8: 46-49.

if he gave his book a "fair and favorable consideration" while remaining "non-committal as to its full conclusions" rather than declare that he was a "convert." His strategy was clear: find those weak points where Darwin was sure to be attacked and turn them against the critics in Darwin's favor. Nothing would satisfy him more, he told his friend Boott, than to "stop Agassiz' mouth with his own words, and to show up his loose way of putting things." He worked hard on his review for two solid weeks and circulated it among several trusted colleagues for their comments. It would appear in the March issue of *The American Journal of Science*.<sup>59</sup>

Agassiz had already stolen the march in the campaign for public opinion. In the first week of January he used his charm, eloquence, and enormous popularity as America's premier naturalist to stir up Boston against Darwin and assure his audience of the Creator's obvious and wonderful plan of creation.

What has the whale in the arctic regions to do with the lion and tiger in the tropical Indies? There is no possible connection between them; and yet they are built respectively according to one & the same idea. . . . There is a design according to which they were built, which must have been conceived before they were called into existence; otherwise these things could not be related in this general manner. Whenever we study the general relations of animals, . . . we study the manner in which it has pleased the Creator to express his thoughts in

<sup>59</sup>Gray to Darwin, 10 Jan. 1860, *CCD* 8: 26-27; Gray to Darwin, 23 Jan. 1860, *CCD* 8: 46-49; Gray to Boott, 16 Jan. 1860, Darwin-Lyell Papers, American Philosophical Society. Daniel Cady Eaton, Gray's student and professor of botany at Yale, reported having seen a part of his review of Darwin, Eaton to Gray, 28 Jan. 1860, *GHA*. Gray also gave William Rogers the proof sheets of the review to look over. William Rogers to Henry Rogers, Feb. 1860, William Rogers to Henry Rogers, 21 Feb. 1860, Mrs. William Barton Rogers, ed., *Life and Letters of William Barton Rogers*, 2 vols. (Boston: Houghton Mifflin, 1896), 2: 20, 22. There is other indirect evidence that Gray circulated drafts of his reviews to colleagues whose views he suspected were similar to his own and opposed to Agassiz's. This is an important indication that Gray was well aware of a growing number of naturalists who shared his dissatisfaction with Agassiz's philosophy, politics, and posturing and were openly considering alternative explanations for the questions they were considering. He took maximum advantage of that disaffection in planning his campaign for Darwin.

living realities; and that is the real value of that study for intellectual Man; for while he traced these thoughts as revealed in nature, he must be conscious that he feels, and attempts as far as it is possible for the limited mind of man to analyze the thoughts of the Creator, to approach if possible into the counsels that preceded the calling into existence of this world with its inhabitants, and there lies really the moral value of the study of nature; for it makes us acquainted with the Creator in a manner in which we cannot learn [of] Him otherwise. As the Author of Nature, we must study Him in the revelation of nature in that which is living before our eyes.<sup>60</sup>

No statement of Agassiz better expressed the way in which he used his eloquence and passion to enrapture his audiences with the truths of his philosophical and religious perspective.

### William Rogers Debates Agassiz

While Agassiz was enthralling his public audiences, he had not yet silenced his

<sup>60</sup>The extract has not been preserved; the exact occasion of Agassiz's remarks are not known. Francis Darwin thought it may have been given to the Mercantile Library Assoc. (LL 2:64-65n.). Gray apparently sent an "extract" from a newspaper article reporting on Agassiz's public lecture to Hooker in his 5 Jan 1860 letter. Hooker passed the letter on to Darwin. Darwin thanked Gray for the "extract from Agassiz" in his letter of 28 Jan. 1860. Darwin, in turn, may have sent this extract to Lyell, as was his custom with other items relating to the reception of *The Origin*. Lyell included an "Extract from Lecture by Agassiz, Boston, January 1860" in his scientific journal covering this period. Leonard G. Wilson, ed., *Sir Charles Lyell's Scientific Journals on the Species Question* (New Haven: Yale University Press, 1970), 348-349; CCD 8: 55, n.11. We assume this is the extract Gray first sent to Hooker.

Both Gray and Darwin were amused that Agassiz's denunciation would, in fact, advertise *The Origin* even more widely; Gray to Darwin, 10 Jan. 1860, CCD 8: 26-27, Darwin to John Murray, 25 Jan. 1860, CCD 8: 51.

Agassiz evidently was busy on both the public and political stage. The *Christian Intelligencer* (Feb. 16, 1860, 135), the newspaper of the Dutch Reformed Church, reported that Agassiz "addressed a legislative meeting in the State House . . . on 'The True Aims of the Study of Natural History'" in which he countered the development theory with his own well-known theory of successive creations. Such an address was well-planned. Just the previous summer he had successfully secured a generous state grant (not to exceed \$100,000) for his museum. Now he wanted to assure these same state legislators that he and his museum could be trusted to support through its teaching and exhibits the traditional harmony of natural history and religion and resist the new heresies of Darwin. This address was evidently a preview of Agassiz's popularization of his *Essay* (1858) which he brought out in eight monthly installments during 1862 in the *Atlantic Monthly* and subsequently brought together as *Methods of Study in Natural History* in 1863.



professional critics who wanted to give Darwin a fair hearing. He had yet to confront the formidable Boston geologist, William Barton Rogers, at the Boston Society of Natural History. Agassiz and Rogers knew each other professionally and were, for the most part, on friendly terms. They were members of several of the same professional associations, such as the American Academy of Arts and Sciences, the American Association for the Advancement of Science, and the Boston Society of Natural History.

They were also political rivals. Rogers had been nursing a dream ever since 1846 of establishing a school of industrial technology in Boston, the heart of American manufacturing leadership. He laid out his bold vision for John A. Lowell, the wealthy Boston textile manufacturer and philanthropist. Lowell, already in Agassiz's corner at the Lawrence Scientific School, declined to fund Rogers' project. Undeterred by Agassiz's rebuff, Rogers moved from the University of Virginia to Boston in 1853 to be in the heart of the city in which he still hoped to build a "practical" school of science and technology. He made ends meet through lecturing, writing, and some technical consulting. He never abandoned his dream. Toward that end he had tried unsuccessfully for some years to fill the chair of geology at Harvard's Lawrence Scientific School. Perhaps this could be the vehicle to advance his dream. Agassiz blocked his appointment every time. Although Agassiz held positions in both zoology and geology, he rarely offered any courses in geology. Even Agassiz's protege, Jules Marcou, himself a geologist who had previously crossed

swords with Rogers, believed that Rogers was well-qualified for the position.<sup>61</sup> Perhaps he was too well-qualified and too independent for Agassiz's ego to handle. They also held fundamentally different ideas about the purpose of science and technology education. Agassiz was fixed on the model of German graduate education; Rogers envisioned a school that would be intellectual engine of America's industrial might.

Not to be outflanked by Agassiz, Rogers finally succeed in fulfilling his ambition. In April 1861 the state legislature, which had granted Agassiz \$100,000 for the Comparative Museum of Zoology, gave Rogers a similar grant and some choice state-owned property to establish the Massachusetts Institute of Technology. Rogers served for many years as its first president.<sup>62</sup>

William and his brother Henry, himself a prominent geologist then serving as professor of geology at the University of Glasgow, shared their early appreciation for the many merits of *The Origin*.<sup>63</sup> Henry believed it to be a "most suggestive book.

<sup>61</sup>Jules Marcou, *Life, Letters, and Works of Louis Agassiz* (New York: Macmillan, 1896), 2: 108. In July 1859 Henry Rogers reported to his brother that during his recent visit to England Agassiz had told him he wanted to concentrate on the museum and give the geology chair to William. In March 1860 Pres. Felton, the new president of Harvard, told William in private how much he and other faculty members wanted him in a new position as professor Geology and Mining they were proposing for the Lawrence Scientific School. There is no record he was ever offered the position. Henry Rogers to William Rogers, 15 July 1859, *LLWBR*, 2: 10; William Rogers to Henry Rogers, 30 Mar. 1860, *LLWBR*, 2: 30.

<sup>62</sup>Edward Justin Pfeifer, "The Reception of Darwinism in the United States, 1859-1880," (Ph.D. diss., Brown University, 1957), 19; Robert V. Bruce, *The Launching of Modern American Science, 1846-1876* (New York: Alfred A. Knopf, 1987), 161-162, 391.

<sup>63</sup>Patsy Gerstner has recently completed a major biography of Henry Rogers, *Henry Darwin Rogers, 1808-1866: American Geologist* (Tuscaloosa: University of Alabama Press, 1994).

full of ingenious arguments in favour of the Lamarckian hypothesis." William was sure that Henry would "approve" the "truth-loving spirit of the book." The only obvious fault they both found was that Darwin, like Lyell, had seriously minimized the effect of "paroxysmal" changes. Although both of them believed in "development" in principle, Henry concluded that Darwin's theory "will never be capable of a strictly scientific proof." But, then, neither could the theory of "supernatural creations." This impossibility of proof prompted both William and Henry to counsel toleration rather than dogmatism. William carried that advice into his debates with Agassiz and his review of *The Origin*.<sup>64</sup>

William set the tone for his "liberal defence" of Darwin against the charges of Agassiz in the earliest scholarly review of *The Origin* in Cambridge and Boston, perhaps in the entire country.<sup>65</sup> It was surely a testimony to the broad cultural significance of the issues Darwin discussed, Rogers observed, that it should already have excited so much eager attention from all sectors of society, from scientists to general reader. While the topic of the origin of species had been discussed for ages, "never before [had it] been presented in a form and connection bringing them so clearly within the pale of inductive reasoning." He was convinced that the book would "stimulate observation and . . . extend the limits of positive knowledge."

<sup>64</sup>Henry Rogers to William Rogers, 23 Dec. 1859, *LLWBR*, 2: 17-18; William Rogers to Henry Rogers, 2 Jan. 1860, *LLWBR*, 2: 18-19; Henry Rogers to William Rogers, 24 Feb. 1860, *LLWBR*, 2: 24. Henry, on friendly terms with Thomas Huxley, reported that Huxley was "much pleased" with William's "liberal defence" of Darwin's book.

<sup>65</sup>His review was published in the *Boston Courier* on 5 Mar. 1860. A liberal extract of the review is printed in *LLWBR*, 2: 26-28.

Though the majority of readers would not be convinced by Darwin's arguments, Rogers was persuaded that since his views had been "presented with such fairness and simplicity, [they] will not only command the earnest attention of scientific men of whatever predilections, but will in many cases win, at least, their partial assent."

Rogers, though present at the Academy meetings of 1859, remained silent while Gray challenged Agassiz and defended the plausibility of Darwin's theory. Now it was his turn to press the attack. In four important meetings of the BSNH during late winter and early spring 1860 Rogers used his considerable knowledge of the geology of the Northeastern United States and Canada to weaken Agassiz's objections and strengthen the geological portion of Darwin's argument.<sup>66</sup> Rogers became a strong professional ally for Gray in challenging Agassiz and making out a credible case for Darwin.<sup>67</sup>

These were strategically important meetings in securing a favorable hearing for Darwin in America. Rogers was the first scientist, on either side of the Atlantic, to

<sup>66</sup>Rogers and Agassiz debated each other on Feb. 15, Mar. 7, Mar. 21, and Apr. 4. William reported to his brother, Henry, that he "had a good deal to say on the subject [of Darwin] at every meeting of both societies since the question came up, and have had to do battle almost unaided." This last comment is puzzling since Rogers is mentioned only briefly and in passing in the *Proc.* of the American Academy. Perhaps he was "unaided" only at the BSNH since Gray was clearly active in the Academy debates. William Rogers to Henry Rogers, 30 Mar. 1860, *LLWBR*, 2: 30.

<sup>67</sup>Rogers to Gray, 28 Jan. 1860, GHA. Unfortunately, Rogers strategic role in gaining Darwin a thoughtful hearing has not received the attention it deserves. Lurie surveyed Rogers' debate with Agassiz at the BSNH, but did not draw attention to the strength and depth of Rogers' argument. *Agassiz*, 292-297. Edward Pfeifer has given a somewhat fuller discussion of this debate in both his dissertation, "The Reception of Darwinism in the United States, 1859-1880" (Ph.D. diss., Brown University, 1957), 19-24, and in his chapter on the "United States" in Thomas F. Glick, ed., *The Comparative Reception of Darwinism* (Chicago: The University of Chicago Press, 1988), 176-181. George Daniels published several substantive portions of the debate in *Darwinism Comes to America* (Waltham, MA: Blaisdell Publishing Co., 1968), 22-29.

defend Darwin at a professional society meeting. That he was able to base his defense on his own thorough and independent geological study, without an appeal to the much more well-known English and European geologists, not even Lyell, is most impressive. Darwin could have benefited from his knowledge in writing chapters 9 and 10 of *The Origin* where he discussed the difficulties the geological record presented for his theory. Darwin was not equal to the task of providing the required geological knowledge to support his claims; he rather relied on weak arguments -- lame excuses, said his critics -- to explain why intermediate forms were not found in geological depositions. Rogers provided this missing geological expertise in his debate with Agassiz, an expertise based on his own thorough study of eastern North American geological formations. He demonstrated in these debates that he was the equal of his English and European peers in the range and depth of his geological knowledge and its bearing on Darwin's theory.

It is clear from the debates that Rogers had already given serious and thoughtful attention to the questions that Darwin's theory posed for the traditional belief in "successive creations" and how his own previous geological investigations could undermine Agassiz's objections and strengthen Darwin's arguments. At the first meeting of the Boston Society of Natural History in February 1860 Rogers demonstrated his thorough grasp of the important issues Darwin was raising for all naturalists. Another society member, no doubt voicing the objections of many others on first reading *The Origin*, complained that Darwin seemed to have abandoned a judicious weighing of the evidence in favor of advocating "a foregone conclusion" and

glossing over the substantial difficulties of his theory with countless "suppositions and ifs."<sup>68</sup> Rogers patiently pointed out that Darwin had time only to give us a "resume" of his argument; he had neither time nor space to give us all of the facts upon which his theory was based.

Mr. Darwin makes no pretensions to an absolute demonstration, but, after an impartial survey of the facts bearing on the subject and a candid appreciation of the opposing considerations, adopts the view set forth in his book, as offering, in his opinion, a more rational and satisfactory explanation of the history of living nature than the hypothesis of innumerable successive creations.  
 . . . [T]he work [was] marked in an extraordinary degree of fairness in the statement of opposing as well as favorable arguments, by the absence of dogmatism, and by all other evidences of a truth-loving spirit, as well as by the extent and variety of its knowledge and the breadth of its philosophical views.<sup>69</sup>

Not even Gray commended *The Origin* as highly.

Agassiz opened the debate by declaring that, while Darwin was justly honored as an English naturalist, he had used his great learning to advance "an ingenious but fanciful theory" that "the primary cell, by a process of differentiation and gradual improvement by natural selection, has produced all the diversities of animals, in geological and present times." Agassiz countered that Darwin had been led to the erroneous idea of "gradual development" of one species from another by comparing the fauna of the present with the fauna of the past. The fact is, Agassiz insisted, "animal representatives were as numerous and diversified in early geological periods as now. . . ."<sup>70</sup> As proof Agassiz offered up the example of *Lingula prima*, a species

<sup>68</sup>BSNH, *Proc.*, 7 (1859-1861), 234.

<sup>69</sup>Ibid.

<sup>70</sup>BSNH, *Proc.*, 231.

of shellfish whose form could be found "in unbroken succession" up to the present living species.

The key words for Agassiz were *form* and *succession*. The Creator, according to Agassiz's philosophical perspective, had used the *same form* of *lingula* in each *successive* epoch of creative activity. That is why what were then typically called "representative" or "prophetic" types of the same form were found in each successive geological layer. These forms were bound together only in the way that the *form* of an artists' work persists in their *successive* creations because they come from the same mind; they were clearly *not* bound together *materially* as though one painting emerged from another. It was on the basis of this perspective that Agassiz claimed that the persistence of this form was "a fatal objection to the theory of gradual development." Where Agassiz saw *succession* Darwin saw "infinitely fine gradations" and *development*; history was transformed from chronological succession into organic transmutation. Such was the profound way that Darwin's theory had transformed the meaning of history, one that Agassiz, and many others, found so difficult to grasp conceptually.<sup>71</sup>

Rogers admitted that, yes, the persistence of *lingula* would seem to present grave difficulties for Darwin.<sup>72</sup> However, Rogers pointed out that Darwin had not

<sup>71</sup>BSNH, *Proc.*, 232.

<sup>72</sup>William told his brother Henry that "the discussions between Agassiz and myself have been very courteous and good-natured. Indeed, I have felt so strong in the ground I have taken, that I could not be unduly excited, and have much enjoyed these friendly contests." This gives a much more congenial picture of Agassiz and the Darwinian debates in Cambridge than is typically given in the literature. William Rogers to Henry Rogers, 30 Mar. 1860, *LLWBR*, 2: 30.

denied that such persistence of species was impossible under his theory. He had, in fact, offered several explanations for why it was possible. For one, Darwin had reasoned "that the vital characters of some animals fit them for resisting change and extinction better than more plastic natures. . . .; some species, like dogs have changed a great deal, while cats seem not to have changed at all over thousands of years. Rogers here caught a basic point of Darwin's theory which most of his critics did not. Darwin did not claim that all species underwent the same degree or rate of change throughout all space and time. He recognized that every individual organism was subject to unique conditions of life. His theory of natural selection was thus meant only to explain the change some species have undergone; it was not meant to be a universal claim that all species were subject to the same pressures in all places and at all times. Thus, Rogers pointed out, Agassiz's claim that the persistence of *lingula* undermined Darwin's theory simply misunderstood Darwin's claim.

Second, Rogers offered, by way of expanding Darwin's explanation in *The Origin*, that the interruptions in the appearance of *L. prima*, which Agassiz had used to show that previous species of *lingula* had been wiped out, could more easily be explained by large scale patterns of migration. Thus, the fact that species are not found in successive layers of a geological formation only means they have migrated out of a region at one period of time and returned at a later period. Rogers believed that it was more sensible to explain the interruption of species as a result of migration than as a "sudden creation without previous parents. . . ." He pointed out that the *Calymene Blumenbachii*, an illustration not mentioned by Darwin, extending from the



Silurian all the way up to the Devonian, demonstrates "a progressive variation amounting to specific difference." Finally, Rogers offered Darwin's defense that the interruptions are simply due to our great ignorance of the vast dimensions of the earth's crust.<sup>73</sup>

Agassiz was skeptical that animals migrated. All study showed that "species are well circumscribed within the limits of their fauna . . ." Darwin's several hypothetical explanations in *The Origin* of how it might be possible for species to migrate from landmass to landmass without connecting bridges did not persuade him. Agassiz urged that if Darwin, and Wallace, had understood the nuances of the zoological understanding of faunae, they would have understood that there were grave weaknesses in their theory of species migration. They should have understood that "[f]aunae are not necessarily . . . like each other because near together, nor unlike because widely separated. . . ." For example, he pointed out that, though the Australian and Indian faunae were separated by only a 15 mile-wide strait, they were, as Wallace noted, remarkably distinct. On the other hand, though separated by thousands of miles of ocean, the faunae of tropical Africa and America resemble each other a great deal. He concluded that the relationships between these different faunae

do not look like migrations, which are at best limited, and in which, if the conditions of life were much changed, the animals would be destroyed; marine animals, in an element which invites migration, are very much circumscribed within limits as to depth of water, and could not migrate from one part of the world to another across ocean abysses. Another obstacle in the way of migration is the transfer of progeny; eggs in most animals cannot bear much change of

<sup>73</sup>BSNH, *Proc.*, 232-233.

temperature or of location, without destruction of the contained embryo.<sup>74</sup>

Until Darwin, or anyone else, could demonstrate that migration had actually taken place, Agassiz reasoned, there was no reason to abandon what all naturalists already understood about the boundaries of the world's faunae.<sup>75</sup>

Agassiz then took aim at one of Darwin's central claims that geology shows that there has been a progressive development of animals from the Silurian up to the Tertiary period. The fossil record shows precisely the opposite, he claimed. The fossils appearing in the earliest known deposits already show "that all the great and principal classes [of animals] were then existing." For example, the Tribolites are "complicated animals, and belong among the highest crustaceans. . . ." The same thing is true for every other geological deposit: "the highest representatives" of each great kingdom are found in each geological period. Finally, if Darwin claimed to have discovered identical species in successive layers, he could claim this only on the basis of superficial examination. More careful, detailed study would reveal distinct

<sup>74</sup>BSNH, *Proc.*, 251.

<sup>75</sup>This was a key point for Agassiz. He held to the theory, variations of which were very common, that all animals, as well as humans, occupied specific zones, called *faunae*, throughout the world. Thus, there were unique faunae for each of these zones. At best, there were only accidental co-mingling of these distinct *faunae*. The burden of proof was on Darwin to demonstrate that migration between faunae had actually taken place. Geology, accordingly, could not answer the prior question of the various zones occupied by these faunae; only zoology could not that. Perhaps he was exasperated with Rogers at this point. After all, Agassiz had spelled all of this out in his *Essay on Classification*, published as vol. 1 of *Contributions to the Natural History of the United States* in 1857. Chapter one was devoted to "The Fundamental Relations of Animals . . . as the Basis of the Natural System of Animals." Edward Lurie has edited a modern translation of the *Essay* (Cambridge: Belknap Press of Harvard University Press, 1962).

differences among species found in successive deposits.<sup>76</sup>

In making this claim Agassiz expressed the conceptual ambiguity in the meaning of the words *high* and *perfect* as used to characterize organic life that Darwin had amplified. Rogers questioned Agassiz's assertion that the "highest" representatives of each kingdom were found in the earliest fossil deposits. He wondered how the earliest forms could be the "highest" if they were, as Agassiz taught, "of an embryonic character." If that were true, Rogers claimed, then "the term 'perfection' is just as indefinite as the word 'species.'" For his part Rogers "considered perfection . . . but specialization in each particular type [of animal or plant]." There was a great philosophical gulf fixed between his and Agassiz's understandings of "perfection" and "highest" that Rogers perceptively underscored. This confusion over these central terms plagued all contemporary naturalists, including Darwin; it was not unique to Agassiz.

Agassiz often adopted the traditional scale of low/high to characterize the relationship between humans and other forms of life. On this scale human would be "highest" and crustacea would be "lowest." But he also used the low/high scale in a much more technical, philosophical sense to characterize his understanding of the relationship between the 'typical' embryo of a kingdom or family and specialized adult members. Agassiz, in harmony with his German teachers Dollinger and Tiedemann, believed that every individual embryo recapitulated in its own growth the stages

<sup>76</sup>BSNH, *Proc.*, 233.

through which all members of its kingdom had passed on its way to permanent individuality. The embryo of every individual contained the universal characteristics of the kingdom to which it belonged; this fish was an exemplar of the great class, vertebrates. Looked at from this perspective the embryo of the fish was "highest," because it most perfectly embodied the characteristics of the kingdom of vertebrates. Embryology was such a critical science for Agassiz precisely because it studied with microscopic exactness the stages of transformation through which the embryo passed. There was change and evolution in this view, to be sure, but only in the growth of the individual.

This is why Agassiz never understood Darwin's claim that species evolved into kingdoms. From his perspective, this simply inverted the true order of nature: primordial kingdoms, everpresent in the Creator's mind, brought forth with the Creator's power the great diversity of species and individuals in each of those kingdoms. On this scale the embryo of any creature was "highest" since it carried within it the universal type of its kingdom or family. Agassiz held that "perfection [was the] embodiment of the highest combinations, the most complex representation of life." For example, the embryo of the fish was superior to the adult fish since, as an embryo, it fully exhibited the marks of the great class of vertebrates. As the embryo grew it acquired the more distinctive, specialized character of a subordinate species within the great vertebrate family and lost its distinctive type as representative of all vertebrates; the whole kingdom of the vertebrates was contained in the single individual fish. He concluded that "[a]s a generalization or philosophic conception,

the vertebrate egg is superior to man himself, inasmuch as it embodies all that may be produced from it."<sup>77</sup>

By identifying the ambiguity in Agassiz's understanding of "high" and "perfection" Rogers was scoring a neat debater's point, but little more. The chances are that the audience was much more sympathetic to Agassiz's traditional philosophical and theological understanding than to Roger's relatively new, more empiricist, definition. The significance of the interchange is found in the clash of philosophical perspectives represented by Rogers (and Darwin) and Agassiz.<sup>78</sup>

Later in the spring Agassiz raised a similar issue that was central to his own research perspective, that is, Darwin's claim that varieties were only incipient species. He made it clear that he did not believe there was any such thing as a variety in the animal kingdom. All such alleged varieties, he claimed, "are stages of growth, within the limits of species. . . ."<sup>79</sup> He offered several instances in which varieties

<sup>77</sup>BSNH, *Proc.*, 235.

<sup>78</sup>BSNH, *Proc.*, 235; Lurie, *Agassiz*, 285-288. For these I reasons I cannot accept Pfeifer's claims that, by forcing Agassiz to recognize the ambiguity of "highest" and "perfection," Rogers had compelled Agassiz to acknowledge "the progressive character of the fossil record" thereby undermining one of his major challenges to Darwin. This would be true only if Rogers and Agassiz shared the same philosophical framework such that they each recognized the same criteria by which to judge the validity of an argument. They clearly did not. That each of them used "highest" and "perfection" in different ways does not point to their being confused; it rather points to their inhabiting two different realms of discourse, the empiricist and the idealist, which overlapped just enough to cause the ambiguity and confusion they were struggling with. Both of them simply reiterated their basic premises without making a dent in the other's case. Neither of them explored this fundamental meta-theoretical disagreement, a disagreement that lay at the root of the debate over Darwinism. There is no indication from the *Proc.* that Agassiz felt cornered or trapped by what Pfeifer's alleges as his "admission." On the contrary, it appears as though Agassiz responded to Rogers by helping him to understand the "true" meaning of "perfection" in his system of thought. Pfeifer, "United States," 177-78.

<sup>79</sup>BSNH, *Proc.*, 271.

were confused with stages of growth from his intimate knowledge of the kingdom Radiata. Through a close study of the order *echini* Agassiz found gradations of species within the order. However, this was evidence of a common plan, not derivation, as that which unites the various species. All of the species of *echini* currently exist, along with the older types. He finds that "in every generation we see the growth proceed from *Cidaris* to *Spatangus*, showing that the laborious growth required by Darwin's theory is not logical."<sup>80</sup> This is an excellent example of how central embryological development was to Agassiz's understanding of *species*. Rogers simply refused to enter Agassiz's philosophical quagmire, preferring rather to move the discussion back to the solid empirical ground of geology.

They next debated whether fossils should be dated by geology, as Darwin and Lyell believed, or whether geology should be dated by fossils, as Agassiz believed. Agassiz contended that dating fossils by geology had led to all sorts of confusion. Zoologists, because they understood the underlying characteristics of the fauna, were in a position superior to the geologists to determine the chronological order of fossils in geological deposits. From Agassiz's perspective, Darwin's challenge included a turf battle between the geologists and zoologists over who took the lead in characterizing the fossil record. Zoologists, he assured the audience, understood that all animals were confined within carefully bounded territories or *faunae* which were, in turn, bounded by larger geographical areas or *realms*. The world was thus

<sup>80</sup>BSNH, *Proc.*, 273.

comprised of various *realms* and their subordinate *faunae*. For example, there was a distinct *fauna* for the East Indies, southern Africa, Canada, and the Arctic. These *fauna* must not, under any circumstance be confounded, he warned. But this is exactly what geologists do who declare that two fossils, taken from different geologic layers and different regions, are identical based only on their superficial observations. They clearly had not determined which *fauna* were native to the particular region in which they are digging. This was crucial.<sup>81</sup>

Agassiz "thought that [the limits of successive *faunae*] should be determined by the fossils; that the rocks should be regarded merely as the tombs of the fossils, that naturalists should try to find out the animals of an epoch, and establish the limits of *faunae* on zoological and not on physical principles."<sup>82</sup> Only by first determining which particular animals inhabit this realm and *fauna* can the geologist correctly identify the fossils he finds. The geologist must rely on the zoologist to identify the fossils he finds; he must not dictate to the zoologist what he has found. When this

<sup>81</sup>BSNH, *Proc.*, 241. Agassiz's stress on the crucial necessity to recognize specific zones of *faunae* puts him in the middle of the current lively debates on the distribution patterns of plants and animals. These debates initially concerned contemporary distribution patterns. Gradually those studies were extended by some paleontologists and geologists to the distribution patterns of the past. They began to ask about distribution patterns of the large numbers and kinds of fossils they were finding. These two lines of inquiry proceeded rather independently at first. Janet Browne provides a cogent history of scientific thinking on biogeography down to Darwin in *The Secular Ark: Studies in the History of Biogeography* (New Haven: Yale University Press, 1983). Darwin fused the two traditions by making the historical development of patterns of distribution a central key to his theory. See Alan Richardson, "Biogeography and the Genesis of Darwin's Ideas on Transmutation," *Journal of the History of Biology* 14 (Spring 1981), 1-41, Janet Browne, "A Science of Empire: British Biogeography before Darwin," *Revue d'Histoire des Sciences* 43(1992), 453-475 for especially thorough studies of the controversies surrounding both past and present patterns of plant and animal distribution that shaped Darwin's thinking and Agassiz's opposition.

<sup>82</sup>BSNH, *Proc.*, 241.

order is observed it will be found, Agassiz noted, that "the representatives of these faunae differ specifically, and do not pass from one [layer] to the other, and this is true from the most ancient to the most recent times."

Agassiz wanted Rogers to understand that the genus to which the household dog belonged had remained constant through the geological ages. That was the crucial point for Agassiz. Darwin and Rogers could have piled the widest and most varied assortment of dogs from all places and times on Agassiz's work table to convince him of transmutation and he would not have been swayed. What he would see in the pile was a single Plan of the genus *Canis* which remained constant and consistent through every *species* and single individual dog. He may, in fact, break out in delight at how inexhaustible the Mind of the Creator was to be able to produce so many wonderful varieties of the one Plan. That Agassiz would not have *seen* the evidence for transmutation in this pile of dogs does not show, as has often been argued, that he was simply being stubborn, ignorant, or mired in his dogmatic beliefs. No, it rather shows how fundamentally different the philosophical lens were through which Agassiz and Darwin (and now Rogers) saw and interpreted the pile.

Agassiz had satisfied himself that the major faunae which had characterized the geological history of the State of New York were quite distinct, and consistent with the well-known study that Hall and Rogers had done in their "Geology of Pennsylvania and Virginia." If geologists had been careful first to define the faunae, they would have understood that fossils found in various places and thought to be identical were, in fact, distinct species. It was easy to overlook crucial differences



without the trained eye of the zoologist. He understood there were varying degrees of differences between specimens that marked them as belonging to distinct species; some specimens may belong to distinct species which vary but little under superficial observation. This is clearly what had misled Lyell, and presumably Darwin, in declaring two specimens to be identical when, in fact, a zoologist would have pointed out that they belonged to distinct species.<sup>83</sup> Geologists. Agassiz smugly declared, ought not to dictate to zoologists what the character of the world's faunae were; they were rather obligated to interpret their geological findings in terms of the faunae the zoologist had already described.

Rogers countered that in order for his objection to Darwin's claim that identical species had been found in successive geological layers to hold, Agassiz would have to demonstrate that each fauna had been completely isolated. This, Rogers urged, would be difficult and contrary to the opinion of most zoologists who "maintain that animal forms do pass from one stratum to another."

But the gravest difficulty Agassiz would have in proving the universal isolation of each fauna, Rogers pointed out, was that the geological layers themselves were not laid down uniformly. While the fauna may have been fairly uniform, the underlying geological strata were broken, uplifted, and inverted; they were anything but uniform.

<sup>83</sup>BSNH, *Proc.*, 242. Agassiz was certainly overreaching in claiming that zoologists were always able to make the careful discriminations among species so critical to their work as taxonomists. This is understandable in public debate. But, in his private moments he, too, confessed to how frustrated he was in being unable to identify clear differences among specimens he was studying that would warrant classifying them as a new species. Mary Windsor has drawn a sensitive portrait of Agassiz laboring under the weight of an idealistic concept of *species* in "Louis Agassiz and the Species Question," *Studies in History of Biology* 3 (1979), 89-117.

Whatever geological divisions there were, those which Agassiz had taken as evidence for complete isolation, were only local divisions. If a trained geologist diligently followed the contours of the layers, he would undoubtedly discover the convergence of once divided strata, along with the once divided species now being shown to exist in successive layers. Rogers gave numerous examples of this generalization based on his intimate knowledge of the geology of eastern America and southern Canada.<sup>84</sup>

He concluded his challenge to Agassiz with devastating finality.

Seeing, then, that faunae are not infrequently mingled in contiguous formations, that they do not hold the same precise geological level or vertical distribution at different localities, [and] that they may even be inverted in relation to each other, . . . the *precise and absolute limitation* of faunae to formations, as advocated by Prof. Agassiz, if true in any case, is at best but of local and partial application, and can not be the basis of a paleontological arrangement of formations. Even according to the ordinary and much less stringent view of geological faunae, it is found that only the few great divisional lines of the geological column are persistent over extensive areas, while the numerous subordinate ones, however distinct at the typical locality, lose themselves as they are traced, to give place to other modes of subdivision. As regards the comparison of the tertiary fossils with corresponding modern forms, . . . we had the authority of . . . paleontologists in general, for the conclusion that a large number of fossils found in the tertiary deposits are identical with existing species. Even supposing, however, that the fauna of the tertiary contains no such identical forms, it will be admitted that the likeness becomes extremely close as we approach the modern epoch, and this would seem to lend support to Mr. Darwin's doctrine of modification by natural selection. In regard to the discrimination of species, the question at last must come to this: What is the limit of specific difference? who shall be the arbiter? what the principle of distinction between species and variety, and what the guide in drawing the lines of demarcation of the successive faunae?<sup>85</sup>

Rogers had reduced Agassiz, who prided himself on his immense store of knowledge, to an amateurish geologist, uninformed paleontologist, and even untutored zoologist.

<sup>84</sup>BSNH, *Proc.*, 242-244.

<sup>85</sup>BSNH, *Proc.*, 244.

Agassiz could only offer the lame response that he really did not expect his views to be adopted at present. Besides, he "believed that after mature examination of his facts they would be generally received." The great irony of this statement is, of course, that his entire life's work--speaking, writing, lecturing, teaching, organizing--had been devoted to getting a "mature examination of the facts" out to the public that supported his perspective. He failed to see how the differences among members of the same species pointed "to a gradation of species or to a confusion of animal forms." He rather believed the differences simply enabled the zoologist to determine more accurately the characteristics of various species. In fact, what were once thought to be a single species has been often shown, upon closer examination, to be distinct. To make his case even more embarrassing, he suggested that this is what has happened in chemistry where platinum and silver were not originally distinguished. Rogers countered that chemistry has also shown just the opposite trend: what were once thought to be separate species, like the diamond and carbon, are now all declared to be one species. Agassiz was sinking quickly.<sup>86</sup>

Rogers was still not satisfied that he had said all that should be said in defense of Darwin's claim that the geological strata were so imperfect that it was impossible to appeal to them in denying the transmutation of species, as Agassiz had done. He offered a long lecture on how our understanding of how the current geographical strata was deposited ought to affect our dating of the fossils found in those deposits.

<sup>86</sup>BSNH, *Proc.*, 244-245.

He was especially interested in what changes we should expect in the deposits found in oceans if we believed the ocean floor was either stationary, subsiding, or rising during deposition. Obviously, if the ocean floor had been stationary, the strata of the surrounding shoreland and the ocean floor should be fairly level. However, if the ocean floor had been gradually rising during deposition, it would mean that "*older deposits* would crop out at the *higher level*, and the successively later ones at a less and less elevation."<sup>87</sup> Agassiz had supported this position. On that basis he declared that there should be fully intact geological strata showing Darwin's alleged intermediate forms.

Rogers claimed that the Appalachian strata between Lake Ontario and the coal region in Pennsylvania demonstrated that the ancient ocean floor had been subsiding. This meant that we now find older deposits at lower and lower levels, while the newer Devonian and Carboniferous formations south of this region are piled thousands of feet above these older layers.<sup>88</sup> Rogers argued that we cannot explain how "our Appalachian Paleozoic deposits" were formed on the hypothesis of an uplifting ocean floor. Judging by "their aggregate thickness, as well as their continuity, composition, and stratigraphical arrangement, we are entitled to conclude that they were accumulated during *a long period of subsidence of the ocean-floor, varied by many long pauses and upward oscillations.*"<sup>89</sup> Thus, he did not accept the theory that there

<sup>87</sup>BSNH, *Proc.*, 246.

<sup>88</sup>BSNH, *Proc.*, 247.

<sup>89</sup>BSNH, *Proc.*, 273-274.

had been a continuous upward thrusting of the ocean floor in this region, as Agassiz believed.

But, Rogers continued, whether the ocean floor had been rising or not during the deposition, there would still have been a great difference between the topographical features of the land at the conclusion of deposition and what those features were at present. This difference could only be explained by "the work of subsequent denudation, of which extensive and unmistakable evidences are apparent throughout the Paleozoic area."<sup>90</sup> Rogers concluded

that the remarkable preservation of the Paleozoic strata in the region referred to has been due to the subsidence which successively removed them in great part from the destructive effect of shore-action, sealing them down under an accumulation of overlying deposits. This preservation, therefore, is entirely consistent with the view of Darwin and other geologists, of the extensive destruction of deposits with their fossils, when, through an *uprising movement*, they are brought, stratum by stratum, within reach of the wasting and dispersing forces of the shore.<sup>91</sup>

Agassiz had no adequate response. He allowed that Rogers had offered an "ingenious" theory on subsidence and denudation which could explain the known facts, but he saw no proof of either subsidence or denudation and remained unpersuaded. He admitted that "during a local upheaval of the shore, the whole bottom was, in his opinion, subsiding from the shrinking caused by the cooling of the earth's crust."<sup>92</sup>

<sup>90</sup>BSNH, *Proc.*, 247.

<sup>91</sup>BSNH, *Proc.*, 247-248.

<sup>92</sup>BSNH, *Proc.*, 274.

Agassiz had again made a fateful blunder that Rogers was quick to work to his advantage. Rogers expressed his "extreme surprise" to hear Agassiz state that the ocean-floor had, in fact, been rising during deposition. He then drove the point home with unmistakable clarity. As he recalled

the discussion was commenced by Prof. Agassiz's denying the correctness of the views of Darwin and others of the extensive destruction of strata and their fossils during a period of slow upheaval, and urging as an insuperable objection the great extent and completeness of the Paleozoic series of New York, which he maintained had been deposited during a *period of upheaval*. As, however, Prof. Agassiz has now stated that he recognizes the *subsidence* of the ocean-bed as essential to the theory of their formation, Prof. Rogers thought it of no importance in this connection how that depression may have been brought about, or whether it was accompanied by a stationary or rising condition of the ancient shore.<sup>93</sup>

Checkmate. The discussion ended without any substantive counter-reply by Agassiz.<sup>94</sup>

It is clear that Rogers was far better prepared for these encounters than was Agassiz, who was at the time preoccupied with the exciting and exhausting work of setting up his new museum. Rogers was evidently persuaded that a strong case could be made for Darwin based almost exclusively on his knowledge of the geological

<sup>93</sup>BSNH, *Proc.*, 275.

<sup>94</sup>The minutes for the meeting on April 4, closing the debates between Rogers and Agassiz, reports that "Mr. Putnam presented from the Museum of Comparative Zoology ten specimens of fishes from Lake Neufchatel, and one from the rivers of Switzerland, all very much resembling their American congeners, interesting as showing their resemblance, *yet perfect specific distinctness, of the representative faunae of Europe and North America.*" [italics mine] Frederic Ward Putnam was one of Agassiz's bright student assistants at the M.C.Z. One can only wonder if Putnam then understood the irony of his explanation of what these specimens demonstrated in light of Rogers' severe challenge to those assumptions. Perhaps he did. Only a few years later in 1863, following a serious falling out with Agassiz, Putnam, along with Morse, Packard, Hyatt, and Verrill, left Agassiz to establish the Peabody Museum of Science in Salem. This group became the core of the dominant neo-Lamarckian tradition of evolution in late nineteenth-century America. Ralph Dexter tells the story of the "Salem Secession" through the eyes of the embittered students in "The 'Salem Secession' of Agassiz Zoologists," *Essex Institute Collections* 101 (1965), 27-39; Mary P. Winsor adds depth and context to the incident in *Reading the Shape of Nature*, 43-65.

formations of Eastern North America. He seldom deviated from that course, despite several tempting offers from Agassiz to explore philosophical issues. Rather curiously, Rogers never mentioned domestic breeding or geographical distribution, and only referred obliquely to natural selection and comparative morphology, all central foundations of Darwin's theory. This seems not to have been because he had only a limited, narrow understanding of Darwin's theory, but precisely because he understood that Agassiz, at least, was particularly vulnerable to attack on geological grounds.<sup>95</sup> That he was became painfully visible to his friends, patrons, and students in what had been *his* natural history society. In winning his debate with Agassiz, Rogers set a high standard for the kind of specialized scientific inquiry, the great strength of the emerging inductivist tradition, that was so essential for exploring and evaluating the value of Darwin's theory.

The Rogers-Agassiz debates are remarkable not only for the display of Roger's geological acumen, but for the steady scientific focus and spirit of the debate. With few exceptions both Agassiz and Rogers stayed focused on complex substantive scientific issues, often at considerable length and depth. Neither the debaters nor the audience, judging from the minutes of the debate, raised any questions about the possible implications of Darwin's theory for theology and religion. This seems rather surprising, given Agassiz's public posturing and the lingering perception that religious

<sup>95</sup>Rogers no doubt had Agassiz's professional affront to his reputation during the Marcou affair fresh in his mind when exposing Agassiz's shallow knowledge of North American geology. The *Proc.* do not indicate whether Marcou was present at the spring meetings; he was present on Nov. 7 and Dec. 5 when Rogers and Agassiz again disputed the geological history of New York.

and theological issues played a central role in denying Darwin a fair public hearing. Both men conducted themselves as thorough-going professionals; there were no snide comments or the *ad hominem* arguments so much more characteristic of the English debate.

America's initial reception of Darwin is noteworthy for the way that Asa Gray and William Rogers, each with an impeccable scientific reputation, ably defended the scientific credibility of Darwin's ideas and challenged Agassiz's idealistic understanding of the origin of species. Both were able to draw extensively on their own first-hand knowledge of botany and geology to support Darwin and do so while upholding the highest standards of professional courtesy and scholarship. Their debates with Agassiz were remarkably free from the bitter acrimony that characterized the response of the British scientific community. Although Gray has traditionally been given the most attention in supporting Darwin against Agassiz, the record shows that William Rogers actually presented the strongest empirical arguments in Darwin's favor and opposed to Agassiz. All in all, Gray and Rogers impressively vindicated the maturity of American science at mid-century.

The debates at the American Academy in spring 1859 and at the Boston Society of Natural History in spring 1860, while they were over the heads of most who attended, alerted the members to the most persuasive case that Darwin had presented to support his solution to the "species problem" and exposed the most serious weaknesses of Agassiz's response. Already by the first week of April 1860 a significant number of Boston's amateur and professional naturalists understood what



was scientifically at stake in the debate over *The Origin*. All, including Agassiz, realized that Darwin had to be addressed on the merits of his argument; he could not be as easily reviled, caricatured, and dismissed as had the earlier author of the "developmentalist" *Vestiges of the Natural History of Creation*. Darwin could not have asked for a more favorable hearing, even in his own country.<sup>96</sup>

<sup>96</sup> There is no evidence that Darwin was aware of Rogers' strong and articulate support. This is surprising since he amassed a sizeable collection of reviews, pamphlets, and books relating to the reception of his theory in England, North America, and Europe. It is unfortunate because he could certainly have used it in subsequent editions of *The Origin*. While Gray kept Darwin informed of his own debates with Agassiz, he evidently did not feel, for some reason, that he would be interested in Rogers' sturdy defense. It is highly unlikely that Gray was unaware of the content of the BSNH debates.