THE CORRESPONDENCE OF CHARLES DARWIN

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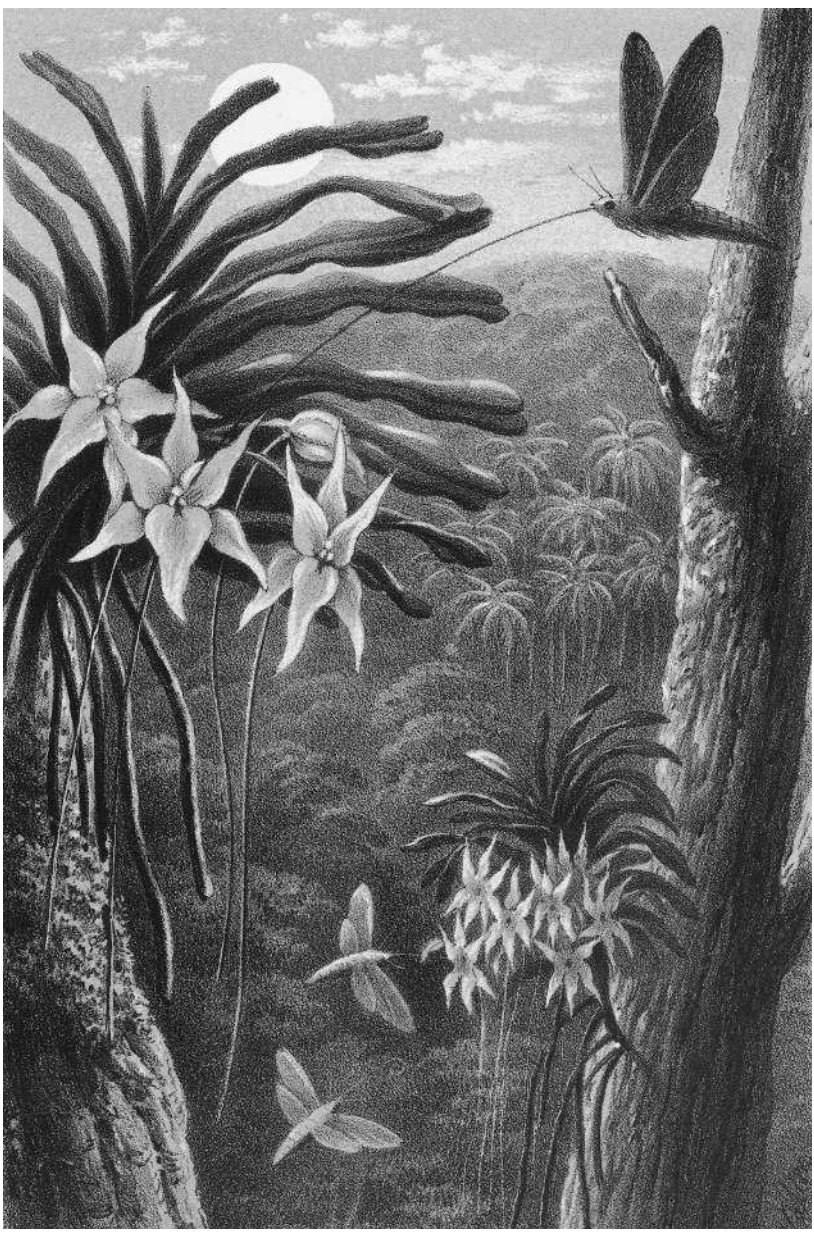

Hypothetical sphinx moth pollinating *Angraecum sesquipedale* in the forests of Madagascar (see pp. 384 and 385 n.5)
Quarterly Journal of Science 4 (1867): facing p. 471
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In Memoriam
ERNST MAYR
1904–2005

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Letters acquired after the publication of the first edition of the *Calendar*, in 1985, have been given numbers corresponding to the chronological ordering of the original *Calendar* listing with the addition of an alphabetical marker. Many of these letters are summarised in a 'Supplement' to a new edition of the *Calendar* (Cambridge University Press, 1994). The marker 'f' denotes letters acquired after the second edition of the *Calendar* went to press in 1994.

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INTRODUCTION

Charles Darwin's major achievement in 1867 was the completion of his large work, *The variation of animals and plants under domestication* (*Variation*). After years of delays, caused by illness and various interruptions, the manuscript, except for the last chapter, had been delivered to the publisher in the final week of 1866. It would take all of 1867 to correct proofs, and just when completion seemed imminent, a further couple of months were needed to index the work, a task that Darwin handed over to someone else for the first time.

The completion of one book marked the beginning of two others, as Darwin decided to exclude the 'chapter on man' from the already oversized two-volume *Variation* and instead write a short (as he then expected) 'Essay on Man'. The focus of the essay was to be the role of sexual selection in forming human races, and there was also to be a chapter on the meaning and cause of the expression of emotions. The 'essay' grew into another two-volume work, *The descent of man and selection in relation to sex* (*Descent*), published in 1871, and the chapter on expression into a book, *The expression of the emotions in man and animals* (*Expression*), published in 1872. Although Darwin had been collecting material and making observations in these areas for decades, it was only now that he began to work with a view to publishing his observations.

The importance of Darwin's network of correspondents becomes vividly apparent in his work on expression in 1867, as he continued to circulate a list of questions on human expression that he may have drawn up in late 1866. His correspondents were asked to copy the list and forward it to those who might best answer the questions, with the result that Darwin began to receive replies from different corners of the world.

Darwin's work was now guaranteed to arouse interest both at home and further afield, and, with *Variation* nearing completion, he received enthusiastic offers of immediate translation, not only into German and French, but also into Russian. Whereas the earlier Russian translation of *Origin* had been based not on the original, but on the German first edition, the new Russian translation of *Variation* would be based on proof-sheets received as Darwin corrected them. Closer to home, two important works, a book by the duke of Argyll, and an anonymous review by an engineer, Henry Charles Fleeming Jenkin, challenged different aspects of Darwin's theory of transmutation as elucidated in *On the origin of species by means of natural selection* (*Origin*) and in *On the various contrivances by which British and foreign orchids are fertilised by insects* (*Orchids*). While Darwin privately gave detailed opinions of these critiques, he was more than happy to leave the public defence of the theory in the capable hands of Alfred Russel Wallace. At the same time, Darwin was persuaded

by some German supporters to write to his most vociferous German champion, Ernst Haeckel, to encourage him to tone down his attacks on critics, which, they felt, were becoming counterproductive.

Throughout the year, Darwin continued to discuss now familiar topics such as dimorphism and trimorphism, self-sterility, pollination, and seed dispersal with a growing network of correspondents who worked on similar topics and were happy to supply him with information and discuss the implications of their findings in light of transmutation theory. Three important new correspondents in 1867 were Hermann Müller and Anton Dohrn in Germany, and Federico Delpino in Italy, who provided Darwin with the collegial support and rigorously scientific atmosphere that he so much needed in what was becoming a highly combative and emotional arena.

Thomas Henry Huxley sent Darwin the New Year's greeting, 'may you be eupeptic through 1867 & your friends & the world in general will profit'. Darwin's health, which had improved considerably the previous year, continued to be good, allowing him to pursue several projects at once. First and foremost was the completion of part of his long-delayed 'big book', started in January 1860, and advertised in the press since 1865 with the unwieldy title, 'Domesticated Animals and Cultivated Plants, or the Principles of Variation, Inheritance, Reversion, Crossing, Interbreeding, and Selection under Domestication'. Having just received the printer's estimate of the size of the two-volume work from his publisher, John Murray, he wrote to Murray on 3 January 1867, 'I cannot tell you how sorry I am to hear of the enormous size of my Book.' He told Murray he would not hold him to his agreement to publish and also informed him that he had finished the concluding chapter and remained doubtful whether or not to include a chapter 'on Man'. After a few days, he wrote back to Murray proposing that some of the more technical passages in the book be printed in smaller type, a plan to which Murray readily agreed. More letters were exchanged, clarifying financial arrangements and discussing the number of copies to be printed, and by the end of the month Darwin promised to send the revised manuscript to the printer as soon as he had marked out passages to be set in smaller type. He added, 'I feel a full conviction that my Chapter on man will excite attention & plenty of abuse & I suppose abuse is as good as praise for selling a Book' (letter to John Murray, 31 January [1867]).

A week later, Darwin had sent the manuscript to the printer, but without the additional chapter. In a letter written on 8 February [1867] to his close friend, Joseph Dalton Hooker, he explained, 'I began a chapter on Man, for which I have long collected materials, but it has grown too long, & I think I shall publish separately a very small volume, "an essay on the origin of mankind"'. Eventually, the chapter would expand into not one but two substantial books, *Descent* and *Expression*. In the same letter, Darwin revealed the conclusion to his newly completed book: a paragraph throwing doubt on Asa Gray's doctrine that each variation had been

'specially ordered or led along a beneficial line'. He added, 'It is foolish to touch such subjects, but there have been so many allusions to what I think about the part which God has played in the formation of organic beings, that I thought it shabby to evade the question.'

As the year progressed, the book continued to consume Darwin's time. The first proof-sheets arrived on 1 March 1867 and the tedious work of correction began. Darwin wrote to Murray on 18 March to say that he preferred the title 'invented' by the compositors, and so the book became *The variation of animals and plants under domestication*. In a letter to his son William dated 27 [March 1867], he admitted, 'I fear the book is by no means worth the confounded labour.' However, offers to translate the work came quickly, and by May, Russian, German, and French translations had been arranged. Darwin had now found sympathetic translators whom he could trust to convey his work without interjecting comments or additions of their own, as the earlier German and French translators had done. The French translator, Jean Jacques Moulinié, had been personally recommended by Carl Vogt and had translated Vogt's own *Vorlesungen über den Menschen* (Lectures on man; Vogt 1863) from German into French. With a background in natural history, native fluency in English, and a desire to devote himself 'body and soul' to the translation, Moulinié was an excellent choice. The offer to undertake a Russian translation was made by a young naturalist equally devoted to Darwin's work, Vladimir Onufrievich Kovalevsky. Kovalevsky included several of his brother's embryological papers with his first letter to Darwin of 15 March 1867, although he described some of Alexander Kovalevsky's ideas as 'a little wild'. Though primarily concerned with finding a good translator for his book, Darwin was always on the lookout for evidence to support his theory of transmutation. Darwin heavily annotated his copy of Alexander's paper on the embryology of ascidians (A. O. Kovalevsky 1866b), revealing his keen interest in one of the first studies to suggest an ancestral link between invertebrates and vertebrates.

Finally, Darwin had made sure that Julius Victor Carus, who had completely revised the German translation of *Origin* in 1866, would be called upon to translate *Variation*. Indeed, he told his publisher, John Murray, in a letter of 4 April [1867], not to send stereotypes of the illustrations to the German publisher until he was certain that Carus would undertake the translation. Darwin had received other offers, notably one from Vogt in April 1867, to translate the new work. Carus had already agreed in principle to translate the work but told Darwin, 'I am so very much occupied just now and within the next twelve months, that I should feel exceedingly obliged if you would kindly tell me, at what rate your work will be published' (letter from J. V. Carus, 5 April 1867). This hint of uncertainty caused Darwin to respond to Vogt somewhat ambiguously, as he wrote, 'Prof. Carus, though he has undertaken the translation informs me that he has much work on hand, & it is possible (though not probable) that when he hears (& I wrote to him on the subject yesterday) of the size of the book, & that several sheets will be printed immediately & sent to him, he may wish to give up the task' (letter

to Carl Vogt, 12 April [1867]). Darwin need not have worried. Carus soon wrote back, assuring Darwin that he could translate the first volume by November 1867, and, apparently alerted to Vogt's approach, warning him that Vogt was not 'the fit person' to introduce the work to the German public (letter from J. V. Carus, 15 April 1867). Darwin may not have fully appreciated how politically radical Vogt was (it was his political activism that led to his forced departure from his native country), but he was quick to reassure Carus, 'The wish never for a moment crossed my mind that Vogt should translate my book in preference to you' (letter to J. V. Carus, 18 April [1867]). Darwin was not disappointed in his choice, for Carus did more than provide an accurate translation. Thanks to his knowledgeable and careful reading of the original, he alerted Darwin to errors in the text and suggested corrections, many of which Darwin was able to incorporate in the English edition.

Although Darwin longed to have the book off his hands, he continued to make additions, especially when new material came to light that seemed to support his beloved theory of heredity, the 'provisional hypothesis of pangenesis'. Such was the case, reported by Charles Victor Naudin, of a fan palm, pollinated by a date palm, whose fruit appeared to show the direct action of the male parent on the female, in that the fruit looked more like that of the date palm. Naudin had sent specimens to Hooker, who reported the 'wonderful discovery' to Darwin on 14 March 1867. Then, in April, Robert Trail wrote from Scotland about a potato he had produced by joining two different varieties at the eye, which resulted in a mottled hybrid (letter from Robert Trail, 5 April 1867). Darwin told his American friend Asa Gray, 'I am repeating this experiment on a large scale, for it seems to me, if true, a wonderful physiological fact' (letter to Asa Gray, 15 April [1867]). Although he did not succeed in duplicating Trail's results, Darwin claimed in *Variation* 1: 396, that the case apparently afforded 'clear evidence of the intimate commingling of the two varieties'. Such a case, if proved, would be evidence of 'the essential identity of sexual and asexual reproduction', Darwin wrote in his chapter on pangenesis (*Variation* 2: 365).

Darwin had developed his provisional hypothesis of pangenesis over many years apparently without discussing it or showing it to anyone until 1865, when he sent a version of it to Huxley, asking whether it should be published. Huxley had pointed out similarities to earlier theories, and, while not discouraging Darwin from publishing altogether, had advised that his views should be presented not as 'formed conclusions' but as 'hypothetical developments' (see *Correspondence* vol. 13, pp. xix–xx). Not surprisingly then, when circulating proofs of the book, Darwin had been especially anxious about the reception of pangenesis. He was happy that Charles Lyell had a positive response, commenting, 'I have been particularly pleased that you have noticed Pangenesis. I do not know whether you ever had the feeling of having thought so much over a subject that you had lost all power of judging it. This is my case with Pangen: (which is 26 or 27 years old!) but I am inclined to think that if it be admitted as a probable hypothesis, it will be a somewhat important step in Biology' (letter to Charles Lyell, 22 August [1867]). Darwin's insecurity persisted,

however, and in November he told Hooker, 'I shall not be at all surprised if you attack it & me with unparalleled ferocity' (letter to J. D. Hooker, 17 November [1867]).

Even when the corrections were complete, in mid-November, the book was further delayed by the time it took William Sweetland Dallas to prepare the index. John Murray had engaged Dallas and Darwin approved the choice but asked to see a specimen of his work. Although he had initially wanted Dallas to index every name that appeared in the text, he eventually conceded, 'On reflection I fear you will find it endless labour to give all author's names in notes. So use your own discretion; anyhow most ought to be introduced' (letter to W. S. Dallas, 8 November [1867]). Dallas resisted the temptation to cut corners, and told Darwin, 'I have adopted your plan of giving every author's name', but added, 'It makes the labour very great, however, & I cannot get on so quickly as I could wish' (letter from W. S. Dallas, 20 November 1867). Dallas, like Carus, alerted Darwin to errors in the text, although some of these corrections were too late to make it into the first printing of the first edition. The relationship between the two men seems to have been uneasy, at least until Dallas finished the index. Although many of Darwin's letters to Dallas have not been found, it is clear from the defensive tone of Dallas's responses that he was under a great deal of pressure to complete his work and was torn between the desire to finish the index quickly and the desire to impress Darwin with the quality of his work.

As the 'horrid tedious dull work' of correcting *Variation* went on, Darwin was at the same time seeking information for his next project, the 'essay on man'. One of the first areas he focused on was expression. In fact, Darwin had been interested in the physical nature of the expression of the emotions in humans for a long time. From around 1838, he had begun making observations on expression, noting the difference or similarity between people and particular animals. He also recorded the expressions of some of his children from infancy, and read books on the anatomy of expression by medical experts such as Charles Bell and Guillaume Benjamin Amand Duchenne. Now Darwin was able to mobilise an ever-widening network of correspondents in an attempt to establish through observation the universality of human expressions. As early as January 1860, he had sent a list of specific queries regarding the expression of emotions in the indigenous people of Tierra del Fuego, whom he had first encountered in 1832 while on the *Beagle* voyage (see *Journal of researches*, pp. 228–9). After hearing from his former *Beagle* shipmate Bartholomew James Sulivan at Christmas 1866, Darwin had written at the end of the year asking again for information on Fuegian expressions. On 11 January 1867, Sulivan replied, enclosing belated answers from Thomas Bridges to the queries Darwin had sent in 1860 and relaying a promise from a missionary friend who was returning to South America to 'look out again' for answers.

In a letter of 22 February [1867] to Fritz Müller in Brazil, in which he asked for information on two subjects – 'sexual selection' and 'expression of countenance' – Darwin explained, 'I am thinking of writing a little essay on the origin of Mankind,

as I have been taunted with concealing my opinions; & I sh[d] do this immediately after the completion of my present book. In this case I sh[d] add a chapter on the cause or meaning of Expression.' With this letter Darwin enclosed a list of questions, handwritten by an amanuensis, headed 'Queries about Expression'. In a postscript to the letter he added, 'But you must not plague yourself on a subject which will appear trifling to you, but has, I am sure, some considerable interest.' Darwin also introduced the subject to Alfred Russel Wallace, who suggested in his response of 11 March [1867] that Darwin send his queries to foreign newspapers. The letter also reveals that he did not share Darwin's interest in studying human expression. He briefly answered some of the queries from memory, and then added, 'But do you think these things are of much importance?' Further, he wrote, 'I would rather see your second volume on "The Struggle for Existence &c." for I doubt if we have a sufficiency of fair & accurate facts to do any thing with Man.' Darwin replied, not altogether ingenuously, 'I fully agree with you that the subject is in no way an important one: it is simply a "hobby-horse" with me about 27 years old; & *after* thinking that I would write an essay on man, it flashed on me that I could work in some "supplemental remarks on expression"' (letter to A. R. Wallace, [12–17] March [1867]). Darwin's doggedness in pursuing answers to his queries reveals a different picture about the importance of the subject to him.

Copies of the queries were sent across the globe to North and South America, Australia, New Zealand, South Africa, India, and elsewhere. Darwin requested that his correspondents pass on the queries to acquaintances in remote areas. On 26 March, Asa Gray wrote, 'You see I have *printed* your queries—privately—50 copies—as the best way of *putting* them where useful answers may be expected', and asked whether he should send more printed copies. Darwin replied, 'I wish I had thought earlier of having them printed, for in that case I might have sent a dozen to each of my few correspondents, as it is I can think of no one to send them to, so do not want any more' (letter to Asa Gray, 15 April [1867]). Nevertheless, at some point during the year he did, in fact, have the queries printed on his own account. Copies of Darwin's own printed questionnaire survive. One of these has been transcribed in Appendix IV. Robert Swinhoe, the British consul in Amoy, had a handwritten version of Darwin's queries published in *Notes and Queries on China and Japan*, 31 August 1867. Another version, possibly derived from Asa Gray's printed queries, was published in 1868 in the *Annual Report of the Board of Regents of the Smithsonian Institution for the year 1867*.

In his 15 April [1867] letter to Gray, Darwin commented, 'I have been lately getting up & looking over my old notes on Expression, & fear that I shall not make so much of my hobby-horse, as I thought I could: nevertheless it seems to me a curious subject, which has been strangely neglected.' CD's doubts did not persist for long, especially as replies to the questionnaire continued to arrive.

'Sexual selection' was the other 'new' subject Darwin devoted his energies to in 1867. Darwin had, in fact, introduced the concept of sexual selection in *Origin*; he proposed that secondary sexual characters had been accumulated by sexual

selection, which, he argued, was 'less rigid' in its action than natural selection since it did not 'entail death', but only resulted in fewer offspring for less favoured males (see *Origin*, pp. 156–7). In *Variation*, Darwin had discussed changes in secondary sexual characters under domestication, noting that such modifications could be explained through sexual selection, but had not elaborated further (*Variation* 2: 75). In notes for his reply to a letter from Edward Blyth dated 19 February 1867, Darwin had written, '*Sexual Selection*.— too many questions to ask'. In the reply itself, written 23 February [1867], Darwin introduced the topic with a compliment to Blyth: 'I have picked up more facts on sexual characters . . . from your writings than from those of any one else.' He then proceeded to ask for still more information on sexual differences in mammals and birds. In his letter to Fritz Müller of 22 February [1867], Darwin was more specific about what he wanted, asking for examples of sexual differences that did not relate to 'different habits of life' in males and females. In his reply of 1 April 1867, Müller supplied Darwin with information about sexual differences in crustaceans, spiders, and annelid worms that might reflect sexual selection.

Although Darwin wrote to several people in his search for material on sexual selection, the individual with whom he discussed and debated the topic on a theoretical level was Alfred Russel Wallace. In a letter to Wallace written on 23 February 1867, Darwin asked whether Wallace could suggest a solution to the puzzling problem of bright colours in caterpillars as well as in butterflies. Wallace was sure that the colours were protective and suggested that John Jenner Weir might conduct experiments in his aviary to see whether this was the case (letter from A. R. Wallace, 24 February [1867]). He also suggested a simple experiment to determine whether female butterflies preferred more colourful males. When Darwin had asked the question about bright caterpillars, the idea that bright colours in male butterflies resulted from sexual selection was implicit. Wallace's response contained much more than a possible explanation about caterpillars; it called into question the whole notion of sexual selection as an explanation of colour in adults of different sexes, and ultimately, the notion of sexual selection as an agent of change.

Darwin was obviously dismayed that his theory of sexual selection was being challenged at a fundamental level. In his response to Wallace (letter to A. R. Wallace, 26 February [1867]), Darwin defended his position about colour in adult insects but turned the discussion to the role of sexual selection in humans, remarking, 'I still strongly think . . . that sexual selection has been the main agent in forming the races of Man.' The two debated the matter over the course of several months. In the 1867 correspondence, Wallace steered clear of the issue of formation of human races, but continued to build his argument about the protective function of colour in both insects and birds. Darwin conceded that Wallace had made a convincing argument concerning protective coloration, but continued to emphasise the importance of sexual selection in humans.

Through their use of polite language and willingness to give ground, Darwin and Wallace were able to sustain a dialogue in an area where they disagreed on many

points. On the practical side, Darwin was happy to take advantage of Wallace's influence in the entomological community in order to gather more information on insects. Moreover, he was still able to engage in fruitful theoretical discussions by allowing a place in his own theory for Wallace's hypothesis about protection. Similarly, Wallace conceded that sexual selection might come into play in some circumstances. In a letter of 5 May [1867], Darwin admitted, referring to Wallace's argument that female birds that used open nesting sites retained dull coloured plumage because of its protective effects, 'your explanation with respect to the females had not occurred to me. I am surprized at my own stupidity, but I have long recognized how much clearer & deeper your insight into matters is than mine.' Darwin had already told Wallace that he would not have much time to devote to this new research until he had finished correcting proofs of *Variation* and added, 'when I return to the work I shall find it much better done by you than I c^{d} have succeeded in doing' (letter to A. R. Wallace, 29 April [1867]). Thus Darwin was able to portray his research as collaborative, with Wallace and him pursuing the same, albeit broadly defined, goal.

Almost a year would pass before Darwin was again ready to take up the subject of sexual selection in earnest. In the meantime, his work on several botanical projects continued and he gained a valuable new correspondent in this area, Hermann Müller. Darwin had already benefited enormously from his correspondence with Hermann's brother, Fritz, who, living in Brazil, could supply Darwin with much information about the rich flora and fauna of the country. Fritz had been writing to his younger brother about his correspondence with Darwin and had even suggested research projects that Hermann, a secondary-school teacher in Westphalia, could pursue with a view to supporting Darwin's theory (letter from Fritz to Hermann Müller, 11 February 1867, in Möller ed. 1915–21, 2: 111–16). Hermann sent Darwin some papers he had published on Westphalian mosses, and Darwin, in a reply that has not been found, evidently wrote encouragingly to him. In his first extant letter to Darwin, that of 23 March 1867, Hermann told Darwin of his research plans. He wrote, 'I think I could not easily find a higher enjoyment and at the same time a better preparation for the researches intended than in repeating your charming observations on the fertilisation of Orchids by insects, as far as the Westfalian Flora offers any opportunities to it and in devoting my attention in general to the fertilisation of flowers by insects.' By the summer, Hermann was already making observations on the pollination of orchids by insects that would confirm points that Darwin had only conjectured in his 1862 study, *On the various contrivances by which British and foreign orchids are fertilised by insects* (*Orchids*).

In October, Müller wrote to thank Darwin for his present of the new German edition of *Origin* and mentioned an observation that he thought might be of some interest. Müller had observed the special adaptation in the mouthparts of various species of hoverflies (Syrphidae) allowing them to eat pollen-grains as well as suck nectar from flowers they visited (letter from Hermann Müller, 23 October 1867). The letter contains two illustrations of Syrphidae mouthparts, the first known

depiction of this singular adaptation. Müller's work not only confirmed many of Darwin's earlier observations on floral mechanisms for ensuring cross-pollination, it complemented Darwin's work on flower structure by focusing on the details of insect morphology such as specialised mouthparts.

Darwin continued to receive a wealth of information from Fritz Müller in Brazil. As well as providing material on sexual selection and answers to Darwin's queries about expression, Fritz continued to add observations on dimorphism and trimorphism and reported on a series of crossing experiments with orchids. Darwin commented, 'You have communicated to me many more cases than any two or three botanists put together' (letter to Fritz Müller, 7 February [1867]). Müller had written the previous year concerning the self-sterility of some orchids, and in his first letter of 1867 he reported on his experiments with an orchid whose pollen had a poisonous effect when applied to its own stigma (letter from Fritz Müller, 1 January 1867). Darwin replied, 'The fact about the own-pollen being poisonous is quite extraordinary', and wondered whether the cause of the decay might be 'parasitic cryptogams' (letter to Fritz Müller, 22 February [1867]). Müller was able to assure him that he had only once seen a parasitic cryptogam on the plant (letter from Fritz Müller, 1 April 1867). Darwin was so interested in Müller's observations on the poisonous effect of a plant's own pollen that he decided, even at such a late stage, to add an abstract of the material to the proof-sheets of *Variation* (letter to Fritz Müller, 31 July [1867]).

Darwin was also interested in experiments crossing different species of orchids that Müller had reported on, but remarked, 'I fear their interest will be greatly lessened by the crossed seeds not germinating', noting, 'One single man in Europe has found out how to make these seeds germinate, & he keeps it a secret in his trade of nurseryman' (letter to Fritz Müller, 26 May [1867]). Darwin was no doubt alluding to the success of the famous nursery of James Veitch & Son in maintaining what was, in 1867, a more than ten-year monopoly in the production of orchid hybrids (Shephard 2003). Darwin frequently received seeds and specimens as well as information from Müller, and, in a letter of 2 November 1867, was able to report on the progress of plants he had grown from some of Müller's seeds. Darwin would continue with these long-term experiments, growing several generations of plants from a wide variety of self-pollinated and cross-pollinated specimens, observing growth rates, vigour, seed production, and other features, and comparing his own results with those of trusted correspondents like Müller and Friedrich Hildebrand.

During the year, two significant critical works appeared, both of which offered very different challenges to the theory of the transmutation of species through natural selection. In January 1867, the duke of Argyll, George Douglas Campbell, published *The reign of law* (G. D. Campbell 1867), a book based on a series of articles that had appeared in 1865. In it he challenged aspects of Darwin's theory, especially the notion that beauty had an adaptive function, arguing instead that beauty in nature was designed by the creator for the aesthetic education of humans. Several

of Darwin's friends commented on it. Huxley, who had not read it, referred to it dismissively as 'the Dukelets book', and noted, 'I hear he is down on both of us' (letter from T. H. Huxley, [before 7 January 1867]). In February, Hooker asked whether Darwin had read it and whether it was worth reading (letter from J. D. Hooker, 4 February 1867). In a letter to his son William, Darwin confided, 'Mamma has several times declared that the Duke did not understand the Origin, but I pooh-poohed her, & as it seems very unjustly' (letter to W. E. Darwin, 27 [March 1867]). Unfortunately, he did not elaborate further on Emma Darwin's view, so we are left to wonder about the nature of her criticisms. By June, Darwin was reading the book, which he thought interesting and 'clever', but with certain weak parts (letter to Charles Lyell, 1 June [1867]). Charles Kingsley found the book 'fair' but told Darwin, 'he [Campbell] writhes about under you as one who feels himself likely to be beat' (letter from Charles Kingsley, 6 June 1867). Darwin was inspired by Kingsley's remarks to give his own critique of the book, which he had just finished reading. He was struck by Campbell's arrogance, pointed out inconsistencies in the remarks on orchids and hummingbirds, and argued, 'With respect to the Deity having created objects beautiful for his own pleasure, I have not a word to say against it but such a view c[d] hardly come into a scientific book' (letter to Charles Kingsley, 10 June [1867]).

After dealing with Campbell's book, Darwin turned to the other work that had posed serious challenges to his theory, an anonymous critique in the *North British Review*, which he described as 'one of the most telling Reviews of the hostile kind' (letter to Charles Kingsley, 10 June [1867]). Kingsley himself had remarked, 'It is a pity the man who wrote it had not studied a little zoology & botany, before writing about them' (letter from Charles Kingsley, 6 June 1867). The review had, in fact, been written by an engineer, Henry Charles Fleeming Jenkin, who had recently collaborated with William Thomson on experiments on electric cable insulation. One of his criticisms in the article, based on Thomson's calculation of the age of the earth, was that there was not enough time for the changes Darwin described to have occurred. This argument had been raised before and Darwin seemed confident in dealing with it, pointing to the uncertainty and disagreement within the mathematical community itself on the question of the age of the earth. Far more thought-provoking, however, was the argument that a variation, no matter how favourable, would tend to be swamped over time, given a theory of heredity that relied on blending inheritance, the generally accepted view of heredity, which Darwin shared. Darwin conceded that he would need to change some of the wording in the relevant section of *Origin*, commenting, 'Instead of saying, as I have sometimes incautiously done a bird suddenly appeared with a beak /particularly/ longer than that of his fellows, I would now say that of all the birds annually born, some will have a beak a shade longer, & some a shade shorter, & that under conditions or habits of life favouring longer beak, all the individuals, with beaks a little longer would be more apt to survive than those with beaks shorter than average' (letter to Charles Kingsley, 10 June [1867]).

Typically, Darwin chose to steer clear of responding publicly to criticism, so he was pleased when Wallace published a long article, 'Creation by law' (A. R. Wallace 1867c), which responded to Jenkin's and Campbell's critiques. Darwin commended Wallace, 'You have just touched on the points which I particularly wished to see noticed. I am glad you had the courage to take up Angræcum after the Duke's attack; for I believe the principle in this case may be widely applied. I like the Figure but I wish the artist had drawn a better sphynx' (letter to A. R. Wallace, 12 and 13 October [1867]). Darwin referred to the full-page illustration at the beginning of the article, an artist's impression of the orchid *Angraecum sesquipedale* with its pollinator, a hypothetical sphinx moth (which Darwin had predicted must exist) with a proboscis long enough to reach the base of the nectary. (The predicted moth, *Xanthopan morgani praedicta*, was eventually discovered in 1903.) The illustration is reproduced as the frontispiece to this volume. In the same letter, Darwin reported on a favourable review from an unlikely source. George Warington, a writer who usually wrote on religious subjects, had written an abstract of *Origin* for the *Journal of the Transactions of the Victoria Institute or Philosophical Society of Great Britain*. The Victoria Institute had been founded a couple of years earlier with the aim of defending 'the great truths revealed in Holy Scripture' against 'the oppositions of Science, falsely so called'. Not surprisingly, Warington's article caused something of a furore within the Victoria Institute itself, which Darwin, having read the proceedings, described as 'very rich from the nonsense talked' (letter to A. R. Wallace, 12 and 13 October [1867]).

Although Darwin himself almost always shied away from public debate, many of his supporters did not. Darwin was called upon to encourage one of his most devoted supporters, the German zoologist Ernst Haeckel, to moderate his public attacks on the opposition. Darwin's German translator, Julius Victor Carus, told Darwin that while Haeckel's recent large work, *Generelle Morphologie der Organismen* (Haeckel 1866), contained much interesting material, it also contained 'personal and quite unnecessary remarks', and pleaded with Darwin to intervene, claiming, 'There is only one man, to whose judgement he would subdue; that is yours' (letter from J. V. Carus, 5 April 1867). Darwin complied, and his letter to Haeckel gives an insight into his own views on criticism in general. He wrote, 'I have long observed that much severity leads the reader to take the side of the attacked person', and concluded, 'I know that it is easy to preach & if I had the power of writing with severity I dare say I sh^{d} triumph in turning poor devils inside out & exposing all their imbecility. Nevertheless I am convinced that this power does no good, only causes pain. I may add that as we daily see men arriving at opposite conclusions from the same premises it seems to me doubtful policy to speak too positively on any complex subject however much a man may feel convinced of the truth of his own conclusions' (letter to Ernst Haeckel, 12 April [1867]).

On a personal level, the year was relatively peaceful. Darwin's own health continued to be good for the most part, although in September he suffered a bout of eczema, evidently accompanied by some temporary memory loss, which caused

Emma to consult his physician, Henry Bence Jones. Jones wrote reassuringly, conjecturing the cause might be 'increased mental work', and suggesting, 'the best course would be to put mental work aside *altogether* for this month; except on wet days' (letter from H. B. Jones, 1 October [1867]). There is no evidence that Darwin took Jones's advice; in fact, he had long maintained that bouts of eczema actually alleviated his other symptoms, and made him feel more alert (see *Correspondence* vol. 13, letter to J. D. Hooker, 9 February [1865] and n. 4). Darwin's wife and children also prospered; after the deaths of two of his sisters the previous year, it must have been a relief to have no serious illness in the family. However, his youngest son, Horace, had a bout of fever, which Darwin mentioned to his close friend Hooker, whose own newborn baby had been worryingly ill with convulsions. At one point Hooker confided, 'We have no hopes for our pretty little baby whose fits increase in number & duration' (letter from J. D. Hooker, 31 March 1867). Within days, however, Frances Harriet Hooker was able to reassure the Darwins that the baby was recovering quickly.

Darwin's family continued to assist him in his work, his wife and daughters reading to him and acting as amanuenses. Henrietta Emma Darwin read and corrected proof-sheets of *Variation*, and while she was away in Cornwall in the summer took some of the work with her. Darwin wrote to commend her work, 'All your remarks, criticisms doubts & corrections are excellent, excellent, excellent' (letter to H. E. Darwin, 26 July [1867]). The year ended as it had begun, with letters about *Variation*. Darwin wrote to Carus on 10 December, informing him of errors discovered by Dallas and asking him to make the changes if not too late in the first volume, then adding, 'The book is delayed by the index-maker'. The letter Darwin sent to Dallas himself has not been found, but must have indicated his impatience, for Dallas replied on 26 December, 'I am vexed to the heart that you should have occasion to write to me again about this Index.' Dallas noted, in his own defence, 'the real cause of delay lies in the nature of the work itself.' *Variation* was published on 30 January 1868.

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LIST OF PROVENANCES

The following list gives the locations of the original versions of the letters printed in this volume. The editors are grateful to all the institutions and individuals listed for allowing access to the letters in their care. Access to material in DAR 261 and DAR 263, formerly at Down House, Downe, Kent, England, is courtesy of English Heritage.

American Philosophical Society, Philadelphia, Pennsylvania, USA
Archives, The New York Botanical Garden, Bronx, New York, USA
Athenæum (publication)
Bayerische Staatsbibliothek, München, Germany
Bibliothèque Publique et Universitaire de Genève, Geneva, Switzerland
British Library, London, England
Cambridge University Library, Cambridge, England
The Charles Darwin Trust, London, England
Christ's College, Cambridge, England
Cleveland Health Sciences Library, Cleveland, Ohio, USA
DAR *see* Cambridge University Library
Edinburgh University Library, Edinburgh, Scotland
Field Museum of Natural History, Chicago, Illinois, USA
Jean-Louis Fischer (private collection)
Fitzwilliam Museum, Cambridge, England
Paul V. Galvin Library, Illinois Institute of Technology, Chicago, Illinois, USA
Gardeners' Chronicle and Agricultural Gazette (publication)
J. L. Gray ed. 1893 (publication)
Gray Herbarium of Harvard University, Cambridge, Massachusetts, USA
B. C. Guild (private collection)
Ernst-Haeckel-Haus, Friedrich-Schiller-Universität Jena, Germany
Hardwicke's Science-Gossip (publication)
S. J. Hessel (private collection)
Eilo Hildebrand (private collection)
Houghton Library, Harvard University, Cambridge, Massachusetts, USA
Imperial College of Science, Technology, and Medicine, London, England
Institut Mittag-Leffler, Djursholm, Sweden
King's College, University of London, Strand, London
Linnean Society of London, Piccadilly, London, England
K. M. Lyell ed. 1881 (publication)

McGill University Libraries, Rare Books and Special Collections Division, Montréal, Québec, Canada
Marchant ed. 1916 (publication)
ML (publication)
Möller ed. 1915–21 (publication)
John Murray Archive, London, England
Museo Civico di Storia Naturale, Milan, Italy
Muséum National d'Histoire Naturelle (Département Systématique & Evolution; Cryptogamie), Paris, France
The Natural History Museum, Cromwell Road, London, England
Notes and Queries on China and Japan (publication)
Quaritch (dealer), London, England
Royal Botanic Gardens, Kew, Richmond, Surrey, England
Royal College of Physicians, Regent's Park, London, England
Royal Entomological Society, Queen's Gate, London, England
Elizabeth Rütimeyer (private collection)
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