THE CORRESPONDENCE OF
CHARLES DARWIN

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THE CORRESPONDENCE OF CHARLES DARWIN

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Dedicated to
the many friends and colleagues in Bennington, Vermont,
who worked with Fred and Anne Burkhardt
between 1974 and 2012

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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 markers 'f' and 'g' denote letters acquired after the second edition of the *Calendar* went to press in 1994.

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INTRODUCTION

Plants always held an important place in Darwin's theorising about species, and botanical research had often been a source of personal satisfaction, providing relief during his periods of severe illness. Yet on 15 January 1875, Darwin confessed to his close friend Joseph Dalton Hooker, 'I am getting sick of insectivorous plants.' Darwin had worked on the subject intermittently since 1859, and had been steadily engaged on a book manuscript for some nine months. The pleasures of observation and experiment had given way to continuous writing and revision, activities that Darwin found less gratifying: 'I am slaving away solely at making detestably bad English a very little less bad.' The process was compounded by the fact that Darwin was also revising another manuscript, the second edition of *Climbing plants*, which he hoped to publish in a single volume along with the material on insectivorous plants. No sooner had he completed these tasks, than he took up the revision of another, much longer book, the second edition of *Variation*. 'I am merely slaving over the sickening work of preparing new Editions', he complained again to Hooker on 18 August. Finally, by autumn, he was able to devote more time to research, returning to the subject of cross and self-fertilisation. On 3 October, he wrote with fresh enthusiasm to the new assistant director at the Royal Botanic Gardens, Kew, William Turner Thiselton-Dyer, about the prospect of obtaining new specimens: 'I have great wish next summer to experimentise on some Marantaceous plant to make out meaning of 2 sets of differently coloured stamens.' At intervals during the year, Darwin was diverted from the onerous task of writing by various controversies. January saw the conclusion of a long-running dispute with the zoologist St George Jackson Mivart. In April and early May, Darwin was occupied with a heated debate over vivisection, working with scientific colleagues and family members to prepare draft legislation for Parliament. At the end of the year, he campaigned vigorously against the blackballing of a young zoologist, Edwin Ray Lankester, who was up for election to the Linnean Society. The 'malcontents' of the Linnean sickened him much more than insectivorous plants. As he confessed to Hooker on 12 December, 'I have not felt so angry for years.'

In January, the protracted dispute with Mivart came to a close. The final chapter of the controversy involved a slanderous attack upon Darwin's son George, in an anonymous review in 1874 (see *Correspondence* vol. 22, Appendix V). Darwin remained bitter and dissatisfied with Mivart's attempts at conciliation, and spent weeks deliberating how to end the matter to his satisfaction. On 8 January, he told Hooker: 'I will write a savage letter & that will do me some good, if I do not send it!' In the end,

with much advice and assistance from his family, he sent a curt note to Mivart on 12 January, breaking off all future communication. Darwin had been supported during the affair by the loyalty of his close friends, Hooker and Thomas Henry Huxley. Because Mivart was a distinguished zoologist, a fellow of the Royal Society of London, and a secretary of the Linnean Society, Darwin's friends had to find ways of coming to his defence while still respecting codes of conduct and communication in scientific society. Huxley chose journalism, depicting the anonymous reviewer (Mivart) as a blind antagonist of 'all things Darwinian' and a mere mouthpiece of 'Jesuitical Rome' (*Academy*, 2 January 1875, pp. 16–17). 'How grandly you have defended me', Darwin wrote on 6 January, 'You have also greatly honoured George. You have indeed been a true friend.' Hooker was hampered by his position as president of the Royal Society from spurning Mivart in public. 'Without cutting him direct', he advised Darwin on 7 January, 'I should avoid him, & if he speaks to me should let him *feel it*.' Hooker also directed some of his anger toward John Murray, the publisher of the *Quarterly Review*, in which Mivart's anonymous essay had appeared. 'I told him that the Review was disgraced, that I should give the cold shoulder to the Editor … Poor Murray shuddered again & again' (letter from J. D. Hooker, 16 January 1875). Darwin had also considered taking up the issue with Murray in 1874, even threatening to break off future dealings with the man who had been his publisher for over thirty years. 'My thirst for vengeance is now quite Satisfied', he told Hooker on 17 January, 'I feel now like a pure forgiving Christian!' Darwin's ire was not fully spent, however, for he set about exposing Mivart's character to other men of science when the chance arose. On 28 January, he sent a note on Royal Society business to Edward Burnett Tylor, whose anthropological work had been reviewed in the same *Quarterly* article that attacked George. Darwin raised the matter at the end of the note: 'I know positively that this article was written by Mr Mivart & I wish to take every opportunity of saying how false a man I consider him to be.'

Just as the Mivart affair was laid to rest, another controversy was brewing. In December 1874, Darwin had been asked to sign a memorial on the practice of vivisection by the religious writer and social reformer Frances Power Cobbe. The memorial raised questions about the cruel treatment of animals in experimental physiology and medical teaching. Cobbe was an acquaintance of the Darwins and part of a circle of philanthropists that included Hensleigh and Frances Wedgwood. She had corresponded with Darwin about the evolution of the moral sense, and shared with Darwin a great fondness for dogs (see *Correspondence* vols. 19 and 20). The vivisection issue was a delicate one within Darwin's family, and he tried to balance his concern for animal suffering with his firm belief in the value of experimental physiology. He expressed his views to his daughter Henrietta on 4 January: 'I wd *gladly* punish severely anyone who operated on an animal not rendered insensible, if the experiment made this possible … Under this point of view I have rejoiced at the present agitation … [but] I certainly could not sign the paper sent me by Miss Cobbe.' Darwin found Cobbe's memorial inflammatory and unfair in its criticism of physiologists. Instead of supporting her, he worked closely with Huxley

and John Burdon Sanderson to draft an alternative proposal. His extensive involvement in preparing legislation on vivisection is evident in the many letters exchanged with physiologists, medical men, and legal experts in April and May, and in various drafts of a bill that was presented to the House of Commons on 12 May, one week after a rival bill based on Cobbe's memorial had been read in the House of Lords (see Appendix VI).

With such divided opinion on vivisection, the government decided to appoint a Royal Commission to advise on future legislation. Huxley served on the commission, which heard testimony from physiologists, medical educators, and other interested parties. Darwin was summoned to testify on 3 November. It caused him much anxiety, but he was asked only to give his general opinion of the value of vivisection and the importance of eliminating unnecessary suffering. A much more controversial witness was Edward Emanuel Klein, a German histologist who worked with John Burdon Sanderson at the Brown Animal Sanatory Institution. When asked about his use of anaesthetics in research, Klein expressed doubt about the validity of experiments conducted under such conditions, and even asserted that he had 'no regard at all' for the sufferings of animals when performing a painful experiment (*Report of the Royal Commission on vivisection*, p. 183). Darwin learned of Klein's testimony from Huxley on 30 October 1875: 'I declare to you I did not believe the man lived who was such an unmitigated cynical brute—as to profess & act upon such principles—and I would willingly agree to any law, which should send him to the treadmill.' Darwin had become acquainted with Klein when his son Francis was studying medicine in London. Klein had assisted in some of Darwin's botanical research and had visited Down House in April 1874 (see *Correspondence* vol. 22, letters from E. E. Klein, 14 May 1874 and 10 July 1874). 'I am astounded & disgusted at what you say about Klein,' Darwin replied to Huxley on 1 November. 'I am very glad he is a foreigner; but it is most painful as I liked the man.'

Darwin's keen interest in the progress of physiology was due in part to his research on the digestive properties of insectivorous plants. This work had led to collaborations with a number of leading physiologists. Indeed, some of the experiments that Darwin performed on plants, such as the application of salts, acids, and poisons, were analogous to those carried out on live animals in laboratories. In January 1875, he received details of experiments by Thomas Lauder Brunton and Joseph Fayrer on the comparative effects of cobra venom on animal and plant tissue. The experiments involved the application of snake poison to the cellular tissue of frogs, newts, and freshwater mussels, as well as to *Vallisneria* (tape grass). Fayrer had previously supplied Darwin with a quantity of the dried poison, which Darwin had applied in varying amounts to the tentacles of *Drosera rotundifolia* (common sundew). He recorded the inflection of the tentacles, noting that little or no harm came to the plant, and that the poison even acted as a kind of stimulant, causing rapid aggregation of protoplasm. He added the details of Brunton and Fayrer's experiments to *Insectivorous plants*, pp. 206–9, remarking on the pronounced differences in the response of plants and animals to various toxic substances.

By January, Darwin was trying to finish the text that he had begun writing in April of the previous year. He did not find the process of writing and revising at all satisfying. On 10 February he complained to Hooker: 'You ask about my book & all that I can say is that I am ready to commit suicide.' Darwin's despair over the revision process may have coloured his perception of the book's appeal to readers, for he warned Murray on 29 April that it might 'sell very poorly'. When a relatively small print run of 1000 was proposed, Darwin offered to pay the costs for printing an additional 250 (letter to John Murray, 3 May 1875). In the event, the book sold well, and Murray's partner, Robert Cooke, politely scolded Darwin on 3 July for underestimating demand: 'You took such a desponding view of your new work that you made us over cautious & we printed but 1250 … & lo & behold we have sold some 1700 Copies!!!' After the initial publication on 2 July, two further printings were needed in quick succession. As usual, Darwin had a number of presentation copies sent to friends, those who had assisted him, and various experts in the field. One of the most enthusiastic responses came from the Swiss botanist Arnold Dodel, an instructor at the University of Zurich. He immediately took the book on a botanical trip with students around the Katzensee, a lake on the border of Zurich, where *Drosera rotundifolia* grew in abundance. 'Your book had to go along on the excursion,' he reported on 6 July, 'and it did us excellent service.' The trapped insects were observed in the field, and some of Darwin's experiments on digestion were then repeated: 'I have the pleasure of demonstrating, in my botanical–microscopical laboratory at the university here, the results of *your* investigations on the living plant to dozens of eager students.' The cunning ways in which plants lured insects to their death were described in a review of the book in the *Academy*, 24 July 1875, by Ellen Frances Lubbock: 'in Utricularia they are enticed into a trap with a spring door … in Sarracenia drink is their ruin, and they fall into a pit of destruction.' She also wrote a plea for the poor creatures in the form of a poem, 'From the Insects to their friend, Charles Darwin'. 'We are very much obliged to you / For now of course we shan't / Be taken in or done for / By any clever Plant … Great plates of honey you will set / For us upon your lawn, / We'll feast away & bless the day / That ever you were born' (letter from E. F. Lubbock, [after 2 July] 1875).

Darwin had originally planned *Insectivorous plants* to be published together with a revised edition of *Climbing plants*. He made corrections to both works from January to March, and completed the two manuscripts about the same time. As was the case with some of Darwin's previous publications, however, the resulting text was judged too large for one volume. *Climbing plants* 2d ed. was delayed until November, allowing Murray to advertise it at his annual sale. In addition to these two botanical publications, Darwin also worked on a second edition of *Variation*, commencing in June, shortly after the proof corrections of *Insectivorous plants* were finished. Darwin's attention seems to have been largely on aspects of generation and development that had some bearing on his hypothesis of pangenesis. He followed up a case of the alleged regrowth of an extra finger after amputation, corresponding in August with Annie Dowie, a daughter of Robert Chambers, in hopes of getting more precise details

about an operation performed in 1851 on her sister. He had described the case in *Variation* 2: 14–16, suggesting that such regrowth could be explained by reversion to 'an enormously remote, lowly-organised, and multidigitate progenitor'. In the end, following the advice of the physician James Paget, he removed the discussion in *Variation* 2d ed. Further information was gathered about graft hybrids. He revisited the case of *Cytisus adami*, a hybrid of two forms of laburnum with flowers of each form on different branches. He also expanded his discussion of hybrid potatoes, adding numerous cases of intermediate varieties produced by splicing tubers or by inserting the eyes of one variety into another (*Variation* 2d ed. 1: 420–4, 2: 360).

Darwin had encouraged further research on the effects of grafting by George John Romanes. A scientific friendship had developed between the men in 1874, and this was enhanced by Romanes's visit to Down House: 'The place was one which I had long wished to see,' he wrote on 21 April 1875, 'and now that I have seen it, I am sure it will ever remain one of the most agreeable and interesting of memory's pictures.' Though trained in zoology and hard at work on the rudimentary nervous system of medusae, Romanes was eager to carry out experiments that might help confirm Darwin's theory of heredity. 'I am a young man yet, and hope to do a good deal of "hammering",' he wrote on 14 July 1875. 'I shall not let Pangenesis alone until I feel quite sure that it does not admit of being any further driven home by experimental work.' Romanes bisected root vegetables and tuberous plants, and boasted about a 'beautifully successful graft' of a red and white carrot: 'You will see that the union is very intimate, and that the originally red half has become wholly white' (letter from G. J. Romanes, [before 4 November 1874]).

Experiments to test Darwin's pangenesis hypothesis had been performed on animals in previous years by Darwin's cousin Francis Galton. These had been unsuccessful, and Galton went on to develop his own theory of heredity in a series of articles in 1875 and 1876, based partly on his studies of twins and the inheritance of 'genius' in families. Galton came to reject the view that characteristics acquired in an individual's lifetime could be transmitted to offspring. According to Galton's theory, some 'germs' developed within the individual, giving rise to characteristics such as hair and eye colour; however, most germs remained dormant and were stored in the reproductive organs in isolation from the effects of environment or habit. He believed that the active germs were rendered 'sterile' by their use in the organism, and so exerted little or no influence on offspring; whereas the inactive or 'residual' germs were passed to offspring unmodified. Galton shared his views with Darwin in several lengthy letters. 'I am very glad indeed of your work,' Darwin replied on 4 November, 'though I cannot yet follow all your reasoning.' Darwin struggled with Galton's unconventional terminology and with the abstractness of his theory, which Galton was unable to improve upon in letters. 'Unless you can make several parts clearer,' Darwin reiterated on 7 November, 'I believe (though I hope I am altogether wrong) that very few will endeavour or succeed in, fathoming your meaning.' Darwin remained committed to the importance of conditions of existence, and the effects of changed habits on fertility and the health of offspring. In the previous year,

he had expanded his discussion in the second edition of *Descent* of the causes of extinction in 'savage races' when their ways of life were altered by European colonists (*Descent* 2d ed., pp. 188–90). He drew attention to this discussion in a letter to George Rolleston, remarking on 2 September: 'the case is strictly parallel to the sterility of many wild animals when made captive. The civilisation of savages & the captivity of wild animals leading to the same result.'

In correcting his manuscripts and proofs, Darwin now relied heavily on his son Francis, who had made the decision in 1873 to abandon his medical studies and work as his father's secretary. On sending the latest batch of corrections to his son in February, Darwin wrote, 'I beg ten thousand pardon & more' (letter to Francis Darwin, [*c.* February 1875?]). By May, having finished *Insectivorous plants*, and moved on to *Variation* 2d ed., Francis signed himself, 'Your affect son … the proofmaniac' (letter from Francis Darwin, 1 and 2 May [1875]). But Francis also found time to pursue his own research, often taking up questions raised by his father's work. He studied aggregation in the tentacles of *Drosera rotundifolia* and prepared a paper on the structure and function of the proboscis of *Ophideres fullonica*, an orange-sucking moth. He observed the hygroscopic properties of seeds, using an instrument designed by his brother Horace, who was doing an apprenticeship at an engineering firm. Darwin was impressed by the device, remarking to Hooker on 13 October: 'Horace has made a hygrometer with a bit of the twisted awn or pistil (or whatever it may be) & mounted it on a graduated circle; & I have never in my life been more astonished than at its sensitiveness. If you blow gently at it from 1 or 2 feet distance, it absorbs moisture & *instantly* rotates.'

George continued to suffer from poor health, for which his chronically invalid father had much sympathy: 'I know well the feeling of life being objectless & all being vanity of vanities,' he wrote on 10 February. 'But this will wear away all the sooner for not trying to work too soon.' George had begun research on tidal friction and the rigidity of the earth, and was discouraged by his early experiments on the flow of pitch. Darwin encouraged him to persevere, writing on 13 October, 'I do not in the least fear that if there is anything to be made out by your method with respect to viscous fluids, you will succeed—such energy as yours almost always succeeds.' 'I'm afraid my letters smell of pitch,' George replied on 26 October, 'but I can think of O else.' In between his physics research and bouts of illness, George still found time to write articles for leading periodicals such as the *Contemporary Review*. Having just emerged from the controversy with Mivart over his paper on cousin marriage, he was embroiled in another as the result of a review of William Dwight Whitney's work on language (G. H. Darwin 1874c). George had taken the American scholar's side in an ongoing debate with the Oxford professor of oriental languages, Friedrich Max Müller. George's article also rehearsed some of Darwin's own arguments in *Descent* about animal language, which had become a debating point between Whitney and Max Müller. In *Descent* 2d ed., pp. 86–8, Darwin had cited Whitney's supportive arguments on the evolution of language through unconscious processes, and had criticised Max Müller's insistence that language was an 'impossible barrier' between

humans and animals. George, in turn, quoted Whitney's favourable assessment of his father's theory, and credited Whitney with a 'successful refutation of the somewhat dogmatic views of the Oxford linguist' (G. H. Darwin 1874c, p. 894).

On previous occasions, Max Müller and Darwin had aired their differences cordially in letters (see *Correspondence* vol. 21), and George's review prompted Max Müller to write to Darwin affirming that his convictions were founded on 'careful consideration' rather than a fear of animal ancestry. 'You know better than anybody', he wrote on 7 January, 'how infinitely great is the difference between man and animal: what I want to know is the first small and hardly perceptible cause of that difference, and I believe I find it in language & what is implied by language.' Max Müller also published an article in response to George's essay, suggesting that 'Mr Darwin, jun.' had used the pretext of a review of Whitney to defend his father. He compared the elder naturalist's writing on language to such opinions as the Pope might offer on astronomy, or the Duke of Wellington on art (Max Müller 1875, pp. 305–7). The debate between Max Müller and Whitney continued in the periodical press and elsewhere, growing more bitter. George, who was on friendly terms with Whitney, wrote to him on 21 December about the perils of entering into a professional dispute between philologists: 'I confess I felt it a little hard on myself to be dragged into the foreground & chaffed—rather savagely however. In my Contemp. article I thought I had made it pretty clear that I wrote as an ignoramus & only intended to represent your views, with such comments as ordinary intelligence *without* linguistic knowledge wd. allow me.'

Tempers flared closer to home when an earlier dispute between Darwin and the local vicar George Sketchley Ffinden resurfaced. In 1873, Charles and Emma Darwin and the Lubbocks had sought Ffinden's support in allocating a reading-room for working men in the evenings as an alternative to the public house. In previous years, they had used a village schoolroom that was under the authority of the Church. After becoming vicar in 1871, Ffinden had opposed their efforts, and had taken issue with Darwin over his involvement in parish affairs (see *Correspondence* vol. 21). Lubbock tried to bring about a reconciliation, writing to Darwin on 5 April: 'Having occasion to write to Mr. Ffinden about another matter, I incidentally expressed my regret at the coolness between you, & my conviction that it must arise from some misapprehension on his part. In reply Mr. Ffinden expressed his regret that there should be any want of harmony between himself & one "so highly gifted both intellectually & morally," but he refers to two matters, the first being your having written to the education department about the school, which he considered an intentional slight. The second was that you ordered five pounds worth of repairs to the Infant school whereas the Committee only sanctioned an expenditure of £4, & he thinks that in this also you intended to slight him.' Darwin assured Lubbock that he never meant to show disrespect, but insisted that it was Ffinden who had given offence. 'Mr. Ffinden accused me in the vestry of having made false statements,' Darwin replied on 8 April. 'This is conduct which a man does not commonly pass over without some sort of apology.— Nevertheless if M^r^. Ffinden bows to M^rs^. Darwin

or myself we will return it; but I fear under present circumstances that we can take no further step.' Relations between the clergyman and the Darwins did not warm thereafter. On 24 December, Emma wrote triumphantly to the former vicar, John Brodie Innes, that a new reading-room had been opened in the village, and a local temperance society had been established by a Down solicitor and his wife: 'Both these undertakings are thorns in Mr Ffinden's side & he has not been content with holding aloof from them; but has used all his influence to prevent their succeeding.'

In the wider public sphere and in scientific communities abroad, Darwin's work continued to elicit strong reactions, both critical and reverential. On 16 July he received a letter from an advocate of women's rights, Charlotte Papé, questioning his views in *Descent* on the superiority of male intellect: 'I myself know so comparatively many striking instances to the contrary, among my friends and my own family, that it seems highly improbable to me. At any rate, every woman ought to try to ascertain as much of the truth in respect to it as she can; for apart from the interest of the question in itself, it is most important for the future of women.' Papé asked Darwin for advice on designing a comparative study of the inheritance of mental powers in women and men, and expressed her frustration at the social constraints that women faced in the pursuit of science: 'of course, like all women, I have had no scientific training … And it is just this very helplessness as to getting information … that must form my excuse for the unwarrantable liberty I am taking.'

Some of Darwin's most ardent support came from Germany. His long-serving translator, Julius Victor Carus, brought out a third German edition of *Descent* (Carus trans. 1875a), and started at once to translate *Insectivorous plants* (Carus trans. 1876a). The German publisher E. Schweizerbart'sche Verlagshandlung began to issue Darwin's other works in the form of a collected edition, *Ch. Darwin's gesammelte Werke*, commencing with his famous narrative of the *Beagle* voyage, *Journal of researches* (Carus trans. 1875b; the series is Carus trans. 1875–87). More controversial was the proposal by the journalist Otto Zacharias to start a monthly periodical titled *Darwinia*. 'Our Journals & weekly publications of Natural History', he explained on 3 June, 'are not sufficiently penetrated with the value & importance of your theory.' Zacharias dubbed the journal 'a special Organ for Darwinismus' and '*transformationist monthly*'. The list of contributors and supporters included long-term correspondents such as Ernst Haeckel, Fritz and Hermann Müller, and Anton Dohrn. Although the periodical never appeared under the proposed title, many of the same contributors were involved in the launch of *Kosmos* in April 1877. From Haeckel, Darwin received a copy of a book titled *Kant und Darwin*, linking his theory of descent to the celebrated German philosopher's ideas on race. 'It is yet another laurel in the wreath of *your fame*', Haeckel wrote on 6 June, 'to have a predecessor in the greatest thinker of Germany!— I hope that this will convert many German philosophers to "Darwinism"!' The author, Fritz Schultze, contacted Darwin himself on 12 June, describing the aims of his book: 'My hope is that Kant, whose standing with *all* parties in Germany is exceedingly high, will convert to Darwinism also *those* who have remained unconvinced … we now view Kant as a "Darwinist before Darwin".'

A more humble tribute was paid by an anonymous correspondent: 'The learned Darwin states that Moses taught confusion / For Man, he boldly says, descends from Ape or Monkey— / I, having read his book, am come to this conclusion / Darwin (at least himself) descends from Ass or Donkey.' On the back of the doggerel, Darwin wrote: 'An anonymous compliment | received Feb 16th 1875'.

Darwin's fame in 1875 was also marked by the increasing number of visitors to Down House. In addition to the usual round of friends, such as the Lubbocks, Huxleys, and Hookers, Darwin hosted many scientific guests and others of social distinction. One of the most keen of visitors was Lady Dorothy Nevill. She had corresponded with Darwin the previous year about insectivorous plants, and had lent him several tropical specimens from her large private collection. She tried to meet Darwin in London on several occasions and finally arranged a visit to Down House on 4 May, but was not content with just one meeting: 'I did so enjoy my afternoon', she wrote on 2 July, 'and if it were not too much to ask—later on—if it were possible I should so like to come down again.' Darwin tried to satisfy her with an autograph, which she requested to accompany her presentation copy of *Insectivorous plants* (letter to D. F. Nevill, 15 July [1875]). Such visitors from the upper ranks of society could be especially taxing. As Emma remarked in a letter to William on 1 May, they required Darwin to be 'so friendly & adoring (if possible)' (DAR 219.1: 89). The most eminent of Darwin's guests was Francis, duke of Teck, a German prince married to a granddaughter of George III. Darwin had hoped to arrange for the meeting to take place at Lubbock's home, High Elms, so that he could get away quickly: 'I do not see how I could get a sort of living Royal Duke out of my house within the short time I can talk to anyone' (letter to John Lubbock, 3 May [1875]). Finally it was arranged for the duke to stop at Down on 19 August before going on to Lubbock's for lunch. Another aristocratic visitor, Lady Derby, prompted a crisis when she proposed coming on the same day as the Darwins were hosting the Russian explorer Nikolai Alekseevich Severtsov and the ornithologist Henry Eeles Dresser. 'The horror was great', Henrietta Emma Litchfield wrote to her brother Leonard on 14 September, '& special messengers had to be sent off to stop them' (DAR 258: 1646). There was no reprieve, however: the Derbys, who had rented a house nearby at Keston, placed their home at the disposal of Thomas Carlyle, who visited Down no less than three times over the summer. Darwin later recalled how the dour sage 'sneered at almost every one. One day in my house he called [George] Grote's *History* "a fetid quagmire, with nothing spiritual about it"… his expression was that of a depressed, almost despondent, yet benevolent man' ('Recollections', p. 407).

Even scientific colleagues could be trying at times. In March, Darwin began corresponding with the Birmingham surgeon Lawson Tait, a specialist in gynaecology. Darwin was interested in his work on ovarian tumours, which Tait believed originated in bud-like tissue through a process that was best explained by Darwin's hypothesis of pangenesis. Over the next few months, Tait wrote a series of long letters on various subjects, suggesting, for example, that bushy tails had evolved as a protective layer for animals to curl up in, and that the shape of the umbilical cord was

analogous to the spiral form of twining plants (letters from Lawson Tait, 16 March [1875] and 27 March [1875]). 'As I am not hampered by worldly exigencies,' he declared on 27 March, 'I do not devote myself to the drudgery of medical life and therefore have time for science.' Tait gave occasional public lectures, presenting himself as a follower and defender of Darwin's theories. In August, he published a favourable review of *Insectivorous plants* for the *Spectator*, and took up the subject himself, focusing on the digestive secretions of *Dionaea* (Venus fly trap), *Drosera* (sundew), and *Nepenthes* (tropical pitcher-plant). He announced that he had isolated a pepsin-like compound from the fluid of *Drosera dichotoma* (the forked-leaf sundew), and sent details of his observations and procedures. His letters came more frequently, even two a day, though sometimes with the proviso that they need not be answered. Darwin could not keep up, and on 22 July, he had Francis reply: 'My Father desires me to say that he is so much engaged with other subjects that he cannot attend to Drosera at present … He desires me to thank you much for your kind desire to help him.' Tait's research not only overlapped considerably with Darwin's, but also with that of Hooker, who had worked periodically on the digestive properties of *Nepenthes* since 1873. 'You are aware that D^r^ Hooker has worked hard at Nepenthes & will soon publish', Darwin warned on 17 July 1875. But Tait was undaunted. He completed a paper in October and asked Darwin to submit it to the Royal Society on his behalf. Darwin complained to Hooker on 13 October, 'It is not at all nice in the confounded man (who has bothered me almost out of my life) to write on Nepenthes when he & all the world know that you have taken up the subject. What had I better do?' Darwin felt obliged to Tait, partly because of his public support for pangenesis and *Insectivorous plants*, but he had reservations about the paper's merit. He confessed to Hooker two days later, 'after agonies of doubt I found that I cd. not endure to refuse … he has been here & wishes to do everything to oblige me, But he is a coarse impudent fellow.' Darwin pleaded that the paper not be referred to him for review. In the end, it was firmly rejected for its faulty chemical methods, and Darwin had to break the news to the author in 1876 that his Royal Society ambitions had been frustrated.

Though Darwin was eventually able to resume observational work on his beloved plants, the year did not end quietly. In December he showed surprising vigour in taking up the cause of Edwin Ray Lankester, who had been blackballed in a bid for election to the Linnean Society. He was the eldest son of Edwin Lankester, a leading microscopist who had assisted Darwin in his work on barnacles (*Correspondence* vol. 5). Though not yet thirty, he had already had a distinguished career, having studied under George Rolleston at Oxford and Huxley at South Kensington, with visiting positions under Haeckel at Jena and Dohrn at Naples. Darwin had expressed his desire to meet Lankester in July, and had agreed to see him at Down with Thiselton-Dyer (letter to W. T. Thiselton-Dyer, 7 July 1875). It was Thiselton-Dyer who nominated Lankester for the Linnean, and he was blackballed on 2 December, the same meeting at which Romanes and Francis Darwin were made fellows. But Thiselton-Dyer had apparently jeopardised his friend's chances by suggesting to the council that his membership fee be waived, thinking that this was appropriate for so distinguished

a nominee. Already in 1875, Lankester had been elected a fellow of the Royal Society, and had been appointed professor of zoology at University College, London. Darwin learned about the blackballing from Hooker, who attributed it to political squabbles within the society, especially among botanists who complained that it was always the zoologists who had their fees remitted. Darwin was incensed that such politicking should take precedence over individual merit, and worried that the result would tarnish Lankester's career. 'It seems to me the most disgraceful act which any scientific Socy. has done in my time,' he told Hooker on 12 December. 'I wish that I knew what the malcontents have to say for themselves.' Darwin spent the next weeks canvassing members of the society to support Lankester at the next meeting, and even planned to attend himself in order to cast a vote. In the event, the election was postponed until February 1876, and Lankester was duly elected.

Darwin was saddened by the deaths during the year of several of his correspondents, including one of his oldest and dearest friends, Charles Lyell. Darwin had learned of Lyell's failing health from Hooker in 1874 and January 1875. On 22 February, he was notified of Lyell's death by Lyell's secretary, Arabella Buckley. Lyell had helped to introduce Darwin to scientific society in London, and offered much advice on his early publications in geology. Replying to Buckley on 23 February, Darwin recalled first meeting Lyell on returning from the *Beagle* voyage: 'how full of sympathy and interest he was about what I could tell him of Coral reefs and South America … almost every thing which I have done in science I owe to the study of his great works.' Later in the year, Darwin received word of the death of one his most avid American supporters, the philosopher Chauncey Wright. Wright had published favourable reviews of Darwin's work, including a defence against Mivart that Darwin had reprinted in Britain. Wright had written to Darwin on 24 February on the lay of hair in eyelashes and on arms, a typically lengthy letter full of personal observations, classical references, and anecdotes of Harvard professors. In September, he died suddenly in his college rooms, and was found at his desk with a copy of *Insectivorous plants* open beside him, and specimens of *Drosera* ready for examination. Though unable to complete an article on Darwin's latest book, Wright did leave his brain to science and on examination it was pronounced to be of a 'high type' (letter from Woodward Emery, 17 September 1875).

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