THE CORRESPONDENCE OF CHARLES DARWIN

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Alfred Russel Wallace. Photograph, Singapore, 1862.
(From James Marchant, *Alfred Russel Wallace: letters and reminiscences*. London: Cassell and Company, 1916. By permission of the Syndics of the Cambridge University Library.)

THE CORRESPONDENCE OF CHARLES DARWIN

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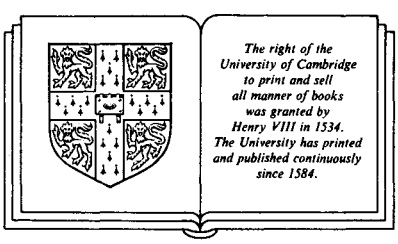

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INTRODUCTION

The two years covered in this volume were, without doubt, the most momentous of Darwin's life. From a quiet rural existence filled with steady work on his 'big book' on species, he was jolted into action by the arrival of an unexpected letter from Alfred Russel Wallace. This letter led to the first announcement of Darwin's and Wallace's respective theories of organic change at the Linnean Society of London in July 1858 and prompted the composition and publication, in November 1859, of Darwin's major treatise *On the origin of species by means of natural selection*. By the end of 1859, Darwin's work was being discussed in publications as diverse as *The Times* and the *English Churchman*, and Darwin himself was busy as never before: answering letters, justifying and explaining his views to friends, relations, and 'bitter opponents'; compiling corrections for a second and then a third edition of his book; and enthusiastically negotiating for possible American, French, and German editions. In particular, he rejoiced in the conversion ('*per*version' as he jokingly called it) to his views of close friends like Charles Lyell, Joseph Dalton Hooker, and Thomas Henry Huxley, who each, in his own way, had hesitated in relinquishing orthodox concepts of creation. 'When I was in spirits', he told Lyell at the end of 1859, 'I sometimes fancied that my book w^{d} be successful; but I never even built a castle-in-the air of such success as it has met with; I do not mean the sale, but the impression it has made on you (whom I have always looked at as chief judge) & Hooker & Huxley. The whole has *infinitely* exceeded my wildest hopes.—' (letter to Charles Lyell, 25 [November 1859]). This transformation in Darwin's personal world and the intellectual turmoil that his writings precipitated are dramatically conveyed by the letters collected here.

The year 1858 opened with Darwin hard at work preparing his 'big book' on species. Begun in May 1856 at the urging of Lyell, the manuscript was already more than half finished. Having completed his ninth chapter, on hybridism, on 29 December 1857, Darwin began in January 1858 to prepare the next chapter, 'Mental powers and the instincts of animals', sorting through his notes collected over two decades, checking his facts with various correspondents, and undertaking research to test the evidence or solve particular problems. He also continued to investigate the subjects of earlier chapters, inserting new material into the manuscript whenever appropriate. The correspondence shows that at any one time Darwin was engaged in a number of projects, fitting together the final pieces of his grand puzzle as they came to him.

The chapter on instinct posed a number of problems for Darwin. 'I find my chapter on Instinct very perplexing', he told his cousin William Darwin Fox,

'from not knowing what to choose from the load of curious facts on record.—' (letter to W. D. Fox, 31 January [1858]). In addition to behaviour such as nest-building in birds, Darwin intended to discuss many other instincts and show how they could be accounted for by his theory of natural selection. Among these, the cell-making instincts of hive-bees posed a particular challenge to his overall theory.

The geometrical architecture of the honey-combs constructed by hive-bees had long been celebrated as a classic example of divine design in nature. Darwin hypothesised that the instinct of the hive-bee to produce these seemingly mathematically conceived hexagonal structures might have evolved from an instinct to produce crude, loosely constructed clusters of circular cells, as exhibited by the Mexican bee *Melipona* or the humble-bee *Bombus*. This led him to observe and experiment with the process of construction as it took place in the hive. As with Darwin's study of poultry and pigeons, many other people were drawn into his researches. William Bernhard Tegetmeier, founder and president of the Apiarian Society, provided Darwin with information and specimens. His old friend George Robert Waterhouse brought him up to date on the natural history of various bees and wasps. For assistance with mathematical measurements and geometry, Darwin called upon William Hallowes Miller, Cambridge professor of mineralogy, who many years earlier had identified the mineralogical specimens from the *Beagle* voyage; on his brother, Erasmus Alvey Darwin; and his son William. Even his apiarian neighbours were asked for help, providing him with bees and bee-hives.

Darwin also continued the botanical work that related to his chapter on variation under nature. Having learned in the summer of 1857 that his method for deriving statistical comparisons of the number of varieties in large and small genera was erroneous, he persevered in reworking the laborious compilations of statistics from botanical works. The question was, Do the species of large genera have a higher proportion of distinct varieties than the species of smaller genera? The inquiry was of great importance to Darwin, for such evidence would support his view that varieties were incipient species (*Natural selection*, p. 145–6):

> From looking at species as only strongly marked & well defined varieties, I was led to anticipate that the species of the larger genera in each country would oftener tend to present varieties, than the species of the smaller genera; for on this view wherever many closely related species, (i.e. species of the same genus) have been formed many varieties, or as I look at them incipient species ought, as a general rule, to be now forming. Where many large trees grow, we expect to find saplings.

Yet the interpretation of the statistics was still problematic. Hooker thought that Darwin was wrong to assume that botanists were equally attentive in recording varieties in large and small genera. Darwin put the point to leading botanists without revealing his reason or his own opinion. Hewett Cottrell Watson

and Charles Cardale Babington thought that in their taxonomical practice they tended to pay more attention to varieties in larger genera, but they were not certain. This was a question new to the experts. Darwin was delighted to hear from Asa Gray that he was not aware of such a tendency in his own work, for this confirmed for Darwin that the positive results of his tabulations could be trusted as evidence for what actually occurred in nature (see letter to Asa Gray, 4 April [1858], and *Natural selection*, p. 161).

By the beginning of May 1858, the statistical tables were completed and his results written up. With some trepidation, Darwin sent his manuscript off to Hooker for his comments. Darwin's relief on hearing of Hooker's approval of his argument is evident. 'Though I sh^{d} not have much cared about throwing away what you have seen,' he told Hooker in his letter of 8 [June 1858], 'yet I have been forced to confess to myself that all was much alike, & if you condemned that you w^{d} condemn all—my life's work—& that I confess made me a little low—but I c^{d} have borne it, for I have the conviction that I have honestly done my best.—'

Other topics discussed in the letters of 1858 also relate to questions that Darwin had begun to explore earlier. Letters to several correspondents concern his attempts to trace the original ancestral types from which modern domestic breeds of animals have been developed. To this end, in a final experiment with fowls, he attempted to obtain, by crossing different breeds, characteristics of what he believed to be the original progenitor of domestic fowls, *Gallus bankiva*. Similarly, he asked his son William, as well as a number of foreign correspondents, to look out for stripes in the coats of dun-coloured horses and ponies. He included a discussion of this research in *Origin*, pp. 163–7, as examples of the occurrence of reversion in nature.

With much of his research completed, Darwin began in mid-June 1858 to write up the results of his study of pigeons, hoping to finish it in a week or two. He had scarcely begun when his work was interrupted by the arrival of the now-famous letter from Alfred Russel Wallace, enclosing an essay in which Wallace enunciated his own theory of natural selection. Darwin's shock and dismay is evident in the letter he subsequently wrote to Charles Lyell, as Wallace had requested, informing Lyell of Wallace's paper. 'Your words have come true with a vengeance that I sh^{d} be forestalled', he lamented to Lyell. 'I never saw a more striking coincidence. if Wallace had my M.S. sketch written out in 1842 he could not have made a better short abstract! Even his terms now stand as Heads of my Chapters.' (letter to Charles Lyell, 18 [June 1858]).

As was his custom, Darwin did not supply a full date on his letter to Lyell. He simply dated the letter '18' and referred to Wallace's letter as having been received 'today'. Following Francis Darwin (*LL* 2: 116–17) and relying on Charles Lyell's endorsement, the editors have dated the letter 18 [June 1858]. However, the accuracy of Darwin's words has been questioned by John L. Brooks and by H. Lewis McKinney, both of whom believe that Darwin received Wallace's communication before 18 June. McKinney has suggested that Darwin

received Wallace's letter and manuscript on 3 June 1858, the same day that another letter from Wallace to his friend Frederick Bates, dated 2 March 1858, arrived in England (McKinney 1972, pp. 138–40). The cover of the letter to Bates bears Wallace's direction 'via Southampton' and was postmarked 'Singapore Apr 21 58' and 'London Ju 3 58'.

Brooks maintains that Darwin received Wallace's letter even earlier, perhaps as early as 14 May. Using records of the schedules of the Dutch East Indies mail service and of the Peninsular & Oriental Company, and assuming that the letter to Darwin was posted at the same time as that to Bates, Brooks suggests that a letter aboard the 9 March steamer could have arrived at the London General Post Office on 14 May via Marseilles or on 20 May via Southampton. According to Brooks, Darwin kept the letter for a month, during which time he revised his species manuscript and appropriated, without acknowledgment, Wallace's theory of divergence. Then, on 18 June he forwarded Wallace's paper to Lyell (Brooks 1984, pp. 262–3). It is of some significance to note that the schedules in Brooks 1984 show that another mail from the East Indies arrived in London on 17 June, a delivery date that is consistent with the arrival of Wallace's communication at Down on 18 June. In the absence of Wallace's letter or of any firm evidence for the date of its arrival in England, the question of whether Darwin held the letter up for any reason is, as one scholar has recently concluded, 'essentially unresolvable' (Beddall 1988, p. 2).

The correspondence between mid-May and mid-June 1858 provides some circumstantial evidence in favour of the 18 June date of receipt. Topics discussed in letters written in this interval are consistent with the normal tenor of Darwin's work, and he shows no sign of anxiety. He says in a letter to Syms Covington, 18 May [1858], that he expects the publication of his species theory to be still some time away. On 16 May [1858], he arranged a meeting with Hooker to discuss his manuscript on large and small genera, stating, 'I am in no sort of hurry, more especially as I know full well you will be dreadfully severe.—' On 18 [May 1858], he again tells Hooker: 'There is not least hurry in world about my M.S.' In his letter to Hooker of 8 June [1858], he indicates that this topic is still foremost in his mind: 'I will try to leave out all allusion to genera coming in & out in this part, till when I discuss the "principle of Divergence", which with "Natural Selection" is the key-stone of my Book & I have very great confidence it is sound.' This does not fit the mood of someone who is distressed, as Darwin clearly was in his letter to Lyell, at the prospect of losing priority for his life's work.

The story has often been told of how Lyell and Hooker suggested that Darwin's years of labour on the species question be acknowledged by publishing jointly with Wallace's paper part of an early sketch of his theory, along with an abstract of his views sent to Asa Gray in September 1857. The correspondence between Darwin, Lyell, and Hooker in this volume contains all of the extant letters pertaining to the reading of the Darwin–Wallace papers at the Linnean Society on 1 July 1858. It also includes an unpublished letter from Hooker to Wallace

informing him of their action. The texts of all the relevant published material, including Wallace's essay, are provided in Appendixes III and IV.

The correspondence in this volume also shows, more vividly than has hitherto been realised, just how distressed Darwin was during the days immediately following his letter to Lyell. On 18 June 1858, his eldest daughter, Henrietta Emma, who had been ill since the beginning of the month, was stricken with diphtheria, then a little-known and frightening illness. Several days later, with Henrietta still very weak, the children's nurse and then the baby, aged 18 months, came down with scarlet fever, currently sweeping through the village. Charles Waring Darwin's condition deteriorated rapidly in the space of a few days and the Darwins were shocked by his unexpected death on 28 June. A tender memorial of the baby written by CD is given in Appendix V. Upon the advice of Fox, the family fled the epidemic and stayed on the Isle of Wight until mid-August. During that time, Darwin learned of the death of his eldest sister, Marianne Parker. It is not surprising, then, to find that Darwin could not give his full attention to the announcement of his views in public and that he did not attend the meeting of the Linnean Society on 1 July 1858.

After the theory of natural selection had finally been brought before a scientific audience, Darwin felt that he should publish a fuller and more formal statement, particularly since the excerpts from his writings in the joint paper had not been written for publication and his 'big book' was still far from finished. At first Darwin considered publishing an 'abstract' of his theory to appear in one or more papers in the Journal of the Linnean Society. But once he had commenced writing, he soon realised that even an abstract of his material would require a 'small volume' (letter to J. D. Hooker, 12 October [1858]). Begun while he was in Sandown on the Isle of Wight, the writing of this 'abstract' continued until March 1859; the resulting volume was published in November.

Since Darwin intended to publish his larger work on species soon after the abstract appeared, he continued to accumulate data and to write lengthy, detailed sections for his 'big book'. In September 1858 he finished his manuscript discussion of pigeons; this figures prominently in his two-volume work on *Variation* published in 1868 but occupies only a few pages in *Origin*. His observations and experiments on bees' cells continued through the autumn of 1858, even though he had completed a draft of the chapter on instinct the previous March.

One of the chapters of both the big book and the abstract dealt with the subject of cross-fertilisation in nature. For several years, Darwin had collected data from the plant and animal kingdoms to support his belief that all organic beings are occasionally fertilised by a distinct individual of the same species. In plants, the Leguminosae presented the most difficult challenge to his views. In November 1858, he communicated a long summary of his work on this topic, entitled 'On the agency of bees in the fertilisation of papilionaceous flowers and on the crossing of kidney beans', to the *Gardeners' Chronicle* (see letter to *Gardeners' Chronicle*, [before 13 November 1858]), in which he presented the evidence for his belief that

occasional cross-fertilisation was a 'law of nature' and described the role of bees in facilitating this process in plants.

Busy as he was, Darwin did not neglect other aspects of his professional activities. He acted as referee for several papers submitted to the Royal Society and served on the society's committee set up to provide advice for an expedition to Arctic North America. Several letters to Hooker, Huxley, and others indicate his concern about the proposed move of the natural history collections of the British Museum to another location. Darwin at first objected to any separation of the natural history collection from the library and other collections of the museum and signed a memorial in opposition. Then, when he was convinced that a move was unavoidable, he joined with other naturalists in support of a separate museum of natural history and the merger of the botanical collections with those of the Royal Botanic Gardens at Kew (see Appendix VII).

The year 1859 began auspiciously with Darwin receiving the Wollaston Medal of the Geological Society, considered to be the highest honour a British geologist could attain. The award citation praised his work on the geology of the Andes and on coral reefs, 'which led to the grand speculation of alternate zones of elevation and depression in the Pacific and Indian Oceans'. It also mentioned his work on the distribution of erratic boulders, the parallel roads of Glen Roy, and his monograph on *Fossil Cirripedia* (1851 and 1854) (*Quarterly Journal of the Geological Society* 15 (1859): xxv).

One of the most interesting exchanges in the correspondence of 1859 is that between Darwin and Hooker concerning Darwin's forthcoming book and Hooker's essay on the flora of Australia, which formed the introduction of *Flora Tasmaniæ*. In an attempt to account for the composition of the Australian flora, Hooker confronted the difficult subjects of geographical distribution, means of migration, and, ultimately, the problem of species change itself. Indeed, it was in the course of writing up this essay that Hooker became convinced of the value of Darwin's theory. As he wrote in his introductory essay (Hooker 1859, p. ii):

> In the present Essay I shall advance the . . . hypothesis, that species are derivative and mutable; and this chiefly because, whatever opinions a naturalist may have adopted with regard to the origin and variation of species, every candid mind must admit that the facts and arguments upon which he has grounded his convictions require revision since the recent publication by the Linnean Society of the ingenious and original reasonings and theories of Mr. Darwin and Mr. Wallace.

The flora of Australia, Hooker stated, seemed especially suited to test such a theory. His essay, published in December 1859, was the first serious study of the validity of Darwin's views.

While writing up their respective views on the transmutation of species, Darwin and Hooker each expressed the concern that they were drawing too heavily on the work of the other. Neither Darwin nor Hooker was quite sure whether his view

was entirely original or whether he had unwittingly appropriated the other's ideas (see letters to J. D. Hooker, 2 March [1859], 11 March [1859], and 7 April [1859]). What is clear is that they both found much of value in each other's work.

By the middle of March 1859, Darwin had finished the last chapter of his abstract and begun to think of a publisher for the work. Again, he called upon Lyell for advice (letter to Charles Lyell, 28 March [1859]). Lyell suggested the firm of John Murray, publishers of the second edition of Darwin's *Journal of researches* and of Lyell's works, and well-known for its scientific list. Murray agreed to publish Darwin's book without even seeing the manuscript, although he subsequently had reservations: Murray was, after all, also the publisher of the conservative *Quarterly Review*. He sent the manuscript to Whitwell Elwin, the editor of the *Quarterly*, for his opinion. Elwin's long and considered reply to Murray is published in this volume. Despite Elwin's suggestions, however, Darwin held firm to the original plan of his book (see letter from Elwin to Murray, 3 May 1859, and letter to John Murray, 6 May [1859]).

The extant correspondence with Murray consists primarily of letters from Darwin. They provide an interesting record of the process of seeing *Origin* through the press and of Darwin's concerns about his book. He worried about its style and its length and whether it would be successful. In particular, he was anxious about the prospects of Murray recovering his expenses and even offered to share in the financial risk—an offer that Murray saw no need of accepting. Darwin took Murray's advice and omitted the words 'an abstract' from the title of the forthcoming book (letter to Charles Lyell, 30 March [1859]). Darwin next considered calling the work 'An essay on the origin of species and varieties' (letters to Charles Lyell, 28 March [1859], and to John Murray, 10 September [1859]), but finally decided on the title 'On the origin of species by means of natural selection, or the preservation of favoured races in the struggle for life'.

In October, having finished the last of the proof-sheets '13 months & 10 days' after he had begun to write the abstract, Darwin left for a hydropathic establishment at Ilkley in Yorkshire to recuperate from the strain of publishing (see 'Journal'; Appendix II). Twice in 1858 and three times in 1859 he had gone to Moor Park in Surrey for a week's water-cure, from which he had seemed to profit. Darwin hoped that he and his daughter Henrietta would benefit from the treatment at the new establishment, but in this he was disappointed. 'We have been here above 6 week,' he wrote to Fox, '& I feel worse than when I came' (letter to W. D. Fox, [16 November 1859]). It was during his stay at Ilkley Wells that *Origin* was published; from there Darwin wrote 'multitudes of letters' to recipients of presentation copies, and it was there too that he began to receive the first indication about the reception of his work.

Darwin was well aware that most of the leading figures in the scientific community would not be sympathetic to his heterodox views. As he told the recipients of presentation copies, in no case did he expect full agreement with the

book, for he freely admitted that there were many difficulties with his theory. He begged Huxley, whom he believed might respond favourably to his ideas, not to be too harsh, explaining that his book was 'a mere rag of an hypothesis with as many flaws & holes as sound parts.— My question is whether the rag is worth anything?' (letter to T. H. Huxley, 2 June [1859]). But as critical letters began to arrive, Darwin could not conceal the discomfort he felt at the severity of some of the attacks.

Adam Sedgwick's negative response to *Origin* was not surprising, but it nevertheless distressed Darwin that Sedgwick should so vehemently condemn his book, telling Darwin that he had abandoned the 'true method of induction' (letter from Adam Sedgwick, 24 November 1859). Equally painful was the news that John Frederick William Herschel, whom he so venerated, had labelled natural selection the 'law of higgledy-piggledy' (letter to Charles Lyell, [10 December 1859]). To each of his critics, Darwin replied by resting his case on a single proposition: 'I cannot think a false theory would explain so many classes of facts, as the theory seems to me to do.' (letter to Adam Sedgwick, 26 November [1859]).

Even his strongest supporters found points in Darwin's work with which they disagreed. Both Gray and Huxley, who were to become Darwin's public champions, were frank in their reservations. Gray was critical of the concept of a blind, accidental process of natural selection and held out for design. Huxley felt that Darwin's hypothesis needed to be tested by observation and experiment to prove that natural selection could produce all the effects Darwin ascribed to it. Hooker, though very much a convert, still argued about the fine points of Darwin's theory (see letter to J. D. Hooker, 6 May 1859). Among the older scientists, only Leonard Horner gave his unqualified approval. Indeed, Darwin came to believe that only those who were of his generation or younger would be fully convinced.

Darwin was particularly interested in Charles Lyell's response to his theory. He often said that if he could convince Lyell, Hooker, and Huxley, he would rest content. During the final stages of proof-reading, he therefore sent sheets to Lyell for his comments. Only one of Lyell's letters has survived, but from Darwin's responses to Lyell's many queries it is clear that Lyell gave the work deep attention. Gradually, in his letters, Lyell indicated that Darwin had made a powerful case for the mutability of species. He spoke out in favour of the book at the September meeting of the British Association for the Advancement of Science. But the great stumbling block for Lyell lay in the implications of the theory for the origin of mankind. As he wrote to Darwin on 3 October 1859, 'the case of Man and his Races & of other animals & that of plants is one & the same & that if a "vera causa" be admitted for one instead of a purely unknown & imaginary one such as the word 'Creation' all the consequences must follow—'. Although in the years to come Lyell served as one of Darwin's most understanding and helpful

advocates, Lyell never could bring himself fully to accept that man could have descended from an ape-like progenitor.

By the middle of November, the first reviews of *Origin* began to appear. One in the *Athenæum* disturbed Darwin, not simply because it was critical but because it dismissed the book on theological grounds instead of evaluating its scientific merits. Darwin told Hooker, it 'sets the Priests at me & leaves me to their mercies' (letter to J. D. Hooker, [22 November 1859]). Late in December, to Darwin's great surprise, *The Times* carried a highly favourable review. 'Certainly I should have said that there was only one man in England,' Darwin wrote to Huxley the morning after he read it, 'who could have written this Essay & that *you* were the man.' (letter to T. H. Huxley, 28 December [1859]). Huxley admitted his authorship to Darwin and wrote a longer review for the December issue of *Macmillan's Magazine*.

Darwin was more than pleased by the few favourable comments in notices of and letters about his book. He told Murray, 'I fear all Reviews of my present Book, will be very unfavourable; but I **now** feel confident my views will ultimately prevail: it is impossible that men like Lyell, Hooker, Huxley, H. C. Watson, Ramsay &c would change their minds without good cause.' (letter to John Murray, 2 December [1859]).

At Murray's trade sale of 22 November, orders for *Origin* exceeded by 250 the 1250 copies of the first printing. Instead of printing additional copies, Murray called for a second edition to be published as soon as possible and asked Darwin to make only a minimal number of corrections. For the second edition, Darwin incorporated some material in response to early criticisms, especially those of Lyell, but because of the lack of time and his absence from Down, he made very few changes. One that Darwin later regretted was the elimination of a passage about the North American bear swimming open-mouthed to feed on insects, where he states: 'I can see no difficulty in a race of bears being rendered, by natural selection, more and more aquatic in their structure and habits, with larger and larger mouths, till a creature was produced as monstrous as a whale.' (*Origin*, p. 184). The passage was ridiculed because it was taken to mean that by natural selection a bear could be transformed into a whale. It is interesting to note that in the list of corrections Darwin sent to Asa Gray for a possible American edition, the story was not deleted (see *Correspondence* vol. 8, letters to Asa Gray, 28 January [1860] and [8 or 9 February 1860]).

A further change in the second edition was the inclusion of a comment on *Origin* by a 'celebrated author and divine' (Charles Kingsley) that 'it is just as noble a conception of the Deity, to believe that he created primal forms capable of self development into all forms needful pro tempore & pro loco, as to believe that He required a fresh act of intervention to supply the lacunas w^{h} he himself had made' (letter from Charles Kingsley, 18 November 1859). This and the two references to the Creator breathing life into the first primordial form were obviously intended to soften the impact of the theory on those for whom creation was central to their religious beliefs.

Amid the flurry of activity surrounding the publication of *Origin*, Darwin took great interest in the activities of his children. His correspondence shows a touching solicitude when they were ill. His old fear that the children had inherited his ill health revived as first Henrietta and then Elizabeth and Leonard suffered similar symptoms. With his sons William and George, he became an enthusiastic billiards player, purchasing an expensive billiard-table for Down House. Darwin eagerly supervised the entomological pursuits of the younger boys, who had become ardent Lepidopterists. Perhaps recalling the thrill of publishing a notice of one of his own youthful captures, he sent off to the *Entomologist's Weekly Intelligencer* a report on some rare species that the boys had collected in the fields around Down. With equal interest, he guided William and Henrietta in their introduction to botanical systematics and plant physiology. The correspondence with William shows Darwin to be attentive to the young man's hobbies and education. Darwin arranged for William to study with a tutor to prepare him for entry to Christ's College, Cambridge, and to coach him for a scholarship examination; he counselled him about setting up rooms in college and about how to handle his financial affairs. His letters are lively and affectionate, full of family gossip and fatherly advice now that William was beginning to make his own way in life. Back in Down, family life was enhanced by the purchase of a pianoforte, new horses, and a carriage, leading Darwin to comment ruefully to William that 'Mamma has got much more larky since we run two horses' (letter to W. E. Darwin, 6 October [1858]). Visitors to Down and trips to London regularly punctuate the correspondence.

As for Darwin's own reaction to the publication of his evolutionary theory after working on its formulation for over twenty years, the correspondence published here reveals important changes in his self-perception and in his plans for the future. Confidence and modesty alternate, swinging Darwin from mood to mood with uncomfortable rapidity. He writes as one who has given his theory to the world for people to make of it what they will. 'You do me injustice', he wrote to Fox, 'when you think that I work for fame: I value it to a certain extent; but, if I know myself, I work from a sort of instinct to try to make out truth' (letter to W. D. Fox, 24 [March 1859]). Yet he desperately wanted people to accept his work. It was now his task, he thought, to publish the fuller version of his 'abstract' in order to provide the evidence necessary to convince thoughtful young naturalists of its validity. As he optimistically told Hooker, 'We shall soon be a good body of working men & shall have, I am convinced, all young & rising naturalists on our side.—' (letter to J. D. Hooker, 14 December [1859]).