“Darwin’s Delay”: A Reassessment of the Evidence

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Abstract: The suggestion that Darwin delayed publishing his species theory has long occupied a central part of his biographical storyline. The notion of a fretful delay reached a melodramatic apogee in Adrian Desmond and James Moore’s best-selling 1991 biography. Janet Browne’s acclaimed work downplayed the pathos but depicted a somewhat hesitant Darwin. In 2007 John van Wyhe upended this tableau, arguing that there was no evidence to support a secretive, fear-based delay. Contrary to van Wyhe, this essay suggests that Darwin was only selectively and strategically open about his belief in transmutation prior to his barnacle project. The 1844 appearance of the anonymously published *Vestiges of the Natural History of Creation* was one in a series of blows that prompted Darwin to reappraise the evidential requirements of his species theory. Nonetheless, much depends on how one interprets the barnacle project. Darwin’s decision to take on the whole group guaranteed its lengthy duration and effectively delayed his species work. The barnacle project could not be considered a necessary preparation, since it was not undertaken to address species theory problems. The evidence and insights Darwin gained from it were largely incidental and came after his decision to tackle the whole group. However, the credentialing motivations behind it were driven by field-generated self-doubts that are difficult to separate from fear. Darwin gained much-needed confidence from it and was far more open about his species theorizing afterward. The project helped Darwin become the authoritative figure he needed to be.

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Biography has never gone out of fashion as far as Darwin is concerned; the multidisciplinary “industry” that bears his name has always circled back to the man himself. The genesis and publication of Darwin’s landmark theory of evolution has played a central role in many biographical narratives. The basic facts are worth recounting: Darwin developed a naturalistic account of evolution in the late 1830s following an epic round-the-world voyage aboard the *Beagle*. He wrote out a sketch version of his theory in 1842 and a far more detailed essay in 1844. While conscious of his theory’s significance, Darwin effectively shelved it for almost a decade to work on barnacles. He resumed his species work in the mid-1850s and finally went public in 1858 when Alfred Russel Wallace threatened to steal his thunder.

The long gap between the basic formulation of Darwin’s theory and its appearance as *The Origin of Species* began to attract the attention of midcentury Darwin scholars, especially those opting for more personal accounts. Interest in the gap years increased in the 1960s after Darwin’s notebooks became available, for they revealed the relatively advanced state of his theorizing in the late 1830s. Given that Darwin was relatively forward in publishing other work, historians discerned a distinct reluctance to go public with his revolutionary theory. The notion of a fearful delay featured in many serious biographies in the second half of the twentieth century and provided an almost irresistible element of drama for popular retellings. The question became not whether there *was* a delay but, rather, *what* stopped Mr. Darwin.¹

Early accounts suggested that Darwin was worried that he might upset someone close to him—such as Robert FitzRoy, the conservative, evangelical captain of the *Beagle*.² Others highlighted Darwin’s trepidation over the way his colleagues might react, his fear of being labeled a materialist, and the cautionary example of the critical reception of *Vestiges of the Natural History of Creation*. Darwin’s reluctance to offend his devout wife Emma was soon added to the explanatory mix.³ By the late 1970s, Darwin’s recurring bouts of illness were implicated in the delay process: some insisted on a somatic etiology for these illnesses; others suggested that they were a psychosomatic manifestation of work-related anxieties and inner turmoil. Finally, a number of scholars argued that Darwin was simply not ready to publish, that he had many facts to gather and problems to work through.⁴

Out of this babble came Adrian Desmond and James Moore’s “defiantly social” blockbuster: *Darwin*. Darwin’s delay was positioned as the “leitmotif” of a richly researched, socially contextualized narrative. Desmond and Moore agreed that Darwin was anxious not to upset his immediate family or conservative scientific colleagues such as John S. Henslow, William D. Fox, and Leonard Jenyns. But they made much of the contrast between Darwin’s outward social conservatism and his radical private theorizing, suggesting that he was constantly fretting over the potential loss of his scientific reputation and social respectability. They insisted that Darwin was acutely aware of the heretical nature of his transformism, particularly the succor it might give to the enemies of church and state. They pictured Darwin as hiding a secret that “was tearing him apart,” converting his fears into illness and preventing him from publishing until

¹ David Quammen, *The Reluctant Mr. Darwin: An Intimate Portrait of Charles Darwin and the Making of His Theory of Evolution* (New York: Norton, 2007).

² Nora Barlow, *Charles Darwin’s Diary of the Voyage of H.M.S. “Beagle”* (Cambridge: Cambridge Univ. Press, 1933); Barlow, *Charles Darwin and the Voyage of the “Beagle”* (London: Pilot, 1945); and Robert E. D. Clark, *Darwin, Before and After: The Story of Evolution* (London: Paternoster, 1948).

³ See Howard Gruber, *Darwin on Man: A Psychological Study of Scientific Creativity* (London: Wildwood, 1974); and Silvan Schweber, “The Origin of the Origin Revisited,” *Journal of the History of Biology*, 1977, 10:229–316.

⁴ For the emphasis on illness see Ralph Colp, *To Be an Invalid: The Illness of Charles Darwin* (Chicago: Univ. Chicago Press, 1977); and John Bowlby, *Charles Darwin: A New Life* (New York: Norton, 1991). For work that focuses on readiness to publish see A. F. Shull, *Evolution* (London: McGraw-Hill, 1951); and Robert Richards, “Why Darwin Delayed; or, Interesting Problems and Models in the History of Science,” *Journal of the History of the Behavioral Sciences*, 1983, 19:45–53.

calmer times.⁵ On our reading, any one of these causes could induce the necessary hesitation, if not paralysis, leaving us to wonder which were the most salient.

Desmond and Moore's best seller garnered many favorable reviews and considerable pop culture traction. A few critics pointed to methodological shortcomings, however, questioning the connections forged between the broader social context and Darwin's mind-set and motivations.⁶ Despite the remarkable scholarship, Darwin's inner state was often inferred from contingent political events rather than directly from primary sources. While Darwin often alluded to the heretical nature of his theory in his correspondence and notebooks, he did not tend to comment on contemporary political agitation.

Other portrayals of Darwin since 1991 include Janet Browne's critically acclaimed two-volume set. Browne's more intimate and sober approach dialed down the drama, but she still suggested that Darwin's terror over the reception of his theory was a factor in his dithering.⁷ Browne otherwise attributed the long lag to publication to Darwin's methodical research process and scientific prudence.

REFRAMING THE DELAY

In 2007 John van Wyhe emphatically rejected the notion of an avoidant delay. Darwin was merely working through a backlog of prior commitments and niggling problems between bouts of debilitating physical illness.⁸ He otherwise stuck to a work schedule he outlined well in advance. The long lag to *Origin* was not due to fear, whether personal, religious, or political. Van Wyhe even suggested that Darwin was not put off by how his scientific peers might receive his species theory.

Van Wyhe's point was that the archival evidence did not support the notion of a fear-based delay; that was a myth invented in the postwar era. Darwin was not as nervously secretive as claimed, given the long list of people he informed about the species work during the gap years. Instead, van Wyhe argued that the lag to publication could be explained by Darwin's scientific caution and the other tasks he had taken on—especially the study of barnacles, which took longer than expected. Once Darwin had finished these tasks he returned to his species work as planned, with publication clearly in mind. Van Wyhe maintained that Darwin was never fearfully delaying, but simply working.

Was van Wyhe correct? Public responses from Darwin scholars have been sparse. One exception was Michael Ruse, who still suggested that Darwin felt he needed to become a recognized expert in the biological sciences.⁹ We aim to build on Ruse's contention and take on the valuable aspects of van Wyhe's analytic framework to gain a more nuanced picture of what Darwin was doing during the gap years.

We agree that the delay was about scheduling, but only up to a point. Our reservations relate to when and why Darwin's schedule changed. Much depends on how one interprets the

⁵ Adrian Desmond and James Moore, *Darwin* (London: Joseph, 1991) (for the quotation see p. 307); and Moore and Desmond, "Transgressing Boundaries," *Journal of Victorian Culture*, 1998, 3:147–168.

⁶ Thomas Söderqvist described Desmond and Moore's *Darwin* as "an 800-page plus masterpiece of historical scholarship": Thomas Söderqvist, "Introduction: A New Look at the Genre of Scientific Biography," in *The History and Poetics of Scientific Biography*, ed. Söderqvist (Aldershot, Surrey: Ashgate, 2007), pp. 1–16, on p. 2. Regarding methodological shortcomings see George Levine, "Essay Review: Darwin Revised and Carefully Edited," *Configurations*, 1994, 2:191–202; and Marjorie Grene, "Recent Biographies of Darwin: The Complexity of Context," *Perspectives on Science*, 1993, 1:659–675.

⁷ See Janet Browne, *Charles Darwin: Voyaging* (London: Cape, 1995), esp. p. 447.

⁸ John van Wyhe, "Mind the Gap: Did Darwin Avoid Publishing His Theory for Many Years?" *Notes and Records of the Royal Society of London*, 2007, 61:177–205.

⁹ Michael Ruse, "The Origin of the *Origin*," in *Cambridge Companion to the "Origin of Species"*, ed. Ruse and Robert J. Richards (Cambridge: Cambridge Univ. Press, 2008), pp. 1–13.

barnacle project. The lead-up to and early stages of the project were punctuated by several deflating events, particularly the publication of *Vestiges of the Natural History of Creation*. These events led Darwin to reassess the evidential requirements of his theory and the personal authority necessary to back it up. Darwin had initially envisaged the barnacles as a small-scale project that would provide “a little zoology & hurrah” for his species work.¹⁰ He subsequently decided to take on the whole group, guaranteeing the project’s lengthy duration. The expanded scope of the project did not come about as an attempt to solve outstanding problems of the species theory. But the credentialing value of the project was always a key motivating factor, one that Darwin became more mindful of over time. While Darwin may have been nervous about the bestial implications of common descent and wary of being labeled a godless materialist, there is little direct evidence that these concerns affected the timing of *Origin*. Instead, we suggest that the lag to publication was all about Darwin becoming the scientist he had to be—a necessity first glimpsed and then grasped.

FIRSTHAND MEMORIALIZING AND OVERBURDENED TESTIMONY

According to van Wyhe, none of the early accounts of Darwin’s life and work, especially those of the family and colleagues who knew him “intimately,” hints of a delay.¹¹ However, the fact that a delay went unmentioned in Darwin’s lifetime, and in biographical accounts immediately after his death, does not make it a fiction. Many important historical accounts—on the causes of the “Great War,” for example—identify factors that were neither obvious nor discernible to the actors involved. Indeed, if this were not the case, the entire discipline of history would be redundant.

When Darwin was buried at Westminster in 1882, most death notices and obituaries took a deeply reverential tone. Many biographies appeared in Britain and the United States in the following two decades, their veneration filtered through conventions of the day. Darwin was celebrated for his courage, honesty, and intellectual integrity. These portrayals were amplified by Darwin’s son Francis, who edited several volumes dedicated to his father’s memory. While van Wyhe claimed that Francis proffered no evidence of an avoidant delay, he never ruled it out either. Francis suggested that his father’s painstaking species research “would perhaps have been indefinitely prolonged, had it not been for the interference of his friends.”¹² He believed that his father *was* a procrastinator, even if he did not fully understand why.

Francis edited an autobiographical sketch that Darwin had written for his children. At the behest of his mother Emma and brother Leonard, Francis published Darwin’s views on religion as a separate, redacted chapter and omitted his father’s comments about his peers altogether. While Francis’s privileged position made his memorializing appear both authoritative and complete, in important respects it was neither. Instead, he became his father’s first biographical gatekeeper, helping to perpetuate the image Darwin cultivated as a retiring, evidence-driven figure who stood apart from controversy.¹³

¹⁰ Charles Darwin to Joseph Hooker, 5 or 12 Nov. 1845, in *The Correspondence of Charles Darwin*, ed. Frederick Burkhardt and Sydney Smith (Cambridge: Cambridge Univ. Press, 1985–) (hereafter cited as **CCD**; publication details will be given for individual volumes as they are cited), Vol. 3: 1844–1846 (1987), pp. 263–265, on p. 264.

¹¹ Van Wyhe, “Mind the Gap” (cit. n. 8), p. 178.

¹² *Ibid.*; and Francis Darwin, “Darwin, Charles Robert,” in *Dictionary of National Biography*, ed. Leslie Stephen and Sydney Lee, Vol. 14 (London: Smith, Elder, 1888), pp. 72–84, on p. 77. Regarding the early biographies see Bernard Lightman, “The Many Lives of Charles Darwin: Early Biographies and the Definitive Evolutionist,” *Notes Rec. Roy. Soc. London*, 2010, 64: 339–358.

¹³ See Nora Barlow, ed., *The Autobiography of Charles Darwin, 1809–1882* (London: Collins, 1958), pp. 11–13 (redacted chapter); and Lightman, “Many Lives of Charles Darwin.” See also Janet Browne, “Charles Darwin as a Celebrity,” *Science in Context*, 2003, 16:175–194.

When Darwin's granddaughter Nora Barlow took over as curator of his legacy, she restored Darwin's autobiographical sketch to its original state. Barlow's efforts came as scientific biography took a more personal, humanizing turn.¹⁴ The inner lives of scientists, their motivations and conflicts, were introduced as important elements of the scientific enterprise. At the same time, the notion of "Darwin's delay" emerged—Barlow was one of the first to raise it—illustrating how changing scholarly priorities shape the stories told.

Van Wyhe argued that Darwin's testimony did not convincingly demonstrate a fear-based avoidance of publication. But as several scholars wondered, is it reasonable to expect Darwin to have been entirely open and forthright? We know he was a rather guarded man who "never tried looking into [his] own mind."¹⁵ Moreover, overinterpreting an absence of evidence as evidence of absence might be one of the unique traps of the abundant Darwin archive.

Van Wyhe considered the limited set of quotations that had been cited *ad nauseam* as indicative of Darwin's nervous frame of mind, suggesting that they could be read in more innocent ways. For example, in an 1844 letter to Joseph D. Hooker, Darwin likened admitting his belief in transmutation to "confessing a murder." This "confession" became a staple of the delay thesis, a signifier of the secret burden Darwin carried.¹⁶ But where Desmond and Moore saw evidence of abject terror, van Wyhe saw self-effacing humor. There were many such jokes in Darwin's correspondence and notes, and they can read as a tacit acknowledgment that the issues at hand were delicate. Darwin's murderous confession still seemed to convey a conflicted mind-set, with his enthusiasm for revealing novel insights tempered by self-conscious, nervous restraint.

Van Wyhe's revisionist reading of such oft-repeated material illustrates the inherent ambiguities of testimony. Fragments such as Darwin's murderous confession have been selectively deployed and made to carry far more weight than they should. But to his credit, van Wyhe looked for other forms of corroborating evidence to argue against a fear-based delay.

"PRIVATE BUT NOT A SECRET"? THE POWER OF A LIST

After surveying Darwin's correspondence and notebooks, van Wyhe concluded that Darwin was far less secretive than had been assumed. From this, he inferred that Darwin could not have been as fearful as portrayed, thus undermining a foundational element of the delay thesis. Van Wyhe pictured Darwin as at ease with transmutation, openly discussing his work with scientific colleagues and taking no great steps to hide his views. He named thirty-three individuals who were privy to Darwin's "secret." They included many of Darwin's scientific colleagues, as well as his family. Most were simply listed as having been told; seven colleagues were listed as told "later" and four as "likely" told.

However, our review of the evidence reveals some interesting patterns to Darwin's disclosures. They fall neatly into two periods: those made around the time of the 1844 essay and those post-1854, after completion of his barnacle project. In between, revelations were few and far between. As we will demonstrate, Darwin was open about his views on transmutation after the barnacles, if not about his proprietary mechanism of natural selection. But the period around 1844

¹⁴ Frederick Churchill, "Darwin and the Historian," *Biological Journal of the Linnean Society*, 1982, 17:45–68; and Janet Browne, "Making Darwin: Biography and the Changing Representations of Charles Darwin," *Journal of Interdisciplinary History*, 2010, 40:347–373.

¹⁵ Darwin admitted this to Francis Galton in 1879 when returning one of Galton's questionnaires for his 1883 work *Inquiries into Human Faculty and Its Development*. See Francis Darwin, ed., *The Life and Letters of Charles Darwin*, 3 vols. (London: John Murray, 1887), Vol. 3, p. 238. On the matter of Darwin's openness see Lucy Odling-Smee, "Darwin and the Twenty-Year Publication Gap," *Nature*, 2007, 446:478–479.

¹⁶ Darwin to Hooker, 11 Jan. 1844, CCD, Vol. 3, pp. 1–3, on p. 2; and Desmond and Moore, *Darwin* (cit. n. 5), pp. 313–338.

was a different story, and it is crucial to the idea of a fear-based delay. If Darwin was being secretive and fearfully avoidant, then this is when.

Van Wyhe’s list included people Darwin merely mentioned his species work to, particularly during the early delay period. Darwin would often introduce the topic using a short stock phrase, telling correspondents that he was “collecting facts about” or “working on” varieties and species. He also used the more suggestive phrase “the origin and variation of species” in this early period but did so more often in the post-barnacles period. Van Wyhe interpreted these ambiguous statements as humility or convenient shorthand, implying that Darwin had already discussed species origins in person. But in all but a few cases, it was far from clear that Darwin had.¹⁷ Others van Wyhe counted were told even less.¹⁸ Treating recipients of these sketchy details as confidants assumes a present-day understanding of Darwin’s private theorizing—theorizing that included the heterodox ideas of species-to-species transmutation and descent from common stock. While we might be familiar with what Darwin had in mind when he used such phrases, his correspondents could interpret them as perfectly consistent with a creationist viewpoint and a quite mainstream interest in species issues. Darwin’s shorthand otherwise appeared deliberately generic—characteristic of a man unwilling to give much away, much less provoke or offend. Although the written record is not likely to account for all of Darwin’s disclosures, the low standard of disclosure that van Wyhe adopted with his lumped-together list appeared to give an inflated sense of Darwin’s openness in this early period.¹⁹

THE CHRONOLOGY AND CONTENT OF DARWIN’S EARLY DISCLOSURES

Darwin’s earliest confidant was probably Charles Lyell. Lyell opposed transmutation on “philosophical” rather than religious grounds. Correspondence between Darwin and Lyell suggests that they were on candid terms regarding species origins, including the issue of transmutation. Darwin was also very open with the museum taxonomist George R. Waterhouse in 1843.²⁰ He likewise confided in Joseph Hooker in early 1844 and then in Leonard Jenyns; both were given

¹⁷ Darwin’s correspondence with Leonard Horner and Edward Cressy does not suggest that they knew of his species thinking during the gap years. Darwin’s dealings with Hugh Strickland illustrate the problems associated with notebook evidence. Darwin recorded his reactions to discussions but not necessarily what was said. Darwin’s notebooks suggest that his discussions with Strickland were compatible with a special creation viewpoint. See DAR205.9.149, Apr. 1842, Darwin Papers, University of Cambridge Library (these materials are also available at Darwin Online: <http://darwin-online.org.uk>). His correspondence with Strickland during the gap years does not mention transmutation. The notebook evidence van Wyhe cited for those Darwin “likely told”—William Herbert, William Yarrell, Hugh Cuming, and John Lubbock—was likewise inconclusive. Herbert and Darwin discussed species issues, but we do not know how much Darwin revealed. Darwin may have been candid in this early period with Yarrell, but the evidence is far from conclusive. Darwin’s limited correspondence with Cuming did not mention species issues. While Darwin’s notebooks did indicate that he canvassed species issues with Cuming early on, there is no evidence that this included any discussion of transmutation. Darwin’s notebooks indicate that he may have discussed species issues with Lubbock, but not until 1855.

¹⁸ E. L. Layard and C. A. Murray fall into this minimal disclosure category; they were informed only in the lead-up to the publication of *Origin*. None of Darwin’s letters to his German translator Ernst Dieffenbach conveyed his theorizing on species. Darwin seemed to distrust the man who helped popularized his *Beagle* journals in Germany. See Darwin to Hooker, 22 Jan. 1845, 19 May 1846, CCD, Vol. 3, pp. 127–128, 319–320.

¹⁹ Van Wyhe’s inclusion of Darwin’s extended family did not address his *collegial* openness. While Emma Darwin may have been well acquainted with her husband’s views, she could be trusted to keep quiet. It is not clear how much other extended family members were told in this early period. Darwin’s correspondence with Hensleigh Wedgewood suggested that they had discussed species issues in the lead-up to *Origin*, but it was not clear that they had done so in the earlier pre-barnacles period, and the notebook evidence is ambiguous. See DAR205.5.60, Apr. 1843. Darwin’s notes in this period relating to Hensleigh’s wife Elizabeth were even more ambiguous. See DAR205.5.30, June 1840. The inclusion of Darwin’s manuscript copyists Mr. Fletcher and Ebenezer Norman looks like padding.

²⁰ Darwin to Charles Lyell, 8 Oct. 1845, CCD, Vol. 3, pp. 258–259; and Darwin to George R. Waterhouse, 26 July 1843, CCD, Vol. 2: 1837–1843 (1986), pp. 375–377.

self-consciously convoluted confessions, including the idea of descent from common stock. Darwin did not give either man much detail about natural selection via Malthusian struggle but was clearly willing to do so, since he offered both Hooker and Jenyns his 1844 essay.

In November 1845 Darwin also cautiously revealed his thoughts on transmutation to Charles J. F. Bunbury. Bunbury's memoirs indicate that while Darwin was cagey, he "avowed himself to some extent a believer in the transmutation of species, though not, he said, according to either the doctrine of Lamarck or *Vestiges*."²¹ It also appears that Darwin discussed species issues with Edward Forbes, Hugh Falconer, and possibly William Lonsdale in the 1844–1847 period.

There were others in Darwin's collegial network that one might expect to have been informed but were not, including John S. Henslow. In many respects, Darwin and Henslow could not have been closer. A lifelong friendship had been forged at Cambridge after Darwin took Henslow's botany classes and became a fixture at his Friday night gatherings. Van Wyhe claimed that Henslow knew about Darwin's leanings early on; in a letter dated 10 November 1839, Darwin alluded to his interest in "the origin & variation of species."²² However, subsequent correspondence does not contain even the most tangential reference to transmutation until 1856.²³ It appeared that Henslow became familiar with Darwin's thinking only following the joint presentation of Darwin's and Wallace's work in July 1858. Since Henslow was the epitome of conservative Anglican science and unlikely to be sympathetic to transmutation, Darwin's restraint is understandable. Similarly, Darwin was reticent with his clerical cousin William D. Fox, indicating only that he was interested in writing about "varieties of species" and collecting facts to this end.²⁴

Darwin was just as reserved in the early gap period with most of his other conservative colleagues. He certainly never confided in Adam Sedgwick. He was also very guarded with the highly regarded comparative anatomist Richard Owen, even though they were on friendly terms during the mid-1840s. Darwin had warily trialed Owen as a potential ally but subsequently told him very little.²⁵ Owen would swiftly transform into Darwin's archenemy following the success of *Origin*.

Our review does not support the inclusion of many names on van Wyhe's list. The evidence for Owen, Ernst Dieffenbach, and Leonard Horner is very limited across the entire gap period. And while Darwin was very close to Henslow, Fox, and Hensleigh Wedgwood, he furnished them only with minimal details, especially early on. Van Wyhe wrote: "it is abundantly clear that Darwin usually kept natural selection to himself while being frank that he believed in transmutation."²⁶ But during the crucial early period, Darwin was quite guarded about airing the notion of transmutation. He revealed his commitment to transmutation to only five colleagues during the early period; they can be ranked according to how open Darwin was with them. First is Hooker, the only one who read Darwin's 1844 essay. Next is Jenyns, who was offered the essay but apparently never read it, followed by Lyell, Waterhouse, and Bunbury.

²¹ Charles J. F. Bunbury, *The Memorials of C. J. F. Bunbury*, Vol. 2: *Middle Life*, Pt. 1, ed. Frances H. Bunbury and Katherine H. Lyell (New York: Cambridge Univ. Press, 2011), p. 77.

²² Darwin to John S. Henslow, 10 Nov. 1839, CCD, Vol. 2, pp. 237–238, on p. 238.

²³ Darwin withheld key aspects of his species theory even at this stage. See Darwin to Henslow, 23 Aug./Sept. 1855, CCD, Vol. 5: 1851–1855 (1989), pp. 415–416.

²⁴ See Darwin to Henslow, 4 Aug. 1858, CCD, Vol. 7: 1858–1859 (1991), p. 146; and Darwin to William D. Fox, 25 Jan. 1841, CCD, Vol. 2, pp. 279–280.

²⁵ Darwin's correspondence with Owen during the gap years contained no mention of transmutation. When presenting a copy of *Origin* to Owen in 1859, Darwin hoped he would not find it "abominable." See Darwin to Richard Owen, 11 Nov. 1859, CCD, Vol. 7, p. 371.

²⁶ Van Wyhe, "Mind the Gap" (cit. n. 8), p. 185.

Darwin may have also informed Forbes, Falconer, and Lonsdale early on, but the evidence is inconclusive.

Our slimmed-down list suggests that Darwin’s early revelations were driven by a range of strategic considerations rather than indiscriminating candor. His early confidants tended toward the liberal end of the spectrum in political and scientific matters but were hardly closet transmutationists. Several would become key members of Darwin’s growing collegial network. At this point, apart from these few, Darwin’s species theory *was* a secret.

DARWIN’S WORK PRIORITIES AND EARLY DISCLOSURES

After completing his species essay in July 1844, Darwin returned to the last of his *Beagle*-related work. He finished drafting a volume on the geology of South America in April 1845 and revised his acclaimed *Journal of Researches* later that year. He then went back to the South American geology volume, which was published in the second half of 1846. But species theorizing was never far from Darwin’s mind during this period. In broaching the subject with selected colleagues, Darwin was looking for more than just pragmatic assistance in gathering specimens and data; he was looking for constructive theoretical input and sympathetic allies and trying to get a sense of the opposition he was likely to encounter. These considerations played out with a greater or lesser emphasis according to the circumstances and purposes of his disclosures, dictating how much he was prepared to give away.

Darwin’s most important sounding board, Joseph Hooker, was privy to it all. Although Darwin did not know Hooker well when he made his murderous confession, the basis for their friendship had been very much prepared. Hooker met Darwin in 1839, just before Hooker embarked on the *Erebus*.²⁷ Darwin sensed a kindred spirit in the young botanist. After Hooker returned from Antarctica in September 1843, the two struck up a correspondence that led to Darwin’s murderous confession in 1844. At Darwin’s invitation, Hooker began a productive series of visits to Down House. Darwin set Hooker small evidence-gathering tasks, encouraging him to see things through transformist eyes. Darwin did not expect to win Hooker over, at least not right away. There was an interaction of equals: two worldly naturalists trading information and sparring over interpretations, while holding each other in the highest regard.

In contrast, Darwin had sought to convert George Waterhouse. Darwin had great respect for Waterhouse as an entomologist and valued his expertise in comparative anatomy and paleontology. But Darwin thought Waterhouse lacked an appreciation of the bigger picture and was particularly skeptical about his engagement with William MacLeay’s Quinarian system. Darwin’s attempts to re-educate Waterhouse came with a frank admission of his own heterodox views on species: “According to my opinion, (which I give every one leave to hoot at, like I should have, six years since, hooted at them, for holding like views) classification consists in grouping beings according to their actual *relationship*, ie their consanguinity, or descent from common stocks.”²⁸

Darwin was seeking out like-minded peers with whom he could exchange ideas. Forbes, Falconer, Bunbury, and Lonsdale were similar in terms of background, intellectual openness, and rising prominence. Forbes, a lecturer in botany at King’s College, was a natural historian. Darwin and Forbes had both trained in medicine at Edinburgh and shared a mutual interest in the distribution and variability of species. Although the evidence is equivocal, it seems likely that

²⁷ See R. J. Berry, “Joseph Hooker: One of Charles Darwin’s Best Friends,” *Curtis’s Botanical Magazine*, 2009, 26:142–174.

²⁸ Darwin to Waterhouse, 3 or 17 Dec. 1843, CCD, Vol. 2, pp. 415–416; and Darwin to Waterhouse, 26 July 1843, *ibid.*, pp. 375–377, on p. 376.

Darwin raised transmutation with Forbes.²⁹ In late November 1845 Darwin invited Hooker to Down for a weekend with “Forbes, Falconer and perhaps Waterhouse,” adding that he would then have “the four most rising naturalists in England round my table and very much enjoy it.” Darwin’s notes suggested that he enjoyed jousting with Forbes and the paleontologist Falconer.³⁰ It was probably one of many occasions at Down in this period when speculation ran fast and furious. Charles Bunbury moved in similar social circles to Darwin, as did the more senior William Lonsdale, an expert in geology who was the curator, librarian, and indexer at the Geological Society.³¹

Leonard Jenyns was the exception among Darwin’s early confidants, a Cambridge Paleyite and the only man of the cloth. Yet Darwin was prepared to share everything with him.³² Why did Darwin confide in Jenyns rather than Henslow? The reasons appear to be generational and personal. Darwin probably feared upsetting Henslow, an influential father figure with a deeply religious worldview. Jenyns was younger and less likely to reprove, and Darwin had always tried to impress him, trading information and beetle specimens. Jenyns and Darwin also shared a special bond. Jenyns had been a pupil of Henslow and was Henslow’s original choice to go on the *Beagle*. When Jenyns reluctantly declined, Henslow nominated Darwin instead. As Darwin’s fame and scientific reputation grew, Jenyns probably wondered what might have been. Did Darwin feel he needed to repay Jenyns with his candor? Perhaps; but Darwin’s motivations were probably more strategic than that.

Darwin understood that he needed to test his conjectures and gauge likely objections. He had already developed a tendency to argue by antithesis, building a case for descent from common stock by dismantling the assumptions underlying design stasis. Aside from any personal obligations Darwin may have felt toward Jenyns, he was keen to trial his ideas with at least one friendly and respected representative of Anglican natural theology. This would help him anticipate the kind of objections that would come from the likes of Adam Sedgwick and Samuel Wilberforce. Darwin’s exchange with Jenyns occurred just prior to the sensational impact of *Vestiges*, which would have rendered this need moot. Darwin chose well. Jenyns did not appear shocked by Darwin’s admissions but remained skeptical nonetheless.³³ Darwin subsequently suggested that he might show Jenyns his 1844 essay, a mooted offer Jenyns seemed quietly to ignore. Their interaction on species issues did not go much further than this brief exchange—suggesting that Darwin’s primary objective was simply canvassing the critical reaction of his clerical friend. In contrast, Darwin spent months pestering Hooker before persuading him to read the essay. Obviously, Darwin hoped to gain much more from him than from Jenyns.

The fact that Darwin mentioned his interest in species to a smattering of others in this early period was hardly a sign of fearless openness. Darwin was in the process of transforming himself into the most effective of networkers. He “prepped” correspondents on the information or specimens he required and piqued their interest by offering details of his own interests and research. But Darwin was very careful about what he revealed of his species work to these professional acquaintances, offering only the most cryptic clues to his theoretical commitments.

Our conclusion is that Darwin was not delaying in the 1844–1847 period. While he busied himself with various *Beagle*-related projects and side interests, his species theory was occupying a significant amount of his time and intellectual energy, even after he put his essay aside. Much

²⁹ See DAR205.9.185, 20 Mar. 1844; DAR205.5.103, n.d.; and DAR45.58, Dec. 1844.

³⁰ Darwin to Hooker, 25 Nov. 1845, CCD, Vol. 3, p. 272; and DAR205.9.187, 20 Mar. 1844 (enjoyment).

³¹ Darwin’s notes for February 1842 do not clearly indicate that he told Lonsdale or Bunbury much at this point. See DAR205.9.146.

³² Darwin to Leonard Jenyns, 12 Oct. 1844, CCD, Vol. 3, pp. 67–68.

³³ Darwin to Jenyns, 25 Nov. 1844, CCD, Vol. 3, pp. 84–85.

of this species work involved various forms of intellectual exchange and the courting and conversion of allies, activities now regarded as a central part of the practice of science.

TRIPPING OVER THE GAP

According to van Wyhe, in late 1846 Darwin did not envisage publishing on species until 1853 at the earliest. He needed to deal with a backlog of long-standing tasks. This included the last of his *Beagle*-related publications on invertebrates, which was to be part of the Treasury-supported “Zoology” series, but a funding shortfall saw Darwin take on the task independently. It was initially envisaged as a short-term study of a limited set of “lower marine animals” that would take “some months, perhaps a year.”³⁴ As van Wyhe had it, the project gradually expanded to encompass the entire Cirripedia subclass. While held up by ill health and bereavement, Darwin returned to his species theory in due course, finally going public with the presentation of the joint paper in July 1858 and the publication of *Origin* the following year. But Darwin’s work timetable was a movable feast—telescoping out in the late 1840s and early 1850s, then abruptly foreshortening in mid-1858 when it seemed that Wallace might preempt him.

We agree that it is “important to date the change from Darwin’s intention to do some barnacle papers to his conducting a full group project,” not least because it radically lengthened his estimate of how long the project would take. For van Wyhe, this was not “a single conscious decision . . . it was a gradual process,” the cumulative function of Darwin’s deepening interest, the need for broader comparisons, and the encouragement of his peers. Van Wyhe stressed that no additional reasons were needed. For example, the suggestion that Darwin changed his mind because he thought he “required the added status and experience to proceed with his species work” was unnecessary and not supported by the evidence.³⁵ We take a different view.

If Darwin’s elastic schedule is to be at least part of the reason for the delay, then reconstructing an accurate timeline is crucial. Van Wyhe suggested that Darwin “seems to have moved to take up the whole group between October and December 1846,” quite soon after he got under way. But Darwin actually made this decision just before Christmas 1847. Darwin had continued to pass up opportunities to obtain a wider range of specimens that year because he “did not then think of describing all the species of Cirripedia.”³⁶ He did not inform Hooker of any such decision before Hooker left for India in early November, and his journal, notebooks, and correspondence are silent on this score through to early December 1847. The clearest pointer is Darwin’s letter of 18 December 1847 to John E. Gray and the British Museum’s trustees. The decision thus appeared to be a relatively sudden change of heart that would prove a pivotal moment in Darwin’s career. While the explanatory factors van Wyhe singled out no doubt exercised a cumulative influence, they are not as adequate as he suggested. In fact, the more one explores the lead-up to Darwin’s important decision, the more intriguing it becomes.

The years leading up to 1844 had been good for Darwin. His *Beagle* publications had secured him a significant reputation as a naturalist voyager, with a public expertise grounded in geology. But this charmed run ended with a series of blows to his confidence. The first was the unexpected appearance of the anonymously authored *Vestiges* late in 1844. *Vestiges* was a taboo-breaking bombshell. Its readership spanned the classes, from royalty to the radical underbelly, and it paved the way for more open discussions of transmutation. Darwin’s conflicted reaction to *Vestiges* is telling. As Browne described it, Darwin was dismayed to see that he was not the only one able to connect a broad array of evidence to explain the origins of species. He remained res-

³⁴ Darwin to Hooker, 2 Oct. 1846, CCD, Vol. 3, pp. 346–347, on p. 346.

³⁵ Van Wyhe, “Mind the Gap” (cit. n. 8), pp. 191, 192.

³⁶ *Ibid.*, p. 191; and Darwin to J. G. Forchhammer, 12 Nov. 1849, CCD, Vol. 4: 1847–1850 (1988), pp. 277–278, on p. 278.

olutely unamused by his identification as a potential author and privately disdainful about the author's grasp of the science. Reviews tended to split along political and religious lines. Reformist publications like the *Lancet* heralded *Vestiges* as a "very remarkable book" and a "breath of fresh air." Conservative periodicals like the *London Medical Gazette* lambasted the "sophistry of this anonymous pseudo-philosopher."³⁷ Many reputable naturalists—especially those with mixed opinions—avoided public comment.³⁸ However, John Herschel went out of his way strongly to condemn the scientific misconceptions and extravagant speculations of *Vestiges* in his presidential address to the British Association in 1845.³⁹

Vestiges left room for an initial designer and downplayed its materialistic implications by making the upward march of species as unthreatening as the birth of a child into a middle-class Victorian family. It advanced a form of species modification that conflated uniserial parallelism with more sophisticated notions of divergent development and structural archetypes. Modification came about through sudden arrests or advances in the developmental sequence. While this was in stark contrast to Darwin's mechanism of natural selection and branching descent, the similarities were uncomfortable enough for Darwin to worry over the criticism *Vestiges* received. He would continue to monitor critical assessments of "improved" versions of the work, most notably Thomas Henry Huxley's damning appraisal of the tenth edition.⁴⁰

Paleyite tracts such as the Bridgewater Treatises had incorporated a progressivist interpretation of the parallels between embryological development, taxonomic orderings, and the sequence of the fossil record. But these parallels were seen as the result of discontinuous special creation; continuity existed only in the mind of God.⁴¹ *Vestiges* debased these divine notions with a romantic naturalism that all but removed God's hand from daily affairs. It marked the author as rather crass, quite possibly a Unitarian comparative anatomist with a medical background, and certainly an outsider to the conservative Oxbridge elite.

Sensing the danger, Adam Sedgwick gave *Vestiges* a savage review in mid-1845, and Darwin took careful note. Although Darwin thought Sedgwick's review was full of heavy-handed sermonizing, he still conceded that it was "a grand piece of argument against mutability of species; & I read it with fear & trembling, but was well pleased to find, that I had not overlooked any of the arguments, though I had put them to myself as feebly as milk & water."⁴²

Darwin's comments were a mixture of self-effacing satisfaction tinged with relief. He seemed genuinely worried that Sedgwick might advance unanticipated counterarguments fatal to his theory but was pleased to see that he did not. Sedgwick gave Darwin a clear idea of the opposition he would face in mounting his case for transmutation and the care that would be needed in doing so. Suitably chastened, Darwin took Hooker's criticism of the species speculations of Frédéric Gérard quite personally—despite Hooker's protestations. Darwin lamented: "How painfully (to me) true is your remark that no one has hardly a right to examine the question of spe-

³⁷ Browne, *Charles Darwin* (cit. n. 7), pp. 416–462. For conflicting reviews see J. Onwhyn, "Review of *Vestiges of the Natural History of Creation*," *Lancet*, 23 Nov. 1844, pp. 265–266; and "Review of *Explanations, A Sequel*," *London Medical Gazette*, 12 June 1946, pp. 1044–1045. See also James A. Secord, *Victorian Sensation: The Extraordinary Publication, Reception, and Secret Authorship of "Vestiges of the Natural History of Creation"* (Chicago: Univ. Chicago Press, 2000).

³⁸ Hooker told Darwin that he thought *Vestiges* was good value for a hack work, despite its errors: Hooker to Darwin, 30 Dec. 1844, CCD, Vol. 3, pp. 101–105. Conversely, *Vestiges* put Owen in a difficult position; his reaction was even more conflicted than Darwin's. See Secord, *Victorian Sensation*, pp. 421–426.

³⁹ John Herschel, "Presidential Address," *Report of the Fifteenth Meeting of British Association for the Advancement of Science* (London: John Murray, 1846), pp. xxxvi–xxxviii.

⁴⁰ See Darwin to Thomas Henry Huxley, 2 Sept. 1854, CCD, Vol. 5, pp. 212–214.

⁴¹ Evelleen Richards, *Darwin and the Making of Sexual Selection* (Chicago: Univ. Chicago Press, 2017), Ch. 9.

⁴² Darwin to Lyell, 8 Oct. 1845, CCD, Vol. 3, pp. 258–259, on p. 258.

cies who has not minutely described many.”⁴³ Darwin’s response suggests that he was deeply troubled by the imputation that he did not possess the necessary authority for grand species theorizing.

TURNING TO THE BARNACLES

In this humble frame of mind, Darwin turned to the last undescribed specimens of his *Beagle* collection. The trigger was a strange creature Darwin had collected on the Chonos Archipelago off the coast of Chile in January 1835. He tentatively ascribed this “little monster” to the Balanidae family, a Cirripedia taxa that included most of the common sessile barnacles that attached themselves to rocks and ships. A complete overhaul of the whole group was seen as sorely needed; Louis Agassiz had called for such work in September 1846, just as Darwin was about to get under way.⁴⁴

Darwin had a long-standing interest in marine invertebrates and had planned to do at least part of the descriptive cataloguing work himself. The fact that he had recognized this “little monster” as a cirripede was testament to his knowledge even while voyaging on the *Beagle*. This tiny species bored its way into the shell of its gastropod host but had no shell itself.⁴⁵ Darwin had discovered a new kind of cirripede. He initially dubbed it “*Arthrobalanus*” and later renamed it *Cryptophtialus minutus*.

Darwin envisaged the project as a limited anatomical study that would boost his zoological credentials and provide another small set of examples for his species theory. He drafted a short anatomical paper on “Mr. Arthrobalanus” in October 1846, but it was never published and no copy survives. Darwin told Hooker that he intended to send his draft to Owen, but it is not clear that he ever did so. In any case, Darwin soon asked Owen for related specimens, with a view to doing “five or six genera” for comparative purposes.⁴⁶

At first the work was unexpectedly intoxicating. Darwin’s letters to Hooker late in 1846 extolled the joy of “pure observation”; he was happy to get his hands dirty as a *bona fide* biological specialist.⁴⁷ Before ill health held him up in late March and early April 1847, Darwin had worked his way through a handful of genera with direct comparative relevance, mainly in the Balanidae family.⁴⁸ But after April, Darwin’s work journal indicates that he tackled no new Cirripedia species until the end of the year. Darwin upgraded his microscopic equipment in the meantime, suggesting that he wanted to tackle the project more seriously. Darwin had been in the habit of sending his *Beagle* haul to fellow naturalists for microscopic analysis, generally deferring to the physician and physiologist William B. Carpenter and the zoologist and compar-

⁴³ Hooker to Darwin, 4–9 Sept. 1845, CCD, Vol. 3, pp. 250–252, on p. 250; and Darwin to Hooker, 10 Sept. 1845, *ibid.*, pp. 252–253, on p. 253.

⁴⁴ Marsha Richmond, “Darwin’s Study of the Cirripedia,” CCD, Vol. 4, App. II, pp. 388–409; and Christoph Irmscher, *Louis Agassiz: Creator of American Science* (Boston: Houghton Mifflin Harcourt, 2013), p. 126.

⁴⁵ See Darwin’s original description: DAR31.2: 305–314. See also Jean Deutsch, “Darwin and Barnacles,” *Comptes Rendus Biologies*, 2010, 333:99–106; and Charles Darwin, *Living Cirripedia*, Vol. 1 (London: Ray Society, 1851 [1852]), p. 566 (regarding the oddity of the dating see http://darwin-online.org.uk/EditorialIntroductions/Newman_barnacledates.html).

⁴⁶ For Darwin’s original plan for a limited anatomical study see Darwin to Hooker, 5 or 12 Nov. 1845, CCD, Vol. 3, pp. 263–265. Regarding the paper see Darwin, “Journal,” *ibid.*, App. II, p. 397; and Darwin to Hooker, 26 Oct. 1846, *ibid.*, pp. 357–358. For the request to Owen see Darwin to Owen, 25 Nov. 1846, *ibid.*, pp. 372–373.

⁴⁷ Darwin to Hooker, 6 Nov. 1846, CCD, Vol. 3, pp. 363–364. See also Darwin to Robert FitzRoy, 28 Oct. 1846, *ibid.*, pp. 359–360.

⁴⁸ After putting “*Arthrobalanus*” aside, Darwin tackled the *Conia* (syn.: *Tetraclita*) and *Megatrema* (syn.: *Pyrgoma*) genera in the Balanidae family in late 1846. He examined *Balanus* specimens in February 1847 and *Acasta* and *Clisia* specimens the following month. The last specimens Darwin listed for 1847, tackled in April, were *Tubinicella Coronula*. See Darwin, “Journal,” CCD, Vol. 4, App. I, pp. 383–384.

ative anatomist Christian Ehrenberg.⁴⁹ But now he determined to take it on himself. He purchased a new, high-magnification instrument with achromatically corrected lenses in April 1847. It was hugely expensive, the best available. He also updated his simple instrument in January 1848. Darwin developed a methodical, stepwise approach that drew from his old mentor Robert Brown and from Ehrenberg. He would work up from the naked eye through the higher magnification powers of his microscopes, first describing gross features and structural homologies and then identifying what were often extraordinarily small details. Darwin's study was bound to be innovative; it was the first systematic microscopic study of both the external and the internal features of these sea creatures.

Other issues would distract Darwin during this period and add to his deflating mood. In May 1847 Hooker took him to task regarding his theories about coal. In July Darwin stood by as Robert Chambers—now widely suspected to be the author of *Vestiges*—took a mauling at the British Association meeting at Oxford. Darwin became increasingly preoccupied by geological matters when his account of the parallel “roads” of Glen Roy began to be eclipsed by the sensational Ice Age theory of the newly appointed Harvard professor Louis Agassiz. Darwin's 1839 Glen Roy paper was his first important research publication, greatly influenced by Lyell's uniformitarianism. However, the careful observations of the Scottish geologist David Milne reinforced the idea that the “roads” were the result of a glacial lake rather than raised former beaches. The potential loss of face in this public debate obviously perturbed Darwin, as his defensive letters to Lyell and the *Scotsman* attested.⁵⁰

Darwin appeared to make little obvious progress with the barnacles in the latter part of 1847.⁵¹ It was becoming increasingly clear that to understand his unusual Chilean specimen fully he would need to go beyond the narrow confines of the common Balanidae species. A personal audience with Owen had reinforced how important it was to situate “Arthrobalanus” within the Cirripedia and with related Crustacea. Other specialists in the field—such as Hugh Cuming and Samuel Stutchbury—encouraged Darwin by donating their collections. More specimens arrived from diverse sources, increasing the sense of obligation.

COMMITTING TO THE WHOLE GROUP

Darwin's decision to commit to the whole group dramatically expanded the project's scope. Early in 1847, his schedule had extended from his initial projection only slightly. He still assured Ernst Dieffenbach that he would return to species work “in a year's time or so.” But in October 1848, ten months after making his decision to do the whole group, Darwin told Agassiz that it would take him another two to three years.⁵² While Darwin was allowing for ill health, he was underestimating the toll it would take. Moreover, the open-ended process of gathering specimens, especially scarce fossils, made it difficult for him to foresee the scale of the task ahead. And Darwin certainly could not have anticipated the tragedy of his daughter Annie's death. Given that Darwin's significantly lengthened timetable was *still* wishfully optimistic, his decision to tackle the whole group does indeed account for the project's lengthy duration. The problem becomes explaining Darwin's decision.

⁴⁹ Boris Jardine, “Between the *Beagle* and the Barnacle: Darwin's Microscopy, 1837–1854,” *Studies in History and Philosophy of Science*, 2009, 40:382–395.

⁵⁰ See Darwin to Hooker, 6 May 1847, CCD, Vol. 4, pp. 40–41; Secord, *Victorian Sensation* (cit. n. 37), pp. 434–436 (BA meeting); Darwin to Lyell, 8 Sept. 1847, CCD, Vol. 4, pp. 65–70; and Darwin to the *Scotsman*, 20 Sept. 1847, *ibid.*, pp. 76–80.

⁵¹ Richmond, “Darwin's Study of the Cirripedia” (cit. n. 44).

⁵² Darwin to Ernst Dieffenbach, 9 Feb. 1847, CCD, Vol. 4, pp. 12–13, on p. 13; and Darwin to Louis Agassiz, 22 Oct. 1848, *ibid.*, pp. 178–181. On the expansion of the barnacles project see Alan C. Love, “Darwin and ‘Cirripedia’ Prior to 1846: Exploring the Origins of the Barnacle Research,” *J. Hist. Biol.*, 2002, 35:251–289.

The clearest precipitating factor was John Gray’s offer of his own collection and his pledge to arrange access to the British Museum’s public collection. Darwin got to know Gray as a frequent presenter at the Zoological Society and had helped him obtain his position as Keeper of the Zoological Collections at the museum. Gray had been collecting Cirripedia specimens for his own research but was now too busy to work on them. Darwin had initially refused Gray’s offer. It is unclear when Gray first made it or why Darwin hesitated. Perhaps Darwin thought he lacked the necessary taxonomic expertise. Darwin felt that specialists such as Gray had “a *perfect right*” to describe this intriguing group first. Gray’s decision to bow out cleared that impediment at least.⁵³ Perhaps Darwin also worried that he could not trust Gray to hand over the whole group unconditionally—a fear partially realized. Or perhaps he thought the task too tedious and time consuming.

By the end of 1847 Darwin overcame his doubts, formally requesting access to the British Museum’s collection on 18 December 1847. He must have felt confident of approval from the museum’s trustees before they met to consider his request on 29 January 1848. Darwin’s work journal recorded that he crossed the taxonomic Rubicon on 18 December 1847 to begin a systematic study of the Lepadidae family, venturing beyond the comparative needs dictated by his initial classification of “*Arthrobalanus*.” Darwin began actively to trawl for the most complete set of specimens he could find, including the fossil collections he had initially turned down.

Darwin later shared the credit for his decision with many of his colleagues. Cuming and Stutchbury were thanked for donating their collections, acts of generosity that predated Gray’s offer. Darwin also cited Agassiz’s call for a complete overhaul of the barnacles as a key prod, even though Agassiz had made it fifteen months prior to Darwin’s decision. Others, especially Hooker, got their share of the credit or the “blame.”⁵⁴ In sum, Darwin was characteristically opaque when it came to explaining just what made him steel himself in December 1847.

The explanation that seems least supported by the evidence is that exciting discoveries relating to sexual differentiation prompted Darwin to commit to the whole group. Van Wyhe largely ignored this suggestion, having dated Darwin’s commitment early in the project. However, Janet Browne argued that the species-theory relevance of these findings prevented Darwin from winding up the project and instead encouraged him to make “a complete scientific survey of the group.”⁵⁵ But a close examination of the timing of Darwin’s findings indicates that they could not have played such a role in his decision.

Up to the 1830s, cirripedes had generally been thought to be hermaphrodites, although there was some debate over the nature of their reproductive organs. Subsequent research linked them with Crustacea, which generally displayed separate sexes. Some naturalists, such as Henry Goodsir, suggested that cirripedes might have separate sexes. In 1843 Goodsir published a paper describing what he thought was his discovery of a small male residing in the sac of what he thought was a female *Balanus balanoides*. Darwin was well aware of Goodsir’s research and sensitized to the issue of sexual differentiation. Late in 1847 he examined *Balanus* specimens like those of Goodsir. Darwin recounted his observations to the noted French zoologist Henri Milne-Edwards, whose taxonomic perspective he had fully absorbed. According to Darwin, the organism residing in the sac of the “female” *Balanus* belonged to an entirely different species. Darwin

⁵³ Regarding Gray’s assistance see Darwin, *Living Cirripedia*, Vol. 1 (cit. n. 45), p. v. On Darwin’s initial refusal of Gray’s offer see Albert E. Gunther, “J. E. Gray, Charles Darwin, and the Cirripedes, 1846–1851,” *Notes Rec. Roy. Soc. London*, 1979, 34:53–63; and Darwin to John Gray, 29 Aug. 1848, CCD, Vol. 4, pp. 162–163, on p. 162 (“*perfect right*”).

⁵⁴ Darwin, *Living Cirripedia*, Vol. 1, p. vi (crediting Cuming and Stutchbury); and Darwin to Hooker, 12 Oct. 1849, CCD, Vol. 4, pp. 268–271.

⁵⁵ Browne, *Charles Darwin* (cit. n. 7), p. 481. Browne implied that Darwin committed to the whole group “just before Christmas” 1846 (p. 472).

described it as “Lernaea like,” without being able to identify it definitively.⁵⁶ This was the female of the species, Darwin wrote, while Goodsir’s “parasites” were actually the smaller larvae of this same species. Thus Goodsir’s work did little to dissuade Darwin that cirripedes were hermaphrodites.

It took some time before Darwin could identify sexual differentiation among Cirripedia. And he did so only after committing to the whole group. Darwin’s “eureka” moment can be dated to late March 1848. He told Henslow on 1 April that “in the last few days” he had dissected a female in the Lepadidae family (almost certainly *Ibla cumingii*) with internal parasitic males. Even more exciting findings would follow. On 10 May 1848, Darwin told Hooker of his sexually differentiated *Ibla* specimens. He then rather breathlessly recounted his findings with another species in a closely related genus (probably *Scalpellum vulgare*) that was hermaphrodite “as usual” but had microscopic “supplemental” males adhering to it. A male accompanying a hermaphrodite (rather than a female) was a new discovery: while known in plants, no one had observed it in the animal kingdom at the time. Darwin told Hooker that he “never sh^d have made this out, had not my species theory convinced me, that an hermaphrodite species must pass into a bisexual species by insensibly small stages.”⁵⁷ Other colleagues such as Lyell and Agassiz were treated to this news, but only Hooker was privy to the species theory input and implications. Darwin maintained this disciplined line in his first volume of *Living Cirripedia* three years later, appearing deliberately to misrepresent his discovery process to emphasize the observation-driven nature of his research and hide his species theory’s role in it.⁵⁸

It is important to note that all such findings took place after Darwin had braced himself for the long haul in December 1847. Whether or not he made these discoveries, it seems that he had determined to plow on. Nonetheless, they appeared to energize him through the middle part of 1848, as he worked through genus after genus. This concentrated effort would take its toll, and the New Year saw Darwin seeking respite in the chill waters of Malvern. By the middle of 1849 he would again get bogged down with more mundane specimens. And things got particularly dire after Darwin turned to the fossils in 1850. Yet he persisted.

THE EVIDENTIAL MOTIVATIONS AND DIVIDENDS OF THE BARNACLES

Van Wyhe dismissed the idea that Darwin’s barnacle project was a “necessary preparation” for his species work. But he still suggested that it gave Darwin a greater appreciation of variation as a spontaneous, omnipresent phenomenon rather than a response to geological change or geographical isolation. Richard Bellon and Costas Mannouris also insisted that the project prompted Darwin to rework the principle of divergence that explained evolutionary branching marked by an absence of transitional forms. But Robert Richards has demonstrated that Darwin already had the basics of this principle sketched out in 1844. It seems that Darwin realized that his account was unsatisfactory only after he had returned to his species work in September 1854. Darwin recalled that the significance of this problem, and its solution, came to him in a flash dur-

⁵⁶ Henry D. S. Goodsir, “On the Sexes, Organs of Reproduction, and Mode of Development, of the Cirripedes,” *Edinburgh New Philosophical Journal*, 1843, 35:88–104; and Darwin to Henri Milne-Edwards, 18 Nov. 1847, *CCD*, Vol. 4, pp. 96–98. Darwin’s familiarity with the taxonomic perspective of Milne-Edwards is reported in Richmond, “Darwin’s Study of the Cirripedia” (cit. n. 44); and Love, “Darwin and ‘Cirripedia’ Prior to 1846” (cit. n. 52).

⁵⁷ Darwin to Henslow, 1 Apr. 1848, *CCD*, Vol. 4, pp. 127–129; and Darwin to Hooker, 10 May 1848, *ibid.*, pp. 138–142, on p. 140. Regarding the novelty of the discovery see Darwin, *Living Cirripedia*, Vol. 1 (cit. n. 45), p. 214; and Richmond, “Darwin’s Study of the Cirripedia.”

⁵⁸ There were some odd inconsistencies between Darwin’s published account of 1851 and his correspondence with Henslow and Hooker in 1848. See Roderick D. Buchanan, “Darwin’s ‘Mr. Arthrobalanus’: Sexual Differentiation, Evolutionary Destiny, and the Expert Eye of the Beholder,” *J. Hist. Biol.*, 2017, 50:315–355.

ing a carriage ride in mid-1855. The crucial additional idea was that nature tended to select extremes in a manner that prevented them from being swamped back into the pack.⁵⁹ Darwin reconceptualized natural selection as a remorseless hourly scrutiny of favorable forms in the competitive economy of nature. However, this shift toward an ecological framework cannot be directly tied to Darwin’s barnacle work and his immersion in systematics.

The barnacles did help Darwin meld his taxonomic thinking with that of the key peer groups he wanted to convince. Darwin echoed Hooker’s disdain for naturalists who were fond of dividing varieties into distinct species. These mostly city-bound “splitters” tended to study flora and fauna in circumscribed areas and had a much narrower view of species limits than the well-traveled “lumpers.” Hooker derided splitters as incompetent, vainglorious “species mongers,” as did Darwin. Endless “splitting” threatened to make taxonomic classification a confusing, arbitrary exercise and to legitimate theories of species mutability. The specter of the “species mongers” was a fundamental obstacle to Hooker’s acceptance of Darwin’s species theory. Like many of his British counterparts, Hooker maintained a provisional belief in special creation as the most parsimonious explanation of species origins. Darwin needed to satisfy Hooker, who stood as a standard-bearer for the theoretically integrated, “philosophical” naturalism Darwin wanted to uphold. The “philosophical” naturalist was not just a keen observer and describer but one who developed causal theories to explain organic and geological patterns and processes.⁶⁰

Despite his belief in the impermanence of species, Darwin lumped aggressively—far more so than Cirripedia taxonomists do today. He developed a version of species type that accommodated variation by emphasizing typical or modal features but remained consistent with British preferences for empirical observation and accuracy.⁶¹ His more nuanced principle of divergence—whereby varieties settled into distinct niches—also gave taxonomic categories a genealogical basis. It made for elegantly interlocking explanations of structural homologies and geographical patterns in accordance with the broad delimitations of species approved of by “lumpers” like Hooker, Lyell, and Henslow. Darwin’s barnacle volumes were a tacit demonstration of “natural classification” based on the principle of branching descent through modification.

Some scholars have cited Darwin’s findings on sexual differentiation as a thrilling vindication of his notebook conjectures.⁶² But the sheer oddness of the rudimentary males posed many questions about sexual differentiation and the forces shaping it. These findings would prove difficult to reconcile with the concept of sexual selection Darwin later developed, being inconveniently at odds with his general assumption that males were the more evolved sex. Darwin only touched on the sexual differentiation he found in the Cirripedia in both *Origin* and *Descent*, even though it was the most notable discovery of the whole project. It was not until 1873 that he seriously attempted to situate these findings in an evolutionary framework.⁶³

Darwin thought of his barnacle project as quite distinct from his species work. In retrospect, he doubted whether the barnacles were worth “so much time” and downplayed their contri-

⁵⁹ See Richard Bellon, “Joseph Hooker Takes a ‘Fixed Post’: Transmutation and the ‘Present Unsatisfactory State of Systematic Botany,’ 1844–1860,” *J. Hist. Biol.*, 2006, 39:1–39; Costas Mannouris, “Darwin’s ‘Beloved Barnacles’: Tough Lessons in Variation,” *History and Philosophy of the Life Sciences*, 2011, 33:51–70; and Robert J. Richards, “Darwin’s Principles of Divergence and Natural Selection: Why Fodor Was Almost Right,” *Studies in History and Philosophy of Biological and Biomedical Sciences*, 2012, 43:256–268.

⁶⁰ See Darwin to Hugh Strickland, 4 Feb. 1849, CCD, Vol. 4, pp. 206–209, on p. 207 (“species mongers”); and Bellon, “Joseph Hooker Takes a ‘Fixed Post.’” Hooker flattered Darwin, labeling Darwin’s botany “philosophical” while modestly describing his own efforts as “professional”: Hooker to Darwin, 11 Apr. 1857, CCD, Vol. 6: 1856–1857 (1990), pp. 369–370.

⁶¹ See Darwin to Hooker, 25 Sept. 1853, CCD, Vol. 5, pp. 155–157; and Mannouris, “Darwin’s ‘Beloved Barnacles’” (cit. n. 59).

⁶² Browne, *Charles Darwin* (cit. n. 7), Ch. 20.

⁶³ Buchanan, “Darwin’s ‘Mr. Arthrobalanus’” (cit. n. 58); and Charles Darwin, “On the Males and Complemental Males of Certain Cirripedes, and on Rudimentary Structures,” *Nature*, 1873, 8:431–432.

bution to his species theorizing. They did provide useful support for his discussion of systematics in *Origin* and a greater appreciation of the incessant nature of variation. The barnacles did not solve the problems of his species theory as much as sensitize him to the way he would have to solve them and whom he would need to satisfy. But there is little evidence to suggest that he started the project with these issues in mind, nor did it appear that such thinking played a significant role in his decision to do the whole group. By December 1847 Darwin had worked through a limited number of species, mainly within the Balanidae family, and had only just begun to get acquainted with the range of variation Cirripedia displayed. The extensive varieties of the Lepadidae and other families still awaited. And Darwin appreciated the close and continuous ordering the Balanidae exhibited fully only when he exhaustively tackled them near the end of the project. While he was alert to the possibility of sexual differentiation among his barnacles from the beginning, he still committed to the whole group before he found it. Moreover, the differentiation he uncovered was not quite what he expected. In sum, the evidential and theoretical payoffs of the project did not stem from a planned attempt to deal with the outstanding issues of his species theory: not at the start, and not in the lead-up to his decision to do the whole group. All outstanding issues—such as the problem of neuter insects, Darwin’s “greatest special difficulty”—had to wait until he finished with the Cirripedia.⁶⁴ The findings and insights Darwin did glean from the barnacles were the unanticipated rewards of a huge task taken on for other reasons.

THE BARNACLES AS A TRAINING, CREDENTIALING, AND SELF-FASHIONING EXERCISE

Darwin was always mindful of the way the barnacles project might buttress his standing among his peers, and these field-oriented motivations played a central role in the way it escalated. Darwin’s peers certainly encouraged him; but only because he was already worried about his lack of experience and expertise did he allow them to push him beyond the small-scale study he initially envisaged. The epic nature of the project can thus be taken as indicative of the ways in which Darwin felt unready, for he felt more than ready to handle the “very presumptuous” nature of his species theorizing afterward.⁶⁵ So while Darwin’s colleagues may have become more receptive to the idea of transmutation and the social and political climate may have calmed, the post-barnacles change was mainly in Darwin himself. When he returned to species work, he did so with renewed vigor and far greater self-assurance.

As Marsha Richmond observed, describing a group had become a rite of passage among naturalists in Britain and abroad.⁶⁶ Overhauling the Cirripedia was Darwin’s initiation. Darwin never explicitly outlined what he hoped would be his return. It would have been ungentlemanly to do so. The most candid insight is revealed in an exchange between Darwin and Gray. Gray had presented two short papers on cirripedes on 14 March 1848 at the Zoological Society of London. Darwin had been warned that Gray was planning to preempt his work. Given that Gray’s papers were on cirripedes closely related to *Ibla* and *Scalpellum*, Darwin probably feared that Gray would scoop him on sexual differentiation. Gray reassured Darwin that the papers were old work; Darwin could have the barnacles all to himself. But Darwin’s terse letter in response gives an inkling of what might make the hard slog worthwhile: “I should wish that what little of novelty there yet remained in the subject, should be the reward of my work, which I assure

⁶⁴ Barlow, ed., *Autobiography of Charles Darwin* (cit. n. 13), p. 118; and Richards, “Why Darwin Delayed” (cit. n. 4), p. 52.

⁶⁵ Darwin to Hooker, 11 Jan. 1944, CCD, Vol. 3, pp. 1–3, on p. 2.

⁶⁶ Agassiz, Owen, Forbes, Milne-Edwards, and Ehrenberg had all done a taxonomic study of at least one major group. See Richmond, “Darwin’s Study of the Cirripedia” (cit. n. 44).

you has been to my utmost every day. — I certainly should not have dreamed of undergoing the labour of making out all the close species, if I had supposed that the most striking, & therefore most interesting & easy forms, were to be described before me.”⁶⁷

Darwin obviously coveted the scientific kudos associated with novel findings—as any naturalist would. But this passage also clearly indicates what he would *not* do if his barnacle work was purely driven by his personal research agenda: cherry-pick the most “striking” and relevant examples to bolster his species theorizing. The drudgery of “making out all the close species” was a dutiful sacrifice for the sake of definitive completeness. It went beyond the need to situate his “little monster” within a broader comparative framework, and it had little to do with deepening interest.

Overhauling the whole group gave Darwin the opportunity to make profound, theoretically embedded statements. In a judgment that essentially survives to this day, Darwin ranked Cirripedia as separate subclass within the Crustacea, rather than subsuming them within an existing subclass (as per Milne-Edwards) or making them a separate class altogether (as per Owen). In the wake of his monumental four-volume publication, Darwin could no longer be dismissed as a well-heeled, “unphilosophical” collector. He could stand on the authority of his personal expertise as a leading specialist in systematics. Darwin was now skilled in dissection, well versed in anatomy, embryology, and paleontology. He had also become an expert in microscopy, a rapidly improving technology that had become synonymous with accuracy and best taxonomic practice.⁶⁸

Darwin became more conscious of these far-reaching benefits as the project wore on. His prolix barnacle news “bulletins” were testament to this. Ahead of his publications, Darwin wanted key allies (e.g., Hooker, Lyell) and even potential foes (e.g., Agassiz, Owen) to be aware of his findings and, by implication, the competence he now possessed.⁶⁹ His subsequent Cirripedia volumes were an overt demonstration of the key epistemic virtues governing systematics: attention to detail, consistency of method, and exhaustive thoroughness. Each species description was presented in a clear and relatively standardized format.⁷⁰ Novel findings and classificatory decisions were embedded in a learned, comparative perspective, backed by careful observation and evidence-based reasoning. Given the widely recognized need to overhaul the group, Darwin’s effort was hardly going to be in vain. But the appreciation he received went beyond that. In 1853 he received the prestigious Royal Medal from the Royal Society, emblematic of the public values of British science.

While Darwin’s Cirripedia volumes were an outstanding technical achievement, they also helped him cultivate a reputation for selfless trustworthiness. His taxonomic descriptions displayed a kind of abstemious presence, apparently stripped of theoretical presumptions. While

⁶⁷ Gray to Darwin, 26 Aug. 1848, CCD, Vol. 4, pp. 161–162; and Darwin to Gray, 29 Aug. 1848, *ibid.*, pp. 162–163, on p. 163. For the papers see John E. Gray, “Description of a New Species of *Anatifa*,” and “On *Thaliella*, a New Genus of Cirripedes Allied to *Scalpellum*,” *Proceedings of the Zoological Society of London*, 1848, 16:44.

⁶⁸ On Darwin’s ranking of the Cirripedia see Richmond, “Darwin’s Study of the Cirripedia” (cit. n. 44), p. 394. Regarding microscopic expertise see Jardine, “Between the *Beagle* and the Barnacle” (cit. n. 49).

⁶⁹ Darwin treated Hooker, Henslow, and Lyell to his barnacle news. But he made his progress known to many outside his inner circle, such as Owen, Milne-Edwards, and Agassiz. See, e.g., Darwin to Agassiz, 22 Oct. 1848, 15 June 1850, CCD, Vol. 4, pp. 178–181, 345–346.

⁷⁰ Darwin’s two huge *Living Cirripedia* volumes covered the gamut of systematics. Volume 1 began with notes on nomenclature. Volume 2 gave an overview of the whole group. Both volumes provided summaries at each major taxonomic level, including key structural features, organs and affinities, metamorphoses and life cycle, range, habitat, and geological history. Individual species descriptions listed known synonyms, range, habitat and provenance, size and general appearance, anatomical structure, and organs. These volumes were fully indexed and illustrated and dwarfed the work of other British naturalists who had published on Cirripedia. For example, the unreliable output of Henry Goodsir looked amateurish in comparison.

Darwin's species theory guided his investigations, discretion demanded that he kept it tacitly enfolded. The humble, evidence-driven posture he crafted was reinforced by the stories he told against himself, of how initial impressions were overturned by repeated observations and careful cross-checking.⁷¹

After committing to the whole group, Darwin quite consciously took responsibility for raising the standards of the field, and his reform agenda had an unmistakably moral dimension. Darwin told Henslow that he felt driven by an "instinct for truth," much like an "instinct for virtue." This alone guaranteed the value of his work, never mind its practical usefulness. With all the drudgery came a higher purpose, a leading opportunity to emphasize the importance of disinterestedness. At the beginning of 1849 Darwin complained to Hugh Strickland about the practice of including the discoverer's surname in the binomial name of a new species and the use of "*mihi*" to indicate responsibility for a proposed name. In Darwin's view, such practices encouraged hasty work and vainglorious "species mongering." The plethora of species synonyms and shoddy three-line descriptions he had to wade through were proof of this. "Naturalists ought to require no such stimulus," Darwin argued, drafting a paper on nomenclature aimed at ridding British systematics of this "curse."⁷² In the end, he simply opted to set a good example: none of the many novel Cirripedia species he catalogued would carry the Darwin name or be followed by "*mihi*."

Darwin's barnacle project positioned him as the kind of methodical and impartial observer of nature the rising salaried elite could call their own. He became an aspirational example, someone they could submit their allegiance to. Huxley would lionize Darwin's barnacle volumes as the embodiment of the new beau ideal of naturalistic science, "one of the most beautiful and complete anatomical and zoological monographs which has appeared in our time."⁷³

THE BARNACLES AS A PERSONAL ODYSSEY

The long and winding road to the publication of *Origin* is also the story of Darwin's relationship with Joseph Hooker, his closest confidant and mentor. Hooker was the one who impressed upon Darwin the need for firsthand taxonomic expertise. He set sail for India in early November 1847 and left Darwin cocooned at Down. But his absent presence loomed large. When Darwin resolved to commit fully to the barnacles, Hooker was the first of his inner circle to hear of it.⁷⁴

Darwin's December 1847 decision represented a change felt on all fronts. The free-ranging weekends wound down in the late 1840s. With Darwin dogged by ill health, collegial visitors to Down became rare.⁷⁵ Just as he began to feel better, his beloved Annie died in April 1851. The two years after her death were a period of deep mourning. Darwin all but ceased to discuss species issues with his peers during the 1848–1854 period, and species concerns also disappeared from his specimen and data gathering. Hooker was a notable exception; but even then

⁷¹ Buchanan, "Darwin's 'Mr. Arthrobalanus'" (cit. n. 58).

⁷² Darwin to Henslow, 1 Apr. 1848, CCD, Vol. 4, pp. 127–129, on p. 128; Darwin to Strickland, 29 Jan. 1849, *ibid.*, pp. 187–188, on p. 188; and Darwin to Strickland, 4 Feb. 1849, *ibid.*, pp. 206–209.

⁷³ Thomas H. Huxley, "Lectures on General Natural History, Lecture XII: The Cirripedia," *Medical Times and Gazette*, 1857, 15:238–240, on p. 238 n. On Darwin as an aspirational model see Richard Bellon, "Joseph Dalton Hooker's Ideals for a Professional Man of Science," *J. Hist. Biol.*, 2001, 34:51–82; and Lightman, "Many Lives of Charles Darwin" (cit. n. 12).

⁷⁴ Hooker to Darwin, 16 Mar. 1848, CCD, Vol. 4, pp. 114–120, on p. 118. The letter Darwin wrote to Hooker has never been found. Based on the estimated time for mail to travel from Downe, England, to Mirzapore, India, Darwin's missing letter can be dated as late January 1848.

⁷⁵ Darwin's attendance at scientific meetings dropped away in the early 1850s. Other social interactions also trailed off, disappearing almost completely between 1851 and 1853. Even Darwin's correspondence dipped markedly in this period, especially in 1852. All these social indices picked up sharply in 1855.

Darwin brought up his species theory only in relation to his barnacle work. The only other allusions to his theory now tended to be rueful laments about how the barnacles were delaying his return to what Darwin clearly thought was a more interesting and important topic.

Darwin *was* delaying after 1847. Preoccupied with systematics, he was not working on his species theory in any tangible way. Darwin’s decision to do the whole barnacle group did not represent the fulfillment of a prior commitment. It was, in effect, a new commitment that involved a significant change in schedule. He was digressing in a manner that postponed his return to species work and the publication of his theory.

This long digression also had a profoundly productive effect. Darwin reemerged late in 1854 with a vengeance, and it is here that van Wyhe’s portrayal of an open and confident man is far more apt. The collegial weekends recommenced, more freewheeling than before. Hooker and company were joined by the combative Huxley and many others. Darwin discussed species issues more casually and in greater detail. He introduced the topic with C. A. Murray, E. L. Layard, J. D. Dana, and possibly John Lubbock and H. C. Watson and went into considerable detail with Asa Gray and T. V. Wollaston, as van Wyhe documented. It is quite clear that Hensleigh Wedgwood was privy to Darwin’s thinking—as were most other family members, one presumes.⁷⁶ Darwin corresponded in some depth about species issues with Edward Blyth, even if it is not clear how much of his theoretical commitments he gave away.⁷⁷ He also raised the topic with G. H. K. Thwaites. Neither was cited on van Wyhe’s list. And during an 1856 exchange with Samuel P. Woodward, a man he hardly knew, Darwin admitted: “I am growing as bad as the worst about species & hardly have a vestige of belief in the permanence of species left in me.”⁷⁸ Woodward did not make van Wyhe’s list either.

Gone was the hedging, furtive restraint. Darwin was now far more inclined to admit to a belief in transmutation rather than just an interest in species variation and origins. People were also becoming aware of his heterodoxy secondhand. Charles Bunbury could hardly believe his ears in 1856, even though Darwin had given him a guarded version of his position a decade earlier. Bunbury wrote incredulously to Darwin: “I was told last summer that you were becoming a believer in the unlimited mutability of species,—almost to the extent of the ‘Vestiges of Creation.’ I suspect this is not strictly correct.”⁷⁹

Darwin still avoided informing Cambridge Paleyites like Sedgwick; most were not part of his inner circle anyway. But to repeat an earlier point: Darwin also did not appear to tell Henslow much at all and was less open with his cousin William Fox than he was with others in his scientific network. Aside from these conservatives, it seems that the post-barnacles Darwin hardly cared who knew of his views—quite a contrast to earlier times. Being less cautious also had its costs, however. Darwin completely misjudged Wollaston. Darwin’s attempts to persuade him of the merits of transmutation backfired spectacularly. Wollaston would become one of Darwin’s most effective critics.⁸⁰

⁷⁶ See, e.g., Darwin to Hensleigh Wedgwood, before 29 Sept. 1857, CCD, Vol. 6, pp. 458–459.

⁷⁷ Blyth’s ideas about speciation came perilously close to natural and sexual selection. See, e.g., Blyth’s discussion of Wallace’s 1855 paper: Edward Blyth to Darwin, 8 Dec. 1855, CCD, Vol. 5, pp. 519–522.

⁷⁸ Darwin to G. H. K. Thwaites, 8 Mar. 1856, CCD, Vol. 6, pp. 54–55; and Darwin to Samuel P. Woodward, 18 July 1856, *ibid.*, p. 189.

⁷⁹ Charles Bunbury to Darwin, 7 Feb. 1856, CCD, Vol. 6, pp. 36–37, on p. 36.

⁸⁰ Darwin to Fox, 19 Mar. 1855, CCD, Vol. 5, pp. 288–289; and, e.g., Darwin to T. V. Wollaston, 6 June 1856, CCD, Vol. 6, pp. 134–135.

A CERTAIN KIND OF FEAR

Where was fear in all this? Darwin's conflicted reaction to *Vestiges* and the way he took Hooker's casual remarks about Gérard to heart illustrate the worries he harbored. As Darwin admitted to his closest colleagues, he feared for his scientific reputation; he feared he could not provide adequate evidence to back his theorizing; he feared he would not be convincing.⁸¹ Darwin's prolonged commitment to the Cirripedia, a specialized study only tangentially related to his species theory, suggested that he feared *he* would be seen as not up to it, as not possessing the kind of experience and expertise required for grand species theorizing. His dogged pursuit of the necessary authority was a testament to these anxieties. Darwin knew the high evidential hurdles he would have to surmount and the scientific credibility he would need to do so. As he admitted to Hooker, Bunbury, Waterhouse, and many others, he was well aware that the weight of British scientific opinion was against transformism.⁸²

Darwin's justification that he "gained much by [his] delay . . . and lost nothing" can be read as a tacit admission that he could have published his species theory earlier. Indeed, he almost lost his priority claim. The fact that Darwin took such a risk with something he held so dear makes it harder to discount the barnacles as simply a scheduled obligation that took longer than anticipated. In the end, he published in a rush, prompted by Wallace's letter of June 1858. Darwin was in the middle of writing up a huge multivolume treatise titled *Natural Selection*. He abruptly changed tack to produce a much shorter "abstract" version. But he could surely have published even earlier if necessary. Darwin finished the proofs of *Origin* in October 1859. Fretting over how his colleagues would receive his book, he dispatched advance copies with apprehensive, apologetic, or self-effacing covering notes that anticipated their critical reactions.⁸³

While Desmond and Moore emphasized Darwin's anxieties, van Wyhe did the opposite. All that was left for van Wyhe were evidential "concerns" unsullied by anything that could be labeled fear. Van Wyhe did concede that Darwin fearfully avoided publishing on the question of human origins, but only because Darwin explicitly admitted as much.⁸⁴ Darwin was keenly aware of the controversial implications of common descent. Even his 1844 essay, which was only to be published posthumously, made no mention of an ape-like ancestry. And Darwin was always wary of being seen as a materialist, a term that had accrued particularly invidious connotations.⁸⁵

Much has been made of the disparity between Darwin's highly disciplined public output during the gap years and the wild speculations of his early private notebooks. Desmond and Moore were not the only ones to cite this self-censorship as evidence of Darwin's trepidation about going public, that he feared his "social respectability" would be put at risk. But as Gowan Dawson pointed out, they did little to explain this ineffable concept or how it was cultivated and maintained.⁸⁶ Desmond and Moore imagined that its effects would translate as an overwhelming fear of exposure, with the only option being a level of secrecy precluding almost any form of dissemination. It was this assumption that van Wyhe sought to undermine.

⁸¹ See Darwin to Hooker, 10 Sept. 1845, 2 Oct. 1846, CCD, Vol. 3, pp. 252–253, 346–347.

⁸² Bunbury, *Memorials of C. J. F. Bunbury*, Vol. 2, Pt. 1 (cit. n. 21), p. 77.

⁸³ See Darwin to Lyell, 30 Sept. 1859, CCD, Vol. 7, pp. 338–339; Darwin to Huxley, 15 Oct. 1859, *ibid.*, pp. 349–350; Darwin to Henslow, 11 Nov. 1859, *ibid.*, p. 370; Darwin to Jenyns, 13 Nov. 1859, *ibid.*, pp. 374–375; and Darwin to Hugh Falconer, 11 Nov. 1859, *ibid.*, p. 368.

⁸⁴ Van Wyhe, "Mind the Gap" (cit. n. 8), p. 195; and Charles Darwin, *The Descent of Man*, 2 vols., Vol. 1 (London: John Murray, 1871), p. 1.

⁸⁵ Gowan Dawson, *Darwin, Literature, and Victorian Respectability* (Cambridge: Cambridge Univ. Press, 2007), Ch. 1.

⁸⁶ *Ibid.*

The moral demands of social respectability applied just as forcefully to naturalists of more modest backgrounds, such as Hooker and Huxley. But this hardly left them frozen with fear. Huxley took the fight to his conservative opponents with undaunted relish—even though or perhaps because he lacked money and connections. Huxley led the way in leveraging a space between the gentlemanly aristocrats and Cambridge Paleyites, on one hand, and the popular hacks and cranks, on the other. However, the secular “philosophical” naturalism Huxley and like-minded colleagues championed had to exemplify the highest scientific standards *and* a very traditional rectitude if they were to assume the right to interpret nature in ways that might not accord with theological scripture.⁸⁷

While maintaining social respectability was hardly the stuff of nightmares and illness that Desmond and Moore suggested, it remained an ongoing concern for Darwin. It colored all his published output and scientific activities, not to mention his involvement in community and parish affairs. While the demands of social respectability may have been a factor in the discretion Darwin exercised over his species theory during the gap years, these demands did not affect the timing of his theory’s eventual publication.

Darwin’s barnacle work was an instructive rehearsal in this sense. His fascination with the sex lives of these sea creatures was one of the most palpable features of the project. But the minefield of Victorian sensibilities Darwin had to negotiate in recounting his findings hardly seemed to perturb or delay him. Instead, he described their unusual sexual apparatus and arrangements in copious detail. The neutral tone Darwin adopted—save for a sense of surprise and wonder—contrasted sharply with the playfully anthropomorphic accounts he privately relayed to Hooker and company.⁸⁸ The ascetic posture of his public presentations, the pure empiricism they demonstrated, provided the warrant that nature was as he found it—and could therefore be neither profane nor blasphemous.

Darwin’s barnacle work helped nurture a reputation for integrity that would help insulate him from the dire accusations of his adversaries.⁸⁹ But this was a payoff Darwin seemed to grasp only after he took responsibility for overhauling the whole barnacle group. We do not get a strong impression that the Darwin of the mid-1840s feared he lacked *moral* authority—and certainly not in the way that he felt vulnerable about his modest firsthand experience and expertise in biological science.

Darwin understood the moral and religious sensitivities that his species work invoked, and he carried the necessary forms of rectitude up to the publication of *Origin* and beyond. Again, any question of a delay can be framed in terms of scheduling. While prepared to adjust, Darwin still planned to publish and be damned—as he repeatedly told his closest friends. However, these sensitivities clearly played a role in the two-stage publication strategy he opted for. With a teasing restraint, Darwin avoided spelling out the implications for human descent in *Origin* so as not to make the book “more *un-* orthodox, than the subject makes inevitable.” The newspapers of the day—not to mention his peers—were not so reticent. When *The Descent of Man* finally appeared in 1871, after its own fraught and protracted preparation process, Darwin and publisher John Murray were careful to edit the text to avoid public offense and accusations of indecency.⁹⁰

⁸⁷ See Lorraine Daston and Peter Galison, *Objectivity* (New York: Zone, 2007).

⁸⁸ Buchanan, “Darwin’s ‘Mr. Arthrobalanus’” (cit. n. 58).

⁸⁹ Dawson, *Darwin, Literature, and Victorian Respectability* (cit. n. 85), Ch. 2.

⁹⁰ Darwin to Hooker, 5 or 12 Nov. 1845, CCD, Vol. 3, pp. 263–265; and Darwin to Lyell, 28 Mar. 1859, CCD, Vol. 7, pp. 269–270, on p. 270 (quotation). Regarding the editing of *Descent* see Dawson, *Darwin, Literature, and Victorian Respectability*, Ch. 2.

FOUNDATIONAL FIGURES AND BIOGRAPHY

While Darwin was clearly not ready to go public in 1844, he did not appear to be spooked by inopportune times, as Desmond and Moore suggested. Social upheavals may have worried him, but he was not in the habit of commenting on political issues.⁹¹ Thus there is little evidence to support the thesis that Darwin was delaying in the wishful anticipation of less volatile circumstances. Van Wyhe's analytic framework also allows us to decouple Darwin's ill health from the notion of a fearfully avoidant delay. His bouts of illness were hardly confined to the species work; he was just as sick, or sicker, during his barnacle project. Nor did it seem that anxiety *per se* was the culprit. To cite one key example: Wallace's arrival on the scene was certainly upsetting, yet Darwin remained relatively well during the delicate negotiations over priority and through much of the pressurized write-up of *Origin*. He succumbed only toward the end of that process. If anything, it was simply hard work that seemed to get the better of him.⁹² And while ill health may have slowed Darwin, it did not significantly change his work priorities or publication plans.

Van Wyhe's account represented an undeniably valuable corrective to Desmond and Moore's version of the delay thesis. But he went too far. Van Wyhe aimed to banish psychobiographical assumptions and speculations but inadvertently affirmed how unavoidable they are when it comes to personal accounts of a scientist's work. We see one straw man exchanged for another: a fretful, secretive, cowering figure supplanted by a fearlessly open and focused researcher driven entirely by his own intellectual priorities.

Foundational figures like Darwin symbolize our view of the scientific past, legitimating the present and mapping possible futures. The Darwin industry shows few signs of retrenchment. While Darwin's legacy remains hotly contested, particularly in the United States, he still stands as a towering figure worthy of attention. One wonders whether his reputation needed this kind of rescuing.

The debate over Darwin's delay has a strong metabiographical element. Each generation of historians has given us a different Darwin, and these differences say as much about changing scholarly fashion as they do about Darwin himself.⁹³ We have come to distrust uncomplicated, bloodless portraits because we have ceased to view science and scientists in this way. Darwin's humanizing contradictions are there for all to see. He was a coolly independent thinker who was also prone to bouts of insecurity. He was quite open with some of his colleagues but surprisingly secretive with others. And he was a consummate social networker yet a self-absorbed semi-recluse for much of his life.

We are now more interested in biography as a way of exploring the cultural identity of the scientist, and we seek to connect individual actors within the microsocial networks in which they worked—as Browne did so well. And we have Desmond and Moore to thank for shining the brightest of lights on Darwin's broader sociopolitical context, even if they failed to sustain the implied connections between this context and Darwin's state of mind and actions.

Thus, the delay remains. While the evidence is circumstantial and indirect, we suggest that Darwin's Cirripedia digression *did* effectively postpone his species work. The barnacle project was in the beginning, and in the end, a credentialing exercise driven by field-generated self-doubts. The solitary years Darwin spent on these small sea creatures can best be characterized as a penance for his youthful hubris.

⁹¹ Darwin's notebooks rarely mentioned politics or social issues, and these concerns were virtually absent from his correspondence. See Grene, "Recent Biographies of Darwin" (cit. n. 6).

⁹² *Ibid.*; and Allen Esterson, "Desmond and Moore's *Darwin's Sacred Cause*: A Misreading of the Historical Record," *SAGE Open*, Apr.–June 2013, pp. 1–7.

⁹³ Browne, "Making Darwin" (cit. n. 14).

The effects of this marathon project were nonetheless remarkable. Darwin reinvented himself as the kind of authority figure recognized by his peers—and later by historians—as the prefigurative embodiment of modern secular science. We agree with van Wyhe that Darwin’s closest peers knew him best. But Darwin also knew what it would take to earn their respect. Only a reordering of the entire barnacle group, with all those monotonous “close species,” would suffice. Hooker suggested that it served as a crucial preparation for Darwin’s species work, telling Darwin’s son Francis:

He [Darwin] talked to me incessantly of beginning to work at his “beloved Barnacles” (his favorite expression) long before he did so methodically. It is impossible to say at what stage of progress he realized the necessity of such a training as monographing the Order offered him; but that he did recognize it and act upon it as a training in systematic biological study, morphological, anatomical, geographical, taxonomic and descriptive, is very certain; he often alluded to it as a valued discipline and added that even the “hateful” work of digging out synonyms, and of describing, not only improved his methods but opened his eyes to the difficulties and merits of the works of the dullest cataloguers.⁹⁴

Darwin’s decision to take on the Cirripedia was a tipping point in his career, one that linked eras old and new, the gentleman amateur and the trained professional. The project changed him irrevocably, upgrading his training and collegial integration and furnishing him with the technical hardware and competencies of the laboratory specialist. The fact that he remained the self-financed, paternal figure of Down House masked the significance of what had taken place. Darwin acquired many of the trappings that would come to define the paid practitioner. More important, his work exemplified the values and standards of those promoting this new identity, one touted as maintaining all that was good about gentlemanly science.⁹⁵ The variegated tableau of the barnacle years—the sophisticated equipment and exotic specimens intermingling with the homespun quaintness of Darwin’s drawing-room study—made for ambiguities that are the stuff of endless debate and revision. They are part of what makes Darwin such a compelling, boundary-crossing figure: working, delaying, and, eventually, triumphing.

⁹⁴ Reconstructed from Darwin, ed., *Life and Letters of Charles Darwin* (cit. n. 15), Vol. 1, p. 346; and Ronald W. Clark, *The Survival of Charles Darwin: A Biography of a Man and an Idea* (London: HarperCollins, 1986), p. 96.

⁹⁵ See Jim Endersby, “A Life More Ordinary: The Dull Life but Interesting Times of Joseph Dalton Hooker,” *J. Hist. Biol.*, 2011, 44:611–631.