
MIND THE GAP: DID DARWIN AVOID PUBLISHING HIS THEORY
FOR MANY YEARS?

by

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It is widely believed that Charles Darwin avoided publishing his theory of evolution for many years. Many explanations have been proposed to identify Darwin's reasons or motives for doing so. This essay demonstrates that Darwin's delay is a recent historiographical theme for which there is no clear evidence, and indeed is overwhelmingly contradicted by the historical evidence. It is also shown that Darwin's belief in evolution was not a secret before publication. Instead of a man afraid of his secret theory's being revealed to his prejudiced contemporaries, it is demonstrated that Darwin was understandably very busy and began his species book when he had completed work in hand, just as he had intended all along. This essay therefore rewrites a fundamental chapter in the story of Darwin's life and work as usually told.

Keywords: Charles Darwin; evolution; 'Darwin's delay'; historiography

Considering that Darwin became an evolutionist in 1837...one would think that he would rush this, the most important theory in biology, to the printer as quickly as possible. Instead, he postponed publication for twenty years and was forced into action only by circumstances. Why this incredible procrastination?

Ernst Mayr¹

The very existence of Darwin's two-decade delay has raised a fundamental question: Why did he refrain from publishing his theory of evolution for so long? Was it simply that he had scientific business to finish, or did fears of ostracism stay the squire's hand?

Adrian Desmond²

It is widely believed that Charles Darwin avoided publishing his theory of evolution, begun in the 1830s, until 1858 when Alfred Russel Wallace hit on a strikingly similar theory. This period between the late 1830s and the late 1850s has been called 'the long wait' (Ruse), 'Darwin's procrastination' (Mayr) and most often 'Darwin's delay'.³ It occupies a major place in popular and scholarly accounts of Darwin, often described as the greatest puzzle about his life. Many authors have sought Darwin's reasons or motives for refraining from publishing. Darwin is said to have avoided publishing because he was afraid of the reactions of his scientific colleagues, damaging his reputation, religious persecution, upsetting his

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religious wife or Captain Fitzroy, or disturbing the social order, or was put off by the reception of *Vestiges of creation* or torn by some inner psychological conflict about his theory's implications.

In this essay it is argued that not only is there no evidence that Darwin avoided publishing his theory for many years, but the evidence is overwhelmingly against that interpretation. By re-examining the historical evidence, without presuming that Darwin avoided publication, it can be shown that there is no reason to introduce such a hypothesis in the first place. If we come to the evidence already believing that Darwin put it off, then vague and ambiguous passages will seem consistent with such a view. Such was the case, for example, with the legend of Darwin's finches. Scholars believed for 20 years that the finches inspired Darwin while on the Galapagos islands to the view that species evolve. In the early 1980s, however, Frank Sulloway's research demonstrated beyond any doubt that this was not the case, that Darwin did not and could not have had a eureka moment on the Galapagos.⁴

A fresh analysis of Darwin's manuscripts, letters, publications and the writings of those who knew him intimately shows the story to be quite different from one of a lifetime of avoiding publication. It will be demonstrated that Darwin's delay is a historiographical theme of quite recent date and unknown not only to Darwin and his contemporaries but also to generations of writers after them. Furthermore, this theme is not the product of the greater knowledge of Darwin produced by modern historical scholarship since the 1960s. Modern writers inherited Darwin's delay from earlier writers who did not have access to the full manuscript corpus.

In fact, Darwin hardly veered from his original plans for working out and publishing his species theory in due course. Finally, it will be shown that, contrary to common belief, Darwin did not keep his belief in evolution (or transmutation as it was then known) a secret before publication in 1858–59. It should be stressed that this essay addresses Darwin's belief in and theory of evolution, not other ideas such as materialism or the origins of humans.

THE DELAYED ADVENT OF DARWIN'S DELAY

After Darwin's death in 1882, countless accounts of his life and work appeared. In none of his obituaries or the many biographies and other accounts of his life in succeeding decades is there a hint that Darwin put off publishing.⁵ Only in the 1940s and 1950s did the modern belief in Darwin's delay begin gradually to appear. Writers before this either did not give any attention to, or explanation for, the widely dispersed dates for conceiving and publishing or simply referred to the period as the time spent working on the subject. Many writers represented it as a virtue that Darwin worked for so long on his species theory. Most simply followed Darwin's rendition on the first page of *Origin of species* (1859). He told his readers that he began to work on the subject in 1837, then 'drew up some short notes' in 1842 and enlarged on these in 1844: 'from that period to the present day I have steadily pursued the same object'.

Perhaps nothing indicates the entire absence of any notion of a delay by those who knew Darwin than the biographical works edited by his son Francis.⁶ Nowhere in these works is there the slightest suggestion that Darwin delayed or dared not publish his evolutionary theory for whatever reason, nor that he treated it as secret. Even more striking is *Foundations of the Origin of species* (1909), which provided, for the first time, Darwin's early 1842 and 1844 species essays. These were also carefully compared with the *Origin*. Francis Darwin even pondered what would have happened if the 1844 essay alone had been published.

Yet there is no sense that Darwin postponed. Nor is any explanation given for why so many years elapsed before publication. What is to us a central part of the Darwin story was entirely absent from the accounts of people who knew Darwin intimately. Why? Why would they forgo what is to us one of the most evocative and romantic parts of the Darwin story, how he avoided publishing his theory for 20 years?

During the course of the twentieth century, after the new synthesis and successive anniversaries were celebrated, Darwin's reputation became more prominent than ever before. There was more historical attention to the man and his life. In the 1930s and 1940s Darwin's granddaughter and historian Nora Barlow still described him as the patient collector of facts during the gap years but also surmised, perhaps for the first time, that Darwin did not publish at first because he was cautious and he might have been afraid of upsetting Captain Robert Fitzroy.⁷ This was offered merely as speculation.

R. E. D. Clark, writing in a popular series for issues relating to 'faith' in 1948, described Darwin as vacillating and procrastinating because 'it became more than ever clear to his mind that evolutionary views were not going to be at all popular'. Indeed, Darwin was made ill by 'the constant uncertainty as to whether or no he should take the final plunge—an uncertainty that he allowed to haunt him for twenty years'.⁸ Here, at last, the modern delay story was beginning to emerge.

A delay component in the Darwin story slowly and haltingly began to preponderate. In 1951, for example, Professor of Zoology A. F. Shull described Darwin as in no hurry to publish because he was still assembling facts.⁹ Julian Huxley, in 1958, described Darwin as unable to 'overcome his extraordinary diffidence over publishing his results'.¹⁰ Charles Gillispie wrote in 1960: '[Darwin] was held back from publication, and even from giving himself joyfully to his conclusions, by a fear of seeming premature'.¹¹

Darwin's early transmutation notebooks were first published in 1960–67.¹² These provided substantial new material on Darwin's early work. Perhaps they fuelled interest in the gap period. By the late 1960s the years before publication became increasingly central to Darwin historiography. Historians like Peter Vorzimmer began to ask, 'why, if the process of natural selection was at last clearly formulated in Darwin's mind after reading Malthus in 1838, did he delay in writing up his account of how that process effected the transmutation of species?'.¹³ Michael Ghiselin surmised in 1969 that Darwin 'was very much discouraged in his attempts to explain natural selection to his colleagues, and this may be one of the reasons why he so long delayed publication'.¹⁴

Postponement because of fear became the central theme of the Darwin story in the work of the psychologist Howard Gruber.¹⁵ Gruber was influenced by the work of the French child psychologist Jean Piaget to understand the psychological processes of the growth of new ideas and their development over time. As Piaget wrote in his preface to the book: '[It is] one of the major themes of this book: Darwin's motives for his long delay in publication. His fear of persecution and ridicule was based not only on the unpopularity of evolutionary theory, but on the fiercer retribution meted out against proponents of materialism.' Gruber put it similarly: 'we need some explanation for Darwin's long delay in publishing his views, and we need some understanding of the way in which this delay affected his inner life'.¹⁶ Mental anguish was Gruber's primary explanation for the delay, although other possible causes such as offending Emma Darwin were considered. Gruber did not assemble a case to show that Darwin had avoided publication; rather, assuming this to be the case, he argued that fear was the main reason. Gruber's rendition of Darwin was very influential and fear has remained the most popular explanation for Darwin's delay.¹⁷

Gruber also interpreted Darwin's 1838 dream in which 'a person was hung & came to life' as not about a third person, as Darwin recorded in his notebook, but actually, in a Freudian sense, about Darwin himself because of his 'fear of dire punishment for thinking'. Gruber also suggested that 'it might be equally reasonable to interpret it as a castration dream'.¹⁸ Surely it is more parsimonious to take Darwin's recording of the 'witty' dream as written, a curious and amusing dream about a man who was hanged but which changed in the strange way of dreams to decapitation and how in a context of 'banter and joking' a succession of ideas came before him which 'I neither doubted or *believed*'.¹⁹ Darwin records no fear at all. He was interested in memory, cognition and the physiology of these processes. Gruber's interpretation highlights his preconception that Darwin must have been afraid of what others would think of his theory.

By 1977 Darwin's delay had become a major episode in the Darwin story. Silvan Schweber accepted Gruber's interpretation and named anxiety of materialism, upsetting Emma, the reception of *Vestiges*, Darwin's 'inability to advance a satisfactory explanation for the observed variations' and the disapproval of colleagues as reasons for postponing.²⁰ Steven Jay Gould also discussed 'Darwin's delay'.²¹ Gould was certain that Darwin had avoided publishing. 'So complex an issue as the motivation for Darwin's delay has no simple resolution, but I feel sure of one thing: the negative effect of fear must have played at least as great a role as the positive need for additional documentation.'²²

In the same year Ralph Colp's important book on Darwin's illness appeared. Here, too, Darwin seems to delay because of his fear of the 'vast opposition' of his colleagues.²³ Colp also argued that Darwin's ill health was related to the torment of concealing his evolutionary and materialist ideas. Also in 1977 Sandra Herbert argued that 'twenty years had been required for an audience to form' and that the absence of which explained why Darwin did not publish in 1839.²⁴

This new delay trend in the literature found its way to the television screen in 1973 in Bronowski's *The ascent of man*, in which viewers were told that Darwin feared his book would be shocking to his wife and that Darwin *would rather die* because he 'did not want to face the public'. Darwin's delay appeared again in 1978 in the BBC series *The voyage of Charles Darwin*. In episode seven, reasons for not publishing were given: he needed more evidence and he was worried about loss of reputation and public censure of his ideas.

So by the late 1970s the delay had become 'the major puzzle in the Darwinian story' and it was read into the past. For example, some writers described T. H. Huxley's 'opinion as to the value of the eight years given to the Cirripedes' as an explanation for Darwin's delay. In fact, there is no sense of a delay or postponement in Huxley's piece.²⁵ Instead, Huxley discussed the importance of the barnacle work as essential training for Darwin. Huxley's point was that it was not a waste of time, as Darwin questioned in his autobiography.²⁶

Robert Richards examined the question of 'Why Darwin delayed' in a perceptive 1983 article. Richards rightly showed that many writers tended to offer one explanation for a complex phenomenon. He also asked a more appropriate question: not 'why did Darwin delay publishing?' but 'why was there a gap?'. Richards accepted that many of the delay explanations were partly true but none was sufficient. He concluded that, among other things, 'it was because of his work agenda that he was not able to get to his species book more quickly'.²⁷ Richards favoured the view that Darwin was not finished and was particularly concerned with the problem of instincts of neuter insects.

New explanations for Darwin's delay were introduced by Adrian Desmond in the afterword to his celebrated book *Politics of evolution* (1989). After an in-depth study of radical London in the 1830s, Desmond concluded that 'there would have been an

establishment outcry' if Darwin had published his evolutionary speculation in the late 1830s.²⁸ 'Yet any fears Darwin might have had on this point would have been dwarfed by the prospect of ultraradical appropriation' for their own radical social and political ends.²⁹ Furthermore, Desmond urged, Darwin must have been aware of this. Desmond concluded, 'Ultimately Darwin was frightened for his respectability.'³⁰

Darwin's delay became the central theme of Desmond and Moore's biography *Darwin* (1991) and the BBC Timewatch documentary made in the same year. The book argued, as they later summarized, '[Darwin] had buried evolution for twenty years, petrified for his respectability, upholding the paternalist order for a generation before being forced into the open.'³¹ In their view Darwin was so afraid of revealing his ideas that it was a 'contradiction that was tearing him apart'.³²

In the documentary Desmond succinctly asked: 'So why didn't [Darwin] publish this new theory in 1838?' Desmond argued that the theory would be despised by social elites and played into the hands of radicals. 'Only a troublemaker would announce an evolutionary theory at such a time' and 'Darwin was no troublemaker'. In the book a similar point is made: 'He had his theory, and a fancy animal analogy; but in these riot-torn years they were safely tucked away in his secret notebooks, and only in the very distant future would he even think of publishing.'³³

The view that Darwin dared not publish because he was afraid has now been very widely diffused.³⁴ In a 1999 television documentary the former head of Down House remarks: 'he delayed publishing for nearly twenty years because he was so terrified of what the public reaction might be'.³⁵ In 2002 the BBC documentary *Great Britons: Charles Darwin* repeated the story of a trembling Darwin.

Desmond and Moore also made use of Gruber's interpretation of the hanging dream as actually about Darwin himself.³⁶ Ten years later, in 2001, this interpretation of Darwin's dream was exaggerated in the documentary *Evolution: Darwin's dangerous idea* into Darwin dragged in terror to the scaffold by Captain Fitzroy and Richard Owen! Awakening in a sweat from this nightmare Darwin tells Emma, 'This book will be the death of me!'

Desmond and Moore deftly argue that Darwin must have consciously avoided publication because of how they believe such a theory would have been received in the late 1830s and mid 1840s, both by men of science and by social radicals. They argue that in such a context Darwin's reputation as a man of science and his respectability as a gentleman would be ruined. Although certainly plausible, no clear evidence is given to show any connection between their assessment of these contextual features and the awareness or actions of Charles Darwin. The plausibility of their case about Darwin's context is not, in itself, enough to warrant such a specific and powerful conclusion about Darwin's thoughts and actions. Additional evidence is necessary to distinguish between cause (the social climate is why Darwin did not publish) and correlation (the social climate was as they characterize it, but was not why Darwin did not publish at that time). Desmond and Moore and Peter Brent suggest that two patriarchal Darwin letters of advice throw light on why he must have postponed. The first is to the young botanist John Scott, whom Darwin was helping with his career. 'I would suggest to you the advantage at present of your being very sparing in introducing theory in your papers (I formerly erred *much* in geology in that way): *let theory guide your observations*, but till your reputation is well established be sparing in publishing theory. It makes persons doubt your observations.'³⁷ The second was to Darwin's son George:

It is an old doctrine of mine that it is of foremost importance for a young author to publish (if with his name) only what is very good & new; so that the public may have faith in him, & read what he writes ... I have marked one or two passages in which you give your own

conviction: remember that an enemy might ask who is this man, & what is his age & what have been his special studies, that he sh^d. give to the world his opinions on the deepest subjects? This sneer might easily be avoided ... but my advice is to pause, pause, pause.³⁸

If we already believe that Darwin postponed, and further are convinced that he must have done so, then such passages seem to be confirmations. But if we do not assume that Darwin postponed there are much more mundane and parsimonious readings of these and other such ambiguous fragments. The first letter is clearly reflecting on Darwin's blunder in his article about the parallel roads of Glen Roy.³⁹ The second is no more about Darwin himself than the hanging dream. The fact that Darwin advised his son not to publish overly ambitious claims about his convictions in 1873 is not evidence that Darwin himself refrained from publishing his evolution theory from 1839 to 1858. As will be discussed below, the very fact that such tangential and ambiguous passages are so often put forward as evidence for Darwin's delay is indicative of the lack of historical evidence for this view.

Many writers have argued that observing the critical reactions to *Vestiges* (1844) caused Darwin to avoid publishing his views.⁴⁰ These have all been proposed after the advent of Darwin's delay in the mid-twentieth century. In fact, there is no evidence that *Vestiges* put Darwin off publishing his theory.⁴¹ Darwin's estimates of when he would publish did not indicate any inclination to postpone after *Vestiges* appeared (see below). He had already written the 1844 sketch and memorandum to his wife requesting her to see that it be enlarged and published in the event of his premature death.

A letter from Darwin to Charles Lyell is often cited as evidence that *Vestiges* put Darwin off: 'I have been much interested with Sedgwick Review [of *Vestiges*] ... it [is] 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 consistently stated that he would publish nonetheless. In any case Darwin did not consider *Vestiges* and its theory like his. He considered *Vestiges* unphilosophical and amateurish. It was also a theory of the progress of the entire universe rather than just a naturalistic explanation of species. Therefore the harsh reactions to and criticisms of a popular, unphilosophical and amateurish book were not reasons for Darwin to fear that his serious and well-grounded theory would receive the same treatment. Furthermore, Darwin's notes on *Vestiges* reveal no fear but instead considerations of how to proceed in the light of this new publication.⁴³

Although Darwin's delay has become a major theme in Darwin historiography, there is by no means a unanimous view. Some historians deny that Darwin postponed. Perhaps the first was Cannon in 1976, who argued that Darwin needed 20 years to develop his case.⁴⁴ The literary critic Joseph Carroll argued that 'there was no "delay"' but 'only a protracted preparation'.⁴⁵ Janet Browne's monumental biography largely downplays a delay after 1844 and rightly stresses that Darwin continued to give attention to his theory while he was incredibly busy with other natural history investigations.⁴⁶ Several other writers have pointed out that Darwin was not ready to publish in 1844 because many aspects and implications of his theory were unfinished.⁴⁷

WAS IT A SECRET?

Many recent writers state that Darwin's belief in transmutation was a secret during the gap years. The first such claim I have found is actually Bettany in 1887 but not again until Irvine (1956). Thereafter the view becomes quite common in Greene (1959), de Beer (1963), Olby (1967), Hopkins (1969), Easley (1979), Durant (1985), Colp (1986), Desmond and Moore

(1992), Bowler and Bowlby (1990), Ruse (2000), Browne (2002), van Wyhe (2002), Stott (2003) and Quammen (2006), to name only a few.⁴⁸

Desmond and Moore stress most strongly that Darwin's theory was 'secret' because they believe Darwin and his contemporaries must have seen 'evolution as a social crime'. 'Darwin could expect a furor among his geological friends if they discovered his secret. No more "hail fellow, well met." He could be labelled as a traitor. His respectability would be compromised. Not only would his science be impugned. He himself would be accused of reckless abandon.'⁴⁹ Therefore they describe his transmutation and expression notebooks as 'secret', 'clandestine' and 'covert'.⁵⁰ Darwin preferred, in their view, 'living a lie'.⁵¹

Was it really a secret? Did Darwin lie? I can find no documentary evidence that Darwin ever referred to it or treated it as secret. The editors of Darwin's correspondence observed: 'Darwin is usually depicted as having been very careful to keep secret his heretical views on species, but the correspondence does not bear out this view, if what is meant is that Darwin was afraid to divulge his conviction that species had evolved.'⁵² Very many references in Darwin's correspondence, notebooks and loose notes reveal open discussion on behalf of transmutation. This evidence provides a long list of people who knew of Darwin's transmutationism. The list includes Emma, his father Robert, his brother Erasmus, Darwin's children, Hensleigh and Elizabeth Wedgwood, William Lonsdale, Hugh Strickland, Edward Forbes, Hugh Falconer, Dieffenbach, Fletcher, Edward Cressy, J. S. Henslow, Leonard Horner, Leonard Jenyns, Richard Owen, G. R. Waterhouse, J. D. Hooker, W. D. Fox, Lyell, C. J. F. Bunbury, Asa Gray and, later, T. H. Huxley, T. V. Wollaston, H. C. Watson, J. D. Dana, E. L. Layard, C. A. Murray and E. Norman.⁵³ It seems likely that Darwin told William Herbert, Hugh Cuming, William Yarrell and John Lubbock.⁵⁴ Darwin consulted very many more people on questions of heredity, distribution, variation, hybridity and so forth, which do not contain explicit mention of transmutation. However, after considering how freely those above were told, there is a difficulty in distinguishing between those who certainly knew, those who probably knew, and the larger number who might have known. I have tried to err on the side of caution. There were, no doubt, others who were told in conversation or in letters that have not been uncovered or of which no traces survive.

The editors of Darwin's correspondence observed: 'It is clear from the correspondence that his close friends were not outraged by Darwin's heterodox opinions'.⁵⁵ The manuscript evidence is consistent with this. His colleagues disagreed, but Darwin does not seem reluctant to discuss his ideas with them. Naturally, Darwin probably did not bring up such a topic with the likes of Adam Sedgwick, who passionately reviled transmutation, although Darwin did with Owen.⁵⁶ The only one of these people who was asked to keep it to himself, as far as we know, was Gray in America. We know that Darwin told a number of people whom he hardly knew. He had been corresponding with the botanist J. D. Hooker for only two months when the latter was told about the theory. Darwin barely knew Hooker and was, as so often, humorously melodramatic in telling his correspondent that he held an unconventional view.

I am almost convinced (quite contrary to opinion I started with) that species are not (it is like confessing a murder) immutable. Heaven forbid me from Lamarck nonsense of a 'tendency to progression' 'adaptations from the slow willing of animals' &c,—but the conclusions I am led to are not widely different from his—though the means of change are wholly so—I think I have found out (here's presumption!) the simple way by which species become exquisitely adapted to various ends.⁵⁷

Many scholars since the 1950s have felt this passage means that Darwin was very afraid. Yet the interpretation that it is humorous is far more plausible. First of all it had been read for

70 years (including many people who knew Darwin) without being interpreted as evidence of fear. Second, I have electronically searched Darwin's correspondence for words like murder, hang(ed), executed, suicide, torture and battle. Every single instance that is not literal is humorously melodramatic. Darwin's almost constant good-natured self-effacing humour in his letters very often took the form of hyperbolic witticisms. Perhaps more important than Darwin's humour in such passages is his modesty. It was difficult, indeed impossible, for Darwin to say, with proper modesty, what he felt he could (and in the end did) achieve in revolutionizing the study of life on Earth.

The fact that Darwin paid copyists on at least two occasions to copy his species theory is also inconsistent with secrecy. The Down schoolmaster, Fletcher, was paid £2 to make a fair copy of the 1844 essay, and Ebenezer Norman copied Darwin's sketch sent to Gray in 1857.⁵⁸ Having his evolution work commercially copied is an inexplicable conundrum if the contents were secret and considered capable of undermining his respectability and reputation. Yet it is consistent with Darwin's claims in the autobiography and elsewhere 'though I cared in the highest degree for the approbation of such men as Lyell and Hooker, who were my friends, I did not care much about the general public'.⁵⁹

In contrast, consider the arrangements of Robert Chambers, who, to keep secret his authorship of *Vestiges* (1844), had the whole manuscript copied out by his wife (so that his handwriting could not be recognized). This copied manuscript was then sent to his friend Alexander Ireland in Manchester, who forwarded it to the publisher, John Churchill, in London.⁶⁰ Darwin's work was in fact so far from a secret that he expressed no surprise or regret when he heard that *Vestiges* 'has been by some attributed to me.—at which I ought to be much flattered & unflattered'.⁶¹

Finally, we have at least three explicit statements to the fact that Darwin discussed evolution with 'very many' people. In the sixth edition of the *Origin* (1872) Darwin stated, 'I formerly spoke to very many naturalists on the subject of evolution, and never once met with any sympathetic agreement'.⁶² In his autobiography he recalled, 'I occasionally sounded not a few naturalists, and never happened to come across a single one who seemed to doubt about the permanence of species. Even Lyell and Hooker, though they would listen with interest to me, never seemed to agree. I tried once or twice to explain to able men what I meant by Natural Selection, but signally failed.'⁶³ And finally there is a remark written in Notebook C (1838): 'State broadly scarcely any novelty in my theory, only slight differences, «the opinion of many people in conversation.» the whole object of the Work is its proof'.⁶⁴ This remark appears to record that many people had observed in conversation by 1838 that there was 'scarcely any novelty' in his theory of descent. Contrary to the conventional view, Darwin seems to have been generally quite open about his belief in transmutation with his family, friends and colleagues. It does not indicate that he revealed natural selection, however.⁶⁵ This, after all, was his unique solution. A more appropriate term for his belief that species evolved is 'private' rather than secret.⁶⁶

THE STRONGEST OBJECTIONS AGAINST THE THEORY

Historians of recent decades have urged that Darwin must have been afraid of the reactions of his scientific colleagues as well as religious and social repercussions. Although there is a contextual case that this might have been a reasonable position to hold, no unambiguous evidence has been found that Darwin was particularly concerned about a hostile reception. In fact, all of the evidence that does exist points to other forms of expected objections, gaps in the theory or its evidence for example. None of Darwin's written considerations of difficulties suggests an

unwillingness or even a reluctance to go public. For example, Darwin made a note of a conversation in August 1842 'Talking with Mr. Strickland—I confess that my theory must necessarily be given up if the fossil record could show the sudden appearance of complex forms were fact. On numerous other occasions Darwin referred to the incompleteness of the fossil record as 'Strongest objection against my theory'.⁶⁷ Similarly, Darwin worried about his inability to demonstrate intermediate steps that led to the formation of some complex organs. 'We never may be able to trace the steps by which the organization of the eye, passed from simpler stage to more perfect This really perhaps greatest difficulty to whole theory.'⁶⁸

Perhaps the most frequently cited passages suggesting fear and anticipation of criticisms are these from the transmutation notebooks:

I fear «great evil» from vast opposition in opinion on all subjects of classification, I must work out hypothesis, & compare it with results. if I acted otherwise, my premises (in di) would be disputed.

If I want some good passages against opposition of divines to progress of knowledge, see Lyell on Scrope.

Mention persecution of early Astronomers.— then add chief good of individual scientific men is to push their science a few years in advance only of their age. (differently from literary men.—) must remember that if they *believe* & do not openly avow their belief, they do as much to retard, as those, whose opinion they believe have endeavoured to advance cause of truth.⁶⁹

There can be no doubt that Darwin was aware that his theories would be, and indeed in conversation often were, disputed and by some despised, and these passages reflect this and his anticipated counter-arguments. But this does not mean that he was terribly afraid of revealing what he believed or indeed went so far as to suppress his work for many years. In fact, these remarks about what critics will say or how to counter them are few and far between. The vast majority of almost 70 000 words in the transmutation notebooks is devoted to the properties and distribution of present and extinct organisms.⁷⁰ In fact, Darwin's consistent statements about possible objections to his theory in his correspondence and loose notes, and the strongest concerns expressed, are evidential, not religious, social or personal.

Yet Darwin often wrote tangentially of his theory in his correspondence. Does this mean it was partly secret? Just as plausibly, and more parsimoniously, we can conclude that this was done to save time, to retain the novelty of his solution, or to spare broaching a touchy subject that a correspondent disliked or was unprepared to appreciate. Darwin used language such as 'my work on varieties and species' with those such as Hooker who did know.⁷¹ Can we therefore conclude that because Darwin used the same language with others that he was attempting to veil his belief in transmutation and furthermore that he did so because he was so afraid? Many of these tangential references actually make more sense if we assume that Darwin had already talked with his correspondent on the subject. Hence referring to 'species' work was enough. If the correspondent did not know what this referred to, such ambiguity would invite questions of clarification. At any rate it is abundantly clear that Darwin usually kept natural selection to himself while being frank that he believed in transmutation.

WHAT DELAY?

In his autobiography (written from 1876) Darwin wrote, 'I gained much by my delay in publishing from about 1839, when the theory was clearly conceived, to 1859; and I lost

nothing by it'.⁷² Here, it might be argued, is a straightforward admission of intentional delay by Darwin himself. But not so. Only with avoidance already in mind can it be read in this way. Darwin's family, friends and contemporaries such as Francis Darwin, Huxley, Wallace and scores of biographers also read this passage but left behind no trace that they thought Darwin dared not publish. Here is the rub. The phrase 'my delay in publishing' has two legitimate and straightforward interpretations.

1. 'Delay' means the time during which publication was avoided.
2. 'Delay' means the span of time that happened to transpire between conceiving and publishing (with no avoidance involved).

There are two other references to 'delays' in publishing books in the same document.⁷³ In both of these a 'delay' means the time that, in the end, happened to elapse between conceiving of an idea and publishing it. This manner of work was typical for Darwin. He would notice something or have an idea and think about it and continue to make relevant observations for some time, and when he finished a current project he would devote himself to one of his earlier ideas or interests. Herbert observed that Darwin, until 1861, always completed one book before starting another.⁷⁴ Many years later Darwin observed, 'it is a great wrench to my mind to change from one subject to another'.⁷⁵

In addition to the two 'delayed' books referred to above, Darwin's notes on the psychological development of his baby son William were not published for 37 years in 1877.⁷⁶ His inheritance theory of pangenesis was not published for 27 years.⁷⁷ He began observing orchids in the 1830s but did not publish his book on the subject until 1862. He began working on cross-fertilization in 1839 but only published his book on the subject in 1876. He first realized the significance of the minuscule actions of worms for modifying the landscape in 1837 and published a short article in 1838 but did not publish his book on the subject until 42 years later, in 1881.⁷⁸

FILLING THE GAP

If Darwin did not avoid publishing for many years, then why was there the long gap between conception and publication? To answer this question we must remember that Darwin came to believe that species are mutable while he was already fully embedded in the early stages of a major publishing and editing campaign that would last more than a decade. As the editors of Darwin's correspondence observed, 'Darwin's investigation of the species question went on, literally and figuratively, in the background of his other work. In the foreground, his major occupation in terms of both energy and time was the publication of the scientific results of the *Beagle* voyage.'⁷⁹

From his letters it is possible to see that throughout the time that he was musing on transmutation, Darwin foresaw his current commitments as extending long into the future. He expected, as he wrote to his fiancée, Emma Wedgwood, in November 1838, to be tied to London for 'rather more than' three years to come.⁸⁰ He spent 'October–November preparing scheme of Zoology of Voyage of Beagle, commenced Geology & finished proofs of Journal—Paper on Worms forming mould'.⁸¹ In February 1838 he 'Finished St. Helena & small islands in Atlantic. Also speculated much about "Existence of Species" & read more than usual'.⁸² Mostly he worked on *Zoology* and *Geology* and occasionally, as he noted in June 1838, 'some little species theory'.⁸³ In May he estimated to his sister that his *Beagle* publications would require hard work 'for the next two or three years'.⁸⁴ In September he noted, 'Frittered these foregoing days away on working on Transmutation theories'.⁸⁵

Clearly, transmutation was a fascinating subject, but not his work of first priority. It was not until 28 September 1838 that he read Thomas Malthus's *Essay on the principle of population*—'for amusement', he later recalled.⁸⁶ Yet, as Janet Browne has written, he was 'clearly following up lines of inquiry relating to individual variation, averages, and chance, as well as seeking information on human population statistics'.⁸⁷ The species work is not mentioned in his journal again until 'End of February & first week in March Earthquake Paper then a little work on Species'.⁸⁸ He wrote of it to his cousin Fox, 'It is my prime hobby & I really think some day, I shall be able to do something on that most intricate subject species & varieties'.⁸⁹ It is obvious from the way in which Darwin wrote in his notebooks and loose notes that his developing theory of transmutation was intended to persuade his sceptical fellow naturalists. But when? From the remark in Notebook C (quoted above) we know that he was already talking about his ideas with many people by 1838. As he later wrote of this time in his autobiography, 'Nor did I ever intermit collecting facts bearing on the origin of species; and I could sometimes do this when I could do nothing else from illness.'⁹⁰

By 1839, when Darwin finished his Notebook E, much of the basic framework of the theory of evolution by natural selection had been sketched out. Darwin, writing in 1876, recalled 'Here then I had at last got a theory by which to work; but I was so anxious to avoid prejudice, that I determined not for some time to write even the briefest sketch of it'. It is unlikely that the 'prejudice' referred to was that of his peers. Janet Browne has suggested that Darwin meant he wished to keep his own mind unprejudiced on the subject.⁹¹ His letters written at the time certainly betray no fear of this kind and none of those he wrote to about transmutation reacted with prejudice. In fact, he wrote to Fox in January 1841, 'I continue to collect all kinds of facts, about "Varieties & Species" for my some-day work to be so entitled'.⁹² In any case this determination not to write a sketch lasted only until 'June 1842 [when] I first allowed myself the satisfaction of writing a very brief abstract of my theory in pencil in 35 pages'.⁹³ He had just finished correcting the proofs of his book on coral reefs, so he had a bit of time before continuing with *Beagle* work.

We should focus on the words 'a theory by which to work'. Although the transmutation notebooks were largely finished by 1840–41, this does not mean that Darwin had completed his theory and could therefore publish it. He continued making loose notes organized into subject portfolios. The notebook period ends full of questions and plans for a very broad and at the same time in-depth research programme.⁹⁴ It is impossible to read over the thousands of pages of Darwin's loose notes from the early 1840s with endless detailed questions about distribution, inheritance, hybridity or the fossil record and expect that publication could occur in the near future. This material demonstrates that Darwin intended to uncover a vast body of evidence about many particular points in support of his general conclusions. The specificity of his queries together with their enormous range equal a research programme of great ambition and vast scope, and not one to be concluded in a few months or even a year or two. Yet at this time Darwin did not have a few years to devote to a new object: he was already fully engaged in his *Beagle* publications.

In a letter to the German naturalist and translator of *Journal of researches*, Ernst Dieffenbach, from July 1843 Darwin remarked on his species work, 'I intend sometime to publish'.⁹⁵ Later that month Darwin wrote freely and frankly to Waterhouse, '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'.⁹⁶

Apart from some occasional species work Darwin returned to writing his book *Volcanic islands* and in October wrote 'Paper on Sagitta and on seeds'.⁹⁷ By January of 1844 he had sent *Volcanic islands* to the printers. He finished corrections of the same in February 1844. With this out of the way he spent some months enlarging and improving his species pencil sketch into a 230-page essay. He then wrote the famous memorandum to his wife on 5 July 1844:

I have just finished my sketch of my species theory. If, as I believe, my theory in time be accepted even by one competent judge, it will be a considerable step in science. I therefore write this in case of my sudden death, as my most solemn and last request ... that you will devote 400 pounds to its publication. ... I wish that my sketch be given to some competent person, with this sum to induce him to take trouble in its improvement and enlargement.

The prospective editor was also to be given all of Darwin's books and notes, as these were essential to expand the essay. He went on to remark that if necessary another hundred pounds should be offered 'as the correcting and enlarging and altering my sketch will also take considerable time' because it was written 'from memory without consulting any works, and with no intention of publication in its present form'.⁹⁸ This 1844 essay was 'Sent ... to Mr. Fletcher to be copied—Corrected it last week in Sept'.⁹⁹ Darwin then returned to his work on the geology of the voyage. As *Vestiges* was published in October, it could not be responsible for Darwin's continuing with other, non-evolutionary, work after writing this essay. Interestingly, species work is not mentioned again in his pocket journal for 10 years, when he finished the barnacle volumes in 1854.¹⁰⁰

So in July 1844 Darwin had written a lengthy essay outlining his theory of transmutation 'with no intention of publication in its present form'. And publication, intended for some unnamed future date, would first require 'considerable time' for 'correcting and enlarging and altering'. The fair copy was intentionally left with plenty of margins and empty pages for further revision. This was not a finished work. The manuscript has many pencil corrections and alterations by Darwin and comments by Emma.¹⁰¹ Six years later Darwin referred to it as 'my rude species = sketch'.¹⁰²

Across his life Darwin consistently referred to this sketch as not intended for publication. In 1858, when portions of the sketch were published with Wallace's essay, Darwin added a footnote: 'This MS. work was never intended for publication, and therefore was not written with care'.¹⁰³ He also added his 1857 sketch for Gray. After this first publication Darwin had an urgent need to publish an abstract of his larger unfinished work, but he did not publish the 1844 essay. Instead he spent a gruelling 13 months writing *Origin of species* (1859). In 1859 Darwin wrote to Wallace that the latter's essay 'puts my extracts, (written in 1839 now just 20 years ago!) which I must say in apology were never for an instant intended for publication, in the shade'.¹⁰⁴ If Darwin wished to ensure priority over Wallace, surely it would have been better to say he was ready to publish long ago but held back to avoid abuse, rather than saying he was still working on it.

In July 1844, the same month in which he wrote the species sketch, he also wrote papers on Atlantic dust¹⁰⁵ and on terrestrial and marine Planariae (flat worms)¹⁰⁶ and sorted his collections. At the end of the month he began *Geological observations on South America. Being the third part of the geology of the voyage of the Beagle*.¹⁰⁷ In September he corrected the fair copy of his species sketch. In August he wrote to the Swiss stratigrapher and archaeologist Adolf von Morlot, 'I regret exceedingly to say, that I cannot undertake to see your Journal published: my health during the last three years has been exceedingly weak, so that I am able to work only two or three hours in the 24: these are more than fully occupied &

I have materials for several years' work, which is almost more than I dare undertake'.¹⁰⁸ So Darwin foresaw that his continuing *Beagle* work would take 'several years' to complete.

On 12 October 1844 Darwin wrote to his friend the clergyman naturalist Leonard Jenyns about the species work: 'I shall not publish on this subject for several years—At present I am on the geology of S. America'.¹⁰⁹ Similarly, in the following month, in the midst of that work, Darwin described his species theory as a 'far distant work'.¹¹⁰ In February 1845 he wrote to Jenyns, 'it will be years before I publish [on species], so that I shall have plenty of time to think of better words'.¹¹¹ Darwin finished his first draft of his book on South American geology after nine months, in April 1845. He then began work on the second edition of his *Journal of researches*, which took him four months, finishing at the end of August. As is well known, many passages in the revised edition indicate that Darwin believed that species change.¹¹² Rather than viewing these as veiled hints, it seems entirely consistent with Darwin's interests and professional conversations. At any rate, Darwin had already published in 1837 remarks that suggested a belief in transmutation.¹¹³

In early September 1845 Hooker wrote to Darwin about the speculative work on species by Frédéric Gérard, the French writer on botanical subjects.¹¹⁴ 'I am not inclined to take much for granted from anyone [who] treats the subject in his way and who does not know what it is to be a specific Naturalist himself.'¹¹⁵ Darwin responded that this remark could also include him.

How painfully (to me) true is your remark that no one has hardly a right to examine the question of species who has not minutely described many. I was, however, pleased to hear from Owen (who is vehemently opposed to any mutability in species) that he thought it was a very fair subject & that there was a mass of facts to be brought to bear on the question, not hitherto collected. My only comfort is, (as I mean to attempt the subject) that I have dabbled in several branches of Nat. Hist: & seen good specific men work out my species & know something of geology; (an indispensable union) & though I shall get more kicks than half-pennies, I will, life serving, attempt my work.¹¹⁶

Passages like this are often interpreted by those looking for Darwin's reasons for avoiding publication as evidence that consideration of critical reaction must have held him back. Yet Darwin explicitly states *twice* that he would persevere, in spite of expected criticism. Despite Hooker's remark, which so troubled Darwin, that some might think him unqualified as a species theorist, Darwin wrote: 'All which you so kindly say about my species work does not alter one iota my long self-acknowledged presumption in accumulating facts & speculating on the subject of variation, without having worked out my due share of species. But now for nine years it has been anyhow the greatest amusement to me.'¹¹⁷ In another letter of November 1845 Darwin wrote, 'I hope this next summer to finish my S. American geology; then to get out a little zoology & hurrah for my species-work, in which, according to every law of probability, I shall stick & be confounded in the mud'.¹¹⁸ Modest humility aside, Darwin actually finished *Geology* in October 1846. If his 'little Zoology' took him another year, this estimate might mean he expected (in late 1845) that he would begin full-time species work by the beginning of 1848. If he then spent five years working on it (as he estimated), publication could result in 1853 (figure 1).

It is crucial to bear in mind that before starting his theory of the origin of species Darwin was fully committed to and embedded in a major series of editing and publishing efforts devoted to his *Beagle* specimens and experiences. Between 1839 and 1846 Darwin brought out 10 books. There were the two editions of his *Journal of researches*, five massive volumes of the *Zoology of the voyage of H.M.S. Beagle*, which he planned, edited and contributed to, the three volumes on the geology of the *Beagle* voyage (including his theory of coral reefs)

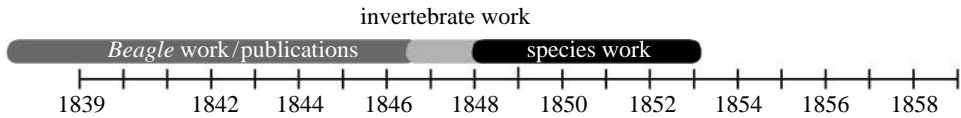


Figure 1. Darwin's November 1845 estimate.

and he made 20 contributions to periodicals. A final volume on invertebrates was planned for the *Zoology* series, which Darwin planned to write himself.¹¹⁹

BARNACLES

So if Darwin intended all along to work out and publish his species theory after completing his long-term *Beagle* publishing projects, why did he not publish around 1853 as originally planned? Hooker wrote to Darwin some time before 3 September 1846, 'When that work [*Geology of South America*] is over you will I suppose attack *Species* as you have long promised I wish you joy of the task'.¹²⁰ It should be remembered that Hooker was not, at this time, a transmutationist. At any rate, Darwin's intimate friend believed that Darwin would soon undertake that work. If the hypotheses that Darwin dared not publish were true, then Darwin's most intimate confidant was unaware of it.

By October 1846 only one thing really remained of the *Beagle* materials: the invertebrates. As Darwin wrote to Henslow, 'You cannot think how delighted I feel at having finished all my *Beagle* materials, except some invertebrata: it is now 10 years since my return, & your words, which I thought preposterous, are come true, that it w^d take twice the number of years to describe, that it took to collect & observe'. Similarly, Darwin wrote to Fitzroy: 'I have now with the exception of some Zoological papers on the lower marine animals completed all which I shall ever attempt on the materials collected during the voyage'.¹²¹

It had become clear as early as 1844 that the Treasury grant of £1000 for the *Zoology* series would not be enough to also cover a work on invertebrates. As the editors of Darwin's correspondence remark, 'the exhaustion of the government grant forced him to abandon the idea. Instead he decided to publish his own observations and descriptions of the specimens that he considered to be important new discoveries, and did so in articles on *Sagitta* and *Planariae*'.¹²²

Darwin planned to write about the invertebrates himself because they were his particular interest and expertise. As far back as 1825 in Edinburgh, marine invertebrates had fascinated Darwin when he studied with Robert Grant. Richard Keynes noted that 'well over half of the pages of the *Zoology* Notes [on the *Beagle*] were concerned with marine invertebrates'.¹²³ Much of Darwin's book on coral reefs, often referred to as a book only of geology, also deals with marine invertebrates.

Darwin's invertebrate collection included a curious new parasitic barnacle from South American waters. He wrote to Hooker in October 1846, 'I am going to begin some papers on the lower marine animals, which will last me some months, perhaps a year, and then I shall begin looking over my ten-year-long accumulation of notes on species and varieties, which, with writing, I dare say will take me five years, and then, when published, I dare say I shall stand infinitely low in the opinion of all sound Naturalists—so this is my prospect for the future'.¹²⁴ So in October 1846 Darwin still intended to write some papers on the anatomy and classification of barnacles and then proceed to his species theory. If we work out the time estimate in this letter we come to around October 1852 at the earliest. This was about the same as his estimate from the year before.

Gradually the barnacle work became increasingly interesting and satisfying for Darwin, who had spent the past decade in writing.¹²⁵ As he wrote to Fitzroy at the end of October, 'for the last half month [I have been] daily hard at work in dissecting a little animal about the size of a pin's head from the Chonos Arch[ipelago]. & I could spend another month on it, & daily see some more beautiful structure!'¹²⁶ In early November he wrote to Hooker, 'there is an extraordinary pleasure in pure observation; not but what I suspect the pleasure in this case is rather derived from comparisons forming in ones mind with allied structures. After having been so many years employed in writing my old geological observations it is delightful to use one's eyes & fingers again.'¹²⁷ By the end of the month he reported to Owen, 'Having worked out pretty carefully my new articulated Balanus (to be call Arthrobalanus) I have become so much interested in the structure of the sessile Cirripedes, that I am dissecting 5 or 6 of the other genera'.¹²⁸ Dieffenbach was told a few months later, in February 1847,

I have for the *present* given up Geology, & am hard at work at pure Zoology & am dissecting various genera of Cirripedia, & am extremely interested in the subject. I always, however, keep on reading & observing on my favourite work on Variation or on Species, & shall in a year's time or so, commence & get my notes in order; should any important paper appear on this subject in any German periodical, I sh^d be greatly indebted for information of it.¹²⁹

So a few months into barnacles his estimate of when he would begin species work, about early 1848, was unchanged.

But the barnacle work was slowly becoming more than a few papers. He wrote to Hooker in February 1847, 'I hope to Heaven I am right in spending such a time over one object'.¹³⁰ Darwin certainly considered them 'beloved' barnacles at first.¹³¹ He did not make a single conscious decision to do the entire group of barnacles in four volumes instead of just some papers; it was a gradual process. As he wrote in his autobiography, 'To understand the structure of my new Cirripede I had to examine and dissect many of the common forms; and this gradually led me on to take up the whole group'.¹³² He was also strongly encouraged by colleagues who thought such a work was needed. Darwin seems to have moved to take up the whole group between October and December 1846.¹³³ Hooker welcomed the project as a grand challenge for Darwin: 'I am so glad you have plucked up courage for the cirripeda', he wrote.¹³⁴

It is important to date the change from Darwin's intention to do some barnacle papers to his conducting a full group project because four years later, in October 1849, Darwin referred to this decision in a letter to Hooker: 'By-the-way, you say in your letter that you care more for my species work than for the Barnacles; now this is too bad of you, for I declare your decided approval of my plain Barnacle work over theoretic species work, had very great influence in deciding me to go on with the former, and defer my species paper'.¹³⁵ Another reason for allowing the barnacle work to expand is explained in a letter to the Swiss-born American geologist and zoologist Louis Agassiz: 'I have been particularly gratified in receiving specimens from yourself, in as much as, when doubting whether to undertake a monograph of the class, or to confine myself to their anatomy, your sentence that "a monograph on the Cirripedia was a pressing desideratum in Zoology" much helped to decide me'.¹³⁶ Darwin was convinced that a monograph on the barnacles was an important desideratum and he was fascinated by them from the beginning. In the same letter Darwin estimated that he would not finish his monograph for two or three more years because his health allowed him to work very little. This would mean that he believed, in October 1848, that his barnacle work would be finished by October 1850 or 1851. If we add his five years of

projected species work to this date we come to the period when Darwin believed his barnacle work would defer species work: instead of 1853 it would be 1855 or 1856.¹³⁷

So, some time after Darwin's October 1846 estimate of publishing species in about 1853 he decided to spend more time on his barnacle work, which would defer species work to 1855 or 1856. Many writers urge that there must have been additional reasons for this decision, such as that any project would be a welcome procrastination, or that he wished to avoid work on species or he required the added status and experience to proceed with his species work. Although certainly plausible suggestions, there is no evidence to support them. The change to a full barnacle programme is well documented and entirely explicable without such hypotheses.¹³⁸

At any rate it is entirely clear from so many statements, to so many people and over so many years, that Darwin never intended to work out and publish his species theory until he had finished publishing the *Beagle*-related works including that on invertebrates, which evolved into the barnacle studies. The amount of time spent on barnacles expanded beyond Darwin's original intentions and, according to his own estimate, he 'lost 1 or 2 years by illness'.¹³⁹ As he wrote playfully to Hooker, 'do not flatter yourself that I shall not yet live, to finish the Barnacles & then make a fool of myself on the subject of Species'.¹⁴⁰

By 1850 Darwin's beloved barnacles had become 'confounded' barnacles.¹⁴¹ Still, he was committed to finishing what he had started. He wrote to the mollusc expert Albany Hancock, 'I have been working like a wretched slave at *mere* [barnacle] *species* & have many more months' work, & till I have completed this slavery, I have not heart to begin work of interest, for I think I shd never get courage to resume the drudgery'.¹⁴² Perhaps transmutation was the 'work of interest' he had in mind? By October 1852 he wrote to Fox, 'I am at work on the second vol. of the Cirripedia, of which creatures I am wonderfully tired: I hate a Barnacle as no man ever did before, not even a Sailor in a slow-sailing ship'.¹⁴³

So the amount of time spent on barnacles and lost to illness was not, and could not have been, foreseen. When he could see some light at the end of the barnacle tunnel he wrote to Hooker in October 1853 that he would be at his 'species book' 'In a year or two's time'.¹⁴⁴ And, indeed, as soon as the final monograph of cirripedia was published, on the very day that he finished packing up borrowed specimens, he noted in his journal 'Sept 9 [1854] Began sorting notes for Species theory'.¹⁴⁵ We should also recall that the barnacle monograph itself contains very many statements consistent with the transmutation of species.¹⁴⁶

'From September 1854', he wrote in his autobiography, 'I devoted all my time to arranging my huge pile of notes, to observing, and experimenting, in relation to the transmutation of species'.¹⁴⁷ He experimented with the dispersal of seeds and took up pigeon breeding. He wrote to Fox, referring to his species work in the most casual way, in March 1855:

I forget whether I ever told you what the objects of my present work is,—it is to view all facts that I can master (eh, eh, how ignorant I find I am) in Nat. History, (as on geograph. distribution, palaeontology, classification Hybridism, domestic animals & plants &c &c &c) to see how far they favour or are opposed to the notion that wild species are mutable or immutable: I mean with my utmost power to give all arguments & facts on both sides. I have a *number* of people helping me in every way, & giving me most valuable assistance; but I often doubt whether the subject will not quite overpower me.¹⁴⁸

This was, again and again, Darwin's explicit concern that the subject would be too much for him because it was so large and complex. He nowhere remarks that expected criticism might influence publishing on the subject. In fact, on the contrary, every one of his statements declares that he would publish despite what others might think of him.

On 14 May 1856 Darwin 'Began by Lyells advice writing species sketch'.¹⁴⁹ From this point onwards he worked almost exclusively on his projected 'big book' on species, as we see from the consistent entries in his journal. Darwin had in mind a great scientific treatise like Lyell's classic three-volume *Principles of geology* (1830–33).¹⁵⁰ By the spring of 1858 Darwin had completed more than 10 chapters covering two-thirds of the topics later discussed in *Origin of species*.¹⁵¹ The fateful letter from Wallace arrived in June 1858 containing a draft essay that proposed a strikingly similar theory for the origin of species.¹⁵²

So it is fair to say that by 9 September 1854, when Darwin began sorting his species notes, he had begun his full-time species work. The 'big book' would probably have been finished by 1860.¹⁵³ This is about eight years beyond Darwin's 1845 estimate of when he would publish his theory. And the difference between these two dates is identical to the amount of time spent working on barnacles and lost to ill health. Thus the gap is filled.

Darwin's barnacle work, like all his projects, took considerably longer than he at first estimated. Even after the publication of his 'abstract', *Origin of species* (1859), Darwin still planned to publish his great work. As Robert Stauffer noted, '[Darwin] did not abandon his long manuscript, nor write on the unused backs of the sheets for drafting other new publications as he so often did with other manuscripts'.¹⁵⁴ So we have every reason to believe that Darwin intended all along to write a great multi-volume work on transmutation, a work that would take at least five years to prepare; such an ambitious and protracted work would begin when current projects were completed.

NOT FINISHED, NOT DELAYED

In addition to the evidence surveyed above that Darwin never planned to publish before 1853, we must remember the many outstanding issues he addressed between 1844 and 1858. There was a great deal of thinking to be done, experiments to be performed, experts to be questioned, problems to be solved and books to be read. There was the great problem of evolutionary divergence.¹⁵⁵ Another difficulty that Darwin, writing in his notes in 1848, called 'the greatest *special* difficulty I have met with' and described rather dramatically in the *Origin* as 'first appeared to me insuperable, and actually fatal to my whole theory. I allude to the neuters or sterile females in insect-communities'.¹⁵⁶ In November 1854 he decided he had solved this with what he called 'family' or kin selection.

Darwin experimented extensively in the 1850s with natural means of dispersal and the individual variations and crossings of pigeons and other domesticated and wild varieties. He also experimented with hive bees, attempting to understand the origins of their geometrical constructions.

Darwin also learned much for the species theory while he worked on the classification and morphology of barnacles. This led Darwin to think less of species originating in geographical isolation, and more towards omnipresent variations ever sifted by natural selection. The barnacles gave him a well-founded understanding of natural variation. The experience he gained in taxonomy, morphology and ontogenetic research can hardly be overestimated.¹⁵⁷ Nevertheless the barnacle work should not be justified in terms of its use for a later project, in terms of either skills gained or reputation earned. To do so would be to restrict Darwin ahistorically to a man interested only in his theory of evolution when in fact, as his broad range of publications demonstrates, he had wide and deep interests in many natural phenomena. Some of the most focused and detailed work by historians on the development of Darwin's theory shows how his thinking changed in some subtle but crucial ways from the

early notebooks to the 1850s. Dov Ospovat argued that during the 1840s and 1850s Darwin gradually lost his teleological way of thinking, pondered questions of whether adaptation was perfect, how species diverge, and whether evolution is a progressive process.¹⁵⁸ This and other work confirms that Darwin was not finished. We should always endeavour to remember the staggering magnitude of Darwin's research programme. Doing so makes the amount of time that elapsed less surprising.

CONCLUSION

It is argued in this essay with a wide range and representative collection of evidence that the belief that Darwin put off publishing his species theory is wrong. Darwin himself consistently described his story in a different way: 'I opened my first note-book for facts in relation to *Origin of Species*, about which I had long reflected, and never ceased working on for the next twenty years'.¹⁵⁹ This should be credited not just because they are Darwin's own words, which could be biased or misleading, but because it is consistent with all the contemporary evidence.

Darwin and his contemporaries referred to the gap years as the time spent working on the theory. Darwin remarked to Sedgwick after publishing the *Origin*, 'I have worked like a slave on the subject for above 20 years. ... But I do not think you would wish anyone to conceal the results at which he has arrived after he has worked, according to the best ability which may be in him.'¹⁶⁰ Gray wrote to Darwin, 'I do not think 20 years too much time to produce such a book in'.¹⁶¹ After finishing the *Origin* Darwin wrote to Lyell:

I suppose that I am a very slow thinker, for you would be surprised at the number of years it took me to see clearly what some of the problems were, which had to be solved,—such as the necessity of the principle of divergence of character—the extinction of intermediate varieties on a continuous area with graduated conditions,—the double problem of sterile first crosses & sterile hybrids, &c &c.—

Looking back, I think it was more difficult to see what the problems were than to solve them, as far as I have succeeded in doing; & this seems to me rather curious.¹⁶²

The essentially consistent estimates in his letters, the renditions of those who knew Darwin, the mass of details he still needed to sort out because of the magnitude of the proposed species project and the amount of time he felt necessary to complete the species work are all consistent with the view that he never meant to publish before the early 1850s. This diverse evidence converges on the conclusion that Darwin did not avoid publishing, as is commonly believed.

It was detailed above how Darwin began to theorize about species at the beginning of his 10-year-long *Beagle* publishing projects. These were to be finished by work on invertebrates from 1846 to 1848 or 1849. The invertebrate work gradually lengthened to become the barnacle monograph, lasting until 1854 (figure 2).

The years of barnacle work should not be described as a digression or excuse to postpone or avoid publishing. The barnacle studies evolved gradually and in an unbroken routine out of the *Beagle* publishing programme. Barnacles were a continuation of what he had been doing all along and, in a sense, a culmination of his work on a particular favourite class of life over the previous 20 years. True, Darwin also saw some relevance for his species theory in the work. Nevertheless the barnacles should not be justified or excused because they contributed to, or were a necessary preparation for, the *Origin*.

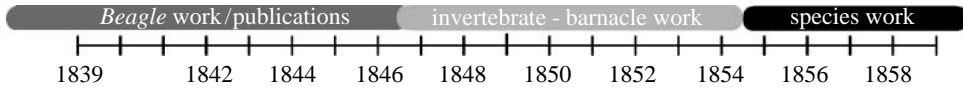


Figure 2. What Darwin actually did.

Richards was right to point out that one explanation cannot be given to so complex a series of phenomenon as Darwin's thoughts and actions over the course of 20 years. One can go a step further to consider that there was also no singular or cohesive block of time to which any decision or plan by Darwin can be hypothesized to apply. Instead, Darwin was occupied with many different, sometimes overlapping, sometimes successive, objectives throughout this period.

The fact that Darwin discussed transmutation so openly in his correspondence and conversation contradicts the widely held view that Darwin kept his views secret because he was afraid of disapproval. The reactions we do have in letters and contemporary diaries bear this out. His friends and colleagues did not agree with him, but they did not ostracize him. This interpretation must be entirely abandoned.

The oft-repeated assertion that Darwin put off publishing his species work because he was afraid is not supported by the evidence. It is abundantly clear that Darwin also made detailed enquiries leading to the support or modification of his theories from hundreds of his contemporaries. It should also be reiterated that there is not a shred of evidence for the widely held view that the reception of *Vestiges* made Darwin put off his own species work or that he hoped to avoid upsetting his wife. The only evidence that has ever been put forward for Darwin's delay is circumstantial or ambiguous passages. It is curious that so many groundless and dubious theories have been put forward for so many years to explain why Darwin delayed. Yet they all share a desire to substantiate the previous belief that Darwin was prevented from publishing by fear(s).

One can go so far as to assert that the contemporary evidence alone would never lead us to believe that Darwin was so afraid that he concealed or postponed his evolutionary views in the 1830s or 1840s. Emma wrote to Darwin in about February 1839, 'Your mind and time are full of the most interesting subjects and thoughts of the most absorbing kind, viz. following up your own discoveries'.¹⁶³ She did not see a Darwin full of fear, but one full of fascination for his study of nature. In November 1844 Darwin wrote about his theory to Jenyns, 'I am a bold man to lay myself open to being thought a complete fool, & a most deliberate one.'¹⁶⁴ Darwin could imagine being thought a fool and of proving not up to the enormous task, but he certainly does not give us the sense that he was very afraid. Darwin consistently maintained that he would carry on regardless of what people might think of his species theory.¹⁶⁵ He felt the same about his views on erratic boulders, as he wrote to the geologist John Phillips: 'I believe I shall get well abused for my paper & not make single convert to my own view, but I am resolved not to show a white feather & bring it out'.¹⁶⁶ Similarly he did not refrain from publishing other controversial theories such as coral reefs, the parallel roads of Glen Roy, or pangenesis.

Rather like the legend of Darwin's finches, the notion of Darwin's delay emerged during the mid-twentieth century. Biographies and accounts of Darwin did not contain the slightest hint that he avoided publishing his theory, until almost a century after 1859. Darwin's son Francis gives us no sense of a postponement in *Life and letters* (1887), *More letters* (1903) or *Foundations* (1909). Instead, those who knew Darwin wrote in ways consistent with Darwin's own rendition.

Why the notion of Darwin's delay emerged in the twentieth century is a complex question that would require its own study to answer adequately. Perhaps, just as Gruber tried to find hidden meaning behind the hanging dream, scholars in the twentieth century were imbued with Freudian expectations that there must be hidden motives behind Darwin's actions that could be uncovered. Freud's biographer Ernest Jones wrote about 'the psychology of discoverers' in 1959:

[Discovering] the relation of Natural Selection to Evolution, ... meant displacing God from His position as a detailed Creator specially concerned with mankind. ... One of them, Darwin, the one who stood in such awe of his own father, said it was 'like committing [sic] murder'—as, indeed, it was unconsciously; in fact, parricide. He paid the penalty in a crippling and lifelong neurosis, and in an astonishing display of modesty, hesitancy, and dubiety concerning his work.¹⁶⁷

There are also insurmountable inconsistencies with theories of avoiding publication. If Darwin dared not publish because the times were wrong, why did he not later mention such a significant life event when the times *were* right? And why did not other Victorians such as Francis Darwin, Huxley or Wallace not say so later? Why not trumpet that their modern late Victorian times were liberated and able to receive Darwin's truths? Or, at the very least, why was this major life event not mentioned somewhere at least once? There is no hint in the recollections of Darwin's friends or family that fear caused putting off publishing.¹⁶⁸

We do know, for example, that Darwin planned not to publish his views on the origins of humans because the times were not ripe. He tells us so twice, once in his autobiography and in the opening lines of the *Descent of man* (1871):¹⁶⁹

During many years I collected notes on the origin or descent of man, without any intention of publishing on the subject, but rather with the determination not to publish, as I thought that I should thus only add to the prejudices against my views. ... Now the case wears a wholly different aspect. ... In consequence of the views now adopted by most naturalists, ... I have been led to put together my notes....¹⁷⁰

His thoughts on humans were held back, and indeed it is only the M and N notebooks (which dealt with these subjects) that Darwin marked as 'Private' on the covers in the 1830s.¹⁷¹ The transmutation notebooks simply carried Darwin's name and address on the inside cover in case he mislaid them at his club or a museum.¹⁷²

So there is an additional problem for the view that Darwin avoided publication: namely that if he did so he must also have concealed the fact because it is never mentioned in countless documents where it should appear when describing how he came to publish *Origin*. But why would he do this when he was happy to say why he held back his views on humans? Instead, Darwin mentioned in letters again and again that he would pursue his species theory when he completed the *Beagle* and barnacle work. And this is precisely what he did in the end.

On the first page of *Origin* Darwin established how long ago he had commenced his work on the origin of species—in 1837. But he did not say he was ready to publish then. Instead, he told his readers 'from that period to the present day I have steadily pursued the same object'. This is strikingly different from Wallace's first line of the *Malay Archipelago* (1869): 'My readers will naturally ask why I have delayed writing this book for six years after my return; and I feel bound to give them full satisfaction on this point.'¹⁷³ His answer was that he was busy. Darwin either did not feel that he had anything to explain or he concealed that he had put off publishing for some reason(s).

Historians can assess the tenor of a context but cannot conclude how this generalization must have affected the brain of an individual in that context. If Darwin was obsessed with the difficulty of his situation, holding an impossibly unorthodox view, why was neither this difficulty nor possible solutions or ways around the difficulty (such as publishing anonymously) ever discussed in the correspondence with his confidants? As already shown, Darwin talked openly about his belief in transmutation with just about everyone he knew. We can also now appreciate, for the first time, that all of the names put down as potential editors of Darwin's 1844 essay knew that Darwin was a transmutationist because he had talked with them about his speculations.

We start off by asking the wrong questions if we ask why Darwin put it off or what his motives were for delaying. Such questions presume that Darwin did put it off. If Darwin did not avoid publishing, then our understanding will be unnecessarily limited. Instead we should ask: what happened? What did Darwin do; how did he work?

In one sense it is quite ahistorical to ask why Darwin did not publish his belief in transmutation at once. It is quite plausible that in the 1830s and 1840s it never even occurred to him to publish his incomplete views but instead, as indicated by all the evidence we have bearing on the question, he considered it a great work that he would shoulder some day, when finished with current projects.

Darwin was a wealthy gentleman of leisure, though he worked very hard at his scientific pursuits. He could not lose his livelihood for publishing unwelcome theories. There was also no culture of 'publish or perish'. It is misleading to demand why Darwin did not publish a draft essay that he explicitly described as written 'from memory, without consulting any works & with no intention of publication in its present form'.¹⁷⁴ Many years later Darwin told Lyell, 'I had not intended to publish any sketch' and Hooker heard, 'I *truly* never dreamed of [publishing a brief sketch], till Lyell suggested it'.¹⁷⁵

This should force us to question that transmutation was quite so taboo as often described. There is the example of a letter written by John Herschel, perhaps the most senior British scientific figure. In this widely read letter to Lyell, in which Herschel praised the approach of *Principles of geology* as a way to find true causes, 'that mystery of mysteries the replacement of extinct species by others' should be due to natural causes as much as extinctions. So the origin of new species 'would be found to be a natural in contradistinction to a miraculous process'.¹⁷⁶ The diary of Bunbury (also a non-transmutationist) also records casual conversation with Darwin on this subject. Martin Rudwick's invaluable work provides abundant reasons why transmutation was a reasonable arrangement of the then-current palaeontological and biological evidence.¹⁷⁷ The subject is mooted in Owen's 1849 lectures on limbs.¹⁷⁸ There are very many further examples. Perhaps historians have tended to overemphasize the heterodoxy of transmutation ideas before 1859. At one time this might have been done because it made a more dramatic story with Darwin triumphing against the blinkered scientific world. But later this evolution taboo-centred perspective may have been given a new lease on life when it seemed to explain the delay that historians had learned was true, but which was entirely absent from the primary evidence.

The myth of Darwin's delay has remained unquestioned for far too long. It generates a cascade of subtle errors that ultimately accumulate to a distorted picture of the man and his science and indeed to early Victorian scientific communities. A varied and overwhelming array of evidence demonstrates that Darwin did not avoid publishing his theory for 20 years. For my part I suspect that being older and having a more established reputation made it easier for Darwin to publish, but there is no evidence for this either. It is just an educated guess.

And this is the ultimate lesson to be drawn from the myth of Darwin's delay: the danger of confirmation bias lurks over historians just as much as scientists, if not more so. Once we, as historians, come to believe a story, it is easy to find apparent confirmations and, when the evidence contradicts it, difficult to let it go.

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- 81 DAR 158 f12. C. Darwin (ed.), *The zoology of the voyage of H.M.S. Beagle* (5 volumes) (Smith, Elder and Co., London, 1838–43); C. Darwin, *The structure and distribution of coral reefs* (Smith, Elder & Co., London, 1842); C. Darwin, *Geological observations on the volcanic islands* (Smith Elder & Co., London, 1844).
- 82 DAR 158 f13.
- 83 DAR 158 f14.
- 84 To S. Darwin [15 May 1838], *CCD* 2:87.
- 85 DAR 158 f15.
- 86 Barlow, *op. cit.* (note 26), p. 120. T. Malthus, *Essay on the principle of population* (Murray, London, 1826).
- 87 Browne, *Voyaging, op. cit.* (note 46), p. 385.
- 88 DAR 158 f17.
- 89 To W. D. Fox [15 June 1838], *CCD* 2:92. See also letter to C. Lyell [14] Sept. [1838], *CCD* 2:107.
- 90 Barlow, *op. cit.* (note 26), p. 99.
- 91 J. Browne, personal communication.
- 92 To W. D. Fox [25 Jan. 1841], *CCD* 2:279.
- 93 Barlow, *op. cit.* (note 26), p. 120.
- 94 This view is shared by Smith, *op. cit.* (note 47). On how unfinished Darwin's notebook speculations end *ca.* July 1839 see Kohn, 'Introduction to Notebook E' in Barrett *et al.*, *op. cit.* (note 18), p. 396.
- 95 To E. Dieffenbach, 4 July [1843], *CCD* 7: 476–478. Darwin enclosed a questionnaire concerning differences between races or varieties and species, 'as "Species" is a subject, which I intend sometime to publish on.' Darwin, *Journal of researches* (Henry Colburn, London, 1839).

- 96 To G. R. Waterhouse [26 July 1843], *CCD* 2:375–376.
- 97 C. Darwin, 'Brief description of several terrestrial *Planariae*'. *Ann. Mag. Nat. Hist.* **14**, 241–251 (1844), and C. Darwin, 'Observations on the structure and propagation of the genus *Sagitta*', *Ann. Mag. Nat. Hist.* **13**, 1–6 (1844).
- 98 *CCD* 3:43.
- 99 DAR 158 f22.
- 100 C. Darwin, *A monograph of the sub-class Cirripedia* (2 volumes) (Ray Society, London, 1851, 1854) and *Fossil Cirripedia of Great Britain* (2 volumes) (Palaeontological Society, London, 1851, 1854).
- 101 DAR 113. This was later published in F. Darwin (ed.) *Foundations of the origin of species* (Cambridge University Press, 1909).
- 102 To J. D. Hooker 13 June [1850], *CCD* 4:343.
- 103 C. Darwin and A. R. Wallace, 'On the tendency of species to form varieties; and on the perpetuation of varieties and species by natural means of selection', *J. Proc. Linn. Soc. Zoo.* **3** (20 Aug.), 45–62 (1858), at p. 45. This point was repeated in his autobiography (Barlow, *op. cit.* (note 26), p. 85).
- 104 To A. R. Wallace 25 Jan. [1859], *CCD* 7:241.
- 105 C. Darwin, 'An account of the fine dust which often falls on vessels in the Atlantic Ocean', *Q. J. Geol. Soc.* **2**, 26–30 (1846).
- 106 C. Darwin, 'Brief descriptions of several terrestrial planariae', *Ann. Mag. Nat. Hist.* **14** (Oct.), 241–251 (1844). On the Planariae see Browne, *Voyaging, op. cit.* (note 46), pp. 215–217.
- 107 Smith Elder & Co., London (1846).
- 108 To A. von Morlot 9 Aug. [1844], *CCD* 3:51.
- 109 *CCD* 3:59.
- 110 To L. Jenyns 25 [Nov. 1844], *CCD* 3:84–85.
- 111 To L. Jenyns, 14 Feb. [1845], *CCD* 3:142–143, at p. 143.
- 112 C. Darwin, *Journal of researches*, 2nd edn (Murray, London, 1845), pp. 52, 173, 380, 393, *passim*.
- 113 C. Darwin, 'On certain areas of elevation and subsidence in the Pacific and Indian oceans, as deduced from the study of coral formations', *Proc. Geol. Soc. Lond.* **2**, 552–554 (1837).
- 114 F. Gérard, 'De l'espèce dans les corps organisés', from A. d'Orbigny (ed.), *Dictionnaire universel d'histoire naturelle* (16 volumes) (Paris, 1841–49).
- 115 Hooker to Darwin [4–9 Sept. 1845], *CCD* 3:250.
- 116 To J. D. Hooker, [10 Sept. 1845], Down *CCD* 3:252–253.
- 117 To J. D. Hooker [18 Sept. 1845], *CCD* 3:255.
- 118 [5 or 12 Nov. 1845], *CCD* 3:263–264.
- 119 To L. Jenyns 10 Apr. [1837], *CCD* 2:15.
- 120 *CCD* 3:337.
- 121 To Henslow [5 Oct. 1846], *CCD* 3:350; to Fitzroy 1 Oct. 1846, *CCD* 3:344–345.
- 122 *CCD* 4:xvii; *CCD* 2:xv.
- 123 R. Keynes, *Charles Darwin's zoology notes and specimen lists from H. M. S. Beagle* (Cambridge University Press, 2000).
- 124 [2 Oct. 1846], *CCD* 3:346.
- 125 [M. Richmond], 'Darwin's study of the Cirripedia', *CCD* 4:388–409.
- 126 To R. FitzRoy 28 Oct. [1846], *CCD* 3:359.
- 127 To J. D. Hooker [6 Nov. 1846], *CCD* 3:363.
- 128 To R. Owen 25 Nov. [1846], *CCD* 3:372.
- 129 To E. Dieffenbach 9 Feb. [1847], *CCD* 4:13.
- 130 To J. D. Hooker 8 [Feb. 1847], *CCD* 4:10.
- 131 To R. Owen [26 Mar. 1848], *CCD* 4:125; to J. D. Hooker 10 May 1848, *CCD* 4:138; to J. D. Hooker 28 Mar. 1849, *CCD* 4:227.
- 132 Barlow, *op. cit.* (note 26), p. 117.

- 133 For a more detailed account see *CCD* 4:xvii–iii. I am grateful to James Moore for discussing this shift with me in detail.
- 134 From J. D. Hooker 20 Feb.–16 [Mar.] 1848, *CCD* 4:114.
- 135 To J. D. Hooker 12 Oct. 1849, *CCD* 4:268.
- 136 To J. L. R. Agassiz 22 Oct. 1848, *CCD* 4:178. For the sentence referred to see letter to J. E. Gray 18 Dec. 1847, *CCD* 4:99.
- 137 See also the letter to E. Cressy [May 1848]. *CCD* 4:135, which states, ‘The Barnacles will put off my species book for a rather long period.’
- 138 See also P. R. Sloan, ‘Darwin’s invertebrate program, 1826–1836’, in *The Darwinian heritage* (ed. D. Kohn), pp. 71–120 (Princeton University Press, 1985), and A. C. Love, ‘Darwin and Cirripedia prior to 1846, exploring the origins of the barnacle research’, *J. Hist. Biol.* **35**, 251–289 (2002).
- 139 9 Sept. 1854, DAR 158 f32.
- 140 To J. D. Hooker 9 Apr. 1849, *CCD* 4:232.
- 141 To J. D. Hooker 13 June [1850], *CCD* 4:343.
- 142 To A. Hancock 25 Dec. [1850], *CCD* 4:378.
- 143 To W. D. Fox 24 [Oct. 1852], *CCD* 5:99.
- 144 To J. D. Hooker, [9 Oct. 1853], *CCD* 5:158–159.
- 145 DAR 158 f32.
- 146 See the useful summary in Ruse, *op. cit.* (note 3), pp. 186–188.
- 147 Barlow, *op. cit.* (note 26), p. 118.
- 148 To W. D. Fox 27 Mar. [1855], *CCD* 5:293. See also the relaxed style in Darwin to Waterhouse, *CCD* 2:375–377. The assertion that ‘to give all arguments & facts on both sides’ was a way of concealing the true aim of his work is quite unnecessary. It is simply another case of Darwin modestly avoiding the admission of how fundamentally revolutionary his work would be.
- 149 DAR 158 f34.
- 150 C. Lyell, *Principles of geology* (3 volumes) (Murray, London, 1830–33).
- 151 R. C. Stauffer (ed.), *Charles Darwin’s natural selection* (Cambridge University Press, 1975), p. 10.
- 152 Darwin and Wallace, *op. cit.* (note 103).
- 153 Darwin’s estimate on the first page of the *Origin* is that it would take ‘two or three more years to complete it’; that is, by 1861–62. If we subtract the 13 months spent writing the *Origin* this would mean he could have published it in about 1860.
- 154 Stauffer, *op. cit.* (note 151), p. xii.
- 155 See Barlow, *op. cit.* (note 26), pp. 120–121. On Darwin’s theory of divergence, see J. Browne, ‘Darwin’s botanical arithmetic and the principle of divergence, 1854–1858’, *J. Hist. Biol.* **13**, 53–89 (1980), p. 13; Ospovat (1981), *op. cit.* (note 47), pp. 146–209; S. Schweber, ‘Darwin and the political economists’, *J. Hist. Biol.* **13**, 195–289 (1980); and D. Kohn (ed.), *The Darwinian heritage* (Princeton University Press, 1985), pp. 245–263.
- 156 Quoted in Richards, *op. cit.* (note 25), p. 52, and *op. cit.* (note 27). Darwin, *op. cit.* (note 67), p. 236.
- 157 Ghiselin, *op. cit.* (note 14); Stott, *op. cit.* (note 48).
- 158 Ospovat, *op. cit.* (note 155); see also Browne, *op. cit.* (note 155), J. Hodge and D. Kohn, ‘The immediate origins of natural selection’, in Kohn (ed.), *op. cit.* (note 155); D. Kohn, ‘Darwin’s ambiguity’, *Br. J. Hist. Sci.* **22**, 215–239 (1989); and Schweber, *op. cit.* (note 155). See also J. Hodge, ‘Darwin studies at work’, in *Nature, experiment* (ed. T. H. Levere and W. Shea), pp. 249–274 (Kluwer, Dordrecht, 1990).
- 159 Barlow, *op. cit.* (note 26), p. 83.
- 160 26 Nov. [1859], *CCD* 7:403.
- 161 23 Jan. 1860, *CCD* 8:46. See also W. Elwin to J. Murray 3 May 1859. *CCD* 7:289 and Bronn’s remark in *CCD* 8:288.
- 162 To C. Lyell 30 Sept. [1859], *CCD* 7:339.
- 163 E. Darwin to C. Darwin c. Feb. 1839, *CCD* 2:171.
- 164 To L. Jenyns 25 [Nov. 1844], *CCD* 4:85.

- 165 See for example the following: to J. D. Hooker [10 Sept. 1845], *CCD* 3:353; to J. D. Hooker [2 Oct. 1846], *CCD* 3:346; to J. D. Hooker 9 Apr. 1849, *CCD* 4:233.
- 166 To J. Phillips [7 or 14 Apr. 1848], *CCD* 4:131. The paper in question was C. Darwin, 'On the transportal of erratic boulders from a lower to a higher level', *Q. J. Geol. Soc. Lond.* **4**, 315–323 (1848).
- 167 E. Jones, *Free associations: memoirs of a psychoanalyst* (Basic Books, New York, 1959), pp. 203–204.
- 168 See F. Darwin, 'Reminiscences of my father's everyday life', in F. Darwin (ed., 1887), *op. cit.* (note 6); H. Litchfield (ed.), *Emma Darwin: a century of family letters* (2 volumes) (Murray, London, 1915); and the many manuscript recollections of Darwin's friends and children in the Cambridge University Library Darwin Archive.
- 169 Barlow, *op. cit.* (note 26), pp. 130–131.
- 170 C. Darwin, *The descent of man* (2 volumes) (Murray, London, 1871), vol. 1, p. 1.
- 171 Transcribed in Gruber, *op. cit.* (note 15) and Barrett *et al.*, *op. cit.* (note 18).
- 172 Gruber (1974), p. 519; note that parts of M and N notebooks go straight into *Descent* and Darwin, *op. cit.* (note 15).
- 173 A. R. Wallace, *The Malay archipelago* (2 volumes) (Macmillan, London, 1869), p. 1.
- 174 To E. Darwin 5 July 1844, *CCD* 3:44.
- 175 To C. Lyell [25 June 1858], *CCD* 7:117; to J. D. Hooker 9 May [1856], *CCD* 6:106–107.
- 176 Herschel to Lyell 20 Feb. 1836, later published in C. Babbage, *Ninth Bridgewater treatise* (2nd edn) (Murray, London, 1838), pp. 226–227.
- 177 Rudwick, *op. cit.* (note 17). See also P. Corsi, *Science and religion* (Cambridge University Press, 1988).
- 178 R. Owen, *On the nature of limbs* (Voorst, London, 1849), p. 86.