MORE LETTERS OF CHARLES DARWIN

VOLUME II

By Charles Darwin

Freeditorial

CHAPTER 2.VII.—GEOGRAPHICAL DISTRIBUTION.

1843-1882 (Continued) (1867-1882.)

LETTER 378. J.D. HOOKER TO CHARLES DARWIN. Kew, January 20th, 1867.

Prof. Miquel, of Utrecht, begs me to ask you for your carte, and offers his in return. I grieve to bother you on such a subject. I am sick and tired of this carte correspondence. I cannot conceive what Humboldt's Pyrenean violet is: no such is mentioned in Webb, and no alpine one at all. I am sorry I forgot to mention the stronger African affinity of the eastern Canary Islands. Thank you for mentioning it. I cannot admit, without further analysis, that most of the peculiar Atlantic Islands genera were derived from Europe, and have since become extinct there. I have rather thought that many are only altered forms of existing European genera; but this is a very difficult point, and would require a careful study of such genera and allies with this object in view. The subject has often presented itself to me as a grand one for analytic botany. No doubt its establishment would account for the community of the peculiar genera on the several groups and islets, but whilst so many species are common we must allow for a good deal of migration of peculiar genera too.

By Jove! I will write out next mail to the Governor of St. Helena for boxes of earth, and you shall have them to grow. Thanks for telling me of having suggested to me the working out of proportions of plants with irregular flowers in islands. I thought it was a deuced deal too good an idea to have arisen spontaneously in my block, though I did not recollect your having done so. No doubt your suggestion was crystallised in some corner of my sensorium. I should like to work out the point.

Have you Kerguelen Land amongst your volcanic islands? I have a curious book of a sealer who was wrecked on the island, and who mentions a volcanic mountain and hot springs at the S.W. end; it is called the "Wreck of the Favourite." (378/1. "Narrative of the Wreck of the 'Favourite' on the Island of Desolation; detailing the Adventures, Sufferings and Privations of John Munn; an Historical Account of the Island and its Whale and Sea Fisheries." Edited by W.B. Clarke: London, 1850.)

LETTER 379. TO J.D. HOOKER. Down, March 17th, 1867.

It is a long time since I have written, but I cannot boast that I have refrained from charity towards you, but from having lots of work...You ask what I have been doing. Nothing but blackening proofs with corrections. I do not believe any man in England naturally writes so vile a style as I do...

In your paper on "Insular Floras" (page 9) there is what I must think an error, which I before pointed out to you: viz., you say that the plants which are wholly distinct from those of nearest continent are often very common instead of very rare. (379/1. "Insular Floras," pamphlet reprinted from the "Gardeners' Chronicle," page 9: "As a general rule the species of the mother continent are proportionally the most abundant, and cover the greatest surface of the islands. The peculiar species are rarer, the peculiar genera of continental affinity are rarer still; whilst the plants having no affinity with those of the mother continent are often very common." In a letter of March 20th, 1867, Sir Joseph explains that in the case of the Atlantic islands it is the "peculiar genera of EUROPEAN AFFINITY that are so rare," while Clethra, Dracaena and the Laurels, which have no European affinity, are common.) Etty (379/2. Mr. Darwin's daughter, now Mrs. Litchfield.), who has read your paper with great interest, was confounded by this sentence. By the way, I have stumbled on two old notes: one, that twenty-two species of European birds occasionally arrive as chance wanderers to the Azores; and, secondly, that trunks of American trees have been known to be washed on the shores of the Canary Islands by the Gulf-stream, which returns southward from the Azores. What poor papers those of A. Murray are in "Gardeners' Chronicle." What conclusions he draws from a single Carabus (379/3. "Dr. Hooker on Insular Floras" ("Gardeners' Chronicle," 1867, pages 152, 181). The reference to the Carabidous beetle (Aplothorax) is at page 181.), and that a widely ranging genus! He seems to me conceited; you and I are fair game geologically, but he refers to Lyell, as if his opinion on a geological point was worth no more than his own. I have just bought, but not read a sentence of, Murray's big book (379/4. "Geographical Distribution of Mammals," 1866.), second-hand, for 30s., new, so I do not envy the publishers. It is clear to me that the man cannot reason. I have had a very nice letter from Scott at Calcutta (379/5.

See Letter 150.): he has been making some good observations on the acclimatisation of seeds from plants of same species, grown in different countries, and likewise on how far European plants will stand the climate of Calcutta. He says he is astonished how well some flourish, and he maintains, if the land were unoccupied, several could easily cross, spreading by seed, the Tropics from north to south, so he knows how to please me; but I have told him to be cautious, else he will have dragons down on him...

As the Azores are only about two-and-a-half times more distant from America (in the same latitude) than from Europe, on the occasional migration view (especially as oceanic currents come directly from West Indies and Florida, and heavy gales of wind blow from the same direction), a large percentage of the flora ought to be American; as it is, we have only the Sanicula, and at present we have no explanation of this apparent anomaly, or only a feeble indication of an explanation in the birds of the Azores being all European.

LETTER 380. TO J.D. HOOKER. Down, March 21st {1867}.

Many thanks for your pleasant and very amusing letter. You have been treated shamefully by Etty and me, but now that I know the facts, the sentence seems to me quite clear. Nevertheless, as we have both blundered, it would be well to modify the sentence something as follows: "whilst, on the other hand, the plants which are related to those of distant continents, but have no affinity with those of the mother continent, are often very common." I forget whether you explain this circumstance, but it seems to me very mysterious (380/1. Sir Joseph Hooker wrote (March 23rd, 1867): "I see you 'smell a rat' in the matter of insular plants that are related to those of {a} distant continent being common. Yes, my beloved friend, let me make a clean breast of it. I only found it out after the lecture was in print!...I have been waiting ever since to 'think it out,' and write to you about it, coherently. I thought it best to squeeze it in, anyhow or anywhere, rather than leave so curious a fact unnoticed.")...Do always remember that nothing in the world gives us so much pleasure as seeing you here whenever you can come. I chuckle over what you say of And. Murray, but I must grapple with his book some day.

LETTER 381. TO C. LYELL. Down, October 31st {1867}.

Mr. {J.P. Mansel} Weale sent to me from Natal a small packet of dry locust dung, under 1/2 oz., with the statement that it is believed that they introduce new plants into a district. (381/1. See Volume I., Letter 221.) This statement, however, must be very doubtful. From this packet seven plants have germinated, belonging to at least two kinds of grasses. There is no error, for I dissected some of the seeds out of the middle of the pellets. It deserves notice that locusts are sometimes blown far out to sea. I caught one 370 miles from Africa, and I have heard of much greater distances. You might like to hear the following case, as it relates to a migratory bird belonging to the most wandering of all orders—viz. the woodcock. (381/2. "Origin," Edition VI., page 328.) The tarsus was firmly coated with mud, weighing when dry 9 grains, and from this the Juncus bufonius, or toad rush, germinated. By the way, the locust case verifies what I said in the "Origin," that many possible means of distribution would be hereafter discovered. I quite agree about the extreme difficulty of the distribution of land mollusca. You will have seen in the last edition of "Origin" (381/3. "Origin," Edition IV., page 429. The reference is to MM. Marten's (381/4. For Marten's read Martins' {the name is wrongly spelt in the "Origin of Species."}) experiments on seeds "in a box in the actual sea.") that my observations on the effects of sea-water have been confirmed. I still suspect that the legs of birds which roost on the ground may be an efficient means; but I was interrupted when going to make trials on this subject, and have never resumed it.

We shall be in London in the middle of latter part of November, when I shall much enjoy seeing you. Emma sends her love, and many thanks for Lady Lyell's note.

LETTER 382. TO J.D. HOOKER. Down, Wednesday {1867}.

I daresay there is a great deal of truth in your remarks on the glacial affair, but we are in a muddle, and shall never agree. I am bigoted to the last inch, and will not yield. I cannot think how you can attach so much weight to the physicists, seeing how Hopkins, Hennessey, Haughton, and Thomson have enormously disagreed about the rate of cooling of the crust; remembering Herschel's speculations about cold space (382/1. The reader will find some account of Herschel's views in Lyell's "Principles," 1872, Edition XI., Volume I., page 283.), and bearing in mind all the recent speculations on change of axis, I will maintain to the death that your case of Fernando Po and Abyssinia is worth ten times more than the belief of a dozen physicists. (382/2. See "Origin," Edition VI., page 337: "Dr. Hooker has also lately shown that several of the plants living on the upper parts of the lofty island of Fernando Po and on the neighbouring Cameroon mountains, in the Gulf of Guinea, are closely related to those in the mountains of Abyssinia, and likewise to those of temperate Europe." Darwin evidently means that such facts as these are better evidence of the gigantic periods of time occupied by evolutionary changes than the discordant conclusions of the physicists. See "Linn. Soc. Journ." Volume VII., page 180, for Hooker's general conclusions; also Hooker and Ball's "Marocco," Appendix F, page 421. For the case of Fernando Po see Hooker ("Linn. Soc. Journ." VI., 1861, page 3, where he sums up: "Hence the result of comparing Clarence Peak flora {Fernando Po} with that of the African continent is—(1) the intimate relationship with Abyssinia, of whose flora it is a member, and from which it is separated by 1800 miles of absolutely unexplored country; (2) the curious relationship with the East African islands, which are still farther off; (3) the almost total dissimilarity from the Cape flora." For Sir J.D. Hooker's general conclusions on the Cameroon plants see "Linn. Soc. Journ." VII., page 180. More recently equally striking cases have come to light: for instance, the existence of a Mediterranean genus, Adenocarpus, in the Cameroons and on Kilima Njaro, and nowhere else in Africa; and the probable migration of South African forms along the highlands from the Natal District to Abysinnia. See Hooker, "Linn. Soc. Journ." XIV., 1874, pages 144-5.) Your remarks on my regarding temperate plants and disregarding the tropical plants made me at first uncomfortable, but I soon recovered. You say that all botanists would agree that many tropical plants could not withstand a somewhat cooler climate. But I have come not to care at all for general beliefs without the special facts. I have suffered too often from this: thus I found in every book the general statement that a host of flowers were fertilised in the bud, that seeds could not withstand salt water, etc., etc. I would far more trust such graphic accounts as that by you of the mixed vegetation on the Himalayas and other such accounts. And with respect to tropical plants withstanding the slowly coming on cool period, I trust to such facts as yours (and others) about seeds of the same species from mountains and plains having acquired a slightly different climatal constitution. I know all that I have said will excite in you savage contempt towards me. Do not answer this rigmarole, but attack me to your heart's content, and to that of mine, whenever you can come here, and may it be soon.

LETTER 383. J.D. HOOKER TO CHARLES DARWIN. Kew, 1870.

(383/1. The following extract from a letter of Sir J.D. Hooker shows the tables reversed between the correspondents.)

Grove is disgusted at your being disquieted about W. Thomson. Tell George from me not to sit upon you with his mathematics. When I threatened your tropical cooling views with the facts of the physicists, you snubbed me and the facts sweetly, over and over again; and now, because a scarecrow of x+y has been raised on the selfsame facts, you boo-boo. Take another dose of Huxley's penultimate G. S. Address, and send George back to college. (383/2. Huxley's Anniversary Address to the Geological Society, 1869 ("Collected Essays," VIII., page 305). This is a criticism of Lord Kelvin's paper "On Geological Time" ("Trans. Geolog. Soc. Glasgow," III.). At page 336 Mr. Huxley deals with Lord Kelvin's "third line of argument, based on the temperature of the interior of the earth." This was no doubt the point most disturbing to Mr. Darwin, since it led Lord Kelvin to ask (as quoted by Huxley), "Are modern geologists prepared to say that all life was killed off the earth 50,000, 100,000, or 200,000 years ago?" Mr. Huxley, after criticising Lord Kelvin's data and conclusion, gives his conviction that the case against Geology has broken down. With regard to evolution, Huxley (page 328) ingeniously points out a case of circular reasoning. "But it may be said that it is biology, and not geology, which asks for so much time—that the succession of life demands vast intervals; but this appears to me to be reasoning in a circle. Biology takes her time from geology. The only reason we have for believing in the slow rate of the change in living forms is the fact that they persist through a series of deposits which, geology informs us, have taken a long while to make. If the geological clock is wrong, all the naturalist will have to do is to modify his notions of the rapidity of change accordingly.")

LETTER 384. TO J.D. HOOKER. February 3rd {1868}.

I am now reading Miquel on "Flora of Japan" (384/1. Miquel, "Flore du Japon": "Archives Neerlandaises" ii., 1867.), and like it: it is rather a relief to me (though, of course, not new to you) to find so very much in common with Asia. I wonder if A. Murray's (384/2. "Geographical Distribution of Mammals," by Andrew Murray, 1866. See Chapter V., page 47. See Letter 379.) notion can be correct, that a {profound} arm of the sea penetrated the west coast of N. America, and prevented the Asiatico-Japan element colonising that side of the continent so much as the eastern side; or will climate suffice? I shall to the day of my death keep up my full interest in Geographical Distribution, but I doubt whether I shall ever have strength to come in any fuller detail than in the "Origin" to this grand subject. In fact, I do not suppose any man could master so comprehensive a subject as it now has become, if all kingdoms of nature are included. I have read Murray's book, and am disappointed—though, as you said, here and there clever thoughts occur. How strange it is, that his view not affording the least explanation of the innumerable adaptations everywhere to be seen apparently does not in the least trouble his mind. One of the most curious cases which he adduces seems to me to be the two allied fresh-water, highly peculiar porpoises in the Ganges and Indus; and the more distantly allied form of the Amazons. Do you remember his explanation of an arm of the sea becoming cut off, like the Caspian, converted into fresh-water, and then divided into two lakes (by upheaval), giving rise to two great rivers. But no light is thus thrown on the affinity of the Amazon form. I now find from Flower's paper (384/3. "Zoolog. Trans." VI., 1869, page 115. The toothed whales are divided into the Physeteridae, the Delphinidae, and the Platanistidae, which latter is placed between the two other families, and is divided into the sub-families Iniinae and Platanistinae.) that these fresh-water porpoises form two sub-families, making an extremely isolated and intermediate, very small family. Hence to us they are clearly remnants of a large group; and I cannot doubt we here have a good instance precisely like that of ganoid fishes, of a large ancient marine group, preserved exclusively in fresh-water, where there has been less competition, and consequently little modification. (384/4. See Volume I., Letter 95.) What a grand fact that is which Miquel gives of the beech not extending beyond the Caucasus, and then reappearing in Japan, like your Himalayan Pinus, and the cedar of Lebanon. (384/5. For Pinus read Deodar. The essential identity of the deodar and the cedar of Lebanon was pointed out in Hooker's "Himalayan Journals" in 1854 (Volume I., page 257.n). In the "Nat. History Review," January, 1862, the question is more fully dealt with by him, and the distribution discussed. The nearest point at which cedars occur is the Bulgar-dagh chain of Taurus—250 miles from Lebanon. Under the name of Cedrus atlantica the tree occurs in mass on the borders of Tunis, and as Deodar it first appears to the east in the cedar forests of Afghanistan. Sir J.D. Hooker supposes that, during a period of greater cold, the cedars on the Taurus and on Lebanon lived many thousand feet nearer the sea-level, and spread much farther to the east, meeting similar belts of trees descending and spreading westward from Afghanistan along the Persian mountains.) I know of nothing that gives one such an idea of the recent mutations in the surface of the land as these living "outlyers." In the geological sense we must, I suppose, admit that every yard of land has been successively covered with a beech forest between the Caucasus and Japan!

I have not yet seen (for I have not sent to the station) Falconer's works. When you say that you sigh to think how poor your reprinted memoirs would appear, on my soul I should like to shake you till your bones rattled for talking such nonsense. Do you sigh over the "Insular Floras," the Introduction to New Zealand Flora, to Australia, your Arctic Flora, and dear Galapagos, etc., etc., etc.? In imagination I am grinding my teeth and choking you till I put sense into you. Farewell. I have amused myself by writing an audaciously long letter. By the way, we heard yesterday that George has won the second Smith's Prize, which I am excessively glad of, as the Second Wrangler by no means always succeeds. The examination consists exclusively of {the} most difficult subjects, which such men as Stokes, Cayley, and Adams can set.

LETTER 385. A.R. WALLACE TO CHARLES DARWIN. March 8th, 1868.

...While writing a few pages on the northern alpine forms of plants on the Java mountains I wanted a few cases to refer to like Teneriffe, where there are no northern forms and scarcely any alpine. I expected the volcanoes of Hawaii would be a good case, and asked Dr. Seemann about them. It seems a man has lately published a list of Hawaiian plants, and the mountains swarm with European alpine genera and some species! (385/1. "This turns out to be inaccurate, or greatly exaggerated. There are no true alpines, and the European genera are comparatively few. See my 'Island Life,' page 323."—A.R.W.) Is not this most extraordinary, and a puzzler? They are, I believe, truly oceanic islands, in the absence of mammals and the extreme poverty of birds and insects, and they are within the Tropics.

Will not that be a hard nut for you when you come to treat in detail on geographical distribution? I enclose Seemann's note, which please return when you have copied the list, if of any use to you.

LETTER 386. TO J.D. HOOKER. Down, February 21st {1870}.

I read yesterday the notes on Round Island (386/1. In Wallace's "Island Life," page 410, Round Island is described as an islet "only about a mile across, and situated about fourteen miles north-east of Mauritius." Wallace mentions a snake, a python belonging to the peculiar and distinct genus Casarea, as found on Round Island, and nowhere else in the world. The palm Latania Loddigesii is quoted by Wallace as "confined to Round Island and two other adjacent islets." See Baker's "Flora of the Mauritius and the Seychelles." Mr. Wallace says that, judging from the soundings, Round Island was connected with Mauritius, and that when it was "first separated {it} would have been both much larger and much nearer the main island.") which I owe to you. Was there ever such an enigma? If, in the course of a week or two, you can find time to let me hear what you think, I should very much like to hear: or we hope to be at Erasmus' on March 4th for a week. Would there be any chance of your coming to luncheon then? What a case it is. Palms, screw-pines, four snakes—not one being in main island—lizards, insects, and not one land bird. But, above everything, such a proportion of individual monocotyledons! The conditions do not seem very different from the Tuff Galapagos Island, but, as far as I remember, very few monocotyledons there. Then, again, the island seems to have been elevated. I wonder much whether it stands out in the line of any oceanic current, which does not so forcibly strike the main island? But why, oh, why should so many monocotyledons have come there? or why should they have survived there more than on the main island, if once connected? So, again, I cannot conceive that four snakes should have become extinct in Mauritius and survived on Round Island. For a moment I thought that Mauritius might be the newer island, but the enormous degradation which the outer ring of rocks has undergone flatly contradicts this, and the marine remains on the summit of Round Island indicate the island to be comparatively new—unless, indeed, they are fossil and extinct marine remains. Do tell me what you think. There never was such an enigma. I rather lean to separate immigration, with, of course, subsequent modification; some forms, of course, also coming from Mauritius. Speaking of Mauritius reminds me that I was so much pleased the day before yesterday by reading a review of a book on the geology of St. Helena, by an officer who knew nothing of my hurried observations, but confirms nearly all that I have said on the general structure of the island, and on its marvellous denudation. The geology of that island was like a novel.

LETTER 387. TO A. BLYTT. Down, March 28th, 1876.

(387/1. The following refers to Blytt's "Essay on the Immigration of the Norwegian Flora during Alternating Rainy and Dry Periods," Christiania, 1876.)

I thank you sincerely for your kindness in having sent me your work on the "Immigration of the Norwegian Flora," which has interested me in the highest degree. Your view, supported as it is by various facts, appears to me the most important contribution towards understanding the present distribution of plants, which has appeared since Forbes' essay on the effects of the Glacial Period.

LETTER 388. TO AUG. FOREL. Down, June 19th, 1876.

I hope you will allow me to suggest an observation, should any opportunity occur, on a point which has interested me for many years—viz., how do the coleoptera which inhabit the nests of ants colonise a new nest? Mr. Wallace, in reference to the presence of such coleoptera in Madeira, suggests that their ova may be attached to the winged female ants, and that these are occasionally blown across the ocean to the island. It would be very interesting to discover whether the ova are adhesive, and whether the female coleoptera are guided by instinct to attach them to the female ants (388/1. Dr. Sharp is good enough to tell us that he is not aware of any such adaptation. Broadly speaking, the distribution of the nest-inhabiting beetles is due to co-migration with the ants, though in some cases the ants transport the beetles. Sitaris and Meloe are beetles which live "at the expense of bees of the genus Anthophora." The eggs are laid not in but near the bees' nest; in the early stage the larva is active and has the instinct to seize any hairy object near it, and in this way they are carried by the Anthophora to the nest. Dr. Sharp states that no such preliminary stage is known in the ant's-nest beetles. For an account of Sitaris and Meloe, see Sharp's "Insects," II., page 272.); or whether the larvae pass through an early stage, as with Sitaris or Meloe, or cling to the bodies of the females. This note obviously requires no answer. I trust that you continue your most interesting investigations on ants.

(PLATE: MR. A.R. WALLACE, 1878. From a photograph by Maull & Fox.)

LETTER 389. TO A.R. WALLACE.

(389/1. Published in "Life and Letters," III., page 230.)

(389/2. The following five letters refer to Mr. Wallace's "Geographical Distribution of Animals," 1876.)

{Hopedene} (389/3. Mr. Hensleigh Wedgwood's house in Surrey.), June 5th, 1876.

I must have the pleasure of expressing to you my unbounded admiration of your book (389/4. "Geographical Distribution," 1876.), though I have read only to page 184—my object having been to do as little as possible while resting. I feel sure that you have laid a broad and safe foundation for all future work on Distribution. How interesting it will be to see hereafter plants treated in strict relation to your views; and then all insects, pulmonate molluscs and fresh-water fishes, in greater detail than I suppose you have given to these lower animals. The point which has interested me most, but I do not say the most valuable point, is your protest against sinking imaginary continents in a quite reckless manner, as was stated by

Forbes, followed, alas, by Hooker, and caricatured by Wollaston and {Andrew} Murray! By the way, the main impression that the latter author has left on my mind is his utter want of all scientific judgment. I have lifted up my voice against the above view with no avail, but I have no doubt that you will succeed, owing to your new arguments and the coloured chart. Of a special value, as it seems to me, is the conclusion that we must determine the areas, chiefly by the nature of the mammals. When I worked many years ago on this subject, I doubted much whether the now-called Palaearctic and Nearctic regions ought to be separated; and I determined if I made another region that it should be Madagascar. I have, therefore, been able to appreciate your evidence on these points. What progress Palaeontology has made during the last twenty years! but if it advances at the same rate in the future, our views on the migration and birthplace of the various groups will, I fear, be greatly altered. I cannot feel quite easy about the Glacial period, and the extinction of large mammals, but I must hope that you are right. I think you will have to modify your belief about the difficulty of dispersal of land molluscs; I was interrupted when beginning to experimentise on the just hatched young adhering to the feet of ground-roosting birds. I differ on one other point—viz. in the belief that there must have existed a Tertiary Antarctic continent, from which various forms radiated to the southern extremities of our present continents. But I could go on scribbling forever. You have written, as I believe, a grand and memorable work, which will last for years as the foundation for all future treatises on Geographical Distribution.

P.S.—You have paid me the highest conceivable compliment, by what you say of your work in relation to my chapters on distribution in the "Origin," and I heartily thank you for it.

LETTER 390. FROM A.R. WALLACE TO CHARLES DARWIN. The Dell, Grays, Essex, June 7th, 1876.

Many thanks for your very kind letter. So few people will read my book at all regularly, that a criticism from one who does so will be very welcome. If, as I suppose, it is only to page 184 of Volume I. that you have read, you cannot yet quite see my conclusions on the points you refer to (land molluscs and Antarctic continent). My own conclusion fluctuated during the progress of the book, and I have, I know, occasionally used expressions (the relics of earlier ideas) which are not quite consistent with what I say further on. I am positively against any Southern continent as uniting South America with Australia or New Zealand, as you will see at Volume I., pages 398-403, and 459-66. My general conclusions as to distribution of land mollusca are at Volume II., pages 522-9. (390/1. "Geographical Distribution" II., pages 524, 525. Mr. Wallace points out that "hardly a small island on the globe but has some land-shells peculiar to it"—and he goes so far as to say that probably air-breathing mollusca have been chiefly distributed by air- or water-carriage, rather than by voluntary dispersal on the land.) When you have read these passages, and looked at the general facts which lead to them, I shall be glad to hear if you still differ from me.

Though, of course, present results as to the origin and migrations of genera of mammals will have to be modified owing to new discoveries, I cannot help thinking that much will remain unaffected, because in all geographical and geological discoveries the great outlines are soon reached, the details alone remain to be modified. I also think much of the geological evidence is now so accordant with, and explanatory of, Geographical Distribution, that it is prima facie correct in outline. Nevertheless, such vast masses of new facts will come out in the next few years that I quite dread the labour of incorporating them in a new edition.

I hope your health is improved; and when, quite at your leisure, you have waded through my book, I trust you will again let me have a few lines of friendly criticism and advice.

LETTER 391. TO A.R. WALLACE. Down, June 17th, 1876.

I have now finished the whole of Volume I., with the same interest and admiration as before; and I am convinced that my judgment was right and that it is a memorable book, the basis of all future work on the subject. I have nothing particular to say, but perhaps you would like to hear my impressions on two or three points. Nothing has struck me more than the admirable and convincing manner in which you treat Java. To allude to a very trifling point, it is capital about the unadorned head of the Argus-pheasant. (391/1. See "Descent of Man," Edition I., pages 90 and 143, for drawings of the Argus pheasant and its markings. The ocelli on the wing feathers were favourite objects of Mr. Darwin, and sometimes formed the subject of the little lectures which on rare occasions he would give to a visitor interested in Natural History. In Mr. Wallace's book the meaning of the ocelli comes in by the way, in the explanation of Plate IX., "A Malayan Forest with some of its peculiar Birds." Mr. Wallace (volume i., page 340) points out that the head of the Argus pheasant is, during the display of the wings, concealed from the view of a spectator in front, and this accounts for the absence of bright colour on the head—a most unusual point in a pheasant. The case is described as a "remarkable confirmation of Mr. Darwin's views, that gaily coloured plumes are developed in the male bird for the purpose of attractive display." For the difference of opinion between the two naturalists on the broad question of coloration see "Life and Letters," III., page 123. See Letters 440-453.) How plain a thing is, when it is once pointed out! What a wonderful case is that of Celebes: I am glad that you have slightly modified your views with respect to Africa. (391/2. "I think this must refer to the following passage in 'Geog. Dist. of Animals,' Volume I., pages 286-7. 'At this period (Miocene) Madagascar was no doubt united with Africa, and helped to form a great southern continent which must at one time have extended eastward as far as Southern India and Ceylon; and over the whole of this the lemurine type no doubt prevailed.' At the time this was written I had not paid so much attention to islands, and in my "Island Life" I have given ample reasons for my belief that the evidence of extinct animals does not require any direct connection between Southern India and Africa."—Note by Mr. Wallace.) And this leads me to say that I cannot swallow the so-called continent of Lemuria—i.e., the direct connection of Africa and Ceylon. (391/3. See "Geographical Distribution," I., page 76. The name Lemuria was proposed by Mr. Sclater for an imaginary submerged continent extending from Madagascar to Ceylon and Sumatra. Mr. Wallace points out that if we confine ourselves to facts Lemuria is reduced to Madagascar, which he makes a subdivision of the Ethiopian Region.) The facts do not seem to me many and strong enough to justify so immense a change of level. Moreover, Mauritius and the other islands appear to me oceanic in character. But do not suppose that I place my judgment on this subject on a level with yours. A wonderfully good paper was published about a year ago on India, in the "Geological Journal," I think by Blanford. (391/4. H.F. Blanford "On the Age and

Correlations of the Plant-bearing Series of India and the Former Existence of an Indo-Oceanic Continent" ("Quart. Journ. Geol. Soc." XXXI., 1875, page 519). The name Gondwana-Land was subsequently suggested by Professor Suess for this Indo-Oceanic continent. Since the publication of Blanford's paper, much literature has appeared dealing with the evidence furnished by fossil plants, etc., in favour of the existence of a vast southern continent.) Ramsay agreed with me that it was one of the best published for a long time. The author shows that India has been a continent with enormous fresh-water lakes, from the Permian period to the present day. If I remember right, he believes in a former connection with S. Africa.

I am sure that I read, some twenty to thirty years ago in a French journal, an account of teeth of Mastodon found in Timor; but the statement may have been an error. (391/5. In a letter to Falconer (Letter 155), January 5th, 1863, Darwin refers to the supposed occurrence of Mastodon as having been "smashed" by Falconer.)

With respect to what you say about the colonising of New Zealand, I somewhere have an account of a frog frozen in the ice of a Swiss glacier, and which revived when thawed. I may add that there is an Indian toad which can resist salt-water and haunts the seaside. Nothing ever astonished me more than the case of the Galaxias; but it does not seem known whether it may not be a migratory fish like the salmon. (391/6. The only genus of the Galaxidae, a family of fresh-water fishes occurring in New Zealand, Tasmania, and Tierra del Fuego, ranging north as far as Queensland and Chile (Wallace's "Geographical Distribution," II., page 448).)

LETTER 392. TO A.R. WALLACE. Down, June 25th, 1876.

I have been able to read rather more quickly of late, and have finished your book. I have not much to say. Your careful account of the temperate parts of South America interested me much, and all the more from knowing something of the country. I like also much the general remarks towards the end of the volume on the land molluscs. Now for a few criticisms.

Page 122. (392/1. The pages refer to Volume II. of Wallace's "Geographical Distribution.")—I am surprised at your saying that "during the whole Tertiary period North America was zoologically far more strongly contrasted with South America than it is now." But we know hardly anything of the latter except during the Pliocene period; and then the mastodon, horse, several great edentata, etc., etc., were common to the north and south. If you are right, I erred greatly in my "Journal," where I insisted on the former close connection between the two.

Page 252 and elsewhere.—I agree thoroughly with the general principle that a great area with many competing forms is necessary for much and high development; but do you not extend this principle too far—I should say much too far, considering how often several species of the same genus have been developed on very small islands?

Page 265.—You say that the Sittidae extend to Madagascar, but there is no number in the tabular heading. {The number (4) was erroneously omitted.—A.R.W.}

Page 359.—Rhinochetus is entered in the tabular heading under No. 3 of the neotropical subregions. {An error: should have been the Australian.—A.R.W.}

Reviewers think it necessary to find some fault; and if I were to review you, the sole point which I should blame is your not giving very numerous references. These would save whoever follows you great labour. Occasionally I wished myself to know the authority for certain statements, and whether you or somebody else had originated certain subordinate views. Take the case of a man who had collected largely on some island, for instance St. Helena, and who wished to work out the geographical relations of his collections: he would, I think, feel very blank at not finding in your work precise references to all that had been written on St. Helena. I hope you will not think me a confoundedly disagreeable fellow.

I may mention a capital essay which I received a few months ago from Axel Blytt (392/2. Axel Blytt, "Essay on the Immigration of the Norwegian Flora." Christiania, 1876. See Letter 387.) on the distribution of the plants of Scandinavia; showing the high probability of there having been secular periods alternately wet and dry, and of the important part which they have played in distribution.

I wrote to Forel (392/3. See Letter 388.), who is always at work on ants, and told him your views about the dispersal of the blind coleoptera, and asked him to observe.

I spoke to Hooker about your book, and feel sure that he would like nothing better than to consider the distribution of plants in relation to your views; but he seemed to doubt whether he should ever have time.

And now I have done my jottings, and once again congratulate you on having brought out so grand a work. I have been a little disappointed at the review in "Nature." (392/4. June 22nd, 1876, pages 165 et seq.)

LETTER 393. A.R. WALLACE TO CHARLES DARWIN. Rosehill, Dorking, July 23rd, 1876.

I should have replied sooner to your last kind and interesting letters, but they reached me in the midst of my packing previous to removal here, and I have only just now got my books and papers in a get-at-able state.

And first, many thanks for your close observation in detecting the two absurd mistakes in the tabular headings.

As to the former greater distinction of the North and South American faunas, I think I am right. The edentata being proved (as I hold) to have been mere temporary migrants into North America in the post-Pliocene epoch, form no part of its Tertiary fauna. Yet in South America they were so enormously developed in the Pliocene epoch that we know, if there is any such thing as evolution, etc., that strange ancestral forms must have preceded them in Miocene times.

Mastodon, on the other hand, represented by one or two species only, appears to have been a late immigrant into South America from the north.

The immense development of ungulates (in varied families, genera, and species) in North America during the whole Tertiary epoch is, however, the great feature which assimilates it to Europe, and contrasts it with South America. True camels, hosts of hog-like animals, true rhinoceroses, and hosts of ancestral horses, all bring the North American {fauna} much nearer to the Old World than it is now. Even the horse, represented in all South America by Equus only, was probably a temporary immigrant from the north.

As to extending too far the principle (yours) of the necessity of comparatively large areas for the development of varied faunas, I may have done so, but I think not. There is, I think, every probability that most islands, etc., where a varied fauna now exists, have been once more extensive—eg., New Zealand, Madagascar: where there is no such evidence (e.g., Galapagos), the fauna is very restricted.

Lastly, as to want of references: I confess the justice of your criticism; but I am dreadfully unsystematic. It is my first large work involving much of the labour of others. I began with the intention of writing a comparatively short sketch, enlarged it, and added to it bit by bit; remodelled the tables, the headings, and almost everything else, more than once, and got my materials in such confusion that it is a wonder it has not turned out far more crooked and confused than it is. I, no doubt, ought to have given references; but in many cases I found the information so small and scattered, and so much had to be combined and condensed from conflicting authorities, that I hardly knew how to refer to them or where to leave off. Had I referred to all authors consulted for every fact, I should have greatly increased the bulk of the book, while a large portion of the references would be valueless in a few years, owing to later and better authorities. My experience of referring to references has generally been most unsatisfactory. One finds, nine times out of ten, the fact is stated, and nothing more; or a reference to some third work not at hand!

I wish I could get into the habit of giving chapter and verse for every fact and extract; but I am too lazy, and generally in a hurry, having to consult books against time, when in London for a day.

However, I will try to do something to mend this matter, should I have to prepare another edition.

I return you Forel's letter. It does not advance the question much; neither do I think it likely that even the complete observation he thinks necessary would be of much use, because it may well be that the ova, or larvae, or imagos of the beetles are not carried systematically by the ants, but only occasionally, owing to some exceptional circumstances. This might produce a great effect in distribution, yet be so rare as never to come under observation.

Several of your remarks in previous letters I shall carefully consider. I know that, compared with the extent of the subject, my book is in many parts crude and ill-considered; but I thought, and still think, it better to make some generalisations wherever possible, as I am not at all afraid of having to alter my views in many points of detail. I was so overwhelmed with zoological details, that I never went through the Geological Society's "Journal" as I ought to have done, and as I mean to do before writing more on the subject.

LETTER 394. TO F. BUCHANAN WHITE.

(394/1. "Written in acknowledgment of a copy of a paper (published by me in the "Proceedings of the Zoological Society") on the Hemiptera of St. Helena, but discussing the origin of the whole fauna and flora of that island."—F.B.W.)

Down, September 23rd. {1878}.

I have now read your paper, and I hope that you will not think me presumptuous in writing another line to say how excellent it seems to me. I believe that you have largely solved the problem of the affinities of the inhabitants of this most interesting little island, and this is a delightful triumph.

LETTER 395. TO J.D. HOOKER. Down, July 22nd {1879}.

I have just read Ball's Essay. (395/1. The late John Ball's lecture "On the Origin of the Flora of the Alps" in the "Proceedings of the R. Geogr. Soc." 1879. Ball argues (page 18) that "during ancient Palaeozoic times, before the deposition of the Coal-measures, the atmosphere contained twenty times as much carbonic acid gas and considerably less oxygen than it does at present." He further assumes that in such an atmosphere the percentage of CO2 in the higher mountains would be excessively different from that at the sea-level, and appends the result of calculations which gives the amount of CO_2 at the sea-level as 100 per 10,000 by weight, at a height of 10,000 feet as 12.5 per 10,000. Darwin understands him to mean that the Vascular Cryptogams and Gymnosperms could stand the sea-level atmosphere, whereas the Angiosperms would only be able to exist in the higher regions where the percentage of CO_2 was small. It is not clear to us that Ball relies so largely on the condition of the atmosphere as regards CO_2. If he does he is clearly in error, for everything we know of assimilation points to the conclusion that 100 per 10,000 (1 per cent.) is by no means a hurtful amount of CO_2, and that it would lead to an especially vigorous assimilation. Mountain plants would be more likely to descend to the plains to share in the rich feast than ascend to higher regions to avoid it. Ball draws attention to the imperfection of our plant records as regards the floras of mountain regions. It is, he thinks, conceivable that there existed a vegetation on the Carboniferous mountains of which no traces have been preserved in the rocks. See "Fossil Plants as Tests of Climate," page 40, A.C. Seward, 1892.

Since the first part of this note was written, a paper has been read (May 29th, 1902) by Dr. H.T. Brown and Mr. F. Escombe, before the Royal Society on "The Influence of varying amounts of Carbon Dioxide in the Air on the Photosynthetic Process of Leaves, and on the Mode of Growth of Plants." The author's experiments included the cultivation of several dicotyledonous plants in an atmosphere containing in one case 180 to 200 times the normal amount of CO_2, and in another between three and four times the normal amount. The general results were practically identical in the two sets of experiments. "All the species of flowering plants, which have been the subject of experiment, appear to be accurately 'tuned' to an atmospheric environment of three parts of CO_2 per 10,000, and the response which they make to slight increases in this amount are in a direction altogether unfavourable to their growth and reproduction." The assimilation of carbon increases with the increase in the partial pressure of the CO_2. But there seems to be a disturbance in metabolism, and the plants fail to take advantage of the increased supply of CO_2. The authors say:—"All we are justified in concluding is, that if such atmospheric variations have occurred since the advent of flowering plants, they must have taken place so slowly as never to outrun the possible adaptation of the plants to their changing conditions."

Prof. Farmer and Mr. S.E. Chandler gave an account, at the same meeting of the Royal Society, of their work "On the Influence of an Excess of Carbon Dioxide in the Air on the Form and Internal Structure of Plants." The results obtained were described as differing in a remarkable way from those previously recorded by Teodoresco ("Rev. Gen. Botanique," II., 1899

It is hoped that Dr. Horace Brown and Mr. Escombe will extend their experiments to Vascular Cryptogams, and thus obtain evidence bearing more directly upon the question of an increased amount of CO_2 in the atmosphere of the Coal-period forests.) It is pretty bold. The rapid development as far as we can judge of all the higher plants within recent geological times is an abominable mystery. Certainly it would be a great step if we could believe that the higher plants at first could live only at a high level; but until it is experimentally {proved} that Cycadeae, ferns, etc., can withstand much more carbonic acid than the higher plants, the hypothesis seems to me far too rash. Saporta believes that there was an astonishingly rapid development of the high plants, as soon {as} flower-frequenting insects were developed and favoured intercrossing. I should like to see this whole problem solved. I have fancied that perhaps there was during long ages a small isolated continent in the S. Hemisphere which served as the birthplace of the higher plants—but this is a wretchedly poor conjecture. It is odd that Ball does not allude to the obvious fact that there must have been alpine plants before the Glacial period, many of which would have returned to the mountains after the Glacial period, when the climate again became warm. I always accounted to myself in this manner for the gentians, etc.

Ball ought also to have considered the alpine insects common to the Arctic regions. I do not know how it may be with you, but my faith in the glacial migration is not at all shaken.

LETTER 396. A.R. WALLACE TO CHARLES DARWIN.

(396/1. This letter is in reply to Mr. Darwin's criticisms on Mr. Wallace's "Island Life," 1880.)

Pen-y-Bryn, St. Peter's Road, Croydon, November 8th, 1880.

Many thanks for your kind remarks and notes on my book. Several of the latter will be of use to me if I have to prepare a second edition, which I am not so sure of as you seem to be.

1. In your remark as to the doubtfulness of paucity of fossils being due to coldness of water, I think you overlook that I am speaking only of water in the latitude of the Alps, in Miocene and Eocene times, when icebergs and glaciers temporarily descended into an otherwise warm sea; my theory being that there was no Glacial epoch at that time, but merely a local and temporary descent of the snow-line and glaciers owing to high excentricity and winter in aphelion.

2. I cannot see the difficulty about the cessation of the Glacial period.

Between the Miocene and the Pleistocene periods geographical changes occurred which rendered a true Glacial period possible with high excentricity. When the high excentricity passed away the Glacial epoch also passed away in the temperate zone; but it persists in the arctic zone, where, during the Miocene, there were mild climates, and this is due to the persistence of the changed geographical conditions. The present arctic climate is itself a comparatively new and abnormal state of things, due to geographical modification.

As to "epoch" and "period," I use them as synonyms to avoid repeating the same word.

3. Rate of deposition and geological time. Here no doubt I may have gone to an extreme, but my "28 million years" may be anything under 100 millions, as I state. There is an enormous difference between mean and maximum denudation and deposition. In the case of the great faults the upheaval along a given line would itself facilitate the denudation (whether sub-aerial or marine) of the upheaved portion at a rate perhaps a hundred times above the average, just as valleys have been denuded perhaps a hundred times faster than plains and plateaux. So local subsidence might itself lead to very rapid deposition. Suppose a portion of the Gulf of Mexico, near the mouths of the Mississippi, were to subside for a few thousand years, it might receive the greater portion of the

sediment from the whole Mississippi valley, and thus form strata at a very rapid rate.

4. You quote the Pampas thistles, etc., against my statement of the importance of preoccupation. But I am referring especially to St. Helena, and to plants naturally introduced from the adjacent continents. Surely if a certain number of African plants reached the island, and became modified into a complete adaptation to its climatic conditions, they would hardly be expelled by other African plants arriving subsequently. They might be so, conceivably, but it does not seem probable. The cases of the Pampas, New Zealand, Tahiti, etc., are very different, where highly developed aggressive plants have been artificially introduced. Under nature it is these very aggressive species that would first reach any island in their vicinity, and, being adapted to the island and colonising it thoroughly, would then hold their own against other plants from the same country, mostly less aggressive in character.

I have not explained this so fully as I should have done in the book. Your criticism is therefore useful.

5. My Chapter XXIII. is no doubt very speculative, and I cannot wonder at your hesitating at accepting my views. To me, however, your theory of hosts of existing species migrating over the tropical lowlands from the N. temperate to the S. temperate zone appears more speculative and more improbable. For where could the rich lowland equatorial flora have existed during a period of general refrigeration sufficient for this? and what became of the wonderfully rich Cape flora, which, if the temperature of tropical Africa had been so recently lowered, would certainly have spread northwards, and on the return of the heat could hardly have been driven back into the sharply defined and very restricted area in which it now exists.

As to the migration of plants from mountain to mountain not being so probable as to remote islands, I think that is fully counterbalanced by two considerations:—

a. The area and abundance of the mountain stations along such a range as the Andes are immensely greater than those of the islands in the N. Atlantic, for example.

b. The temporary occupation of mountain stations by migrating plants (which I think I have shown to be probable) renders time a much more important element in increasing the number and variety of the plants so dispersed than in the case of islands, where the flora soon acquires a fixed and endemic character, and where the number of species is necessarily limited.

No doubt direct evidence of seeds being carried great distances through the air is wanted, but I am afraid can hardly be obtained. Yet I feel the greatest confidence that they are so carried. Take, for instance, the two peculiar orchids of the Azores (Habenaria sp.) What other mode of transit is conceivable? The whole subject is one of great difficulty, but I hope my chapter may call attention to a hitherto neglected factor in the distribution of plants.

Your references to the Mauritius literature are very interesting, and will be useful to me; and I again thank you for your valuable remarks.

LETTER 397. TO J.D. HOOKER.

(397/1. The following letters were written to Sir J.D. Hooker when he was preparing his Address as President of the Geographical Section of the British Association at its fiftieth meeting, at York. The second letter (August 12th) refers to an earlier letter of August 6th, published in "Life and Letters," III., page 246.)

4, Bryanston Street, W., Saturday, 26th {February, 1881}.

I should think that you might make a very interesting address on Geographical Distribution. Could you give a little history of the subject. I, for one, should like to read such history in petto; but I can see one very great difficulty—that you yourself ought to figure most prominently in it; and this you would not do, for you are just the man to treat yourself in a dishonourable manner. I should very much like to see you discuss some of Wallace's views, especially his ignoring the all-powerful effects of the Glacial period with respect to alpine plants. (397/2. "Having been kindly permitted by Mr. Francis Darwin to read this letter, I wish to explain that the above statement applies only to my rejection of Darwin's view that the presence of arctic and north temperate plants in the SOUTHERN HEMISPHERE was brought about by the lowering of the temperature of the tropical regions during the Glacial period, so that even 'the lowlands of these great continents were everywhere tenanted under the equator by a considerable number of temperate forms ("Origin of Species," Edition VI., page 338). My own views are fully explained in Chapter XXIII. of my "Island Life," published in 1880. I quite accept all that Darwin, Hooker, and Asa Gray have written about the effect of the Glacial epoch in bringing about the present distribution of alpine and arctic plants in the NORTHERN HEMISPHERE."—Note by Mr. Wallace.) I do not know what you think, but it appears to me that he exaggerates enormously the influence of debacles or slips and new surface of soil being exposed for the reception of wind-blown seeds. What kinds of seeds have the plants which are common to the distant mountain-summits in Africa? Wallace lately wrote to me about the mountain plants of Madagascar being the same with those on mountains in Africa, and seemed to think it proved dispersal by the wind, without apparently having inquired what sorts of seeds the plants bore. (397/3. The affinity with the flora of the Eastern African islands was long ago pointed out by Sir J.D. Hooker, "Linn. Soc. Journal," VI., 1861, page 3. Speaking of the plants of Clarence Peak in Fernando Po, he says, "The next affinity is with Mauritius, Bourbon, and Madagascar: of the whole 76 species, 16 inhabit these places and 8 more are closely allied to plants from there. Three temperate species are peculiar to Clarence Peak and the East African islands..." The facts to which Mr. Wallace called Darwin's attention are given by Mr. J.G. Baker in "Nature," December 9th, 1880, page 125. He mentions the Madagascar Viola, which occurs elsewhere only at 7,000 feet in the Cameroons, at 10,000 feet in Fernando Po and in the Abyssinian mountains; and the same thing is true of the Madagascar Geranium. In Mr. Wallace's letter to Darwin, dated January 1st, 1881, he evidently uses the expression "passing through the air" in contradistinction to the migration of a species by gradual extension of its area on land. "Through the air" would moreover include occasional modes of transport other than simple carriage by wind: e.g., the seeds might be carried by birds, either attached to the feathers or to the mud on their feet, or in their crops or intestines.)

I suppose it would be travelling too far (though for the geographical section the discussion ought to be far-reaching), but I should like to see the European or northern element in the Cape of Good Hope flora discussed. I cannot swallow Wallace's view that European plants travelled down the Andes, tenanted the hypothetical Antarctic continent (in which I quite believe), and thence spread to South Australia and the Cape of Good Hope.

Moseley told me not long ago that he proposed to search at Kerguelen Land the coal beds most carefully, and was absolutely forbidden to do so by Sir W. Thomson, who said that he would undertake the work, and he never once visited them. This puts me in a passion. I hope that you will keep to your intention and make an address on distribution. Though I differ so much from Wallace, his "Island Life" seems to me a wonderful book.

Farewell. I do hope that you may have a most prosperous journey. Give my kindest remembrances to Asa Gray.

LETTER 398. TO J.D. HOOKER. Down, August 12th, 1881.

...I think that I must have expressed myself badly about Humboldt. I should have said that he was more remarkable for his astounding knowledge than for originality. I have always looked at him as, in fact, the founder of the geographical distribution of organisms. I thought that I had read that extinct fossil plants belonging to Australian forms had lately been found in Australia, and all such cases seem to me very interesting, as bearing on development.

I have been so astonished at the apparently sudden coming in of the higher phanerogams, that I have sometimes fancied that development might have slowly gone on for an immense period in some isolated continent or large island, perhaps near the South Pole. I poured out my idle thoughts in writing, as if I had been talking with you.

No fact has so interested me for a heap of years as your case of the plants on the equatorial mountains of Africa; and Wallace tells me that some one (Baker?) has described analogous cases on the mountains of Madagascar (398/1. See Letter 397, note.)...I think that you ought to allude to these cases.

I most fully agree that no problem is more interesting than that of the temperate forms in the southern hemisphere, common to the north. I remember writing about this after Wallace's book appeared, and hoping that you would take it up. The frequency with which the drainage from the land passes through mountain-chains seems to indicate some general law—viz., the successive formation of cracks and lines of elevation between the nearest ocean and the already upraised land; but that is too big a subject for a note.

I doubt whether any insects can be shown with any probability to have been flower feeders before the middle of the Secondary period. Several of the asserted cases have broken down.

Your long letter has stirred many pleasant memories of long past days, when we had many a discussion and many a good fight.

LETTER 399. TO J.D. HOOKER. Down, August 21st, 1881.

I cannot aid you much, or at all. I should think that no one could have thought on the modification of species without thinking of representative species. But I feel sure that no discussion of any importance had been published on this subject before the "Origin," for if I had known of it I should assuredly have alluded to it in the "Origin," as I wished to gain support from all quarters. I did not then know of Von Buch's view (alluded to in my Historical Introduction in all the later editions). Von Buch published his "Isles Canaries" in 1836, and he here briefly argues that plants spread over a continent and vary, and the varieties in time come to be species. He also argues that closely allied species have been thus formed in the SEPARATE valleys of the Canary Islands, but not on the upper and open parts. I could lend you Von Buch's book, if you like. I have just consulted the passage.

I have not Baer's papers; but, as far as I remember, the subject is not fully discussed by him.

I quite agree about Wallace's position on the ocean and continent question.

To return to geographical distribution: As far as I know, no one ever discussed the meaning of the relation between representative species before I did, and, as I suppose, Wallace did in his paper before the Linnean Society. Von Buch's is the nearest approach to such discussion known to me.

LETTER 400. TO W.D. CRICK.

(400/1. The following letters are interesting not only for their own sake, but because they tell the history of the last of Mr. Darwin's publications—his letter to "Nature" on the "Dispersal of Freshwater Bivalves," April 6th, 1882.)

Down, February 21st, 1882.

Your fact is an interesting one, and I am very much obliged to you for communicating it to me. You speak a little doubtfully about the name of the shell, and it would be indispensable to have this ascertained with certainty. Do you know any good conchologist in Northampton who could name it? If so I should be obliged if you would inform me of the result.

Also the length and breadth of the shell, and how much of leg (which leg?) of the Dytiscus {a large water-beetle} has been caught. If you cannot get the shell named I could take it to the British Museum when I next go to London; but this probably will not occur for about six weeks, and you may object to lend the specimen for so long a time.

I am inclined to think that the case would be worth communicating to "Nature."

P.S.—I suppose that the animal in the shell must have been alive when the Dytiscus was captured, otherwise the adductor muscle of the shell would have relaxed and the shell dropped off.

LETTER 401. TO W.D. CRICK. Down, February 25th, 1882.

I am much obliged for your clear and distinct answers to my questions. I am sorry to trouble you, but there is one point which I do not fully understand. Did the shell remain attached to the beetle's leg from the 18th to the 23rd, and was the beetle kept during this time in the air?

Do I understand rightly that after the shell had dropped off, both being in water, that the beetle's antenna was again temporarily caught by the shell?

I presume that I may keep the specimen till I go to London, which will be about the middle of next month.

I have placed the shell in fresh-water, to see if the valve will open, and whether it is still alive, for this seems to me a very interesting point. As the wretched beetle was still feebly alive, I have put it in a bottle with chopped laurel leaves, that it may die an easy and quicker death. I hope that I shall meet with your approval in doing so.

One of my sons tells me that on the coast of N. Wales the bare fishing hooks often bring up young mussels which have seized hold of the points; but I must make further enquiries on this head.

LETTER 402. TO W.D. CRICK. Down, March 23rd, 1882.

I have had a most unfortunate and extraordinary accident with your shell. I sent it by post in a strong box to Mr. Gwyn Jeffreys to be named, and heard two days afterwards that he had started for Italy. I then wrote to the servant in charge of his house to open the parcel (within which was a cover stamped and directed to myself) and return it to me. This servant, I suppose, opened the box and dropped the glass tube on a stone floor, and perhaps put his foot on it, for the tube and shell were broken into quite small fragments. These were returned to me with no explanation, the box being quite uninjured. I suppose you would not care for the fragments to be returned or the Dytiscus; but if you wish for them they shall be returned. I am very sorry, but it has not been my fault.

It seems to me almost useless to send the fragments of the shell to the British Museum to be named, more especially as the umbo has been lost. It is many years since I have looked at a fresh-water shell, but I should have said that the shell was Cyclas cornea. (402/1. It was Cyclas cornea.) Is Sphaenium corneum a synonym of Cyclas? Perhaps you could tell by looking to Mr. G. Jeffreys' book. If so, may we venture to call it so, or shall I put an (?) to the name?

As soon as I hear from you I will send my letter to "Nature." Do you take in "Nature," or shall I send you a copy?

CHAPTER 2.VIII.—MAN.

I. Descent of Man.—II. Sexual Selection.—III. Expression of the Emotions.

2.VIII.I. DESCENT OF MAN, 1860-1882.

LETTER 403. TO C. LYELL. Down, April 27th {1860}.

I cannot explain why, but to me it would be an infinite satisfaction to believe that mankind will progress to such a pitch that we should {look} back at {ourselves} as mere Barbarians. I have received proof-sheets (with a wonderfully nice letter) of very hostile review by Andrew Murray, read before the Royal Society of Edinburgh. (403/1. "On Mr. Darwin's Theory of the Origin of Species," by Andrew Murray. "Proc. Roy. Soc., Edinb." Volume IV., pages 274-91, 1862. The review concludes with the following sentence: "I have come to be of opinion that Mr. Darwin's theory is unsound, and that I am to be spared any collision between my inclination and my convictions" (referring to the writer's belief in Design).) But I am tired with answering it. Indeed I have done nothing the whole day but answer letters.

LETTER 404. TO L. HORNER.

(404/1. The following letter occurs in the "Memoir of Leonard Horner, edited by his daughter Katherine M. Lyell," Volume II., page 300 (privately printed, 1890).)

Down, March 20th {1861}.

I am very much obliged for your Address (404/2. Mr. Horner's Anniversary Address to the Geological Society ("Proc. Geol. Soc." XVII., 1861).) which has interested me much...I thought that I had read up pretty well on the antiquity of man; but you bring all the facts so well together in a condensed focus, that the case seems much clearer to me. How curious about the Bible! (404/3. At page lxviii. Mr. Horner points out that the "chronology, given in the margin of our Bibles," i.e., the statement that the world was created 4004 B.C., is the work of Archbishop Usher, and is in no way binding on those who believe in the inspiration of Scripture. Mr.

Horner goes on (page lxx): "The retention of the marginal note in question is by no means a matter of indifference; it is untrue, and therefore it is mischievous." It is interesting that Archbishop Sumner and Dr. Dawes, Dean of Hereford, wrote with approbation of Mr. Horner's views on Man. The Archbishop says: "I have always considered the first verse of Genesis as indicating, rather than denying, a PREADAMITE world" ("Memoir of Leonard Horner, II.", page 303).) I declare I had fancied that the date was somehow in the Bible. You are coming out in a new light as a Biblical critic. I must thank you for some remarks on the "Origin of Species" (404/4. Mr. Horner (page xxxix) begins by disclaiming the qualifications of a competent critic, and confines himself to general remarks on the philosophic candour and freedom from dogmatism of the "Origin": he does, however, give an opinion on the geological chapters IX. and X. As a general criticism he quotes Mr. Huxley's article in the "Westminster Review," which may now be read in "Collected Essays," II., page 22.) (though I suppose it is almost as incorrect to do so as to thank a judge for a favourable verdict): what you have said has pleased me extremely. I am the more pleased, as I would rather have been well attacked than have been handled in the namby-pamby, old-woman style of the cautious Oxford Professor. (404/5. This no doubt refers to Professor Phillips' "Life on the Earth," 1860, a book founded on the author's "Rede Lecture," given before the University of Cambridge. Reference to this work will be found in "Life and Letters," II., pages 309, 358, 373.)

LETTER 405. TO J.D. HOOKER.

(405/1. Mr. Wallace was, we believe, the first to treat the evolution of Man in any detail from the point of view of Natural Selection, namely, in a paper in the "Anthropological Review and Journal of the Anthropological Society," May 1864, page clviii. The deep interest with which Mr. Darwin read his copy is graphically recorded in the continuous series of pencil-marks along the margins of the pages. His views are fully given in Letter 406. The phrase, "in this case it is too far," refers to Mr. Wallace's habit of speaking of the theory of Natural Selection as due entirely to Darwin.)

May 22nd 1864.

I have now read Wallace's paper on Man, and think it MOST striking and original and forcible. I wish he had written Lyell's chapters on Man. (405/2. See "Life and Letters," III., page 11 et seq. for Darwin's disappointment over Lyell's treatment of the evolutionary question in his "Antiquity of Man"; see also page 29 for Lyell's almost pathetic words about his own position between the discarded faith of many years and the new one not yet assimilated. See also Letters 132, 164, 170.) I quite agree about his high-mindedness, and have long thought so; but in this case it is too far, and I shall tell him so. I am not sure that I fully agree with his views about Man, but there is no doubt, in my opinion, on the remarkable genius shown by the paper. I agree, however, to the main new leading idea.

LETTER 406. TO A.R. WALLACE.

(406/1. This letter was published in "Life and Letters," III., page 89.)

Down, {May} 28th {1864}.

I am so much better that I have just finished a paper for the Linnean Society (406/2. On the three forms, etc., of Lythrum.); but I am not yet at all strong, I felt much disinclination to write, and therefore you must forgive me for not having sooner thanked you for your paper on Man (406/3. "Anthropological Review," May 1864.) received on the 11th. (406/4. Mr. Wallace wrote, May 10th, 1864: "I send you now my little contribution to the theory of the origin of man. I hope you will be able to agree with me. If you are able {to write} I shall be glad to have your criticisms. I was led to the subject by the necessity of explaining the vast mental and cranial differences between man and the apes combined with such small structural differences in other parts of the body,—and also by an endeavour to account for the diversity of human races combined with man's almost perfect stability of form during all historical epochs." But first let me say that I have hardly ever in my life been more struck by any paper than that on "Variation," etc., etc., in the "Reader." (406/5. "Reader," April 16th, 1864, an abstract of Mr. Wallace: "On the Phenomena of Variation and Geographical Distribution as illustrated by the Papilionidae of the Malayan Region." "Linn. Soc. Trans." XXV.) I feel sure that such papers will do more for the spreading of our views on the modification of species than any separate treatises on the simple subject itself. It is really admirable; but you ought not in the Man paper to speak of the theory as mine; it is just as much yours as mine. One correspondent has already noticed to me your "high-minded" conduct on this head.

But now for your Man paper, about which I should like to write more than I can. The great leading idea is quite new to me—viz. that during late ages the mind will have been modified more than the body; yet I had got as far as to see with you, that the struggle between the races of man depended entirely on intellectual and moral qualities. The latter part of the paper I can designate only as grand and most eloquently done. I have shown your paper to two or three persons who have been here, and they have been equally struck with it. I am not sure that I go with you on all minor points: when reading Sir G. Grey's account of the constant battles of Australian savages, I remember thinking that Natural Selection would come in, and likewise with the Esquimaux, with whom the art of fishing and managing canoes is said to be hereditary. I rather differ on the rank, under a classificatory point of view, which you assign to man; I do not think any character simply in excess ought ever to be used for the higher divisions. Ants would not be separated from other hymenopterous insects, however high the instinct of the one, and however low the instincts of the other. With respect to the differences of race, a conjecture has occurred to me that much may be due to the correlation of complexion (and consequently hair) with constitution. Assume that a dusky individual best escaped miasma, and you will readily see what I mean. I persuaded the Director-General of the Medical Department of the Army to send printed forms to the surgeons of all regiments in tropical countries to ascertain this point, but I daresay I shall never get any returns. Secondly, I suspect that a sort of sexual selection has been the most powerful means of changing the races of man. I can show that the different races have a widely different standard of beauty. Among savages the most powerful men will have the pick of the women, and they will generally leave the most descendants. I have collected a few notes on man, but I do not suppose I shall ever use them. Do you intend to follow out your views? and if so, would you like at some future time to have my few references and notes? I am sure I hardly know whether they are of any value, and they are at present in a state of chaos.

There is much more that I should like to write, but I have not strength.

P.S. Our aristocracy is handsomer (more hideous according to a Chinese or Negro) than the middle classes, from {having the} pick of the women; but oh, what a scheme is primogeniture for destroying Natural Selection! I fear my letter will be barely intelligible to you.

LETTER 406 A.R. WALLACE TO CHARLES DARWIN. 5, Westbourne Grove Terrace, W., May 29th {1864}.

You are always so ready to appreciate what others do, and especially to overestimate my desultory efforts, that I cannot be surprised at your very kind and flattering remarks on my papers. I am glad, however, that you have made a few critical observations (and am only sorry that you were not well enough to make more), as that enables me to say a few words in explanation.

My great fault is haste. An idea strikes me, I think over it for a few days, and then write away with such illustrations as occur to me while going on. I therefore look at the subject almost solely from one point of view. Thus, in my paper on Man (406/1. Published in the "Anthropological Review," 1864.), I aim solely at showing that brutes are modified in a great variety of ways by Natural Selection, but that in none of these particular ways can Man be modified, because of the superiority of his intellect. I therefore no doubt overlook a few smaller points in which Natural Selection may still act on men and brutes alike. Colour is one of them, and I have alluded to this in correlation to constitution, in an abstract I have made at Sclater's request for the "Natural History Review." (406/2. "Nat. Hist. Review," 1864, page 328.) At the same time, there is so much evidence of migrations and displacements of races of man, and so many cases of peoples of distinct physical characters inhabiting the same or similar regions, and also of races of uniform physical characters inhabiting widely dissimilar regions,—that the external characteristics of the chief races of man must, I think, be older than his present geographical distribution, and the modifications produced by correlation to favourable variations of constitution be only a secondary cause of external modification. I hope you may get the returns from the Army. (406/3. Measurements taken of more than one million soldiers in the United States showed that "local influences of some kind act directly on structure."—"Descent of Man," 1901, page 45.) They would be very interesting, but I do not expect the results would be favourable to your view.

With regard to the constant battles of savages leading to selection of physical superiority, I think it would be very imperfect and subject to so many exceptions and irregularities that it would produce no definite result. For instance: the strongest and bravest men would lead, and expose themselves most, and would therefore be most subject to wounds and death. And the physical energy which led to any one tribe delighting in war, might lead to its extermination, by inducing quarrels with all surrounding tribes and leading them to combine against it. Again, superior cunning, stealth, and swiftness of foot, or even better weapons, would often lead to victory as well as mere physical strength. Moreover, this kind of more or less perpetual war goes on amongst savage peoples. It could lead, therefore, to no differential characters, but merely to the keeping up of a certain average standard of bodily and mental health and vigour.

So with selection of variations adapted to special habits of life as fishing, paddling, riding, climbing, etc., etc., in different races, no doubt it must act to some extent, but will it be ever so rigid as to induce a definite physical modification, and can we imagine it to have had any part in producing the distinct races that now exist?

The sexual selection you allude to will also, I think, have been equally uncertain in its results. In the very lowest tribes there is rarely much polygamy, and women are more or less a matter of purchase. There is also little difference of social condition, and I think it rarely happens that any healthy and undeformed man remains without wife and children. I very much doubt the often-repeated assertion that our aristocracy are more beautiful than the middle classes. I allow that they present specimens of the highest kind of beauty, but I doubt the average. I have noticed in country places a greater average amount of good looks among the middle classes, and besides we unavoidably combine in our idea of beauty, intellectual expression, and refinement of manner, which often makes the less appear the more beautiful. Mere physical beauty—i.e. a healthy and regular development of the body and features approaching to the mean and type of European man, I believe is quite as frequent in one class of society as the other, and much more frequent in rural districts than in cities.

With regard to the rank of man in zoological classification, I fear I have not made myself intelligible. I never meant to adopt Owen's or any other such views, but only to point out that from one point of view he was right. I hold that a distinct family for Man, as Huxley allows, is all that can possibly be given him zoologically. But at the same time, if my theory is true, that while the animals which surrounded him have been undergoing modification in all parts of their bodies to a generic or even family degree of difference, he has been changing almost wholly in the brain and head—then in geological antiquity the SPECIES man may be as old as many mammalian families, and the origin of the FAMILY man may date back to a period when some of the ORDERS first originated.

As to the theory of Natural Selection itself, I shall always maintain it to be actually yours and yours only. You had worked it out in details I had never thought of, years before I had a ray of light on the subject, and my paper would never have convinced anybody or been noticed as more than an ingenious speculation, whereas your book has revolutionised the study of Natural History, and carried away captive the best men of the present age. All the merit I claim is the having been the means of inducing you to write and publish at once. I may possibly some day go a little more into this subject (of Man), and if I do will accept the kind offer of your notes.

I am now, however, beginning to write the "Narrative of my Travels," which will occupy me a long time, as I hate writing narrative, and after Bates' brilliant success rather fear to fail.

I shall introduce a few chapters on Geographical Distribution and other such topics. Sir C. Lyell, while agreeing with my main argument on Man, thinks I am wrong in wanting to put him back into Miocene times, and thinks I do not appreciate the immense interval even to the later Pliocene. But I still maintain my view, which in fact is a logical result of my theory; for if man originated in later Pliocene, when almost all mammalia were of closely allied species to those now living, and many even identical, then man has not been stationary in bodily structure while animals have been varying, and my theory will be proved to be all wrong.

In Murchison's address to the Geographical Society, just delivered, he points out Africa as being the oldest existing land. He says there is no evidence of its having been ever submerged during the Tertiary epoch. Here then is evidently the place to find early man. I hope something good may be found in Borneo, and that the means may be found to explore the still more promising regions of tropical Africa, for we can expect nothing of man very early in Europe.

It has given me great pleasure to find that there are symptoms of improvement in your health. I hope you will not exert yourself too soon or write more than is quite agreeable to you. I think I made out every word of your letter, though it was not always easy.

(406/4. For Wallace's later views see Letter 408, note.)

LETTER 407. TO W. TURNER.

(407/1. Sir William Turner is frequently referred to in the "Descent of Man" as having supplied Mr. Darwin with information.)

Down, December 14th {1866}.

Your kindness when I met you at the Royal Society makes me think that you would grant me the favour of a little information, if in your power. I am preparing a book on Domestic Animals, and as there has been so much discussion on the bearing of such views as I hold on Man, I have some thoughts of adding a chapter on this subject. The point on which I want information is in regard to any part which may be fairly called rudimentary in comparison with the same part in the Quadrumana or any other mammal. Now the os coccyx is rudimentary as a tail, and I am anxious to hear about its muscles. Mr. Flower found for me in some work that its one muscle (with striae) was supposed only to bring this bone back to its proper position after parturition. This seems to me hardly credible. He said he had never particularly examined this part, and when I mentioned your name, he said you were the most likely man to give me information.

Are there any traces of other muscles? It seems strange if there are none. Do you know how the muscles are in this part in the anthropoid apes? The muscles of the ear in man may, I suppose, in most cases be considered as rudimentary; and so they seem to be in the anthropoids; at least, I am assured in the Zoological Gardens they do not erect their ears. I gather there are a good many muscles in various parts of the body which are in this same state: could you specify any of the best cases? The mammae in man are rudimentary. Are there any other glands or other organs which you can think of? I know I have no right whatever to ask all these questions, and can only say that I should be grateful for any information. If you tell me anything about the os coccyx or other structures, I hope that you will permit me to quote the statement on your authority, as that would add so greatly to its value.

Pray excuse me for troubling you, and do not hurry yourself in the least in answering me.

I do not know whether you would care to possess a copy, but I told my publisher to send you a copy of the new edition of the "Origin" last month.

LETTER 408. TO W. TURNER. Down, February 1st {1867}.

I thank you cordially for all your full information, and I regret much that I have given you such great trouble at a period when your time is so much occupied. But the facts were so valuable to me that I cannot pretend that I am sorry that I did trouble you; and I am the less so, as from what you say I hope you may be induced some time to write a full account of all rudimentary structures in Man: it would be a very curious and interesting memoir. I shall at present give only a brief abstract of the chief facts which you have so very kindly communicated to me, and will not touch on some of the doubtful points. I have received far more information than I ventured to anticipate. There is one point which has occurred to me, but I suspect there is nothing in it. If, however, there should be, perhaps you will let me have a brief note from you, and if I do not hear I will understand there is nothing in the notion. I have included the down on the human body and the lanugo on the foetus as a rudimentary representation of a hairy coat. (408/1. "Descent of Man" I., page 25; II., page 375.) I do not know whether there is any direct functional connection between the presence of hair and the panniculus carnosus (408/2. Professor Macalister draws our attention to the fact that Mr. Darwin uses the term panniculus in the generalised sense of any sheet of muscle acting on the skin.) (to put the question under another point of view, is it the primary or aboriginal function of the panniculus to move the dermal appendages or the skin itself?); but both are superficial, and would perhaps together become rudimentary. I was led to think of this by the places (as far as my ignorance of anatomy has allowed me to judge) of the rudimentary muscular fasciculi which you specify. Now, some persons can move the skin of their hairy heads; and is this not effected by the panniculus? How is it with the eyebrows? You specify the axillae and the front region of the chest and lower part of scapulae: now, these are all hairy spots in man. On the other hand, the neck, and as I suppose the covering of the gluteus medius, are not hairy; so, as I said, I presume there is nothing in this notion. If there were, the rudiments of the panniculus ought perhaps to occur more plainly in man than in woman...

P.S.—If the skin on the head is moved by the panniculus, I think I ought just to allude to it, as some men alone having power to move the skin shows that the apparatus is generally rudimentary.

(408/3. In March 1869 Darwin wrote to Mr. Wallace: "I shall be intensely curious to read the "Quarterly." I hope you have not murdered too completely your own and my child." The reference is to Mr. Wallace's review, in the April number of the "Quarterly," of Lyell's "Principles of Geology" (tenth edition), and of the sixth edition of the "Elements of Geology." Mr. Wallace points out that here for the first time Sir C. Lyell gave up his opposition to evolution; and this leads Mr. Wallace to give a short account of the views set forth in the "Origin of Species." In this article Mr. Wallace makes a definite statement as to his views on the evolution of man, which were opposed to those of Mr. Darwin. He upholds the view that the brain of man, as well as the organs of speech, the hand and the external form, could not have been evolved by Natural Selection (the child he is supposed to murder). At page 391 he writes: "In the brain of the lowest savages, and, as far as we know, of the prehistoric races, we have an organ...little inferior in size and complexity to that of the highest types...But the mental requirements of the lowest savages, such as the Australians or the Andaman Islanders, are very little above those of many animals...How, then, was an organ developed so far beyond the needs of its possessor? Natural Selection could only have endowed the savage with a brain a little superior to that of an ape, whereas he actually possesses one but very little inferior to that of the average members of our learned societies." This passage is marked in Mr. Darwin's copy with a triply underlined "No," and with a shower of notes of exclamation. It was probably the first occasion on which he realised the extent of this great and striking divergence in opinion between himself and his colleague.

He had, however, some indication of it in Wallace's paper on Man, "Anthropological Review," 1864. (See Letter 406). He wrote to Lyell, May 4th, 1869, "I was dreadfully disappointed about Man; it seems to me incredibly strange." And to Mr. Wallace, April 14th, 1869, "If you had not told me, I should have thought that {your remarks on Man} had been added by some one else. As you expected, I differ grievously from you, and I am very sorry for it."

LETTER 409. TO T.H. HUXLEY. Down, Thursday, February 21st {1868-70?}.

I received the Jermyn Street programme, but have hardly yet considered it, for I was all day on the sofa on Tuesday and Wednesday. Bad though I was, I thought with constant pleasure of your very great kindness in offering to read the proofs of my essay on man. I do not know whether I said anything which might have appeared like a hint, but I assure you that such a thought had never even momentarily passed through my mind. Your offer has just made all the difference, that I can now write, whether or no my essay is ever printed, with a feeling of satisfaction instead of vague dread.

Beg my colleague, Mrs. Huxley, not to forget the corrugator supercilii: it will not be easy to catch the exact moment when the child is on the point of crying, and is struggling against the wrinkling up {of} its little eyes; for then I should expect the corrugator, from being little under the command of the will, would come into play in checking or stopping the wrinkling. An explosion of tears would tell nothing.

LETTER 410. TO FRANCIS GALTON. Down, December 23rd {1870?}.

I have only read about fifty pages of your book (to the Judges) (410/1. "Hereditary Genius: an Inquiry into its Laws and Consequences," by Francis Galton, London, 1869. "The Judges of England between 1660 and 1865" is the heading of a section of this work (page 55). See "Descent of Man" (1901), page 41.), but I must exhale myself, else something will go wrong in my inside. I do not think I ever in all my life read anything more interesting and original. And how well and clearly you put every point! George, who has finished the book, and who expressed himself just in the same terms, tells me the earlier chapters are nothing in interest to the later ones! It will take me some time to get to these later chapters, as it is read aloud to me by my wife, who is also much interested. You have made a convert of an opponent in one sense, for I have always maintained that, excepting fools, men did not differ much in intellect, only in zeal and hard work; and I still think {this} is an eminently important difference. I congratulate you on producing what I am convinced will prove a memorable work. I look forward with intense interest to each reading, but it sets me thinking so much that I find it very hard work; but that is wholly the fault of my brain, and not of your beautifully clear style.

LETTER 411. TO W.R. GREG. March 21st {1871?}.

Many thanks for your note. I am very glad indeed to read remarks made by a man who possesses such varied and odd knowledge as you do, and who is so acute a reasoner. I have no doubt that you will detect blunders of many kinds in my book. (411/1. "The Descent of Man.") Your MS. on the proportion of the sexes at birth seems to me extremely curious, and I hope that some day you will publish it. It certainly appears that the males are decreasing in the London districts, and a most strange fact it is. Mr. Graham, however, I observe in a note enclosed, does not seem inclined to admit your conclusion. I have never much considered the subject of the causes of the proportion. When I reflected on queen bees producing only males when not impregnated, whilst some other parthenogenetic insects produced, as far as known, only females, the subject seemed to me hopelessly obscure. It is, however, pretty clear that you have taken the one path for its solution. I wished only to ascertain how far with various animals the males exceeded the females, and I have given all the facts which I could collect. As far as I know, no other data have been published. The equality of the sexes with race-horses is surprising. My remarks on mankind are quite superficial, and given merely as some sort of standard for comparison with the lower animals. M. Thury is the writer who makes the sex depend on the period of impregnation. His pamphlet was sent me from Geneva. (411/2. "Memoire sur la loi de Production des Sexes," 2nd edition, 1863 (a pamphlet published by Cherbuliez, Geneva).) I can lend it you if you like. I subsequently read an account of experiments which convinced me that M. Thury was in error; but I cannot remember what they were, only the impression that I might safely banish this view from my mind. Your remarks on the less ratio of males in illegitimate births strikes me as the most doubtful point in your MS.—requiring two assumptions, viz. that the fathers in such cases are relatively too young, and that the result is the same as when the father is relatively too old.

My son, George, who is a mathematician, and who read your MS. with much interest, has suggested, as telling in the right direction, but whether sufficient is another question, that many more illegitimate children are murdered and concealed shortly after birth, than in the case of legitimate children; and as many more males than females die during the first few days of life, the census of illegitimate children practically applies to an older age than with legitimate children, and would thus slightly reduce the excess of males. This might possibly be worth consideration. By a strange coincidence a stranger writes to me this day, making the very same suggestion.

I am quite delighted to hear that my book interests you enough to lead you to read it with some care.

LETTER 412. TO FRANCIS GALTON. Down, January 4th, 1873.

Very many thanks for "Fraser" (412/1. "Hereditary Improvement," by Francis Galton, "Fraser's Magazine," January 1873, page 116.): I have been greatly interested by your article. The idea of castes being spontaneously formed and leading to intermarriage (412/2. "My object is to build up, by the mere process of extensive enquiry and publication of results, a sentiment of caste among those who are naturally gifted, and to procure for them, before the system has fairly taken root, such moderate social favours and preference, no more no less, as would seem reasonable to those who were justly informed of the precise measure of their importance to the nation" (loc. cit., page 123).) is quite new to me, and I should suppose to others. I am not, however, so hopeful as you. Your proposed Society (412/3. Mr. Galton proposes that "Some society should undertake three scientific services: the first, by means of a moderate number of influential local agencies, to institute continuous enquiries into the facts of human heredity; the second to be a centre of information on heredity for breeders of animals and plants; and the third to discuss and classify the facts that were collected" (loc. cit., page 124).) would have awfully laborious work, and I doubt whether you could ever get efficient workers. As it is, there is much concealment of insanity and wickedness in families; and there would be more if there was a register. But the greatest difficulty, I think, would be in deciding who deserved to be on the register. How few are above mediocrity in health, strength, morals and intellect; and how difficult to judge on these latter heads. As far as I see, within the same large superior family, only a few of the children would deserve to be on the register; and these would naturally stick to their own families, so that the superior children of distinct families would have no good chance of associating much and forming a caste. Though I see so much difficulty, the object seems a grand one; and you have pointed out the sole feasible, yet I fear utopian, plan of procedure in improving the human race. I should be inclined to trust more (and this is part of your plan) to disseminating and insisting on the importance of the all-important principle of inheritance. I will make one or two minor criticisms. Is it not possible that the inhabitants of malarious countries owe their degraded and miserable appearance to the bad atmosphere, though this does not kill them, rather than to "economy of structure"? I do not see that an orthognathous face would cost more than a prognathous face; or a good morale than a bad one. That is a fine simile (page 119) about the chip of a statue (412/4. "...The life of the individual is treated as of absolutely no importance, while the race is as everything; Nature being wholly careless of the former except as a contributor to the maintenance and evolution of the latter. Myriads of inchoate lives are produced in what, to our best judgment, seems a wasteful and reckless manner, in order that a few selected specimens may survive, and be the parents of the next generation. It is as though individual lives were of no more consideration than are the senseless chips which fall from the chisel of the artist who is elaborating some ideal form from a rude block" (loc. cit., page 119).); but surely Nature does not more carefully regard races than individuals, as (I believe I have misunderstood what you mean) evidenced by the multitude of races and species which have become extinct. Would it not be truer to say that Nature cares only for the superior individuals and then makes her new and better races? But we ought both to shudder in using so freely the word "Nature" (412/5. See Letter 190, Volume I.) after what De Candolle has said. Again let me thank you for the interest received in reading your essay.

Many thanks about the rabbits; your letter has been sent to Balfour: he is a very clever young man, and I believe owes his cleverness to Salisbury blood. This letter will not be worth your deciphering. I have almost finished Greg's "Enigmas." (412/6. "The Enigmas of Life," 1872.) It is grand poetry—but too Utopian and too full of faith for me; so that I have been rather disappointed. What do you think about it? He must be a delightful man.

I doubt whether you have made clear how the families on the Register are to be kept pure or superior, and how they are to be in course of time still further improved.

LETTER 413. TO MAX MULLER. Down, July 3rd, 1873.

(413/1. In June, 1873, Professor Max Muller sent to Mr. Darwin a copy of the sixth edition of his "Lectures on the Science of Language" (413/2. A reference to the first edition occurs in "Life and Letters," II., page 390.), with a letter concluding with these words: "I venture to send you my three lectures, trusting that, though I differ from some of your conclusions, you will believe me to be one of your diligent readers and sincere admirers.")

I am much obliged for your kind note and present of your lectures. I am extremely glad to have received them from you, and I had intended ordering them.

I feel quite sure from what I have read in your works that you would never say anything of an honest adversary to which he would have any just right to object; and as for myself, you have often spoken highly of me—perhaps more highly than I deserve.

As far as language is concerned I am not worthy to be your adversary, as I know extremely little about it, and that little learnt from very few books. I should have been glad to have avoided the whole subject, but was compelled to take it up as well as I could. He who is fully convinced, as I am, that man is descended from some lower animal, is almost forced to believe a priori that articulate language has been developed from inarticulate cries (413/3. "Descent of Man" (1901), page 133.); and he is therefore hardly a fair judge of the arguments opposed to this belief.

(413/4. In October, 1875, Mr. Darwin again wrote cordially to Professor Max Muller on receipt of a pamphlet entitled "In Self-Defence" (413/5. Printed in "Chips from a German Workshop," Volume IV., 1875, page 473.), which is a reply to Professor Whitney's "Darwinism and Language" in the "North American Review," July 1874. This essay had been brought before the "general reader" in England by an article of Mr. G. Darwin's in the "Contemporary Review," November, 1874, page 894, entitled, "Professor Whitney on the Origin of Language." The article was followed by "My reply to Mr. Darwin," contributed by Professor Muller to the "Contemporary Review," January, 1875, page 305.)

LETTER 414. G. ROLLESTON TO CHARLES DARWIN. British Association, Bristol, August 30th, 1875.

(414/1. In the first edition of the "Descent of Man" Mr. Darwin wrote: "It is a more curious fact that savages did not formerly waste away, as Mr. Bagehot has remarked, before the classical nations, as they now do before modern civilised nations..."(414/2. Bagehot, "Physics and Politics," "Fortnightly Review," April, 1868, page 455.) In the second edition (page 183) the statement remains, but a mass of evidence (pages 183-92) is added, to which reference occurs in the reply to the following letter.)

At pages 4-5 of the enclosed Address (414/3. "British Association Reports," 1875, page 142.) you will find that I have controverted Mr. Bagehot's view as to the extinction of the barbarians in the times of classical antiquity, as also the view of Poppig as to there being some occult influence exercised by civilisation to the disadvantage of savagery when the two come into contact.

I write to say that I took up this subject without any wish to impugn any views of yours as such, but with the desire of having my say upon certain anti-sanitarian transactions and malfeasance of which I had had a painful experience.

On reading however what I said, and had written somewhat hastily, it has struck me that what I have said might bear the former interpretation in the eyes of persons who might not read other papers of mine, and indeed other parts of the same Address, in which my adhesion, whatever it is worth, to your views in general is plainly enough implied. I have ventured to write this explanation to you for several reasons.

LETTER 415. TO G. ROLLESTON. Bassett, Southampton, September 2nd {1875}.

I am much obliged to you for having sent me your Address, which has interested me greatly. I quite subscribe to what you say about Mr. Bagehot's striking remark, and wish I had not quoted it. I can perceive no sort of reflection or blame on anything which I have written, and I know well that I deserve many a good slap on the face. The decrease of savage populations interests me much, and I should like you some time to look at a discussion on this subject which I have introduced in the second edition of the "Descent of Man," and which you can find (for I have no copy here) in the list of additions. The facts have convinced me that lessened fertility and the poor constitution of the children is one chief cause of such decrease; and that the case is strictly parallel to the sterility of many wild animals when made captive, the civilisation of savages and the captivity of wild animals leading to the same result.

LETTER 416. TO ERNST KRAUSE. Down, June 30th, 1877.

I have been much interested by your able argument against the belief that the sense of colour has been recently acquired by man. (416/1. See "Kosmos," June 1877, page 264, a review of Dr. Hugo Magnus' "Die Geschichtliche Entwickelung des Farbensinnes," 1877. The first part is chiefly an account of the author's views; Dr. Krause's argument begins at page 269. The interest felt by Mr. Darwin is recorded by the numerous pencil-marks on the margin of his copy.) The following observation bears on this subject.

I attended carefully to the mental development of my young children, and with two, or as I believe three of them, soon after they had come to the age when they knew the names of all common objects, I was startled by observing that they seemed quite incapable of affixing the right names to the colours in coloured engravings, although I tried repeatedly to teach them. I distinctly remember declaring that they were colour-blind, but this afterwards proved a groundless fear.

On communicating this fact to another person he told me that he had observed a nearly similar case. Therefore the difficulty which young children experience either in distinguishing, or more probably in naming colours, seems to deserve further investigation. I will add that it formerly appeared to me that the gustatory sense, at least in the case of my own infants, and very young children, differed from that of grown-up persons. This was shown by their not disliking rhubarb mixed with a little sugar and milk, which is to us abominably nauseous; and in their strong taste for the sourest and most austere fruits, such as unripe gooseberries and crabapples.

(PLATE: G.J. ROMANES, 1891. Elliott & Fry, photo. Walker and Cockerell, ph. sc.)

LETTER 417. TO G.J. ROMANES. {Barlaston}, August 20th, 1878.

(417/1. Part of this letter (here omitted) is published in "Life and Letters," III., page 225, and the whole in the "Life and Letters of G.J. Romanes," page 74. The lecture referred to was on animal intelligence, and was given at the Dublin meeting of the British Association.)

...The sole fault which I find with your lecture is that it is too short, and this is a rare fault. It strikes me as admirably clear and interesting. I meant to have remonstrated that you had not discussed sufficiently the necessity of signs for the formation of abstract ideas of any complexity, and then I came on the discussion on deaf mutes. This latter seems to me one of the richest of all the mines, and is worth working carefully for years, and very deeply. I should like to read whole chapters on this one head, and others on the minds of the higher idiots. Nothing can be better, as it seems to me, than your several lines or sources of evidence, and the manner in which you have arranged the whole subject. Your book will assuredly be worth years of hard labour; and stick to your subject. By the way, I was pleased at your discussing the selection of varying instincts or mental tendencies; for I have often been disappointed by no one having ever noticed this notion.

I have just finished "La Psychologie, son Present et son Avenir," 1876, by Delboeuf (a mathematician and physicist of Belgium) in about a hundred pages. It has interested me a good deal, but why I hardly know; it is rather like Herbert Spencer. If you do not know it, and would care to see it, send me a postcard.

Thank Heaven, we return home on Thursday, and I shall be able to go on with my humdrum work, and that makes me forget my daily discomfort.

Have you ever thought of keeping a young monkey, so as to observe its mind? At a house where we have been staying there were Sir A. and Lady Hobhouse, not long ago returned from India, and she and he kept {a} young monkey and told me some curious particulars. One was that her monkey was very fond of looking through her eyeglass at objects, and moved the glass nearer and further so as to vary the focus. This struck me, as Frank's son, nearly two years old (and we think much of his intellect!!) is very fond of looking through my pocket lens, and I have quite in vain endeavoured to teach him not to put the glass close down on the object, but he always will do so. Therefore I conclude that a child under two years is inferior in intellect to a monkey.

Once again I heartily congratulate you on your well-earned present, and I feel assured, grand future success.

(417/2. Later in the year Mr. Darwin wrote: "I am delighted to hear that you mean to work the comparative Psychology well. I thought your letter to the "Times" very good indeed. (417/3. Romanes wrote to the "Times" August 28th, 1878, expressing his views regarding the distinction between man and the lower animals, in reply to criticisms contained in a leading article in the "Times" of August 23rd on his lecture at the Dublin meeting of the British Association.) Bartlett, at the Zoological Gardens, I feel sure, would advise you infinitely better about hardiness, intellect, price, etc., of monkey than F. Buckland; but with him it must be viva voce.

"Frank says you ought to keep a idiot, a deaf mute, a monkey, and a baby in your house.")

LETTER 418. TO G.A. GASKELL. Down, November 15th, 1878.

(418/1. This letter has been published in Clapperton's "Scientific Meliorism," 1885, page 340, together with Mr. Gaskell's letter of November 13th (page 337). Mr. Gaskell's laws are given in his letter of November 13th, 1878. They are:—

I. The Organological Law:
Natural Selection, or the Survival of the Fittest.

II. The Sociological Law:
Sympathetic Selection, or Indiscriminate Survival.

III. The Moral Law:
Social Selection, or the Birth of the Fittest.)

Your letter seems to me very interesting and clearly expressed, and I hope that you are in the right. Your second law appears to be largely acted on in all civilised countries, and I just alluded to it in my remarks to the effect (as far as I remember) that the evil which would follow by checking benevolence and sympathy in not fostering the weak and diseased would be greater than by allowing them to survive and then to procreate.

With regard to your third law, I do not know whether you have read an article (I forget when published) by F. Galton, in which he proposes certificates of health, etc., for marriage, and that the best should be matched. I have lately been led to reflect a little, (for, now that I am growing old, my work has become {word indecipherable} special) on the artificial checks, but doubt greatly whether such would be advantageous to the world at large at present, however it may be in the distant future. Suppose that such checks had been in action during the last two or three centuries, or even for a shorter time in Britain, what a difference it would have made in the world, when we consider America, Australia, New Zealand, and S. Africa! No words can exaggerate the importance, in my opinion, of our colonisation for the future history of the world.

If it were universally known that the birth of children could be prevented, and this were not thought immoral by married persons, would there not be great danger of extreme profligacy amongst unmarried women, and might we not become like the "arreoi" societies in the Pacific? In the course of a century France will tell us the result in many ways, and we can already see that the French nation does not spread or increase much.

I am glad that you intend to continue your investigations, and I hope ultimately may publish on the subject.

LETTER 419. TO K. HOCHBERG. Down, January 13th, 1879.

I am much obliged for your note and for the essay which you have sent me. I am a poor german scholar, and your german is difficult; but I think that I understand your meaning, and hope at some future time, when more at leisure, to recur to your essay. As far as I can judge, you have made a great advance in many ways in the subject; and I will send your paper to Mr. Edmund Gurney (The late Edmund Gurney, author of "The Power of Sound," 1880.), who has written on and is much interested in the origin of the taste for music. In reading your essay, it occurred to me that facility in the utterance of prolonged sounds (I do not think that you allude to this point) may possibly come into play in rendering them musical; for I have heard it stated that those who vary their voices much, and use cadences in long continued speaking, feel less fatigued than those who speak on the same note.

LETTER 420. TO G.J. ROMANES. Down, February 5th, 1880.

(420/1. Romanes was at work on what ultimately came to be a book on animal intelligence. Romanes's reply to this letter is given in his "Life," page 95. The table referred to is published as a frontispiece to his "Mental Evolution in Animals," 1885.)

As I feared, I cannot be of the least use to you. I could not venture to say anything about babies without reading my Expression book and paper on Infants, or about animals without reading the "Descent of Man" and referring to my notes; and it is a great wrench to my mind to change from one subject to another.

I will, however, hazard one or two remarks. Firstly, I should have thought that the word "love" (not sexual passion), as shown very low in the scale, to offspring and apparently to comrades, ought to have come in more prominently in your table than appears to be the case. Secondly, if you give any instance of the appreciation of different stimulants by plants, there is a much better case than that given by you—namely, that of the glands of Drosera, which can be touched roughly two or three times and do not transmit any effect, but do so if pressed by a weight of 1/78000 grain ("Insectivorous Plants" 263). On the other hand, the filament of Dionoea may be quietly loaded with a much greater weight, while a touch by a hair causes the lobes to close instantly. This has always seemed to me a marvellous fact. Thirdly, I have been accustomed to look at the coming in of the sense of pleasure and pain as one of the most important steps in the development of mind, and I should think it ought to be prominent in your table. The sort of progress which I have imagined is that a stimulus produced some effect at the point affected, and that the effect radiated at first in all directions, and then that certain definite advantageous lines of transmission were acquired, inducing definite reaction in certain lines. Such transmission afterwards became associated in some unknown way with pleasure or pain. These sensations led at first to all sorts of violent action, such as the wriggling of a worm, which was of some use. All the organs of sense would be at the same time excited. Afterwards definite lines of action would be found to be the most useful, and so would be practised. But it is of no use my giving you my crude notions.

LETTER 421. TO S. TOLVER PRESTON. Down, May 22nd, 1880.

(421/1. Mr. Preston wrote (May 20th, 1880) to the effect that "self-interest as a motive for conduct is a thing to be commended—and it certainly {is} I think...the only conceivable rational motive of conduct: and always is the tacitly recognised motive in all rational actions." Mr. Preston does not, of course, commend selfishness, which is not true self-interest.

There seem to be two ways of looking at the case given by Darwin. The man who knows that he is risking his life,—realising that the personal satisfaction that may follow is not worth the risk—is surely admirable from the strength of character that leads him to follow the social instinct against his purely personal inclination. But the man who blindly obeys the social instinct is a more useful member of a social community. He will act with courage where even the strong man will fail.)

Your letter appears to me an interesting and valuable one; but I have now been working for some years exclusively on the physiology of plants, and all other subjects have gone out of my head, and it fatigues me much to try and bring them back again into my head. I am, moreover, at present very busy, as I leave home for a fortnight's rest at the beginning of next week. My conviction as yet remains unchanged, that a man who (for instance) jumps into a river to save a life without a second's reflection (either from an innate tendency or from one gained by habit) is deservedly more honoured than a man who acts deliberately and is conscious, for however short a time, that the risk and sacrifice give him some inward satisfaction.

You are of course familiar with Herbert Spencer's writings on Ethics.

(422/1. The observations to which the following letters refer were continued by Mr. Wallis, who gave an account of his work in an interesting paper in the "Proceedings of the Zoological Society," March 2nd, 1897. The results on the whole confirm the belief that traces of an ancestral pointed ear exist in man.)

LETTER 422. TO H.M. WALLIS. Down, March 22nd, 1881.

I am very much obliged for your courteous and kind note. The fact which you communicate is quite new to me, and as I was laughed at about the tips to human ears, I should like to publish in "Nature" some time your fact. But I must first consult Eschricht, and see whether he notices this fact in his curious paper on the lanugo on human embryos; and secondly I ought to look to monkeys and other animals which have tufted ears, and observe how the hair grows. This I shall not be able to do for some months, as I shall not be in London until the autumn so as to go to the Zoological Gardens. But in order that I may not hereafter throw away time, will you be so kind as to inform me whether I may publish your observation if on further search it seems desirable?

LETTER 423. TO H.M. WALLIS. Down, March 31st, 1881.

I am much obliged for your interesting letter. I am glad to hear that you are looking to other ears, and will visit the Zoological Gardens. Under these circumstances it would be incomparably better (as more authentic) if you would publish a notice of your observations in "Nature" or some scientific journal. Would it not be well to confine your attention to infants, as more likely to retain any primordial character, and offering less difficulty in observing. I think, though, it would be worth while to observe whether there is any relation (though probably none) between much hairiness on the ears of an infant and the presence of the "tip" on the folded margin. Could you not get an accurate sketch of the direction of the hair of the tip of an ear?

The fact which you communicate about the goat-sucker is very curious. About the difference in the power of flight in Dorkings, etc., may it not be due merely to greater weight of body in the adults?

I am so old that I am not likely ever again to write on general and difficult points in the theory of Evolution.

I shall use what little strength is left me for more confined and easy subjects.

LETTER 424. TO MRS. TALBOT.

(Mrs. Emily Talbot was secretary of the Education Department of the American Social Science Association, Boston, Mass. A circular and register was issued by the Department, and answers to various questions were asked for. See "Nature," April 28th, page 617, 1881. The above letter was published in "The Field Naturalist," Manchester, 1883, page 5, edited by Mr. W.E. Axon, to whom we are indebted for a copy.)

Down, July 19th {1881?}

In response to your wish, I have much pleasure in expressing the interest which I feel in your proposed investigation on the mental and bodily development of infants. Very little is at present accurately known on this subject, and I believe that isolated observations will add but little to our knowledge, whereas tabulated results from a very large number of observations, systematically made, would probably throw much light on the sequence and period of development of the several faculties. This knowledge would probably give a foundation for some improvement in our education of young children, and would show us whether the system ought to be followed in all cases.

I will venture to specify a few points of inquiry which, as it seems to me, possess some scientific interest. For instance, does the education of the parents influence the mental powers of their children at any age, either at a very early or somewhat more advanced stage? This could perhaps be learned by schoolmasters and mistresses if a large number of children were first classed according to age and their mental attainments, and afterwards in accordance with the education of their parents, as far as this could be discovered. As observation is one of the earliest faculties developed in young children, and as this power would probably be exercised in an equal degree by the children of educated and uneducated persons, it seems not impossible that any transmitted effect from education could be displayed only at a somewhat advanced age. It would be desirable to test statistically, in a similar manner, the truth of the oft-repeated statement that coloured children at first learn as quickly as white children, but that they afterwards fall off in progress. If it could be proved that education acts not only on the individual, but, by transmission, on the race, this would be a great encouragement to all working on this all-important subject. It is well known that children sometimes exhibit, at a very early age, strong special tastes, for which no cause can be assigned, although occasionally they may be accounted for by reversion to the taste or occupation of some progenitor; and it would be interesting to learn how far such early tastes are persistent and influence the future career of the individual. In some instances such tastes die away without apparently leaving any after effect, but it would be desirable to know how far this is commonly the case, as we should then know whether it were important to direct as far as this is possible the early tastes of our children. It may be more beneficial that a child should follow energetically some pursuit, of however trifling a nature, and thus acquire perseverance, than that he should be turned from it because of no future advantage to him. I will mention one other small point of inquiry in relation to very young children, which may possibly prove important with respect to the origin of language; but it could be investigated only by persons possessing an accurate musical ear. Children, even before they can articulate, express some of their feelings and desires by noises uttered in different notes. For instance, they make an interrogative noise, and others of assent and dissent, in different tones; and it would, I think, be worth while to ascertain whether there is any uniformity in different children in the pitch of their voices under various frames of mind.

I fear that this letter can be of no use to you, but it will serve to show my sympathy and good wishes in your researches.

2.VIII.II. SEXUAL SELECTION, 1866-1872.

LETTER 425. TO JAMES SHAW. Down, February 11th {1866}.

I am much obliged to you for your kindness in sending me an abstract of your paper on beauty. (425/1. A newspaper report of a communication to the "Dumfries Antiquarian and Natural History Society.") In my opinion you take quite a correct view of the subject. It is clear that Dr. Dickson has either never seen my book, or overlooked the discussion on sexual selection. If you have any precise facts on birds' "courtesy towards their own image in mirror or picture," I should very much like to hear them. Butterflies offer an excellent instance of beauty being displayed in conspicuous parts; for those kinds which habitually display the underside of the wing have this side gaudily coloured, and this is not so in the reverse case. I daresay you will know that the males of many foreign butterflies are much more brilliantly coloured than the females, as in the case of birds. I can adduce good evidence from two large classes of facts (too large to specify) that flowers have become beautiful to make them conspicuous to insects. (425/2. This letter is published in "A Country Schoolmaster, James Shaw." Edited by Robert Wallace, Edinburgh, 1899.)

(425/3. Mr. Darwin wrote again to Mr. Shaw in April, 1866:—)

I am much obliged for your kind letter and all the great trouble which you have taken in sending to all the various and interesting facts on birds admiring themselves. I am very glad to hear of these facts. I have just finished writing and adding to a new edition of the "Origin," and in this I have given, without going into details (so that I shall not be able to use your facts), some remarks on the subject of beauty.

LETTER 426. TO A.D. BARTLETT. Down, February 16th {1867?}

I want to beg two favours of you. I wish to ascertain whether the Bower-Bird discriminates colours. (426/1. Mr. Bartlett does not seem to have supplied any information on the point in question. The evidence for the Bower-Bird's taste in colour is in "Descent of Man," II., page 112.) Will you have all the coloured worsted removed from the cage and bower, and then put all in a row, at some distance from bower, the enclosed coloured worsted, and mark whether the bird AT FIRST makes any selection. Each packet contains an equal quantity; the packets had better be separate, and each thread put separate, but close together; perhaps it would be fairest if the several colours were put alternately—one thread of bright scarlet, one thread of brown, etc., etc. There are six colours. Will you have the kindness to tell me whether the birds prefer one colour to another?

Secondly, I very much want several heads of the fancy and long-domesticated rabbits, to measure the capacity of skull. I want only small kinds, such as Himalaya, small Angora, Silver Grey, or any small-sized rabbit which has long been domesticated. The Silver

Grey from warrens would be of little use. The animals must be adult, and the smaller the breed the better. Now when any one dies would you send me the carcase named; if the skin is of any value it might be skinned, but it would be rather better with skin, and I could make a present to any keeper to whom the skin is a perquisite. This would be of great assistance to me, if you would have the kindness thus to aid me.

LETTER 427. TO W.B. TEGETMEIER.

(427/1. We are not aware that the experiment here suggested has ever been carried out.)

Down, March 5th {1867}.

I write on the bare and very improbable chance of your being able to try, or get some trustworthy person to try, the following little experiment. But I may first state, as showing what I want, that it has been stated that if two long feathers in the tail of the male Widow-Bird at the Cape of Good Hope are pulled out, no female will pair with him.

Now, where two or three common cocks are kept, I want to know, if the tail sickle-feathers and saddle-feathers of one which had succeeded in getting wives were cut and mutilated and his beauty spoiled, whether he would continue to be successful in getting wives. This might be tried with drakes or peacocks, but no one would be willing to spoil for a season his peacocks. I have no strength or opportunity of watching my own poultry, otherwise I would try it. I would very gladly repay all expenses of loss of value of the poultry, etc. But, as I said, I have written on the most improbable chance of your interesting any one to make the trial, or having time and inclination yourself to make it. Another, and perhaps better, mode of making the trial would be to turn down to some hens two or three cocks, one being injured in its plumage.

I am glad to say that I have begun correcting proofs. (427/2. "The Variation of Animals and Plants.") I hope that you received safely the skulls which you so kindly lent me.

LETTER 428. TO W.B. TEGETMEIER. Down, March 30th {1867}.

I am much obliged for your note, and shall be truly obliged if you will insert any question on the subject. That is a capital remark of yours about the trimmed game cocks, and shall be quoted by me. (428/1. "Descent of Man," Edition I., Volume II., page 117. "Mr. Tegetmeier is convinced that a game cock, though disfigured by being dubbed with his hackles trimmed, would be accepted as readily as a male retaining all his natural ornaments.") Nevertheless I am still inclined from many facts strongly to believe that the beauty of the male bird determines the choice of the female with wild birds, however it may be under domestication. Sir R. Heron has described how one pied peacock was extra attentive to the hens. This is a subject which I must take up as soon as my present book is done.

I shall be most particularly obliged to you if you will dye with magenta a pigeon or two. (428/2. "Mr. Tegetmeier, at my request, stained some of his birds with magenta, but they were not much noticed by the others."—"Descent of Man" (1901), page 637.) Would it not be better to dye the tail alone and crown of head, so as not to make too great difference? I shall be very curious to hear how an entirely crimson pigeon will be received by the others as well as his mate.

P.S.—Perhaps the best experiment, for my purpose, would be to colour a young unpaired male and turn him with other pigeons, and observe whether he was longer or quicker than usual in mating.

LETTER 429. TO A.R. WALLACE. Down, April 29th {1867}.

I have been greatly interested by your letter, but your view is not new to me. (429/1. We have not been able to find Mr. Wallace's letter to which this is a reply. It evidently refers to Mr. Wallace's belief in the paramount importance of protection in the evolution of colour. This is clear from the P.S. to the present letter and from the passages in the "Origin" referred to. The first reference, Edition IV., page 240, is as follows: "We can sometimes plainly see the proximate cause of the transmission of ornaments to the males alone; for a peahen with the long tail of the male bird would be badly fitted to sit on her eggs, and a coal-black female capercailzie would be far more conspicuous on her nest, and more exposed to danger, than in her present modest attire." The passages in Edition I. (pages 89, 101) do not directly bear on the question of protection.) If you will look at page 240 of the fourth edition of the "Origin" you will find it very briefly given with two extreme examples of the peacock and black grouse. A more general statement is given at page 101, or at page 89 of the first edition, for I have long entertained this view, though I have never had space to develop it. But I had not sufficient knowledge to generalise as far as you do about colouring and nesting. In your paper perhaps you will just allude to my scanty remark in the fourth edition, because in my Essay on Man I intend to discuss the whole subject of sexual selection, explaining as I believe it does much with respect to man. I have collected all my old notes, and partly written my discussion, and it would be flat work for me to give the leading idea as exclusively from you. But, as I am sure from your greater knowledge of Ornithology and Entomology that you will write a much better discussion than I could, your paper will be of great use to me. Nevertheless I must discuss the subject fully in my Essay on Man. When we met at the Zoological Society, and I asked you about the sexual differences in kingfishers, I had this subject in view; as I had when I suggested to Bates the difficulty about gaudy caterpillars, which you have so admirably (as I believe it will prove) explained. (429/2. See a letter of February 26th, 1867, to Mr. Wallace, "Life and Letters" III., page 94.) I have got one capital case (genus forgotten) of a {Australian} bird in which the female has long tail-plumes, and which consequently builds a different nest from all her allies. (429/3. Menura superba: see "Descent of Man" (1901), page 687. Rhynchoea, mentioned a line or two lower down, is discussed in the "Descent," page 727. The female is more brightly coloured than the male, and has a convoluted trachea, elsewhere a masculine character. There seems some reason to suppose that "the male undertakes the duty of incubation.") With respect to certain female birds being more brightly coloured than the males, and the latter incubating, I have gone a little into the subject, and cannot say that I am fully satisfied. I remember mentioning to you the case of Rhynchoea, but its nesting seems unknown. In some other cases the difference in brightness seemed to me hardly sufficiently accounted for by the principle of protection. At the Falkland Islands there is a carrion hawk in which the female (as I ascertained by dissection) is the brightest coloured, and I doubt whether protection will here apply; but I wrote several months ago to the Falklands to make enquiries. The conclusion to which I have been leaning is that in some of these abnormal cases the colour happened to vary in the female alone, and was transmitted to females alone, and that her variations have been selected through the admiration of the male.

It is a very interesting subject, but I shall not be able to go on with it for the next five or six months, as I am fully employed in correcting dull proof-sheets. When I return to the work I shall find it much better done by you than I could have succeeded in doing.

It is curious how we hit on the same ideas. I have endeavoured to show in my MS. discussion that nearly the same principles account for young birds not being gaily coloured in many cases, but this is too complex a point for a note.

On reading over your letter again, and on further reflection, I do not think (as far as I remember my words) that I expressed myself nearly strongly enough on the value and beauty of your generalisation (429/4. See Letter 203, Volume I.), viz., that all birds in which the female is conspicuously or brightly coloured build in holes or under domes. I thought that this was the explanation in many, perhaps most cases, but do not think I should ever have extended my view to your generalisation. Forgive me troubling you with this P.S.

LETTER 430. TO A.R. WALLACE. Down, May 5th {1867}.

The offer of your valuable notes is most generous, but it would vex me to take so much from you, as it is certain that you could work up the subject very much better than I could. Therefore I earnestly, and without any reservation, hope that you will proceed with your paper, so that I return your notes. You seem already to have well investigated the subject. I confess on receiving your note that I felt rather flat at my recent work being almost thrown away, but I did not intend to show this feeling. As a proof how little advance I had made on the subject, I may mention that though I had been collecting facts on the colouring, and other sexual differences in mammals, your explanation with respect to the females had not occurred to me. I am surprised at my own stupidity, but I have long recognised how much clearer and deeper your insight into matters is than mine. I do not know how far you have attended to the laws of inheritance, so what follows may be obvious to you. I have begun my discussion on sexual selection by showing that new characters often appear in one sex and are transmitted to that sex alone, and that from some unknown cause such characters apparently appear oftener in the male than in the female. Secondly, characters may be developed and be confined to the male, and long afterwards be transferred to the female. Thirdly, characters may arise in either sex and be transmitted to both sexes, either in an equal or unequal degree. In this latter case I have supposed that the survival of the fittest has come into play with female birds and kept the female dull-coloured. With respect to the absence of spurs in the female gallinaceous birds, I presume that they would be in the way during incubation; at least I have got the case of a German breed of fowls in which the hens were spurred, and were found to disturb and break their eggs much. With respect to the females of deer not having horns, I presume it is to save the loss of organised matter. In your note you speak of sexual selection and protection as sufficient to account for the colouring of all animals, but it seems to me doubtful how far this will come into play with some of the lower animals, such as sea anemones, some corals, etc., etc. On the other hand Hackel (430/1. See "Descent of Man" (1901) page 402.) has recently well shown that the transparency and absence of colour in the lower oceanic animals, belonging to the most different classes, may be well accounted for on the principle of protection.

Some time or other I should like much to know where your paper on the nests of birds has appeared, and I shall be extremely anxious to read your paper in the "Westminster Review." (430/2. "Westminster Review," July, 1867.) Your paper on the sexual colouring of birds will, I have no doubt, be very striking. Forgive me, if you can, for a touch of illiberality about your paper.

LETTER 431. TO A.R. WALLACE. March 19th, 1868.

(431/1. "The Variation of Animals and Plants" having been published on January 30th, 1868, Mr. Darwin notes in his diary that on February 4th he "Began on Man and Sexual Selection." He had already (in 1864 and 1867) corresponded with Mr. Wallace on these questions—see for instance the "Life and Letters," III., page 89; but, owing to various interruptions, serious work on the subject did not begin until 1869. The following quotations show the line of work undertaken early in 1868.

Mr. Wallace wrote (March 19th, 1868): "I am glad you have got good materials on sexual selection. It is no doubt a difficult subject. One difficulty to me is, that I do not see how the constant MINUTE variations, which are sufficient for Natural Selection to work with, could be SEXUALLY selected. We seem to require a series of bold and abrupt variations. How can we imagine that an inch in the tail of the peacock, or 1/4-inch in that of the Bird of Paradise, would be noticed and preferred by the female.")

In regard to sexual selection. A girl sees a handsome man, and without observing whether his nose or whiskers are the tenth of an inch longer or shorter than in some other man, admires his appearance and says she will marry him. So, I suppose, with the pea-hen; and the tail has been increased in length merely by, on the whole, presenting a more gorgeous appearance. J. Jenner Weir, however, has given me some facts showing that birds apparently admire details of plumage.

LETTER 432. TO F. MULLER. March 28th {1868}.

I am particularly obliged to you for your observations on the stridulation of the two sexes of Lamellicorns. (432/1. We are unable to find any mention of F. Muller's observations on this point; but the reference is clearly to Darwin's observations on Necrophorus and Pelobius, in which the stridulating rasp was bigger in the males in the first individuals examined, but not so in succeeding specimens. "Descent of Man," Edition II., Volume I., page 382.) I begin to fear that I am completely in error owing to that common cause, viz. mistaking at first individual variability for sexual difference.

I go on working at sexual selection, and, though never idle, I am able to do so little work each day that I make very slow progress. I knew from Azara about the young of the tapir being striped, and about young deer being spotted (432/2. Fritz Muller's views are discussed in the "Descent of Man," Edition II., Volume II., page 305.); I have often reflected on this subject, and know not what to conclude about the loss of the stripes and spots. From the geographical distribution of the striped and unstriped species of Equus there seems to be something very mysterious about the loss of stripes; and I cannot persuade myself that the common ass has lost its stripes owing to being rendered more conspicuous from having stripes and thus exposed to danger.

LETTER 433. TO J. JENNER WEIR.

(433/1. Mr. John Jenner Weir, to whom the following letters are addressed, is frequently quoted in the "Descent of Man" as having supplied Mr. Darwin with information on a variety of subjects.)

Down, February 27th {1868}.

I must thank you for your paper on apterous lepidoptera (433/2. Published by the West Kent Natural History, Microscopical and Photographic Society, Greenwich, 1867. Mr. Weir's paper seems chiefly to have interested Mr. Darwin as affording a good case of gradation in the degree of degradation of the wings in various species.), which has interested me exceedingly, and likewise for the very honourable mention which you make of my name. It is almost a pity that your paper was not published in some Journal in which it would have had a wider distribution. It contained much that was new to me. I think the part about the relation of the wings and spiracles and tracheae might have been made a little clearer. Incidentally, you have done me a good service by reminding me of the rudimentary spurs on the legs of the partridge, for I am now writing on what I have called sexual selection. I believe that I am not mistaken in thinking that you have attended much to birds in confinement, as well as to insects. If you could call to mind any facts bearing on this subject, with birds, insects, or any animals—such as the selection by a female of any particular male—or conversely of a particular female by a male, or on the rivalry between males, or on the allurement of the females by the males, or any such facts, I should be most grateful for the information, if you would have the kindness to communicate it.

P.S.—I may give as instance of {this} class of facts, that Barrow asserts that a male Emberiza (?) at the Cape has immensely long tail-feathers during the breeding season (433/3. Barrow describes the long tail feathers of Emberiza longicauda as enduring "but the season of love." "An Account of Travels into the Interior of Southern Africa": London, 1801, Volume I., page 244.); and that if these are cut off, he has no chance of getting a wife. I have always felt an intense wish to make analogous trials, but have never had an opportunity, and it is not likely that you or any one would be willing to try so troublesome an experiment. Colouring or staining the fine red breast of a bullfinch with some innocuous matter into a dingy tint would be an analogous case, and then putting him and ordinary males with a female. A friend promised, but failed, to try a converse experiment with white pigeons—viz., to stain their tails and wings with magenta or other colours, and then observe what effect such a prodigious alteration would have on their courtship. (433/4. See Letter 428.) It would be a fairer trial to cut off the eyes of the tail-feathers of male peacocks; but who would sacrifice the beauty of their bird for a whole season to please a mere naturalist?

LETTER 434. TO J. JENNER WEIR. Down, February 29th {1868}.

I have hardly ever received a note which has interested me more than your last; and this is no exaggeration. I had a few cases of birds perceiving slight changes in the dress of their owners, but your facts are of tenfold value. I shall certainly make use of them, and need not say how much obliged I should be for any others about which you feel confident.

Do you know of any birds besides some of the gallinaceae which are polygamous? Do you know of any birds besides pigeons, and, as it is said, the raven, which pair for their whole lives?

Many years ago I visited your brother, who showed me his pigeons and gave me some valuable information. Could you persuade him (but I fear he would think it high treason) to stain a male pigeon some brilliant colour, and observe whether it excited in the other pigeons, especially the females, admiration or contempt?

For the chance of your liking to have a copy and being able to find some parts which would interest you, I have directed Mr. Murray to send you my recent book on "Variation under Domestication."

P.S.—I have somewhere safe references to cases of magpies, of which one of a pair has been repeatedly (I think seven times) killed, and yet another mate was always immediately found. (434/1. On this subject see "Descent of Man," Edition I., Volume II., page 104, where Mr. Weir's observations were made use of. This statement is quoted from Jenner ("Phil. Trans." 1824) in the "Descent of Man" (1901), page 620.) A gamekeeper told me yesterday of analogous case. This perplexes me much. Are there many unmarried birds? I can hardly believe it. Or will one of a pair, of which the nest has been robbed, or which are barren, always desert his or her mate for a strange mate with the attraction of a nest, and in one instance with young birds in the nest? The gamekeeper said during breeding season he had never observed a single or unpaired partridge. How can the sexes be so equally matched?

P.S. 2nd.—I fear you will find me a great bore, but I will be as reasonable as can be expected in plundering one so rich as you.

P.S. 3rd.—I have just received a letter from Dr. Wallace (434/2. See "Descent of Man," Edition I., Volume I., pages 386-401, where Dr. Wallace's observations are quoted.), of Colchester, about the proportional numbers of the two sexes in Bombyx; and in this note, apropos to an incidental remark of mine, he stoutly maintains that female lepidoptera never notice the colours or appearance of the male, but always receive the first male which comes; and this appears very probable. He says he has often seen fine females receive old battered and pale-tinted males. I shall have to admit this very great objection to sexual selection in insects. His observations no doubt apply to English lepidoptera, in most of which the sexes are alike. The brimstone or orange-tip would be good to observe in this respect, but it is hopelessly difficult. I think I have often seen several males following one female; and what decides which male shall succeed? How is this about several males; is it not so?

LETTER 435. TO J. JENNER WEIR. 6, Queen Anne Street, Cavendish Square, W. {March 6th, 1868}.

I have come here for a few weeks, for a little change and rest. Just as I was leaving home I received your first note, and yesterday a second; and both are most interesting and valuable to me. That is a very curious observation about the goldfinch's beak (435/1.

"Descent of Man," Edition I., Volume I., page 39. Mr. Weir is quoted as saying that the birdcatchers can distinguish the males of the goldfinch, Carduelis elegans, by their "slightly longer beaks."), but one would hardly like to trust it without measurement or comparison of the beaks of several male and female birds; for I do not understand that you yourself assert that the beak of the male is sensibly longer than that of the female. If you come across any acute birdcatchers (I do not mean to ask you to go after them), I wish you would ask what is their impression on the relative numbers of the sexes of any birds which they habitually catch, and whether some years males are more numerous and some years females. I see that I must trust to analogy (an unsafe support) for sexual selection in regard to colour in butterflies. You speak of the brimstone butterfly and genus Edusa (435/2. Colias Edusa.) (I forget what this is, and have no books here, unless it is Colias) not opening their wings. In one of my notes to Mr. Stainton I asked him (but he could or did not answer) whether butterflies such as the Fritillaries, with wings bright beneath and above, opened and shut their wings more than Vanessae, most of which, I think, are obscure on the under surface. That is a most curious observation about the red underwing moth and the robin (435/3. "Descent of Man," Edition I., Volume I., page 395. Mr. Weir describes the pursuit of a red-underwing, Triphoena pronuba, by a robin which was attracted by the bright colour of the moth, and constantly missed the insect by breaking pieces off the wing instead of seizing the body. Mr. Wallace's facts are given on the same page.), and strongly supports a suggestion (which I thought hardly credible) of A.R. Wallace, viz. that the immense wings of some exotic lepidoptera served as a protection from difficulty of birds seizing them. I will probably quote your case.

No doubt Dr. Hooker collected the Kerguelen moth, for I remember he told me of the case when I suggested in the "Origin," the explanation of the coleoptera of Madeira being apterous; but he did not know what had become of the specimens.

I am quite delighted to hear that you are observing coloured birds (435/4. "Descent of Man," Edition I., Volume II., page 110.), though the probability, I suppose, will be that no sure result will be gained. I am accustomed with my numerous experiments with plants to be well satisfied if I get any good result in one case out of five.

You will not be able to read all my book—too much detail. Some of the chapters in the second volume are curious, I think. If any man wants to gain a good opinion of his fellow-men, he ought to do what I am doing, pester them with letters.

LETTER 436. TO J. JENNER WEIR. 4, Chester Place, Regent's Park, N.W., March 13th {1868}.

You make a very great mistake when you speak of "the risk of your notes boring me." They are of the utmost value to me, and I am sure I shall never be tired of receiving them; but I must not be unreasonable. I shall give almost all the facts which you have mentioned in your two last notes, as well as in the previous ones; and my only difficulty will be not to give too much and weary my readers. Your last note is especially valuable about birds displaying the beautiful parts of their plumage. Audubon (436/1. In his "Ornithological Biography," 5 volumes, Edinburgh, 1831-49.) gives a good many facts about the antics of birds during courtship, but nothing nearly so much to the purpose as yours. I shall never be able to resist giving the whole substance of your last note. It is quite a new light to me, except with the peacock and Bird of Paradise. I must now look to turkey's wings; but I do not think that their wings are beautiful when opened during courtship. Its tail is finely banded. How about the drake and Gallus bankiva? I forget how their wings look when expanded. Your facts are all the more valuable as I now clearly see that for butterflies I must trust to analogy altogether in regard to sexual selection. But I think I shall make out a strong case (as far as the rather deceitful guide of analogy will serve) in the sexes of butterflies being alike or differing greatly—in moths which do not display the lower surface of their wings not having them gaudily coloured, etc., etc.—nocturnal moths, etc.—and in some male insects fighting for the females, and attracting them by music.

My discussion on sexual selection will be a curious one—a mere dovetailing of information derived from you, Bates, Wallace, etc., etc., etc.

We remain at above address all this month, and then return home. In the summer, could I persuade you to pay us a visit of a day or two, and I would try and get Bates and some others to come down?

But my health is so precarious, I can ask no one who will not allow me the privilege of a poor old invalid; for talking, I find by long and dear-bought experience, tries my head more than anything, and I am utterly incapable of talking more than half an hour, except on rare occasions.

I fear this note is very badly written; but I was very ill all yesterday, and my hand shakes to-day.

LETTER 437. TO J. JENNER WEIR. 4, Chester Place, Regent's Park, N.W., March 22nd {1868}.

I hope that you will not think me ungrateful that I have not sooner answered your note of the 16th; but in fact I have been overwhelmed both with calls and letters; and, alas! one visit to the British Museum of an hour or hour and a half does for me for the whole day.

I was particularly glad to hear your and your brother's statement about the "gay" deceiver-pigeons. (437/1. Some cock pigeons "called by our English fanciers gay birds are so successful in their gallantries that, as Mr. H. Weir informs me, they must be shut up, on account of the mischief which they cause.") I did not at all know that certain birds could win the affections of the females more than other males, except, indeed, in the case of the peacock. Conversely, Mr. Hewitt, I remember, states that in making hybrids the cock pheasant would prefer certain hen fowls and strongly dislike others. I will write to Mr. H. in a few days, and ask him whether he has observed anything of this kind with pure unions of fowls, ducks, etc. I had utterly forgotten the case of the ruff (437/2. The ruff, Machetes pugnax, was believed by Montague to be polygamous. "Descent of Man," Edition I., Volume I., page 270.), but now I remember having heard that it was polygamous; but polygamy with birds, at least, does not seem common enough to have played an important part. So little is known of habits of foreign birds: Wallace does not even know whether Birds of Paradise are polygamous. Have you been a large collector of caterpillars? I believe so. I inferred from a letter from Dr. Wallace, of Colchester, that he would account for Mr. Stainton and others rearing more female than male by their having collected the larger and finer caterpillars. But I misunderstood him, and he maintains that collectors take all caterpillars, large and small, for that they collect the caterpillars alone of the rarer moths or butterflies. What think you? I hear from Professor Canestrini (437/3. See "Descent of Man" (1901), page 385.) in Italy that females are born in considerable excess with Bombyx mori, and in greater excess of late years than formerly! Quatrefages writes to me that he believes they are equal in France. So that the farther I go the deeper I sink into the mire. With cordial thanks for your most valuable letters.

We remain here till April 1st, and then hurrah for home and quiet work.

LETTER 438. TO J. JENNER WEIR. 4, Chester Place, N.W., March 27th {1868}.

I hardly know which of your three last letters has interested me most. What splendid work I shall have hereafter in selecting and arranging all your facts. Your last letter is most curious—all about the bird-catchers—and interested us all. I suppose the male chaffinch in "pegging" approaches the captive singing-bird, from rivalry or jealousy—if I am wrong please tell me; otherwise I will assume so. Can you form any theory about all the many cases which you have given me, and others which have been published, of when one {of a} pair is killed, another soon appearing? Your fact about the bullfinches in your garden is most curious on this head. (438/1. Mr. Weir stated that at Blackheath he never saw or heard a wild bullfinch, yet when one of his caged males died, a wild one in the course of a few days generally came and perched near the widowed female, whose call-note is not loud. "Descent of Man" (1901), page 623.) Are there everywhere many unpaired birds? What can the explanation be?

Mr. Gould assures me that all the nightingales which first come over are males, and he believes this is so with other migratory birds. But this does not agree with what the bird-catchers say about the common linnet, which I suppose migrates within the limits of England.

Many thanks for very curious case of Pavo nigripennis. (438/2. See "Animals and Plants," Edition II., Volume I., page 306.) I am very glad to get additional evidence. I have sent your fact to be inserted, if not too late, in four foreign editions which are now printing. I am delighted to hear that you approve of my book; I thought every mortal man would find the details very tedious, and have often repented of giving so many. You will find pangenesis stiff reading, and I fear will shake your head in disapproval. Wallace sticks up for the great god Pan like a man.

The fertility of hybrid canaries would be a fine subject for careful investigation.

LETTER 439. TO J. JENNER WEIR. Down, April 4th {1868}.

I read over your last ten (!) letters this morning, and made an index of their contents for easy reference; and what a mine of wealth you have bestowed on me. I am glad you will publish yourself on gay-coloured caterpillars and birds (439/1. See "Descent of Man," Edition I., Volume I., page 417, where Mr. Weir's experiments are given; they were made to test Mr. Wallace's theory that caterpillars, which are protected against birds by an unpleasant taste, have been rendered conspicuous, so that they are easily recognised. They thus escape being pecked or tasted, which to soft-skinned animals would be as fatal as being devoured. See Mr. Jenner Weir's papers, "Transact. Entomolog. Soc." 1869, page 2; 1870, page 337. In regard to one of these papers Mr. Darwin wrote (May 13th, 1869): "Your verification of Wallace's suggestion seems to me to amount to quite a discovery."); it seems to me much the best plan; therefore, I will not forward your letter to Mr. Wallace. I was much in the Zoological Gardens during my month in London, and picked up what scraps of knowledge I could. Without my having mentioned your most interesting observations on the display of the Fringillidae (439/2. "Descent of Man" (1901), page 738.), Mr. Bartlett told me how the Gold Pheasant erects his collar and turns from side to side, displaying it to the hen. He has offered to give me notes on the display of all Gallinaceae with which he is acquainted; but he is so busy a man that I rather doubt whether he will ever do so.

I received about a week ago a remarkably kind letter from your brother, and I am sorry to hear that he suffers much in health. He gave me some fine facts about a Dun Hen Carrier which would never pair with a bird of any other colour. He told me, also, of some one at Lewes who paints his dog! and will inquire about it. By the way, Mr. Trimen tells me that as a boy he used to paint butterflies, and that they long haunted the same place, but he made no further observations on them. As far as colour is concerned, I see I shall have to trust to mere inference from the males displaying their plumage, and other analogous facts. I shall get no direct evidence of the preference of the hens. Mr. Hewitt, of Birmingham, tells me that the common hen prefers a salacious cock, but is quite indifferent to colour.

Will you consider and kindly give me your opinion on the two following points. Do very vigorous and well-nourished hens receive the male earlier in the spring than weaker or poorer hens? I suppose that they do. Secondly, do you suppose that the birds which pair first in the season have any advantage in rearing numerous and healthy offspring over those which pair later in the season? With respect to the mysterious cases of which you have given me so many, in addition to those previously collected, of when one bird of a pair is shot another immediately supplying its place, I was drawing to the conclusion that there must be in each district several unpaired birds; yet this seems very improbable. You allude, also, to the unknown causes which keep down the numbers of birds; and often and often have I marvelled over this subject with respect to many animals.

LETTER 440. TO A.R. WALLACE.

(440/1. The following refers to Mr. Wallace's article "A Theory of Birds' Nests," in Andrew Murray's "Journal of Travel," Volume I., page 73. He here treats in fuller detail the view already published in the "Westminster Review," July 1867, page 38. The rule which Mr. Wallace believes, with very few exceptions, to hold good is, "that when both sexes are of strikingly gay and conspicuous colours, the nest is...such as to conceal the sitting bird; while, whenever there is a striking contrast of colours, the male being gay and conspicuous, the female dull and obscure, the nest is open, and the sitting bird exposed to view." At this time Mr. Wallace allowed considerably more influence to sexual selection (in combination with the need of protection) than in his later writings. The following extract from a letter from Mr. Wallace to Darwin (July 23rd, 1877) fixes the period at which the change in his views occurred: "I am almost afraid to tell you that in going over the subject of the colours of animals, etc., etc., for a small volume of essays, etc., I am preparing, I have come to conclusions directly opposed to voluntary sexual selection, and believe that I can explain (in a general way) all the phenomena of sexual ornaments and colours by laws of development aided by simple 'Natural Selection.'" He finally rejected Mr. Darwin's theory that colours "have been developed by the preference of the females, the more ornamented males becoming the parents of each successive generation." "Darwinism," 1889, page 285. See also Letters 442, 443, 449, 450, etc.)

Down, April 15th, {1868}.

I have been deeply interested by your admirable article on birds' nests. I am delighted to see that we really differ very little,—not more than two men almost always will. You do not lay much or any stress on new characters spontaneously appearing in one sex (generally the male), and being transmitted exclusively, or more commonly only in excess, to that sex. I, on the other hand, formerly paid far too little attention to protection. I had only a glimpse of the truth; but even now I do not go quite as far as you. I cannot avoid thinking rather more than you do about the exceptions in nesting to the rule, especially the partial exceptions, i.e., when there is some little difference between the sexes in species which build concealed nests. I am not quite satisfied about the incubating males; there is so little difference in conspicuousness between the sexes. I wish with all my heart I could go the whole length with you. You seem to think that male birds probably select the most beautiful females; I must feel some doubt on this head, for I can find no evidence of it. Though I am writing so carping a note, I admire the article thoroughly.

And now I want to ask a question. When female butterflies are more brilliant than their males you believe that they have in most cases, or in all cases, been rendered brilliant so as to mimic some other species, and thus escape danger. But can you account for the males not having been rendered equally brilliant and equally protected? (440/2. See Wallace in the "Westminster Review," July, 1867, page 37, on the protection to the female insect afforded by its resemblance either to an inanimate object or to another insect protected by its unpalatableness. The cases are discussed in relation to the much greater importance (to the species as a whole) of the preservation of the female insect with her load of eggs than the male who may safely be sacrificed after pairing. See Letter 189, note.) Although it may be most for the welfare of the species that the female should be protected, yet it would be some advantage, certainly no disadvantage, for the unfortunate male to enjoy an equal immunity from danger. For my part, I should say that the female alone had happened to vary in the right manner, and that the beneficial variations had been transmitted to the same sex alone. Believing in this, I can see no improbability (but from analogy of domestic animals a strong probability) that variations leading to beauty must often have occurred in the males alone, and been transmitted to that sex alone. Thus I should account in many cases for the greater beauty of the male over the female, without the need of the protective principle. I should be grateful for an answer on the point.

LETTER 441. TO J. JENNER WEIR. Down, April 18th {1868}.

You see that I have taken you at your word, and have not (owing to heaps of stupid letters) earlier noticed your three last letters, which as usual are rich in facts. Your letters make almost a little volume on my table. I daresay you hardly knew yourself how much curious information was lying in your mind till I began the severe pumping process. The case of the starling married thrice in one day is capital, and beats the case of the magpies of which one was shot seven times consecutively. A gamekeeper here tells me that he has repeatedly shot one of a pair of jays, and it has always been immediately replaced. I begin to think that the pairing of birds must be as delicate and tedious an operation as the pairing of young gentlemen and ladies. If I can convince myself that there are habitually many unpaired birds, it will be a great aid to me in sexual selection, about which I have lately had many troubles, and am therefore rejoiced to hear in your last note that your faith keeps staunch. That is a curious fact about the bullfinches all appearing to listen to the German singer (441/1. See Letter 445, note.); and this leads me to ask how much faith may I put in the statement that male birds will sing in rivalry until they injure themselves. Yarrell formerly told me that they would sometimes even sing themselves to death. I am sorry to hear that the painted bullfinch turns out to be a female; though she has done us a good turn in exhibiting her jealousy, of which I had no idea.

Thank you for telling me about the wildness of the hybrid canaries: nothing has hardly ever surprised me more than the many cases of reversion from crossing. Do you not think it a very curious subject? I have not heard from Mr. Bartlett about the Gallinaceae, and I daresay I never shall. He told me about the Tragopan, and he is positive that the blue wattle becomes gorged with blood, and not air.

Returning to the first of the last three letters. It is most curious the number of persons of the name of Jenner who have had a strong taste for Natural History. It is a pity you cannot trace your connection with the great Jenner, for a duke might be proud of his blood.

I heard lately from Professor Rolleston of the inherited effects of an injury in the same eye. Is the scar on your son's leg on the same side and on exactly the same spot where you were wounded? And did the wound suppurate, or heal by the first intention? I cannot persuade myself of the truth of the common belief of the influence of the mother's imagination on the child. A point just occurs to me (though it does not at present concern me) about birds' nests. Have you read Wallace's recent articles? (441/2. A full discussion of Mr. Wallace's views is given in "Descent of Man," Edition I., Volume II., Chapter XV. Briefly, Mr. Wallace's point is that the dull colour of the female bird is protective by rendering her inconspicuous during incubation. Thus the relatively bright colour of the male would not simply depend on sexual selection, but also on the hen being "saved, through Natural Selection, from acquiring the conspicuous colours of the male" (loc. cit., page 155).) I always distrust myself when I differ from him; but I cannot admit that birds learn to make their nests from having seen them whilst young. I must think it as true an instinct as that which leads a caterpillar to suspend its cocoon in a particular manner. Have you had any experience of birds hatched under a foster-mother making their nests in the proper manner? I cannot thank you enough for all your kindness.

LETTER 442. TO A.R. WALLACE.

(442/1. Dr. Clifford Allbutt's view probably had reference to the fact that the sperm-cell goes, or is carried, to the germ-cell, never vice versa. In this letter Darwin gives the reason for the "law" referred to. Mr. A.R. Wallace has been good enough to give us the following note:—"It was at this time that my paper on 'Protective Resemblance' first appeared in the 'Westminster Review,' in which I adduced the greater, or rather, the more continuous, importance of the female (in the lower animals) for the race, and my 'Theory of Birds' Nests' ('Journal of Travel and Natural History,' No. 2) in which I applied this to the usually dull colours of female butterflies and birds. It is to these articles as well as to my letters that Darwin chiefly refers."—Note by Mr. Wallace, May 27th, 1902.)

Down, April 30th {1868}.

Your letter, like so many previous ones, has interested me much. Dr. Allbutt's view occurred to me some time ago, and I have written a short discussion on it. It is, I think, a remarkable law, to which I have found no exception. The foundation lies in the fact that in many cases the eggs or seeds require nourishment and protection by the mother-form for some time after impregnation. Hence the spermatozoa and antherozoids travel in the lower aquatic animals and plants to the female, and pollen is borne to the female organ. As organisms rise in the scale it seems natural that the male should carry the spermatozoa to the female in his own body. As the male is the searcher, he has required and gained more eager passions than the female; and, very differently from you, I look at this as one great difficulty in believing that the males select the more attractive females; as far as I can discover, they are always ready to seize on any female, and sometimes on many females. Nothing would please me more than to find evidence of males selecting the more attractive females. I have for months been trying to persuade myself of this. There is the case of man in favour of this belief, and I know in hybrid unions of males preferring particular females, but, alas, not guided by colour. Perhaps I may get more evidence as I wade through my twenty years' mass of notes.

I am not shaken about the female protected butterflies. I will grant (only for argument) that the life of the male is of very little value,—I will grant that the males do not vary, yet why has not the protective beauty of the female been transferred by inheritance to the male? The beauty would be a gain to the male, as far as we can see, as a protection; and I cannot believe that it would be repulsive to the female as she became beautiful. But we shall never convince each other. I sometimes marvel how truth progresses, so difficult is it for one man to convince another, unless his mind is vacant. Nevertheless, I myself to a certain extent contradict my own remark, for I believe far more in the importance of protection than I did before reading your articles.

I do not think you lay nearly stress enough in your articles on what you admit in your letters: viz., "there seems to be some production of vividness...of colour in the male independent of protection." This I am making a chief point; and have come to your conclusion so far that I believe that intense colouring in the female sex is often checked by being dangerous.

That is an excellent remark of yours about no known case of male alone assuming protective colours; but in the cases in which protection has been gained by dull colours, I presume that sexual selection would interfere with the male losing his beauty. If the male alone had acquired beauty as a protection, it would be most readily overlooked, as males are so often more beautiful than their females. Moreover, I grant that the life of the male is somewhat less precious, and thus there would be less rigorous selection with the male, so he would be less likely to be made beautiful through Natural Selection for protection. (442/2. This does not apply to sexual selection, for the greater the excess of males, and the less precious their lives, so much the better for sexual selection. {Note in original.}) But it seems to me a good argument, and very good if it could be thoroughly established. I do not know whether you will care to read this scrawl.

LETTER 443. TO A.R. WALLACE. Down, May 5th {1868?}.

I am afraid I have caused you a great deal of trouble in writing to me at such length. I am glad to say that I agree almost entirely with your summary, except that I should put sexual selection as an equal, or perhaps as even a more important agent in giving colour than Natural Selection for protection. As I get on in my work I hope to get clearer and more decided ideas. Working up from the bottom of the scale, I have as yet only got to fishes. What I rather object to in your articles is that I do not think any one would infer from them that you place sexual selection even as high as No. 4 in your summary. It was very natural that you should give only a line to sexual selection in the summary to the "Westminster Review," but the result at first to my mind was that you attributed hardly anything to its power. In your penultimate note you say "in the great mass of cases in which there is great differentiation of colour between the sexes, I believe it is due almost wholly to the need of protection to the female." Now, looking to the whole animal kingdom, I can at present by no means admit this view; but pray do not suppose that because I differ to a certain extent, I do not thoroughly admire your several papers and your admirable generalisation on birds' nests. With respect to this latter point, however, although, following you, I suspect that I shall ultimately look at the whole case from a rather different point of view.

You ask what I think about the gay-coloured females of Pieris. (443/1. See "Westminster Review," July, 1867, page 37; also Letter 440.) I believe I quite follow you in believing that the colours are wholly due to mimicry; and I further believe that the male is not brilliant from not having received through inheritance colour from the female, and from not himself having varied; in short, that he has not been influenced by selection.

I can make no answer with respect to the elephants. With respect to the female reindeer, I have hitherto looked at the horns simply as the consequence of inheritance not having been limited by sex.

Your idea about colour being concentrated in the smaller males seems good, and I presume that you will not object to my giving it as your suggestion.

LETTER 444. TO J. JENNER WEIR. Down, May 7th {1868}.

I have now to thank you for no less than four letters! You are so kind that I will not apologise for the trouble I cause you; but it has lately occurred to me that you ought to publish a paper or book on the habits of the birds which you have so carefully observed. But should you do this, I do not think that my giving some of the facts for a special object would much injure the novelty of your work.

There is such a multitude of points in these last letters that I hardly know what to touch upon. Thanks about the instinct of nidification, and for your answers on many points. I am glad to hear reports about the ferocious female bullfinch. I hope you will have another try in colouring males. I have now finished lepidoptera, and have used your facts about caterpillars, and as a caution the case of the yellow-underwings. I have now begun on fishes, and by comparing different classes of facts my views are getting a little more decided. In about a fortnight or three weeks I shall come to birds, and then I dare say that I shall be extra troublesome. I will now enclose a few queries for the mere chance of your being able to answer some of them, and I think it will save you trouble if I write them on a separate slip, and then you can sometimes answer by a mere "no" or "yes."

Your last letter on male pigeons and linnets has interested me much, for the precise facts which you have given me on display are of the utmost value for my work. I have written to Mr. Bartlett on Gallinaceae, but I dare say I shall not get an answer. I had heard before, but am glad to have confirmation about the ruffs being the most numerous. I am greatly obliged to your brother for sending out circulars. I have not heard from him as yet. I want to ask him whether he has ever observed when several male pigeons are courting one female that the latter decides with which male she will pair. The story about the black mark on the lambs must be a hoax. The inaccuracy of many persons is wonderful. I should like to tell you a story, but it is too long, about beans growing on the wrong side of the pod during certain years.

Queries:

Does any female bird regularly sing?

Do you know any case of both sexes, more especially of the female, {being} more brightly coloured whilst young than when come to maturity and fit to breed? An imaginary instance would be if the female kingfisher (or male) became dull coloured when adult.

Do you know whether the male and female wild canary bird differ in plumage (though I believe I could find this out for myself), and do any of the domestic breeds differ sexually?

Do you know any gallinaceous bird in which the female has well developed spurs?

It is very odd that my memory should fail me, but I cannot remember whether, in accordance with your views, the wing of Gallus bankiva (or Game-Cock, which is so like the wild) is ornamental when he opens and scrapes it before the female. I fear it is not; but though I have often looked at wing of the wild and tame bird, I cannot call to mind the exact colours. What a number of points you have attended to; I did not know that you were a horticulturist. I have often marvelled at the different growth of the flowering and creeping branches of the ivy; but had no idea that they kept their character when propagated by cuttings. There is a S. American genus (name forgotten just now) which differs in an analogous manner but even greater degree, but it is difficult to cultivate in our hot-house. I have tried and failed.

LETTER 445. TO J. JENNER WEIR. Down, May 30th {1868}.

I am glad to hear your opinion on the nest-making instinct, for I am Tory enough not to like to give up all old beliefs. Wallace's view (445/1. See Letter 440, etc.) is also opposed to a great mass of analogical facts. The cases which you mention of suddenly reacquired wildness seem curious. I have also to thank you for a previous valuable letter. With respect to spurs on female Gallinaceae, I applied to Mr. Blyth, who has wonderful systematic knowledge, and he tells me that the female Pavo muticus and Fire-back pheasants are spurred. From various interruptions I get on very slowly with my Bird MS., but have already often and often referred to your volume of letters, and have used various facts, and shall use many more. And now I am ashamed to say that I have more questions to ask; but I forget—you told me not to apologise.

1. In your letter of April 14th you mention the case of about twenty birds which seemed to listen with much interest to an excellent piping bullfinch. (445/2. Quoted in the "Descent of Man" (1901), page 564. "A bullfinch which had been taught to pipe a German waltz...when this bird was first introduced into a room where other birds were kept and he began to sing, all the others, consisting of about twenty linnets and canaries, ranged themselves on the nearest side of their cages, and listened with the greatest

interest to the new performer.") What kind of birds were these twenty?

2. Is it true, as often stated, that a bird reared by foster-parents, and who has never heard the song of its own species, imitates to a certain extent the song of the species which it may be in the habit of hearing?

Now for a more troublesome point. I find it very necessary to make out relation of immature plumage to adult plumage, both when the sexes differ and are alike in the adult state. Therefore, I want much to learn about the first plumage (answering, for instance, to the speckled state of the robin before it acquires the red breast) of the several varieties of the canary. Can you help me? What is the character or colour of the first plumage of bright yellow or mealy canaries which breed true to these tints? So with the mottled-brown canaries, for I believe that there are breeds which always come brown and mottled. Lastly, in the "prize-canaries," which have black wing- and tail-feathers during their first (?) plumage, what colours are the wings and tails after the first (?) moult or when adult? I should be particularly glad to learn this. Heaven have mercy on you, for it is clear that I have none. I am going to investigate this same point with all the breeds of fowls, as Mr. Tegetmeier will procure for me young birds, about two months old, of all the breeds.

In the course of this next month I hope you will come down here on the Saturday and stay over the Sunday. Some months ago Mr. Bates said he would pay me a visit during June, and I have thought it would be pleasanter for you to come here when I can get him, so that you would have a companion if I get knocked up, as is sadly too often my bad habit and great misfortune.

Did you ever hear of the existence of any sub-breed of the canary in which the male differs in plumage from the female?

LETTER 446. TO F. MULLER. Down, June 3rd {1868}.

Your letter of April 22nd has much interested me. I am delighted that you approve of my book, for I value your opinion more than that of almost any one. I have yet hopes that you will think well of pangenesis. I feel sure that our minds are somewhat alike, and I find it a great relief to have some definite, though hypothetical view, when I reflect on the wonderful transformations of animals, the re-growth of parts, and especially the direct action of pollen on the mother form, etc. It often appears to me almost certain that the characters of the parents are "photographed" on the child, only by means of material atoms derived from each cell in both parents, and developed in the child. I am sorry about the mistake in regard to Leptotes. (446/1. See "Animals and Plants," Edition I., Volume II., page 134, where it is stated that Oncidium is fertile with Leptotes, a mistake corrected in the 2nd edition.) I daresay it was my fault, yet I took pains to avoid such blunders. Many thanks for all the curious facts about the unequal number of the sexes in crustacea, but the more I investigate this subject the deeper I sink in doubt and difficulty. Thanks, also, for the confirmation of the rivalry of Cicadae. (446/2. See "Descent of Man," Edition I., Volume I., page 351, for F. Muller's observations; and for a reference to Landois' paper.) I have often reflected with surprise on the diversity of the means for producing music with insects, and still more with birds. We thus get a high idea of the importance of song in the animal kingdom. Please to tell me where I can find any account of the auditory organs in the orthoptera? Your facts are quite new to me. Scudder has described an annectant insect in Devonian strata, furnished with a stridulating apparatus. (446/3. The insect is no doubt Xenoneura antiquorum, from the Devonian rocks of New Brunswick. Scudder compared a peculiar feature in the wing of this species to the stridulating apparatus of the Locustariae, but afterwards stated that he had been led astray in his original description, and that there was no evidence in support of the comparison with a stridulating organ. See the "Devonian Insects of New Brunswick," reprinted in S.H. Scudder's "Fossil Insects of N. America," Volume I., page 179, New York, 1890.) I believe he is to be trusted, and if so the apparatus is of astonishing antiquity. After reading Landois' paper I have been working at the stridulating organ in the lamellicorn beetles, in expectation of finding it sexual, but I have only found it as yet in two cases, and in these it was equally developed in both sexes. I wish you would look at any of your common lamellicorns and take hold of both males and females and observe whether they make the squeaking or grating noise equally. If they do not, you could perhaps send me a male and female in a light little box. How curious it is that there should be a special organ for an object apparently so unimportant as squeaking. Here is another point: have you any Toucans? if so, ask any trustworthy hunter whether the beaks of the males, or of both sexes, are more brightly coloured during the breeding season than at other times of the year? I have also to thank you for a previous letter of April 3rd, with some interesting facts on the variation of maize, the sterility of Bignonia and on conspicuous seeds. Heaven knows whether I shall ever live to make use of half the valuable facts which you have communicated to me...

LETTER 447. TO J. JENNER WEIR. Down, June 18th {1868}.

Many thanks. I am glad that you mentioned the linnet, for I had much difficulty in persuading myself that the crimson breast could be due to change in the old feathers, as the books say. I am glad to hear of the retribution of the wicked old she-bullfinch. You remember telling me how many Weirs and Jenners have been naturalists; now this morning I have been putting together all my references about one bird of a pair being killed, and a new mate being soon found; you, Jenner Weir, have given me some most striking cases with starlings; Dr. Jenner gives the most curious case of all in "Philosophical Transactions" (447/1. "Phil. Trans." 1824.), and a Mr. Weir gives the next most striking in Macgillivray. (447/2. Macgillivray's "History of British Birds," Volume I., page 570. See "Descent of Man" (1901), page 621.) Now, is this not odd? Pray remember how very glad we shall be to see you here whenever you can come.

Did some ancient progenitor of the Weirs and Jenners puzzle his brains about the mating of birds, and has the question become indelibly fixed in all your minds?

LETTER 448. TO A.R. WALLACE. August 19th {1868}.

I had become, before my nine weeks' horrid interruption of all work, extremely interested in sexual selection, and was making fair progress. In truth it has vexed me much to find that the farther I get on the more I differ from you about the females being dull-coloured for protection. I can now hardly express myself as strongly, even, as in the "Origin." This has much decreased the pleasure of my work. In the course of September, if I can get at all stronger, I hope to get

Mr. J. Jenner Weir (who has been wonderfully kind in giving me information) to pay me a visit, and I will then write for the chance of your being able to come, and I hope bring with you Mrs. Wallace. If I could get several of you together it would be less dull for you, for of late I have found it impossible to talk with any human being for more than half an hour, except on extraordinary good days.

(448/1. On September 16th Darwin wrote to Wallace on the same subject:—)

You will be pleased to hear that I am undergoing severe distress about protection and sexual selection; this morning I oscillated with joy towards you; this evening I have swung back to the old position, out of which I fear I shall never get.

LETTER 449. TO A.R. WALLACE.

(449/1. From "Life and Letters," Volume III., page 123.)

Down, September 23rd {1868}.

I am very much obliged for all your trouble in writing me your long letter, which I will keep by me and ponder over. To answer it would require at least 200 folio pages! If you could see how often I have rewritten some pages you would know how anxious I am to arrive as near as I can to the truth. I lay great stress on what I know takes place under domestication; I think we start with different fundamental notions on inheritance. I find it is most difficult, but not, I think, impossible to see how, for instance, a few red feathers appearing on the head of a male bird, and which are at first transmitted to both sexes, would come to be transmitted to males alone. It is not enough that females should be produced from the males with red feathers, which should be destitute of red feathers; but these females must have a latent tendency to produce such feathers, otherwise they would cause deterioration in the red head-feathers of their male offspring. Such latent tendency would be shown by their producing the red feathers when old, or diseased in their ovaria. But I have no difficulty in making the whole head red if the few red feathers in the male from the first tended to be sexually transmitted. I am quite willing to admit that the female may have been modified, either at the same time or subsequently, for protection by the accumulation of variations limited in their transmission to the female sex. I owe to your writings the consideration of this latter point. But I cannot yet persuade myself that females alone have often been modified for protection. Should you grudge the trouble briefly to tell me, whether you believe that the plainer head and less bright colours of female chaffinch, the less red on the head and less clean colours of female goldfinch, the much less red on the breast of the female bullfinch, the paler crest of golden-crested wren, etc., have been acquired by them for protection? I cannot think so, any more than I can that the considerable differences between female and male house-sparrow, or much greater brightness of male Parus caeruleus (both of which build under cover) than of female Parus, are related to protection. I even misdoubt much whether the less blackness of female blackbird is for protection.

Again, can you give me reasons for believing that the moderate differences between the female pheasant, the female Gallus bankiva, the female of black grouse, the pea-hen, the female partridge, have all special references to protection under slightly different conditions? I, of course, admit that they are all protected by dull colours, derived, as I think, from some dull-ground progenitor; and I account partly for their difference by partial transference of colour from the male, and by other means too long to specify; but I earnestly wish to see reason to believe that each is specially adapted for concealment to its environment.

I grieve to differ from you, and it actually terrifies me and makes me constantly distrust myself. I fear we shall never quite understand each other. I value the cases of bright-coloured, incubating male fisher, and brilliant female butterflies, solely as showing that one sex may be made brilliant without any necessary transference of beauty to the other sex; for in these cases I cannot suppose that beauty in the other sex was checked by selection.

I fear this letter will trouble you to read it. A very short answer about your belief in regard to the female finches and Gallinaceae would suffice.

LETTER 450. A.R. WALLACE TO CHARLES DARWIN. 9, St. Mark's Crescent, N.W., September 27th, 1868.

Your view seems to be that variations occurring in one sex are transmitted either to that sex exclusively or to both sexes equally, or more rarely partially transferred. But we have every gradation of sexual colours, from total dissimilarity to perfect identity. If this is explained solely by the laws of inheritance, then the colours of one or other sex will be always (in relation to the environment) a matter of chance. I cannot think this. I think selection more powerful than laws of inheritance, of which it makes use, as shown by cases of two, three or four forms of female butterflies, all of which have, I have little doubt, been specialised for protection.

To answer your first question is most difficult, if not impossible, because we have no sufficient evidence in individual cases of slight sexual difference, to determine whether the male alone has acquired his superior brightness by sexual selection, or the female been made duller by need of protection, or whether the two causes have acted. Many of the sexual differences of existing species may be inherited differences from parent forms, which existed under different conditions and had greater or less need of protection.

I think I admitted before, the general tendency (probably) of males to acquire brighter tints. Yet this cannot be universal, for many female birds and quadrupeds have equally bright tints.

To your second question I can reply more decidedly. I do think the females of the Gallinaceae you mention have been modified or been prevented from acquiring the brighter plumage of the male, by need of protection. I know that the Gallus bankiva frequents drier and more open situations than the pea-hen of Java, which is found among grassy and leafy vegetation, corresponding with the colours of the two. So the Argus pheasant, male and female, are, I feel sure, protected by their tints corresponding to the dead leaves of the lofty forest in which they dwell, and the female of the gorgeous fire-back pheasant Lophura viellottii is of a very similar rich brown colour.

I do not, however, at all think the question can be settled by individual cases, but by only large masses of facts. The colours of the mass of female birds seem to me strictly analogous to the colours of both sexes of snipes, woodcocks, plovers, etc., which are undoubtedly protective.

Now, supposing, on your view, that the colours of a male bird become more and more brilliant by sexual selection, and a good deal of that colour is transmitted to the female till it becomes positively injurious to her during incubation, and the race is in danger of extinction; do you not think that all the females who had acquired less of the male's bright colours, or who themselves varied in a protective direction, would be preserved, and that thus a good protective colouring would soon be acquired?

If you admit that this could occur, and can show no good reason why it should not often occur, then we no longer differ, for this is the main point of my view.

Have you ever thought of the red wax-tips of the Bombycilla beautifully imitating the red fructification of lichens used in the nest, and therefore the FEMALES have it too? Yet this is a very sexual-looking character.

If sexes have been differentiated entirely by sexual selection the females can have no relation to environment. But in groups when both sexes require protection during feeding or repose, as snipes, woodcock, ptarmigan, desert birds and animals, green forest birds, etc., arctic birds of prey, and animals, then both sexes are modified for protection. Why should that power entirely cease to act when sexual differentiation exists and when the female requires protection, and why should the colour of so many FEMALE BIRDS seem to be protective, if it has not been made protective by selection.

It is contrary to the principles of "Origin of Species," that colour should have been produced in both sexes by sexual selection and never have been modified to bring the female into harmony with the environment. "Sexual selection is less rigorous than Natural Selection," and will therefore be subordinate to it.

I think the case of female Pieris pyrrha proves that females alone can be greatly modified for protection. (450/1. My latest views on this subject, with many new facts and arguments, will be found in the later editions of my "Darwinism," Chapter X. (A.R.W.))

LETTER 451. A.R. WALLACE TO CHARLES DARWIN.

(451/1. On October 4th, 1868, Mr. Wallace wrote again on the same subject without adding anything of importance to his arguments of September 27th. We give his final remarks:—)

October 4th, 1868.

I am sorry to find that our difference of opinion on this point is a source of anxiety to you. Pray do not let it be so. The truth will come out at last, and our difference may be the means of setting others to work who may set us both right. After all, this question is only an episode (though an important one) in the great question of the "Origin of Species," and whether you or I are right will not at all affect the main doctrine—that is one comfort.

I hope you will publish your treatise on "Sexual Selection" as a separate book as soon as possible; and then, while you are going on with your other work, there will no doubt be found some one to battle with me over your facts on this hard problem.

LETTER 452. TO A.R. WALLACE. Down, October 6th {1868}.

Your letter is very valuable to me, and in every way very kind. I will not inflict a long answer, but only answer your queries. There are breeds (viz. Hamburg) in which both sexes differ much from each other and from both sexes of Gallus bankiva; and both sexes are kept constant by selection. The comb of the Spanish male has been ordered to be upright, and that of Spanish female to lop over, and this has been effected. There are sub-breeds of game fowl, with females very distinct and males almost identical; but this, apparently, is the result of spontaneous variation, without special selection. I am very glad to hear of case of female Birds of Paradise.

I have never in the least doubted possibility of modifying female birds alone for protection, and I have long believed it for butterflies. I have wanted only evidence for the female alone of birds having had their colour modified for protection. But then I believe that the variations by which a female bird or butterfly could get or has got protective colouring have probably from the first been variations limited in their transmission to the female sex. And so with the variations of the male: when the male is more beautiful than the female, I believe the variations were sexually limited in their transmission to the males.

LETTER 453. TO B.D. WALSH. Down, October 31st, 1868.

(453/1. A short account of the Periodical Cicada (C. septendecim) is given by Dr. Sharp in the Cambridge Natural History, Insects II., page 570. We are indebted to Dr. Sharp for calling our attention to Mr. C.L. Marlatt's full account of the insect in "Bulletin No. 14 {NS.} of the U.S. Department of Agriculture," 1898. The Cicada lives for long periods underground as larva and pupa, so that swarms of the adults of one race (septendecim) appear at intervals of 17 years, while those of the southern form or race (tredecim) appear at intervals of 13 years. This fact was first made out by Phares in 1845, but was overlooked or forgotten, and was only re-discovered by Walsh and Riley in 1868, who published a joint paper in the "American Entomologist," Volume I., page 63. Walsh appears to have adhered to the view that the 13- and 17-year forms are distinct species, though, as we gather from Marlatt's paper (page 14), he published a letter to Mr. Darwin in which he speaks of the 13-year form as an incipient species; see "Index to Missouri Entomolog. Reports Bull. 6," U.S.E.C., page 58 (as given by Marlatt). With regard to the cause of the difference in period of the two forms, Marlatt (pages 15, 16) refers doubtfully to difference of temperature as the determining factor. Experiments have been instituted by moving 17-year eggs to the south, and vice versa with 13-year eggs. The results were, however, not known at the time of publication of Marlatt's paper.)

I am very much obliged for the extracts about the "drumming," which will be of real use to me.

I do not at all know what to think of your extraordinary case of the Cicadas. Professor Asa Gray and Dr. Hooker were staying here, and I told them of the facts. They thought that the 13-year and the 17-year forms ought not to be ranked as distinct species, unless other differences besides the period of development could be discovered. They thought the mere rarity of variability in such a point was not sufficient, and I think I concur with them. The fact of both the forms presenting the same case of dimorphism is very curious. I have long wished that some one would dissect the forms of the male stag-beetle with smaller mandibles, and see if they were well developed, i.e., whether there was an abundance of spermatozoa; and the same observations ought, I think, to be made on the rarer form of your Cicada. Could you not get some observer, such as Dr. Hartman (453/2. Mr. Walsh sent Mr. Darwin an extract from Dr. Hartman's "Journal of the doings of a Cicada septendecim," in which the females are described as flocking round the drumming males. "Descent of Man" (1901), page 433.), to note whether the females flocked in equal numbers to the "drumming" of the rarer form as to the common form? You have a very curious and perplexing subject of investigation, and I wish you success in your work.

LETTER 454. TO A.R. WALLACE. Down, June 15th {1869?}.

You must not suppose from my delay that I have not been much interested by your long letter. I write now merely to thank you, and just to say that probably you are right on all the points you touch on, except, as I think, about sexual selection, which I will not give up. My belief in it, however, is contingent on my general belief in sexual selection. It is an awful stretcher to believe that a peacock's tail was thus formed; but, believing it, I believe in the same principle somewhat modified applied to man.

LETTER 455. TO G.H.K. THWAITES. Down, February 13th {N.D.}

I wrote a little time ago asking you an odd question about elephants, and now I am going to ask you an odder. I hope that you will not think me an intolerable bore. It is most improbable that you could get me an answer, but I ask on mere chance. Macacus silenus (455/1. Macacus silenus L., an Indian ape.) has a great mane of hair round neck, and passing into large whiskers and beard. Now what I want most especially to know is whether these monkeys, when they fight in confinement (and I have seen it stated that they are sometimes kept in confinement), are protected from bites by this mane and beard. Any one who watched them fighting would, I think, be able to judge on this head. My object is to find out with various animals how far the mane is of any use, or a mere ornament. Is the male Macacus silenus furnished with longer hair than the female about the neck and face? As I said, it is a hundred or a thousand to one against your finding out any one who has kept these monkeys in confinement.

LETTER 456. TO F. MULLER. Down, August 28th {1870}.

I have to thank you very sincerely for two letters: one of April 25th, containing a very curious account of the structure and morphology of Bonatea. I feel that it is quite a sin that your letters should not all be published! but, in truth, I have no spare strength to undertake any extra work, which, though slight, would follow from seeing your letters in English through the press—not but that you write almost as clearly as any Englishman. This same letter also contained some seeds for Mr. Farrer, which he was very glad to receive.

Your second letter, of July 5th, was chiefly devoted to mimicry in lepidoptera: many of your remarks seem to me so good, that I have forwarded your letter to Mr. Bates; but he is out of London having his summer holiday, and I have not yet heard from him. Your remark about imitators and imitated being of such different sizes, and the lower surface of the wings not being altered in colour, strike me as the most curious points. I should not be at all surprised if your suggestion about sexual selection were to prove true; but it seems rather too speculative to be introduced in my book, more especially as my book is already far too speculative. The very same difficulty about brightly coloured caterpillars had occurred to me, and you will see in my book what, I believe, is the true explanation from Wallace. The same view probably applies in part to gaudy butterflies. My MS. is sent to the printers, and, I suppose, will be published in about three months: of course I will send you a copy. By the way, I settled with Murray recently with respect to your book (456/1. The translation of "Fur Darwin," published in 1869.), and had to pay him only 21 pounds 2 shillings 3 pence, which I consider a very small price for the dissemination of your views; he has 547 copies as yet unsold. This most terrible war will stop all science in France and Germany for a long time. I have heard from nobody in Germany, and know not whether your brother, Hackel, Gegenbaur, Victor Carus, or my other friends are serving in the army. Dohrn has joined a cavalry regiment. I have not yet met a soul in England who does not rejoice in the splendid triumph of Germany over France

(456/2. See Letter 239, Volume I.): it is a most just retribution against that vainglorious, war-liking nation. As the posts are all in confusion, I will not send this letter through France. The Editor has sent me duplicate copies of the "Revue des Cours Scientifiques," which contain several articles about my views; so I send you copies for the chance of your liking to see them.

LETTER 457. A.R. WALLACE TO CHARLES DARWIN. Holly House, Barking, E., January 27th, 1871.

Many thanks for your first volume (457/1. "The Descent of Man".), which I have just finished reading through with the greatest pleasure and interest; and I have also to thank you for the great tenderness with which you have treated me and my heresies.

On the subject of "sexual selection" and "protection," you do not yet convince me that I am wrong; but I expect your heaviest artillery will be brought up in your second volume, and I may have to capitulate. You seem, however, to have somewhat misunderstood my exact meaning, and I do not think the difference between us is quite so great as you seem to think it. There are a number of passages in which you argue against the view that the female has in any large number of cases been "specially modified" for protection, or that colour has generally been obtained by either sex for purposes of protection. But my view is, as I thought I had made it clear, that the female has (in most cases) been simply prevented from acquiring the gay tints of the male (even when there was a tendency for her to inherit it), because it was hurtful; and that, when protection is not needed, gay colours are so generally acquired by both sexes as to show that inheritance by both sexes of colour variations is the most usual, when not prevented from acting by Natural Selection. The colour itself may be acquired either by sexual selection or by other unknown causes.

There are, however, difficulties in the very wide application you give to sexual selection which at present stagger me, though no one was or is more ready than myself to admit the perfect truth of the principle or the immense importance and great variety of its applications.

Your chapters on "Man" are of intense interest—but as touching my special heresy, not as yet altogether convincing, though, of course, I fully agree with every word and every argument which goes to prove the "evolution" or "development" of man out of a lower form. My ONLY difficulties are, as to whether you have accounted for EVERY STEP of the development by ascertained laws.

I feel sure that the book will keep up and increase your high reputation, and be immensely successful, as it deserves to be...

LETTER 458. TO G.B. MURDOCH. Down, March 13th, 1871.

(458/1. We are indebted to Mr. Murdoch for a draft of his letter dated March 10th, 1871. It is too long to be quoted at length; the following citations give some idea of its contents: "In your 'Descent of Man,' in treating of the external differences between males and females of the same variety, have you attached sufficient importance to the different amount and kind of energy expended by them in reproduction?" Mr. Murdoch sums up: "Is it wrong, then, to suppose that extra growth, complicated structure, and activity in one sex exist as escape-valves for surplus vigour, rather than to please or fight with, though they may serve these purposes and be modified by them?")

I am much obliged for your valuable letter. I am strongly inclined to think that I have made a great and complete oversight with respect to the subject which you discuss. I am the more surprised at this, as I remember reflecting on some points which ought to have led me to your conclusion. By an odd chance I received the day before yesterday a letter from Mr. Lowne (author of an excellent book on the anatomy of the Blow-fly) (458/2. "The Anatomy and Physiology of the Blow-fly (Musca vomitaria L.)," by B.T. Lowne. London, 1870.) with a discussion very nearly to the same effect as yours. His conclusions were drawn from studying male insects with great horns, mandibles, etc. He informs me that his paper on this subject will soon be published in the "Transact. Entomolog. Society." (458/3. "Observations on Immature Sexuality and Alternate Generation in Insects." By B.T. Lowne. "Trans. Entomolog. Soc." 1871 {Read March 6th, 1871}. "I believe that certain cutaneous appendages, as the gigantic mandibles and thoracic horns of many males, are complemental to the sexual organs; that, in point of fact, they are produced by the excess of nutriment in the male, which in the female would go to form the generative organs and ova" (loc. cit., page 197).) I am inclined to look at your and Mr. Lowne's view as specially valuable from probably throwing light on the greater variability of male than female animals, which manifestly has much bearing on sexual selection. I will keep your remarks in mind whenever a new edition of my book is demanded.

LETTER 459. TO GEORGE FRASER.

(459/1. The following letter refers to two letters to Mr. Darwin, in which Mr. Fraser pointed out that illustrations of the theory of Sexual Selection might be found amongst British butterflies and moths. Mr. Fraser, in explanation of the letters, writes: "As an altogether unknown and far from experienced naturalist, I feared to send my letters for publication without, in the first place, obtaining Mr. Darwin's approval." The information was published in "Nature," Volume III., April 20th, 1871, page 489. The article was referred to in the second edition of the "Descent of Man" (1874), pages 312, 316, 319. Mr. Fraser adds: "This is only another illustration of Mr. Darwin's great conscientiousness in acknowledging suggestions received by him from the most humble sources." (Letter from Mr. Fraser to F. Darwin, March 21, 1888.)

Down, April 14th {1871}.

I am very much obliged for your letter and the interesting facts which it contains, and which are new to me. But I am at present so much engaged with other subjects that I cannot fully consider them; and, even if I had time, I do not suppose that I should have anything to say worth printing in a scientific journal. It would obviously be absurd in me to allow a mere note of thanks from me to be printed. Whenever I have to bring out a corrected edition of my book I will well consider your remarks (which I hope that you will send to "Nature"), but the difficulty will be that my friends tell me that I have already introduced too many facts, and that I ought to prune rather than to introduce more.

LETTER 460. TO E.S. MORSE. Down, December 3rd, 1871.

I am much obliged to you for having sent me your two interesting papers, and for the kind writing on the cover. I am very glad to have my error corrected about the protective colouring of shells. (460/1. "On Adaptive Coloration of the Mollusca," "Boston Society of Natural History Proc." Volume XIV., April 5th, 1871. Mr. Morse quotes from the "Descent of Man," I., page 316, a passage to the effect that the colours of the mollusca do not in general appear to be protective. Mr. Morse goes on to give instances of protective coloration.) It is no excuse for my broad statement, but I had in my mind the species which are brightly or beautifully coloured, and I can as yet hardly think that the colouring in such cases is protective.

LETTER 461. TO AUG. WEISMANN. Down, February 29th, 1872.

I am rejoiced to hear that your eyesight is somewhat better; but I fear that work with the microscope is still out of your power. I have often thought with sincere sympathy how much you must have suffered from your grand line of embryological research having been stopped. It was very good of you to use your eyes in writing to me. I have just received your essay (461/1. "Ueber der Einfluss der Isolirung auf die Artbildung": Leipzig, 1872.); but as I am now staying in London for the sake of rest, and as German is at all times very difficult to me, I shall not be able to read your essay for some little time. I am, however, very curious to learn what you have to say on isolation and on periods of variation. I thought much about isolation when I wrote in Chapter IV. on the circumstances favourable to Natural Selection. No doubt there remains an immense deal of work to do on "Artbildung." I have only opened a path for others to enter, and in the course of time to make a broad and clear high-road. I am especially glad that you are turning your attention to sexual selection. I have in this country hardly found any naturalists who agree with me on this subject, even to a moderate extent. They think it absurd that a female bird should be able to appreciate the splendid plumage of the male; but it would take much to persuade me that the peacock does not spread his gorgeous tail in the presence of the female in order to fascinate or excite her. The case, no doubt, is much more difficult with insects. I fear that you will find it difficult to experiment on diurnal lepidoptera in confinement, for I have never heard of any of these breeding in this state. (461/2. We are indebted to Mr. Bateson for the following note: "This belief does not seem to be well founded, for since Darwin's time several species of Rhopalocera (e.g. Pieris, Pararge, Caenonympha) have been successfully bred in confinement without any special difficulty; and by the use of large cages members even of strong-flying genera, such as Vanessa, have been induced to breed.") I was extremely pleased at hearing from Fritz Muller that he liked my chapter on lepidoptera in the "Descent of Man" more than any other part, excepting the chapter on morals.

LETTER 462. TO H. MULLER. Down {May, 1872}.

I have now read with the greatest interest your essay, which contains a vast amount of matter quite new to me. (462/1. "Anwendung der Darwin'schen Lehre auf Bienen," "Verhandl. d. naturhist. Vereins fur preuss. Rheinld. u. Westf." 1872. References to Muller's paper occur in the second edition of the "Descent of Man.") I really have no criticisms or suggestions to offer. The perfection of the gradation in the character of bees, especially in such important parts as the mouth-organs, was altogether unknown to me. You bring out all such facts very clearly by your comparison with the corresponding organs in the allied hymenoptera. How very curious is the case of bees and wasps having acquired, independently of inheritance from a common source, the habit of building hexagonal cells and of producing sterile workers! But I have been most interested by your discussion on secondary sexual differences; I do not suppose so full an account of such differences in any other group of animals has ever been published. It delights me to find that we have independently arrived at almost exactly the same conclusion with respect to the more important points deserving investigation in relation to sexual selection. For instance, the relative number of the two sexes, the earlier emergence of the males, the laws of inheritance, etc. What an admirable illustration you give of the transference of characters acquired by one sex—namely, that of the male of Bombus possessing the pollen-collecting apparatus. Many of your facts about the differences between male and female bees are surprisingly parallel with those which occur with birds. The reading your essay has given me great confidence in the efficacy of sexual selection, and I wanted some encouragement, as extremely few naturalists in England seem inclined to believe in it. I am, however, glad to find that Prof. Weismann has some faith in this principle.

The males of Bombus follow one remarkable habit, which I think it would interest you to investigate this coming summer, and no one could do it better than you. (462/2. Mr. Darwin's observations on this curious subject were sent to Hermann Muller, and after his death were translated and published in Krause's "Gesammelte kleinere Schriften von Charles Darwin," 1887, page 84. The male bees had certain regular lines of flight at Down, as from the end of the kitchen garden to the corner of the "sand-walk," and certain regular "buzzing places" where they stopped on the wing for a moment or two. Mr. Darwin's children remember vividly the pleasure of helping in the investigation of this habit.) I have therefore enclosed a briefly and roughly drawn-up account of this habit. Should you succeed in making any observations on this subject, and if you would like to use in any way my MS. you are perfectly welcome. I could, should you hereafter wish to make any use of the facts, give them in rather fuller detail; but I think that I have given enough.

I hope that you may long have health, leisure, and inclination to do much more work as excellent as your recent essay.

2.VIII.III. EXPRESSION, 1868-1874.

LETTER 463. TO F. MULLER. Down, January 30th {1868}.

I am very much obliged for your answers, though few in number (October 5th), about expression. I was especially glad to hear about shrugging the shoulders. You say that an old negro woman, when expressing astonishment, wonderfully resembled a Cebus when astonished; but are you sure that the Cebus opened its mouth? I ask because the Chimpanzee does not open its mouth when astonished, or when listening. (463/1. Darwin in the "Expression of the Emotions," adheres to this statement as being true of monkeys in general.) Please have the kindness to remember that I am very anxious to know whether any monkey, when screaming violently, partially or wholly closes its eyes.

LETTER 464. TO W. BOWMAN.

(464/1. The late Sir W. Bowman, the well-known surgeon, supplied a good deal of information of value to Darwin in regard to the expression of the emotions. The gorging of the eyes with blood during screaming is an important factor in the physiology of weeping, and indirectly in the obliquity of the eyebrows—a characteristic expression of suffering. See "Expression of the Emotions," pages 160 and 192.)

Down, March 30th {1868}.

I called at your house about three weeks since, and heard that you were away for the whole month, which I much regretted, as I wished to have had the pleasure of seeing you, of asking you a question, and of thanking you for your kindness to my son George. You did not quite understand the last note which I wrote to you—viz., about Bell's precise statement that the conjunctiva of an infant or young child becomes gorged with blood when the eyes are forcibly opened during a screaming fit. (464/2. Sir C. Bell's statement in his "Anatomy of Expression" (1844, page 106) is quoted in the "Expression of the Emotions," page 158.) I have carefully kept your previous note, in which you spoke doubtfully about Bell's statement. I intended in my former note only to express a wish that if, during your professional work, you were led to open the eyelids of a screaming child, you would specially observe this point about the eye showing signs of becoming gorged with blood, which interests me extremely. Could you ask any one to observe this for me in an eye-dispensary or hospital? But I now have to beg you kindly to consider one other question at any time when you have half an hour's leisure.

When a man coughs violently from choking or retches violently, even when he yawns, and when he laughs violently, tears come into the eyes. Now, in all these cases I observe that the orbicularis muscle is more or less spasmodically contracted, as also in the crying of a child. So, again, when the muscles of the abdomen contract violently in a propelling manner, and the breath is, I think, always held, as during the evacuation of a very costive man, and as (I hear) with a woman during severe labour-pains, the orbicularis contracts, and tears come into the eyes. Sir J.E. Tennant states that tears roll down the cheeks of elephants when screaming and trumpeting at first being captured; accordingly I went to the Zoological Gardens, and the keeper made two elephants trumpet, and when they did this violently the orbicularis was invariably plainly contracted. Hence I am led to conclude that there must be some relation between the contraction of this muscle and the secretion of tears. Can you tell me what this relation is? Does the orbicularis press against, and so directly stimulate, the lachrymal gland? As a slight blow on the eye causes, by reflex action, a copious effusion of tears, can the slight spasmodic contraction of the orbicularis act like a blow? This seems hardly possible. Does the same nerve which runs to the orbicularis send off fibrils to the lachrymal glands; and if so, when the order goes for the muscle to contract, is nervous force sent sympathetically at the same time to the glands? (464/3. See "Expression of the Emotions," page 169.)

I should be extremely much obliged if you {would} have the kindness to give me your opinion on this point.

LETTER 465. TO F.C. DONDERS.

(465/1. Mr. Darwin was indebted to Sir W. Bowman for an introduction to Professor Donders, whose work on Sir Charles Bell's views is quoted in the "Expression of the Emotions," pages 160-62.)

Down, June 3rd {1870?}.

I do not know how to thank you enough for the very great trouble which you have taken in writing at such length, and for your kind expressions towards me. I am particularly obliged for the abstract with respect to Sir C. Bell's views (465/2. See "Expression of the Emotions," pages 158 et seq.: Sir Charles Bell's view is that adopted by Darwin—viz. that the contraction of the muscles round the eyes counteracts the gorging of the parts during screaming, etc. The essay of Donders is, no doubt, "On the Action of the Eyelids in Determination of Blood from Expiratory Effort" in Beale's "Archives of Medicine," Volume V., 1870, page 20, which is a translation of the original in Dutch.), as I shall now proceed with some confidence; but I am intensely curious to read your essay in full when translated and published, as I hope, in the "Dublin Journal," as you speak of the weak point in the case—viz., that injuries are not known to follow from the gorging of the eye with blood. I may mention that my son and his friend at a military academy tell me that when they perform certain feats with their heads downwards their faces become purple and veins distended, and that they then feel an uncomfortable sensation in their eyes; but that as it is necessary for them to see, they cannot protect their eyes by closing the eyelids. The companions of one young man, who naturally has very prominent eyes, used to laugh at him when performing such feats, and declare that some day both eyes would start out of his head.

Your essay on the physiological and anatomical relations between the contraction of the orbicular muscles and the secretion of tears is wonderfully clear, and has interested me greatly. I had not thought about irritating substances getting into the nose during vomiting; but my clear impression is that mere retching causes tears. I will, however, try to get this point ascertained. When I reflect that in vomiting (subject to the above doubt), in violent coughing from choking, in yawning, violent laughter, in the violent downward action of the abdominal muscle...and in your very curious case of the spasms (465/3. In some cases a slight touch to the eye causes spasms of the orbicularis muscle, which may continue for so long as an hour, being accompanied by a flow of tears. See "Expression of the Emotions," page 166.)—that in all these cases the orbicular muscles are strongly and unconsciously contracted, and that at the same time tears often certainly flow, I must think that there is a connection of some kind between these phenomena; but you have clearly shown me that the nature of the relation is at present quite obscure.

LETTER 466. TO A.D. BARTLETT. 6, Queen Anne Street, W., December 19th {1870?}.

I was with Mr. Wood this morning, and he expressed himself strongly about your and your daughter's kindness in aiding him. He much wants assistance on another point, and if you would aid him, you would greatly oblige me. You know well the appearance of a dog when approaching another dog with hostile intentions, before they come close together. The dog walks very stiffly, with tail rigid and upright, hair on back erected, ears pointed and eyes directed forwards. When the dog attacks the other, down go the ears, and the canines are uncovered. Now, could you anyhow arrange so that one of your dogs could see a strange dog from a little distance, so that Mr. Wood could sketch the former attitude, viz., of the stiff gesture with erected hair and erected ears. (466/1. In Chapter II. of the "Expression of the Emotions" there are sketches of dogs in illustration of the "Principle of Antithesis," drawn by Mr. Riviere and by Mr. A. May. Mr. T.W. Wood supplied similar drawings of a cat, also a sketch of the head of a snarling dog.) And then he could afterwards sketch the same dog, when fondled by his master and wagging his tail with drooping ears. These two sketches I want much, and it would be a great favour to Mr. Wood, and myself, if you could aid him.

P.S.—When a horse is turned out into a field he trots with high, elastic steps, and carries his tail aloft. Even when a cow frisks about she throws up her tail. I have seen a drawing of an elephant, apparently trotting with high steps, and with the tail erect. When the elephants in the garden are turned out and are excited so as to move quickly, do they carry their tails aloft? How is this with the rhinoceros? Do not trouble yourself to answer this, but I shall be in London in a couple of months, and then perhaps you will be able to answer this trifling question. Or, if you write about wolves and jackals turning round, you can tell me about the tails of elephants, or of any other animals. (466/2. In the "Expression of the Emotions," page 44, reference is made under the head of "Associated habitual movements in the lower animals," to dogs and other animals turning round and round and scratching the ground with their fore-paws when they wish to go to sleep on a carpet, or other similar surface.)

LETTER 467. TO A.D. BARTLETT. Down, January 5th, {1871?}

Many thanks about Limulus. I am going to ask another favour, but I do not want to trouble you to answer it by letter. When the Callithrix sciureus screams violently, does it wrinkle up the skin round the eyes like a baby always does? (467/1. "Humboldt also asserts that the eyes of the Callithrix sciureus 'instantly fill with tears when it is seized with fear'; but when this pretty little monkey in the Zoological Gardens was teased, so as to cry out loudly, this did not occur. I do not, however, wish to throw the least doubt on the accuracy of Humboldt's statement." ("The Expression of the Emotions in Man and Animals," 1872, page 137.) When thus screaming do the eyes become suffused with moisture? Will you ask Sutton to observe carefully? (467/2. One of the keepers who made many observations on monkeys for Mr. Darwin.) Could you make it scream without hurting it much? I should be truly obliged some time for this information, when in spring I come to the Gardens.

LETTER 468. TO W. OGLE. Down, March 7th {1871}.

I wrote to Tyndall, but had no clear answer, and have now written to him again about odours. (468/1. Dr. Ogle's work on the Sense of Smell ("Medico-Chirurgical Trans." LIII., page 268) is referred to in the "Expression of the Emotions," page 256.) I write now to ask you to be so kind (if there is no objection) to tell me the circumstances under which you saw a man arrested for murder. (468/2. Given in the "Expression of the Emotions," page 294.) I say in my notes made from your conversation: utmost horror—extreme pallor—mouth relaxed and open—general prostration—perspiration—muscle of face contracted—hair observed on account of having been dyed, and apparently not erected. Secondly, may I quote you that you have often (?) seen persons (young or old? men or women?) who, evincing no great fear, were about to undergo severe operation under chloroform, showing resignation by (alternately?) folding one open hand over the other on the lower part of chest (whilst recumbent?)—I know this expression, and think I ought to notice it. Could you look out for an additional instance?

I fear you will think me very troublesome, especially when I remind you (not that I am in a hurry) about the Eustachian tube.

LETTER 469. TO J. JENNER WEIR. Down, June 14th {1870}.

As usual, I am going to beg for information. Can you tell me whether any Fringillidae or Sylviadae erect their feathers when frightened or enraged? (469/1. See "Expression of the Emotions," page 99.) I want to show that this expression is common to all or most of the families of birds. I know of this only in the fowl, swan, tropic-bird, owl, ruff and reeve, and cuckoo. I fancy that I remember having seen nestling birds erect their feathers greatly when looking into nests, as is said to be the case with young cuckoos. I should much like to know whether nestlings do really thus erect their feathers. I am now at work on expression in animals of all kinds, and birds; and if you have any hints I should be very glad for them, and you have a rich wealth of facts of all kinds. Any cases like the following: the sheldrake pats or dances on the tidal sands to make the sea-worms come out; and when Mr. St. John's tame sheldrakes came to ask for their dinners they used to pat the ground, and this I should call an expression of hunger and impatience. How about the Quagga case? (469/2. See Letter 235, Volume I.)

I am working away as hard as I can on my book; but good heavens, how slow my progress is.

LETTER 470. TO F.C. DONDERS. Down, March 18th, 1871.

Very many thanks for your kind letter. I have been interested by what you tell me about your views published in 1848, and I wish I could read your essay. It is clear to me that you were as near as possible in preceding me on the subject of Natural Selection.

You will find very little that is new to you in my last book; whatever merit it may possess consists in the grouping of the facts and in deductions from them. I am now at work on my essay on Expression. My last book fatigued me much, and I have had much correspondence, otherwise I should have written to you long ago, as I often intended to tell you in how high a degree your essay published in Beale's Archives interested me. (470/1. Beale's "Archives of Medicine," Volume V., 1870.) I have heard others express their admiration at the complete manner in which you have treated the subject. Your confirmation of Sir C. Bell's rather loose statement has been of paramount importance for my work. (470/2. On the contraction of the muscles surrounding the eye. See "Expression of the Emotions," page 158. See Letters 464, 465.) You told me that I might make further enquiries from you.

When a person is lost in meditation his eyes often appear as if fixed on a distant object (470/3. The appearance is due to divergence of the lines of vision produced by muscular relaxation. See "Expression of the Emotions," Edition II., page 239.), and the lower eyelids may be seen to contract and become wrinkled. I suppose the idea is quite fanciful, but as you say that the eyeball advances in adaptation for vision for close objects, would the eyeball have to be pushed backwards in adaptation for distant objects? (470/4. Darwin seems to have misunderstood a remark of Donders.) If so, can the wrinkling of the lower eyelids, which has often perplexed me, act in pushing back the eyeball?

But, as I have said, I daresay this is quite fanciful. Gratiolet says that the pupil contracts in rage, and dilates enormously in terror. (470/5. See "Expression of the Emotions," Edition II., page 321.) I have not found this great anatomist quite trustworthy on such points, and am making enquiries on this subject. But I am inclined to believe him, as the old Scotch anatomist Munro says, that the iris of parrots contracts and dilates under passions, independently of the amount of light. Can you give any explanation of this statement? When the heart beats hard and quick, and the head becomes somewhat congested with blood in any illness, does the pupil contract? Does the pupil dilate in incipient faintness, or in utter prostration, as when after a severe race a man is pallid, bathed in perspiration, with all his muscles quivering? Or in extreme prostration from any illness?

LETTER 471. TO W. TURNER. Down, March 28th {1871}.

I am much obliged for your kind note, and especially for your offer of sending me some time corrections, for which I shall be truly grateful. I know that there are many blunders to which I am very liable. There is a terrible one confusing the supra-condyloid foramen with another one. (471/1. In the first edition of the "Descent of Man," I., page 28, in quoting Mr. Busk "On the Caves of Gibraltar," Mr. Darwin confuses together the inter-condyloid foramen in the humerus with the supra-condyloid foramen. His attention was called to the mistake by Sir William Turner, to whom he had been previously indebted for other information on the anatomy of man. The error is one, as Sir William Turner points out in a letter, "which might easily arise where the writer is not minutely acquainted with human anatomy." In speaking of his correspondence with Darwin, Sir William remarks on a characteristic of Darwin's method of asking for information, namely, his care in avoiding leading questions.) This, however, I have corrected in all the copies struck off after the first lot of 2500. I daresay there will be a new edition in the course of nine months or a year, and this I will correct as well as I can. As yet the publishers have kept up type, and grumble dreadfully if I make heavy corrections. I am very far from surprised that "you have not committed yourself to full acceptation" of the evolution of man. Difficulties and objections there undoubtedly are, enough and to spare, to stagger any cautious man who has much knowledge like yourself.

I am now at work at my hobby-horse essay on Expression, and I have been reading some old notes of yours. In one you say it is easy to see that the spines of the hedgehog are moved by the voluntary panniculus. Now, can you tell me whether each spine has likewise an oblique unstriped or striped muscle, as figured by Lister? (472/2. "Expression of the Emotions," page 101.) Do you know whether the tail-coverts of peacock or tail of turkey are erected by unstriped or striped muscles, and whether these are homologous with the panniculus or with the single oblique unstriped muscles going to each separate hair in man and many animals? I wrote some time ago to Kolliker to ask this question (and in relation to quills of porcupine), and I received a long and interesting letter, but he could not answer these questions. If I do not receive any answer (for I know how busy you must be), I will understand you cannot aid me.

I heard yesterday that Paget was very ill; I hope this is not true. What a loss he would be; he is so charming a man.

P.S.—As I am writing I will trouble you with one other question. Have you seen anything or read of any facts which could induce you to think that the mind being intently and long directed to any portion of the skin (or, indeed, any organ) would influence the action of the capillaries, causing them either to contract or dilate? Any information on this head would be of great value to me, as bearing on blushing.

If I remember right, Paget seems to be a great believer in the influence of the mind in the nutrition of parts, and even in causing disease. It is awfully audacious on my part, but I remember thinking (with respect to the latter assertion on disease) when I read the passage that it seemed rather fanciful, though I should like to believe in it. Sir H. Holland alludes to this subject of the influence of the mind on local circulation frequently, but gives no clear evidence. (472/3. Ibid., pages 339 et seq.)

LETTER 472. TO W. TURNER. Down, March 29th {1871}.

Forgive me for troubling you with one line. Since writing my P.S. I have read the part on the influence of the nervous system on the nutrition of parts in your last edition of Paget's "Lectures." (472/1. "Lectures on Surgical Pathology," Edition III., revised by Professor Turner, 1870.) I had not read before this part in this edition, and I see how foolish I was. But still, I should be extremely grateful for any hint or evidence of the influence of mental attention on the capillary or local circulation of the skin, or of any part to which the mind may be intently and long directed. For instance, if thinking intently about a local eruption on the skin (not on the face, for shame might possibly intervene) caused it temporarily to redden, or thinking of a tumour caused it to throb, independently of increased heart action.

LETTER 473. TO HUBERT AIRY.

(473/1. Dr. Airy had written to Mr. Darwin on April 3rd:—

"With regard to the loss of voluntary movement of the ears in man and monkey, may I ask if you do not think it might have been caused, as it is certainly compensated, by the facility and quickness in turning the head, possessed by them in virtue of their more erect stature, and the freedom of the atlanto-axial articulation? (in birds the same end is gained by the length and flexibility of the neck.) The importance, in case of danger, of bringing the eyes to help the ears would call for a quick turn of the head whenever a new sound was heard, and so would tend to make superfluous any special means of moving the ears, except in the case of quadrupeds and the like, that have great trouble (comparatively speaking) in making a horizontal turn of the head—can only do it by a slow bend of the whole neck." (473/2. We are indebted to Dr. Airy for furnishing us with a copy of his letter to Mr. Darwin, the original of which had been mislaid.)

Down, April 5th {1871}.

I am greatly obliged for your letter. Your idea about the easy turning of the head instead of the ears themselves strikes me as very good, and quite new to me, and I will keep it in mind; but I fear that there are some cases opposed to the notion.

If I remember right the hedgehog has very human ears, but birds support your view, though lizards are opposed to it.

Several persons have pointed out my error about the platysma. (473/3. The error in question occurs on page 19 of the "Descent of Man," Edition I., where it is stated that the Platysma myoides cannot be voluntarily brought into action. In the "Expression of the Emotions" Darwin remarks that this muscle is sometimes said not to be under voluntary control, and he shows that this is not universally true.) Nor can I remember how I was misled. I find I can act on this muscle myself, now that I know the corners of the mouth have to be drawn back. I know of the case of a man who can act on this muscle on one side, but not on the other; yet he asserts positively that both contract when he is startled. And this leads me to ask you to be so kind as to observe, if any opportunity should occur, whether the platysma contracts during extreme terror, as before an operation; and secondly, whether it contracts during a shivering fit. Several persons are observing for me, but I receive most discordant results.

I beg you to present my most respectful and kind compliments to your honoured father {Sir G.B. Airy}.

LETTER 474. TO FRANCIS GALTON.

(474/1. Mr. Galton had written on November 7th, 1872, offering to send to various parts of Africa Darwin's printed list of questions intended to guide observers on expression. Mr. Galton goes on: "You do not, I think, mention in "Expression" what I thought was universal among blubbering children (when not trying to see if harm or help was coming out of the corner of one eye) of pressing the knuckles against the eyeballs, thereby reinforcing the orbicularis.")

Down, November 8th {1872}.

Many thanks for your note and offer to send out the queries; but my career is so nearly closed that I do not think it worth while. What little more I can do shall be chiefly new work. I ought to have thought of crying children rubbing their eyes with their knuckles, but I did not think of it, and cannot explain it. As far as my memory serves, they do not do so whilst roaring, in which case compression would be of use. I think it is at the close of the crying fit, as if they wished to stop their eyes crying, or possibly to relieve the irritation from the salt tears. I wish I knew more about the knuckles and crying.

What a tremendous stir-up your excellent article on prayer has made in England and America! (474/2. The article entitled "Statistical Inquiries into the Efficacy of Prayer" appeared in the "Fortnightly Review," 1872. In Mr. Francis Galton's book on "Enquiries into Human Faculty and its Development," London, 1883, a section (pages 277-94) is devoted to a discussion on the "Objective Efficacy of Prayer.")

LETTER 475. TO F.C. DONDERS.

(475/1. We have no means of knowing whether the observations suggested in the following letter were made—if not, the suggestion is worthy of record.)

Down, December 21st, 1872.

You will have received some little time ago my book on Expression, in writing which I was so deeply indebted to your kindness. I want now to beg a favour of you, if you have the means to grant it. A clergyman, the head of an institution for the blind in England (475/2. The Rev. R.H. Blair, Principal of the Worcester College: "Expression of the Emotions," Edition II., page 237.), has been observing the expression of those born blind, and he informs me that they never or very rarely frown. He kept a record of several cases, but at last observed a frown on two of the children who he thought never frowned; and then in a foolish manner tore up his notes, and did not write to me until my book was published. He may be a bad observer and altogether mistaken, but I think it would be worth while to ascertain whether those born blind, when young, and whilst screaming violently, contract the muscles round the eyes like ordinary infants. And secondly, whether in after years they rarely or never frown. If it should prove true that infants born blind do not contract their orbicular muscles whilst screaming (though I can hardly believe it) it would be interesting to know whether they shed tears as copiously as other children. The nature of the affection which causes blindness may possibly influence the contraction of the muscles, but on all such points you will judge infinitely better than I can. Perhaps you could get some trustworthy superintendent of an asylum for the blind to attend to this subject. I am sure that you will forgive me asking this favour.

LETTER 476. TO D. HACK TUKE. Down, December 22nd, 1872.

I have now finished your book, and have read it with great interest. (476/1. "Influence of the Mind upon the Body. Designed to elucidate the Power of the Imagination." 1872.)

Many of your cases are very striking. As I felt sure would be the case, I have learnt much from it; and I should have modified several passages in my book on Expression, if I had had the advantage of reading your work before my publication. I always felt, and said so a year ago to Professor Donders, that I had not sufficient knowledge of Physiology to treat my subject in a proper way.

With many thanks for the interest which I have felt in reading your work...

LETTER 477. TO A.R. WALLACE. Down, January 10th {1873}.

I have read your Review with much interest, and I thank you sincerely for the very kind spirit in which it is written. I cannot say that I am convinced by your criticisms. (477/1. "Quarterly Journal of Science," January, 1873, page 116: "I can hardly believe that when a cat, lying on a shawl or other soft material, pats or pounds it with its feet, or sometimes sucks a piece of it, it is the persistence of the habit of pressing the mammary glands and sucking during kittenhood." Mr. Wallace goes on to say that infantine habits are generally completely lost in adult life, and that it seems unlikely that they should persist in a few isolated instances.) If you have ever actually observed a kitten sucking and pounding, with extended toes, its mother, and then seen the same kitten when a little older doing the same thing on a soft shawl, and ultimately an old cat (as I have seen), and do not admit that it is identically the same action, I am astonished. With respect to the decapitated frog, I have always heard of Pfluger as a most trustworthy observer. (477/2. Mr. Wallace speaks of "a readiness to accept the most marvellous conclusions or interpretations of physiologists on what seem very insufficient grounds," and he goes on to assert that the frog experiment is either incorrectly recorded or else that it "demonstrates volition, and not reflex action.") If, indeed, any one knows a frog's habits so well as to say that it never rubs off a bit of leaf or other object which may stick to its thigh, in the same manner as it did the acid, your objection would be valid. Some of Flourens' experiments, in which he removed the cerebral hemispheres from a pigeon, indicate that acts apparently performed consciously can be done without consciousness. I presume through the force of habit, in which case it would appear that intellectual power is not brought into play. Several persons have made suggestions and objections as yours about the hands being held up in astonishment; if there was any straining of the muscles, as with protruded arms under fright, I would agree; as it is I must keep to my old opinion, and I dare say you will say that I am an obstinate old blockhead. (477/3. The raising of the hands in surprise is explained ("Expression of Emotions," Edition I., page 287) on the doctrine of antithesis as being the opposite of listlessness. Mr. Wallace's view (given in the 2nd edition of "Expression of the Emotions," page 300) is that the gesture is appropriate to sudden defence or to the giving of aid to another person.)

The book has sold wonderfully; 9,000 copies have now been printed.

LETTER 478. TO CHAUNCEY WRIGHT. Down, September 21st, 1874.

I have read your long letter with the greatest interest, and it was extremely kind of you to take such great trouble. Now that you call my attention to the fact, I well know the appearance of persons moving the head from side to side when critically viewing any object; and I am almost sure that I have seen the same gesture in an affected person when speaking in exaggerated terms of some beautiful object not present. I should think your explanation of this gesture was the true one. But there seems to me a rather wide difference between inclining or moving the head laterally, and moving it in the same plane, as we do in negation, and, as you truly add, in disapprobation. It may, however, be that these two movements of the head have been confounded by travellers when speaking of the Turks. Perhaps Prof. Lowell would remember whether the movement was identically the same. Your remarks on the effects of viewing a sunset, etc., with the head inverted are very curious. (478/1. The letter dated September 3rd, 1874, is published in Mr. Thayer's "Letters" of Chauncey Wright, privately printed, Cambridge, Mass., 1878. Wright quotes Mr. Sophocles, a native of Greece, at the time Professor of Modern and Ancient Greek at Harvard University, to the effect that the Turks do not express affirmation by a shake of the head, but by a bow or grave nod, negation being expressed by a backward nod. From the striking effect produced by looking at a landscape with the head inverted, or by looking at its reflection, Chauncey Wright was led to the lateral movement of the head, which is characteristic of critical inspection—eg. of a picture. He thinks that in this way a gesture of deliberative assent arose which may have been confused with our ordinary sign of negation. He thus attempts to account for the contradictions between Lieber's statement that a Turk or Greek expresses "yes" by a shake of the head, and the opposite opinion of Prof. Sophocles, and lastly, Mr. Lowell's assertion that in Italy our negative shake of the head is used in affirmation (see "Expression of the Emotions," Edition II., page 289).) We have a looking-glass in the drawing-room opposite the flower-garden, and I have often been struck how extremely pretty and strange the flower garden and surrounding bushes appear when thus viewed. Your letter will be very useful to me for a new edition of my Expression book; but this will not be for a long time, if ever, as the publisher was misled by the very large sale at first, and printed far too many copies.

I daresay you intend to publish your views in some essay, and I think you ought to do so, for you might make an interesting and instructive discussion.

I have been half killing myself of late with microscopical work on plants. I begin to think that they are more wonderful than animals.

P.S., January 29th, 1875.—You will see that by a stupid mistake in the address this letter has just been returned to me. It is by no means worth forwarding, but I cannot bear that you should think me so ungracious and ungrateful as not to have thanked you for your long letter.

As I forget whether "Cambridge" is sufficient address, I will send this through Asa Gray.

(PLATE: CHARLES LYELL. Engraved by G.I. (J). Stodart from a photograph.)

CHAPTER 2.IX. GEOLOGY, 1840-1882.

I. Vulcanicity and Earth-movements.—II. Ice-action.—III. The Parallel Roads of Glen Roy.—IV. Coral Reefs, Fossil and Recent.—V. Cleavage and Foliation.—VI. Age of the World.—VII. Geological Action of Earthworms.—VIII. Miscellaneous.

2.IX.I. VULCANICITY AND EARTH-MOVEMENTS, 1840-1881.

LETTER 479. TO DAVID MILNE. 12, Upper Gower Street, Thursday {March} 20th {1840}.

I much regret that I am unable to give you any information of the kind you desire. You must have misunderstood Mr. Lyell concerning the object of my paper. (479/1. "On the Connexion of certain Volcanic Phenomena, and on the Formation of Mountain-chains and the Effects of Continental Elevations." "Trans. Geol. Soc." Volume V., 1840, pages 601-32 {March 7th, 1838}.) It is an account of the shock of February, 1835, in Chile, which is particularly interesting, as it ties most closely together volcanic eruptions and continental elevations. In that paper I notice a very remarkable coincidence in volcanic eruptions in S. America at very distant places. I have also drawn up some short tables showing, as it appears to me, that there are periods of unusually great volcanic activity affecting large portions of S. America. I have no record of any coincidences between shocks there and in Europe. Humboldt, by his table in the "Pers. Narrative" (Volume IV., page 36, English Translation), seems to consider the elevation of Sabrina off the Azores as connected with S. American subterranean activity: this connection appears to be exceedingly vague. I have during the past year seen it stated that a severe shock in the northern parts of S. America coincided with one in Kamstchatka. Believing, then, that such coincidences are purely accidental, I neglected to take a note of the reference; but I believe the statement was somewhere in "L'Institut" for 1839. (479/2. "L'Institut, Journal General des Societes et Travaux Scientifiques de la France et de l'Etranger," Tome VIII. page 412, Paris, 1840. In a note on some earthquakes in the province Maurienne it is stated that they occurred during a change in the weather, and at times when a south wind followed a north wind, etc.) I was myself anxious to see the list of the 1200 shocks alluded to by you, but I have not been able to find out that the list has been published. With respect to any coincidences you may discover between shocks in S. America and Europe, let me venture to suggest to you that it is probably a quite accurate statement that scarcely one hour in the year elapses in S. America without an accompanying shock in some part of that large continent. There are many regions in which earthquakes take place every three and four days; and after the severer shocks the ground trembles almost half-hourly for months. If, therefore, you had a list of the earthquakes of two or three of these districts, it is almost certain that some of them would coincide with those in Scotland, without any other connection than mere chance.

My paper will be published immediately in the "Geological Transactions," and I will do myself the pleasure of sending you a copy in the course of (as I hope) a week or ten days. A large part of it is theoretical, and will be of little interest to you; but the account of the Concepcion shock of 1835 will, I think, be worth your perusal. I have understood from Mr. Lyell that you believe in some connection between the state of the weather and earthquakes. Under the very peculiar climate of Northern Chile, the belief of the inhabitants in such connection can hardly, in my opinion, be founded in error. It must possibly be worth your while to turn to pages 430-433 in my "Journal of Researches during the Voyage of the 'Beagle'," where I have stated this circumstance. (479/3. "Journal of Researches into the Natural History and Geology of the Countries visited during the Voyage of H.M.S. 'Beagle' round the World." London, 1870, page 351.) On the hypothesis of the crust of the earth resting on fluid matter, would the influence of the moon (as indexed by the tides) affect the periods of the shocks, when the force which causes them is just balanced by the resistance of the solid crust? The fact you mention of the coincidence between the earthquakes of Calabria and Scotland appears most curious. Your paper will possess a high degree of interest to all geologists. I fancied that such uniformity of action, as seems here indicated, was probably confined to large continents, such as the Americas. How interesting a record of volcanic phenomena in Iceland would be, now that you are collecting accounts of every slight trembling in Scotland. I am astonished at their frequency in that quiet country, as any one would have called it. I wish it had been in my power to have contributed in any way to your researches on this most interesting subject.

LETTER 480. TO L. HORNER. Down, August 29th {1844}.

I am greatly obliged for your kind note, and much pleased with its contents. If one-third of what you say be really true, and not the verdict of a partial judge (as from pleasant experience I much suspect), then should I be thoroughly well contented with my small volume which, small as it is, cost me much time. (480/1. "Geological Observations on the Volcanic Islands visited during the Voyage of H.M.S. 'Beagle'": London, 1844. A French translation has been made by Professor Renard of Ghent, and published by Reinwald of Paris in 1902.) The pleasure of observation amply repays itself: not so that of composition; and it requires the hope of some small degree of utility in the end to make up for the drudgery of altering bad English into sometimes a little better and sometimes worse. With respect to craters of elevation (480/2. "Geological Observations," pages 93-6.), I had no sooner printed off the few pages on that subject than I wished the whole erased. I utterly disbelieve in Von Buch and de Beaumont's views; but on the other hand, in the case of the Mauritius and St. Jago, I cannot, perhaps unphilosophically, persuade myself that they are merely the basal fragments of ordinary volcanoes; and therefore I thought I would suggest the notion of a slow circumferential elevation, the central part being left unelevated, owing to the force from below being spent and {relieved?} in eruptions. On this view, I do not consider these so-called craters of elevation as formed by the ejection of ashes, lava, etc., etc., but by a peculiar kind of elevation acting round and modified by a volcanic orifice. I wish I had left it all out; I trust that there are in other parts of the volume more facts and less theory. The more I reflect on volcanoes, the more I appreciate the importance of E. de Beaumont's measurements (480/3. Elie de Beaumont's views are discussed by Sir Charles Lyell both in the "Principles of Geology" (Edition X., 1867, Volume I. pages 633 et seq.) and in the "Elements of Geology" (Edition III., 1878, pages 495, 496). See also Darwin's "Geological Observations," Edition II., 1876, page 107.) (even if one does not believe them implicitly) of the natural inclination of lava-streams, and even more the importance of his view of the dikes, or unfilled fissures, in every volcanic mountain, being the proofs and measures of the stretching and consequent elevation which all such mountains must have undergone. I believe he thus unintentionally explains most of his cases of lava-streams being inclined at a greater angle than that at which they could have flowed.

But excuse this lengthy note, and once more let me thank you for the pleasure and encouragement you have given me—which, together with Lyell's never-failing kindness, will help me on with South America, and, as my books will not sell, I sometimes want such aid. I have been lately reading with care A. d'Orbigny's work on South America (480/4. "Voyage dans l'Amerique Meridionale—execute pendant les annees 1826-33": six volumes, Paris, 1835-43.), and I cannot say how forcibly impressed I am with the infinite superiority of the Lyellian school of Geology over the continental. I always feel as if my books came half out of Lyell's brain, and that I never acknowledge this sufficiently; nor do I know how I can without saying so in so many words—for I have always thought that the great merit of the "Principles" was that it altered the whole tone of one's mind, and therefore that, when seeing a thing never seen by Lyell, one yet saw it partially through his eyes—it would have been in some respects better if I had done this less: but again excuse my long, and perhaps you will think presumptuous, discussion. Enclosed is a note from Emma to Mrs. Horner, to beg you, if you can, to give us the great pleasure of seeing you here. We are necessarily dull here, and can offer no amusements; but the weather is delightful, and if you could see how brightly the sun now shines you would be tempted to come. Pray remember me most kindly to all your family, and beg of them to accept our proposal, and give us the pleasure of seeing them.

LETTER 481. TO C. LYELL. Down, {September, 1844}.

I was glad to get your note, and wanted to hear about your work. I have been looking to see it advertised; it has been a long task. I had, before your return from Scotland, determined to come up and see you; but as I had nothing else to do in town, my courage has gradually eased off, more especially as I have not been very well lately. We get so many invitations here that we are grown quite dissipated, but my stomach has stood it so ill that we are going to have a month's holidays, and go nowhere.

The subject which I was most anxious to talk over with you I have settled, and having written sixty pages of my "S. American Geology," I am in pretty good heart, and am determined to have very little theory and only short descriptions. The two first chapters will, I think, be pretty good, on the great gravel terraces and plains of Patagonia and Chili and Peru.

I am astonished and grieved over D'Orbigny's nonsense of sudden elevations. (481/1. D'Orbigny's views are referred to by Lyell in chapter vii. of the "Principles," Volume I. page 131. "This mud {i.e. the Pampean mud} contains in it recent species of shells, some of them proper to brackish water, and is believed by Mr. Darwin to be an estuary or delta deposit. M.A. D'Orbigny, however, has advanced an hypothesis...that the agitation and displacement of the waters of the ocean, caused by the elevation of the Andes, gave rise to a deluge, of which this Pampean mud, which reaches sometimes the height of 12,000 feet, is the result and monument.") I must give you one of his cases: He finds an old beach 600 feet above sea. He finds STILL ATTACHED to the rocks at 300 feet six species of truly littoral shells. He finds at 20 to 30 feet above sea an immense accumulation of chiefly littoral shells. He argues the whole 600 feet uplifted at one blow, because the attached shells at 300 feet have not been displaced. Therefore when the sea formed a beach at 600 feet the present littoral shells were attached to rocks at 300 feet depth, and these same shells were accumulating by thousands at 600 feet.

Hear this, oh Forbes. Is it not monstrous for a professed conchologist? This is a fair specimen of his reasoning.

One of his arguments against the Pampas being a slow deposit, is that mammifers are very seldom washed by rivers into the sea!

Because at 12,000 feet he finds the same kind of clay with that of the Pampas he never doubts that it is contemporaneous with the Pampas {debacle?} which accompanied the right royal salute of every volcano in the Cordillera. What a pity these Frenchmen do not catch hold of a comet, and return to the good old geological dramas of Burnett and Whiston. I shall keep out of controversy, and just give my own facts. It is enough to disgust one with Geology; though I have been much pleased with the frank, decided, though courteous manner with which D'Orbigny disputes my conclusions, given, unfortunately, without facts, and sometimes rashly, in my journal.

Enough of S. America. I wish you would ask Mr. Horner (for I forgot to do so, and am unwilling to trouble him again) whether he thinks there is too much detail (quite independent of the merits of the book) in my volcanic volume; as to know this would be of some real use to me. You could tell me when we meet after York, when I will come to town. I had intended being at York, but my courage has failed. I should much like to hear your lecture, but still more to read it, as I think reading is always better than hearing.

I am very glad you talk of a visit to us in the autumn if you can spare the time. I shall be truly glad to see Mrs. Lyell and yourself here; but I have scruples in asking any one—you know how dull we are here. Young Hooker (481/2. Sir J.D. Hooker.) talks of coming; I wish he might meet you,—he appears to me a most engaging young man.

I have been delighted with Prescott, of which I have read Volume I. at your recommendation; I have just been a good deal interested with W. Taylor's (of Norwich) "Life and Correspondence."

On your return from York I shall expect a great supply of Geological gossip.

LETTER 482. TO C. LYELL. {October 3rd, 1846.}

I have been much interested with Ramsay, but have no particular suggestions to offer (482/1. "On the Denudation of South Wales and the Adjacent Counties of England." A.C. Ramsay, "Mem. Geol. Survey Great Britain," Volume I., London, 1846.); I agree with all your remarks made the other day. My final impression is that the only argument against him is to tell him to read and re-read the "Principles," and if not then convinced to send him to Pluto. Not but what he has well read the "Principles!" and largely profited thereby. I know not how carefully you have read this paper, but I think you did not mention to me that he does (page 327) (482/2. Ramsay refers the great outlines of the country to the action of the sea in Tertiary times. In speaking of the denudation of the coast, he says: "Taking UNLIMITED time into account, we can conceive that any extent of land might be so destroyed...If to this be added an EXCEEDINGLY SLOW DEPRESSION of the land and sea bottom, the wasting process would be materially assisted by this depression" (loc. cit., page 327).) believe that the main part of his great denudation was effected during a vast (almost gratuitously assumed) slow Tertiary subsidence and subsequent Tertiary oscillating slow elevation. So our high cliff argument is inapplicable. He seems to think his great subsidence only FAVOURABLE for great denudation. I believe from the general nature of the off-shore sea's bottoms that it is almost necessary; do look at two pages—page 25 of my S. American volume—on this subject. (482/3. "Geological Observations on S. America," 1846, page 25. "When viewing the sea-worn cliffs of Patagonia, in some parts between 800 and 900 feet in height, and formed of horizontal Tertiary strata, which must once have extended far seaward...a difficulty often occurred to me, namely, how the strata could possibly have been removed by the action of the sea at a considerable depth beneath its surface." The cliffs of St. Helena are referred to in illustration of the same problem; speaking of these, Darwin adds: "Now, if we had any reason to suppose that St. Helena had, during a long period, gone on slowly subsiding, every difficulty would be removed...I am much inclined to suspect that we shall hereafter find in all such cases that the land with the adjoining bed of the sea has in truth subsided..." (loc. cit., pages 25-6).)

The foundation of his views, viz., of one great sudden upheaval, strikes me as threefold. First, to account for the great dislocations. This strikes me as the odder, as he admits that a little northwards there were many and some violent dislocations at many periods during the accumulation of the Palaeozoic series. If you argue against him, allude to the cool assumption that petty forces are conflicting: look at volcanoes; look at recurrent similar earthquakes at same spots; look at repeatedly injected intrusive masses. In my paper on Volcanic Phenomena in the "Geol. Transactions." (482/4. "On the Connection of certain Volcanic Phenomena, and on the Formation of Mountain-chains and the Effects of Continental Elevations." "Geol. Soc. Proc." Volume II., pages 654-60, 1838; "Trans. Geol. Soc." Volume V., pages 601-32, 1842. {Read March 7th, 1838.}) I have argued (and Lonsdale thought well of the argument, in favour, as he remarked, of your original doctrine) that if Hopkins' views are correct, viz., that mountain chains are subordinate consequences to changes of level in mass, then, as we have evidence of such horizontal movements in mass having been slow, the foundation of mountain chains (differing from volcanoes only in matter being injected instead of ejected) must have been slow.

Secondly, Ramsay has been influenced, I think, by his Alpine insects; but he is wrong in thinking that there is any necessary connection of tropics and large insects—videlicet—Galapagos Arch., under the equator. Small insects swarm in all parts of tropics, though accompanied generally with large ones.

Thirdly, he appears influenced by the absence of newer deposits on the old area, blinded by the supposed necessity of sediment accumulating somewhere near (as no doubt is true) and being PRESERVED—an example, as I think, of the common error which I wrote to you about. The preservation of sedimentary deposits being, as I do not doubt, the exception when they are accumulated during periods of elevation or of stationary level, and therefore the preservation of newer deposits would not be probable, according to your view that Ramsay's great Palaeozoic masses were denuded, whilst slowly rising. Do pray look at end of Chapter II., at what little I have said on this subject in my S. American volume. (482/5. The second chapter of the "Geological Observations" concludes with a Summary on the Recent Elevations of the West Coast of South America, (page 53).)

I do not think you can safely argue that the whole surface was probably denuded at same time to the level of the lateral patches of Magnesian conglomerate.

The latter part of the paper strikes me as good, but obvious.

I shall send him my S. American volume for it is curious on how many similar points we enter, and I modestly hope it may be a half-oz. weight towards his conversion to better views. If he would but reject his great sudden elevations, how sound and good he would be. I doubt whether this letter will be worth the reading.

LETTER 483. TO C. LYELL. Down {September 4th, 1849}.

It was very good of you to write me so long a letter, which has interested me much. I should have answered it sooner, but I have not been very well for the few last days. Your letter has also flattered me much in many points. I am very glad you have been thinking over the relation of subsidence and the accumulation of deposits; it has to me removed many great difficulties; please to observe that I have carefully abstained from saying that sediment is not deposited during periods of elevation, but only that it is not accumulated to sufficient thickness to withstand subsequent beach action; on both coasts of S. America the amount of sediment deposited, worn away, and redeposited, oftentimes must have been enormous, but still there have been no wide formations produced: just read my discussion (page 135 of my S. American book (483/1. See Letter 556, note. The discussion referred to ("Geological Observations on South America," 1846) deals with the causes of the absence of recent conchiferous deposits on the coasts of South America.)) again with this in your mind. I never thought of your difficulty (i.e. in relation to this discussion) of where was the land whence the three miles of S. Wales strata were derived! (483/2. In his classical paper "On the Denudation of South Wales and the Adjacent Counties of England" ("Mem. Geol. Survey," Volume I., page 297, 1846), Ramsay estimates the thickness of certain Palaeozoic formations in South Wales, and calculates the cubic contents of the strata in the area they now occupy together with the amount removed by denudation; and he goes on to say that it is evident that the quantity of matter employed to form these strata was many times greater than the entire amount of solid land they now represent above the waves. "To form, therefore, so great a thickness, a mass of matter of nearly equal cubic contents must have been worn by the waves and the outpourings of rivers from neighbouring lands, of which perhaps no original trace now remains" (page 334.)) Do you not think that it may be explained by a form of elevation which I have always suspected to have been very common (and, indeed, had once intended getting all facts together), viz. thus?—

The frequency of a DEEP ocean close to a rising continent bordered with mountains, seems to indicate these opposite movements of rising and sinking CLOSE TOGETHER; this would easily explain the S. Wales and Eocene cases. I will only add that I should think there would be a little more sediment produced during subsidence than during elevation, from the resulting outline of coast, after long period of rise. There are many points in my volume which I should like to have discussed with you, but I will not plague you: I should like to hear whether you think there is anything in my conjecture on Craters of Elevation (483/3. In the "Geological Observations on Volcanic Islands," 1844, pages 93-6, Darwin speaks of St. Helena, St. Jago and Mauritius as being bounded by a ring of basaltic mountains which he regards as "Craters of Elevation." While unable to accept the theory of Elie de Beaumont and attribute their formation to a dome-shaped elevation and consequent arching of the strata, he recognises a "very great difficulty in admitting that these basaltic mountains are merely the basal fragments of great volcanoes, of which the summits have been either blown off, or, more probably, swallowed by subsidence." An explanation of the origin and structure of these volcanic islands is suggested which would keep them in the class of "Craters of Elevation," but which assumes a slow elevation, during which the central hollow or platform having been formed "not by the arching of the surface, but simply by that part having been upraised to a less height."); I cannot possibly believe that Saint Jago or Mauritius are the basal fragments of ordinary volcanoes; I would sooner even admit E. de Beaumont's views than that—much as I would sooner in my own mind in all cases follow you. Just look at page 232 in my "S. America" for a trifling point, which, however, I remember to this day relieved my mind of a considerable difficulty. (483/4. This probably refers to a paragraph (page 232) "On the Eruptive Sources of the Porphyritic Claystone and Greenstone Lavas." The opinion is put forward that "the difficulty of tracing the streams of porphyries to their ancient and doubtless numerous eruptive sources, may be partly explained by the very general disturbance which the Cordillera in most parts has suffered"; but, Darwin adds, "a more specific cause may be that 'the original points of eruption tend to become the points of injection'...On this view of there being a tendency in the old points of eruption to become the points of subsequent injection and disturbance, and consequently of denudation, it ceases to be surprising that the streams of lava in the porphyritic claystone conglomerate formation, and in other analogous cases, should most rarely be traceable to their actual sources." The latter part of this letter is published in "Life and Letters," I., pages 377, 378.) I remember being struck with your discussion on the Mississippi beds in relation to Pampas, but I should wish to read them over again; I have, however, re-lent your work to Mrs. Rich, who, like all whom I have met, has been much interested by it. I will stop about my own Geology. But I see I must mention that Scrope did suggest (and I have alluded to him, page 118 (483/5. "Geological Observations," Edition II., 1876. Chapter VI. opens with a discussion "On the Separation of the Constituent Minerals of Lava, according to their Specific Gravities." Mr. Darwin calls attention to the fact that Mr. P. Scrope had speculated on the subject of the separation of the trachytic and basaltic series of lavas (page 113).), but without distinct reference and I fear not sufficiently, though I utterly forgot what he wrote) the separation of basalt and trachyte; but he does not appear to have thought about the crystals, which I believe to be the keystone of the phenomenon. I cannot but think this separation of the molten elements has played a great part in the metamorphic rocks: how else could the basaltic dykes have come in the great granitic districts such as those of Brazil? What a wonderful book for labour is d'Archiac!...(483/6. Possibly this refers to d'Archiac's "Histoire des Progres de la Geologie," 1848.)

LETTER 484. TO LADY LYELL. Down, Wednesday night {1849?}.

I am going to beg a very very great favour of you: it is to translate one page (and the title) of either Danish or Swedish or some such language. I know not to whom else to apply, and I am quite dreadfully interested about the barnacles therein described. Does Lyell know Loven, or his address and title? for I must write to him. If Lyell knows him I would use his name as introduction; Loven I know by name as a first-rate naturalist.

Accidentally I forgot to give you the "Footsteps," which I now return, having ordered a copy for myself.

I sincerely hope the "Craters of Denudation" prosper; I pin my faith to this view. (484/1. "On Craters of Denudation, with Observations on the Structure and Growth of Volcanic Cones." "Proc. Geol. Soc." Volume VI., 1850, pages 207-34. In a letter to Bunbury (January 17th, 1850) Lyell wrote:..."Darwin adopts my views as to Mauritius, St. Jago, and so-called elevation craters, which he has examined, and was puzzled with."—"Life of Sir Charles Lyell," Volume II., page 158.)

Please tell Sir C. Lyell that outside the crater-like mountains at St. Jago, even throughout a distance of two or three miles, there has been much denudation of the older volcanic rocks contemporaneous with those of the ring of mountains. (484/2. The island of St. Jago, one of the Cape de Verde group, is fully described in the "Volcanic Islands," Chapter 1.)

I hope that you will not find the page troublesome, and that you will forgive me asking you.

LETTER 485. TO C. LYELL. {November 6th, 1849}.

I have been deeply interested in your letter, and so far, at least, worthy of the time it must have cost you to write it. I have not much to say. I look at the whole question as settled. Santorin is splendid! it is conclusive! it is perfect! (485/1. "The Gulf of Santorin, in the Grecian Archipelago, has been for two thousand years a scene of active volcanic operations. The largest of the three outer islands of the groups (to which the general name of Santorin is given) is called Thera (or sometimes Santorin), and forms more than two-thirds of the circuit of the Gulf" ("Principles of Geology," Volume II., Edition X., London, 1868, page 65). Lyell attributed "the moderate slope of the beds in Thera...to their having originally descended the inclined flanks of a large volcanic cone..."; he refuted the theory of "Elevation Craters" by Leopold von Buch, which explained the slope of the rocks in a volcanic mountain by assuming that the inclined beds had been originally horizontal and subsequently tilted by an explosion.) You have read Dufrenoy in a hurry, I think, and added to the difficulty—it is the whole hill or "colline" which is composed of tuff with cross-stratification; the central boss or "monticule" is simply trachyte. Now, I have described one tuff crater at Galapagos (page 108) (485/2. The pages refer to Darwin's "Geological Observations on the Volcanic Islands, etc." 1844.) which has broken through a great solid sheet of basalt: why should not an irregular mass of trachyte have been left in the middle after the explosion and emission of mud which produced the overlying tuff? Or, again, I see no difficulty in a mass of trachyte being exposed by subsequent dislocations and bared or cleaned by rain. At Ascension (page 40), subsequent to the last great aeriform explosion, which has covered the country with fragments, there have been dislocations and a large circular subsidence...Do not quote Banks' case (485/3. This refers to Banks' Cove: see "Volcanic Islands," page 107.) (for there has been some denudation there), but the "elliptic one" (page 105), which is 1,500 yards (three-quarters of a nautical mile) in internal diameter...and is the very one the inclination of whose mud stream on tuff strata I measured (before I had ever heard the name Dufrenoy) and found varying from 25 to 30 deg. Albemarle Island, instead of being a crater of elevation, as Von Buch foolishly guessed, is formed of four great subaerial basaltic volcanoes (page 103), of one of which you might like to know the external diameter of the summit or crater was above three nautical miles. There are no "craters of denudation" at Galapagos. (485/4. See Lyell "On Craters of Denudation, with Observations on the Structure and Growth of Volcanic Cones," "Quart. Journ. Geol. Soc." Volume VI., 1850, page 207.)

I hope you will allude to Mauritius. I think this is the instance on the largest scale of any known, though imperfectly known.

If I were you I would give up consistency (or, at most, only allude in note to your old edition) and bring out the Craters of Denudation as a new view, which it essentially is. You cannot, I think, give it prominence as a novelty and yet keep to consistency and passages in old editions. I should grudge this new view being smothered in your address, and should like to see a separate paper. The one great channel to Santorin and Palma, etc., etc., is just like the one main channel being kept open in atolls and encircling barrier reefs, and on the same principle of water being driven in through several shallow breaches.

I of course utterly reprobate my wild notion of circular elevation; it is a satisfaction to me to think that I perceived there was a screw loose in the old view, and, so far, I think I was of some service to you.

Depend on it, you have for ever smashed, crushed, and abolished craters of elevation. There must be craters of engulfment, and of explosion (mere modifications of craters of eruption), but craters of denudation are the ones which have given rise to all the discussions.

Pray give my best thanks to Lady Lyell for her translation, which was as clear as daylight to me, including "leglessness."

LETTER 486. TO C. LYELL.

Down {November 20th, 1849}.

I remembered the passage in E. de B. {Elie de Beaumont} and have now re-read it. I have always and do still entirely disbelieve it; in such a wonderful case he ought to have hammered every inch of rock up to actual junction; he describes no details of junction, and if I were in your place I would absolutely dispute the fact of junction (or articulation as he oddly calls it) on such evidence. I go farther than you; I do not believe in the world there is or has been a junction between a dike and stream of lava of exact shape of either (1) or (2)

If dike gave immediate origin to volcanic vent we should have craters of {an} elliptic shape. I believe that when the molten rock in a dike comes near to the surface, some one two or three points will always certainly chance to afford an easier passage upward to the actual surface than along the whole line, and therefore that the dike will be connected (if the whole were bared and dissected) with the vent by a column or cone (see my elegant drawing) of lava. I do not doubt that the dikes are thus indirectly connected with eruptive vents. E. de B. seems to have observed many of his T; now without he supposes the whole line of fissure or dike to have poured out lava (which implies, as above remarked, craters of an elliptic or almost linear shape) on both sides, how extraordinarily improbable it is, that there should have been in a single line of section so many intersections of points eruption; he must, I think, make his orifices of eruption almost linear or, if not so, astonishingly numerous. One must refer to what one has seen oneself: do pray, when you go home, look at the section of a minute cone of eruption at the Galapagos, page 109 (486/1. "Geological Observations on Volcanic Islands." London, 1890, page 238.), which is the most perfect natural dissection of a crater which I have ever heard of, and the drawing of which you may, I assure you, trust; here the arching over of the streams as they were poured out over the lip of the crater was evident, and are now thus seen united to the central irregular column. Again, at St. Jago I saw some horizontal sections of the bases of small craters, and the sources or feeders were circular. I really cannot entertain a doubt that E. de B. is grossly wrong, and that you are right in your view; but without most distinct evidence I will never admit that a dike joins on rectangularly to a stream of lava. Your argument about the perpendicularity of the dike strikes me as good.

The map of Etna, which I have been just looking at, looks like a sudden falling in, does it not? I am not much surprised at the linear vent in Santorin (this linear tendency ought to be difficult to a circular-crater-of-elevation-believer), I think Abich (486/2. "Geologische Beobachtungen uber die vulkanischen Erscheinungen und Bildungen in Unter- und Mittel-Italien." Braunschweig, 1841.) describes having seen the same actual thing forming within the crater of Vesuvius. In such cases what outline do you give to the upper surface of the lava in the dike connecting them? Surely it would be very irregular and would send up irregular cones or columns as in my above splendid drawing.

At the Royal on Friday, after more doubt and misgiving than I almost ever felt, I voted to recommend Forbes for Royal Medal, and that view was carried, Sedgwick taking the lead.

I am glad to hear that all your party are pretty well. I know from experience what you must have gone through. From old age with suffering death must be to all a happy release. (486/3. This seems to refer to the death of Sir Charles Lyell's father, which occurred on November 8th, 1849.)

I saw Dan Sharpe the other day, and he told me he had been working at the mica schist (i.e. not gneiss) in Scotland, and that he was quite convinced my view was right. You are wrong and a heretic on this point, I know well.

LETTER 487. TO C.H.L. WOODD. Down, March 4th {1850}.

(487/1. The paper was sent in MS., and seems not to have been published. Mr. Woodd was connected by marriage with Mr. Darwin's cousin, the late Rev. W. Darwin Fox. It was perhaps in consequence of this that Mr. Darwin proposed Mr. Woodd for the Geological Society.)

I have read over your paper with attention; but first let me thank you for your very kind expressions towards myself. I really feel hardly competent to discuss the questions raised by your paper; I feel the want of mathematical mechanics. All such problems strike me as awfully complicated; we do not even know what effect great pressure has on retarding liquefaction by heat, nor, I apprehend, on expansion. The chief objection which strikes me is a doubt whether a mass of strata, when heated, and therefore in some slight degree at least softened, would bow outwards like a bar of metal. Consider of how many subordinate layers each great mass would be composed, and the mineralogical changes in any length of any one stratum: I should have thought that the strata would in every case have crumpled up, and we know how commonly in metamorphic strata, which have undergone heat, the subordinate layers are wavy and sinuous, which has always been attributed to their expansion whilst heated.

Before rocks are dried and quarried, manifold facts show how extremely flexible they are even when not at all heated. Without the bowing out and subsequent filling in of the roof of the cavity, if I understand you, there would be no subsidence. Of course the crumpling up of the strata would thicken them, and I see with you that this might compress the underlying fluidified rock, which in its turn might escape by a volcano or raise a weaker part of the earth's crust; but I am too ignorant to have any opinion whether force would be easily propagated through a viscid mass like molten rock; or whether such viscid mass would not act in some degree like sand and refuse to transmit pressure, as in the old experiment of trying to burst a piece of paper tied over the end of a tube with a stick, an inch or two of sand being only interposed. I have always myself felt the greatest difficulty in believing in waves of heat coming first to this and then to that quarter of the world: I suspect that heat plays quite a subordinate part in the upward and downward movements of the earth's crust; though of course it must swell the strata where first affected. I can understand Sir J. Herschel's manner of bringing heat to unheated strata—namely, by covering them up by a mile or so of new strata, and then the heat would travel into the lower ones.

But who can tell what effect this mile or two of new sedimentary strata would have from mere gravity on the level of the supporting surface? Of course such considerations do not render less true that the expansion of the strata by heat would have some effect on the level of the surface; but they show us how awfully complicated the phenomenon is. All young geologists have a great turn for speculation; I have burned my fingers pretty sharply in that way, and am now perhaps become over-cautious; and feel inclined to cavil at speculation when the direct and immediate effect of a cause in question cannot be shown. How neatly you draw your diagrams; I wish you would turn your attention to real sections of the earth's crust, and then speculate to your heart's content on them; I can have no doubt that speculative men, with a curb on, make far the best observers. I sincerely wish I could have made any remarks of more interest to you, and more directly bearing on your paper; but the subject strikes me as too difficult and complicated. With every good wish that you may go on with your geological studies, speculations, and especially observations...

LETTER 488. TO C. LYELL. Down, March 24th {1853}.

I have often puzzled over Dana's case, in itself and in relation to the trains of S. American volcanoes of different heights in action at the same time (page 605, Volume V. "Geological Transactions." (488/1. "On the Connection of certain Volcanic Phenomena in South America, and on the Formation of Mountain Chains and Volcanoes, as the Effect of the same Power by which Continents are Elevated" ("Trans. Geol. Soc." Volume V., page 601, 1840). On page 605 Darwin records instances of the simultaneous activity after an earthquake of several volcanoes in the Cordillera.)) I can throw no light on the subject. I presume you remember that Hopkins (488/2. See "Report on the Geological Theories of Elevation and Earthquakes," by W. Hopkins, "Brit. Assoc. Rep." 1847, page 34.) in some one (I forget which) of his papers discusses such cases, and urgently wishes the height of the fluid lava was known in adjoining volcanoes when in contemporaneous action; he argues vehemently against (as far as I remember) volcanoes in action of different heights being connected with one common source of liquefied rock. If lava was as fluid as water, the case would indeed be hopeless; and I fancy we should be led to look at the deep-seated rock as solid though intensely hot, and becoming fluid as soon as a crack lessened the tension of the superincumbent strata. But don't you think that viscid lava might be very slow in communicating its pressure equally in all directions? I remember thinking strongly that Dana's case within the one crater of Kilauea proved too much; it really seems monstrous to suppose that the lava within the same crater is not connected at no very great depth.

When one reflects on (and still better sees) the enormous masses of lava apparently shot miles high up, like cannon-balls, the force seems out of all proportion to the mere gravity of the liquefied lava; I should think that a channel a little straightly or more open would determine the line of explosion, like the mouth of a cannon compared to the touch-hole. If a high-pressure boiler was cracked across, no one would think for a moment that the quantity of water and steam expelled at different points depended on the less or greater height of the water within the boiler above these points, but on the size of the crack at these points; and steam and water might be driven out both at top and bottom. May not a volcano be likened to a protruding and cracked portion on a vast natural high-pressure boiler, formed by the surrounding area of country? In fact, I think my simile would be truer if the difference consisted only in the cracked case of the boiler being much thicker in some parts than in others, and therefore having to expel a greater thickness or depth of water in the thicker cracks or parts—a difference of course absolutely as nothing.

I have seen an old boiler in action, with steam and drops of water spurting out of some of the rivet-holes. No one would think whether the rivet-holes passed through a greater or less thickness of iron, or were connected with the water higher or lower within the boiler, so small would the gravity be compared with the force of the steam. If the boiler had been not heated, then of course there would be a great difference whether the rivet-holes entered the water high or low, so that there was greater or less pressure of gravity. How to close my volcanic rivet-holes I don't know.

I do not know whether you will understand what I am driving at, and it will not signify much whether you do or not. I remember in old days (I may mention the subject as we are on it) often wishing I could get you to look at continental elevations as THE phenomenon, and volcanic outbursts and tilting up of mountain chains as connected, but quite secondary, phenomena. I became deeply impressed with the truth of this view in S. America, and I do not think you hold it, or if so make it clear: the same explanation, whatever it may be, which will account for the whole coast of Chili rising, will and must apply to the volcanic action of the Cordillera, though modified no doubt by the liquefied rock coming to the surface and reaching water, and so {being} rendered explosive. To me it appears that this ought to be borne in mind in your present subject of discussion. I have written at too great length; and have amused myself if I have done you no good—so farewell.

LETTER 489. TO C. LYELL. Down, July 5th {1856}.

I am very much obliged for your long letter, which has interested me much; but before coming to the volcanic cosmogony I must say that I cannot gather your verdict as judge and jury (and not as advocate) on the continental extensions of late authors (489/1. See "Life and Letters," II., page 74; Letter to Lyell, June 25th, 1856: also letters in the sections of the present work devoted to Evolution and Geographical Distribution.), which I must grapple with, and which as yet strikes me as quite unphilosophical, inasmuch as such extensions must be applied to every oceanic island, if to any one, as to Madeira; and this I cannot admit, seeing that the skeletons, at least, of our continents are ancient, and seeing the geological nature of the oceanic islands themselves. Do aid me with your judgment: if I could honestly admit these great {extensions}, they would do me good service.

With respect to active volcanic areas being rising areas, which looks so pretty on the coral maps, I have formerly felt "uncomfortable" on exactly the same grounds with you, viz. maritime position of volcanoes; and still more from the immense thicknesses of Silurian, etc., volcanic strata, which thicknesses at first impress the mind with the idea of subsidence. If this could be proved, the theory would be smashed; but in deep oceans, though the bottom were rising, great thicknesses of submarine lava might accumulate. But I found, after writing Coral Book, cases in my notes of submarine vesicular lava-streams in the upper masses of the

Cordillera, formed, as I believe, during subsidence, which staggered me greatly. With respect to the maritime position of volcanoes, I have long been coming to the conclusion that there must be some law causing areas of elevation (consequently of land) and of subsidence to be parallel (as if balancing each other) and closely approximate; I think this from the form of continents with a deep ocean on one side, from coral map, and especially from conversations with you on immense subsidences of the Carboniferous and {other} periods, and yet with continued great supply of sediment. If this be so, such areas, with opposite movements, would probably be separated by sets of parallel cracks, and would be the seat of volcanoes and tilts, and consequently volcanoes and mountains would be apt to be maritime; but why volcanoes should cling to the rising edge of the cracks I cannot conjecture. That areas with extinct volcanic archipelagoes may subside to any extent I do not doubt.

Your view of the bottom of Atlantic long sinking with continued volcanic outbursts and local elevations at Madeira, Canaries, etc., grates (but of course I do not know how complex the phenomena are which are thus explained) against my judgment; my general ideas strongly lead me to believe in elevatory movements being widely extended. One ought, I think, never to forget that when a volcano is in action we have distinct proof of an action from within outwards. Nor should we forget, as I believe follows from Hopkins (489/2. "Researches in Physical Geology," W. Hopkins, "Trans. Phil. Soc. Cambridge," Volume VI., 1838. See also "Report on the Geological Theories of Elevation and Earthquakes," W. Hopkins, "Brit. Assoc. Rep." page 33, 1847 (Oxford meeting).), and as I have insisted in my Earthquake paper, that volcanoes and mountain chains are mere accidents resulting from the elevation of an area, and as mountain chains are generally long, so should I view areas of elevation as generally large. (489/3. "On the Connexion of certain Volcanic Phenomena in S. America, and on the Formation of Mountain Chains and Volcanoes, as the Effect of the same Power by which Continents are Elevated," "Trans. Geol. Soc." Volume V., page 601, 1840. "Bearing in mind Mr. Hopkins' demonstration, if there be considerable elevation there must be fissures, and, if fissures, almost certainly unequal upheaval, or subsequent sinking down, the argument may be finally thus put: mountain chains are the effects of continental elevations; continental elevations and the eruptive force of volcanoes are due to one great motive, now in progressive action..." (loc. cit., page 629).)

Your old original view that great oceans must be sinking areas, from there being causes making land and yet there being little land, has always struck me till lately as very good. But in some degree this starts from the assumption that within periods of which we know anything there was either a continent in such areas, or at least a sea-bottom of not extreme depth.

LETTER 490. TO C. LYELL. King's Head Hotel, Sandown, Isle of Wight, July 18th {1858}.

I write merely to thank you for the abstract of the Etna paper. (490/1. "On the Structure of Lavas which have Consolidated on Steep Slopes, with Remarks on the Mode of Origin of Mount Etna and on the Theory of 'Craters of Elevation,'" by C. Lyell, "Phil. Trans. R. Soc." Volume CXLVIII., page 703, 1859.) It seems to me a very grand contribution to our volcanic knowledge. Certainly I never expected to see E. de B.'s {Elie de Beaumont} theory of slopes so completely upset. He must have picked out favourable cases for measurement. And such an array of facts he gives! You have scotched, and will see die, I now think, the Crater of Elevation theory. But what vitality there is in a plausible theory! (490/2. The rest of this letter is published in "Life and Letters," II., page 129.)

LETTER 491. TO C. LYELL. Down, November 25th {1860}.

I have endeavoured to think over your discussion, but not with much success. You will have to lay down, I think, very clearly, what foundation you argue from—four parts (which seems to me exceedingly moderate on your part) of Europe being now at rest, with one part undergoing movement. How it is, that from this you can argue that the one part which is now moving will have rested since the commencement of the Glacial period in the proportion of four to one, I do not pretend to see with any clearness; but does not your argument rest on the assumption that within a given period, say two or three million years, the whole of Europe necessarily has to undergo movement? This may be probable or not so, but it seems to me that you must explain the foundation of your argument from space to time, which at first, to me was very far from obvious. I can, of course, see that if you can make out your argument satisfactorily to yourself and others it would be most valuable. I can imagine some one saying that it is not fair to argue that the great plains of Europe and the mountainous districts of Scotland and Wales have been at all subjected to the same laws of movement. Looking to the whole world, it has been my opinion, from the very size of the continents and oceans, and especially from the enormous ranges of so many mountain-chains (resulting from cracks which follow from vast areas of elevation, as Hopkins argues (491/1. See "Report on the Geological Theories of Elevation and Earthquakes." by William Hopkins. "Brit. Assoc. Rep." 1847, pages 33-92; also the Anniversary Address to the Geological Society by W. Hopkins in 1852 ("Quart. Journ. Geol. Soc." Volume VIII.); in this Address, pages lxviii et seq.) reference is made to the theory of elevation which rests on the supposition "of the simultaneous action of an upheaving force at every point of the area over which the phenomena of elevation preserve a certain character of continuity...The elevated mass...becomes stretched, and is ultimately torn and fissured in those directions in which the tendency thus to tear is greatest...It is thus that the complex phenomena of elevation become referable to a general and simple mechanical cause...")) and from other reasons, it has been my opinion that, as a general rule, very large portions of the world have been simultaneously affected by elevation or subsidence. I can see that this does not apply so strongly to broken Europe, any more than to the Malay Archipelago. Yet, had I been asked, I should have said that probably nearly the whole of Europe was subjected during the Glacial period to periods of elevation and of subsidence. It does not seem to me so certain that the kinds of partial movement which we now see going on show us the kind of movement which Europe has been subjected to since the commencement of the Glacial period. These notions are at least possible, and would they not vitiate your argument? Do you not rest on the belief that, as Scandinavia and some few other parts are now rising, and a few others sinking, and the remainder at rest, so it has been since the commencement of the Glacial period? With my notions I should require this to be made pretty probable before I could put much confidence in your calculations. You have probably thought this all over, but I give you the reflections which come across me, supposing for the moment that you took the proportions of space at rest and in movement as plainly applicable to time. I have no doubt that you have sufficient evidence that, at the commencement of the Glacial period, the land in Scotland, Wales, etc., stood as high or higher than at present, but I forget the proofs.

Having burnt my own fingers so consumedly with the Wealden, I am fearful for you, but I well know how infinitely more cautious, prudent, and far-seeing you are than I am; but for heaven's sake take care of your fingers; to burn them severely, as I have done, is very unpleasant.

Your 2 1/2 feet for a century of elevation seems a very handsome allowance. can D. Forbes really show the great elevation of Chili? I am astounded at it, and I took some pains on the point.

I do not pretend to say that you may not be right to judge of the past movements of Europe by those now and recently going on, yet it somehow grates against my judgment,—perhaps only against my prejudices.

As a change from elevation to subsidence implies some great subterranean or cosmical change, one may surely calculate on long intervals of rest between. Though, if the cause of the change be ever proved to be astronomical, even this might be doubtful.

P.S.—I do not know whether I have made clear what I think probable, or at least possible: viz., that the greater part of Europe has at times been elevated in some degree equably; at other times it has all subsided equably; and at other times might all have been stationary; and at other times it has been subjected to various unequal movements, up and down, as at present.

LETTER 492. TO C. LYELL. Down, December 4th {1860}.

It certainly seems to me safer to rely solely on the slowness of ascertained up-and-down movement. But you could argue length of probable time before the movement became reversed, as in your letter. And might you not add that over the whole world it would probably be admitted that a larger area is NOW at rest than in movement? and this I think would be a tolerably good reason for supposing long intervals of rest. You might even adduce Europe, only guarding yourself by saying that possibly (I will not say probably, though my prejudices would lead me to say so) Europe may at times have gone up and down all together. I forget whether in a former letter you made a strong point of upward movement being always interrupted by long periods of rest. After writing to you, out of curiosity I glanced at the early chapters in my "Geology of South America," and the areas of elevation on the E. and W. coasts are so vast, and proofs of many successive periods of rest so striking, that the evidence becomes to my mind striking. With regard to the astronomical causes of change: in ancient days in the "Beagle" when I reflected on the repeated great oscillations of level on the very same area, and when I looked at the symmetry of mountain chains over such vast spaces, I used to conclude that the day would come when the slow change of form in the semi-fluid matter beneath the crust would be found to be the cause of volcanic action, and of all changes of level. And the late discussion in the "Athenaeum" (492/1. "On the Change of Climate in Different Regions of the Earth." Letters from Sir Henry James, Col. R.E., "Athenaeum," August 25th, 1860, page 256; September 15th, page 355; September 29th, page 415; October 13th, page 483. Also letter from J. Beete Jukes, Local Director of the Geological Survey of Ireland, loc. cit., September 8th, page 322; October 6th, page 451.), by Sir H. James (though his letter seemed to me mighty poor, and what Jukes wrote good), reminded me of this notion. In case astronomical agencies should ever be proved or rendered probable, I imagine, as in nutation or precession, that an upward movement or protrusion of fluidified matter below might be immediately followed by movement of an opposite nature. This is all that I meant.

I have not read Jamieson, or yet got the number. (492/2. Possibly William Jameson, "Journey from Quito to Cayambe," "Geog. Soc. Journ." Volume XXXI., page 184, 1861.) I was very much struck with Forbes' explanation of n{itrate} of soda beds and the saliferous crust, which I saw and examined at Iquique. (492/3. "On the Geology of Bolivia and Southern Peru," by D. Forbes, "Quart. Journ. Geol. Soc." Volume XVII., page 7, 1861. Mr. Forbes attributes the formation of the saline deposits to lagoons of salt water, the communication of which with the sea has been cut off by the rising of the land (loc. cit., page 13).) I often speculated on the greater rise inland of the Cordilleras, and could never satisfy myself...

I have not read Stur, and am awfully behindhand in many things...(492/4. The end of this letter is published as a footnote in "Life and Letters," II., page 352.)

LETTER 493. TO C. LYELL. Down, July 18th {1867}.

(493/1. The first part of this letter is published in "Life and Letters," III., page 71.)

(493/2. Tahiti (Society Islands) is coloured blue in the map showing the distribution of the different kinds of reefs in "The Structure and Distribution of Coral Reefs," Edition III., 1889, page 185. The blue colour indicates the existence of barrier reefs and atolls which, on Darwin's theory, point to subsidence.)

Tahiti is, I believe, rightly coloured, for the reefs are so far from the land, and the ocean so deep, that there must have been subsidence, though not very recently. I looked carefully, and there is no evidence of recent elevation. I quite agree with you versus Herschel on Volcanic Islands. (493/3. Sir John Herschel suggested that the accumulation on the sea-floor of sediment, derived from the waste of the island, presses down the bed of the ocean, the continent being on the other hand relieved of pressure; "this brings about a state of strain in the crust which will crack in its weakest spot, the heavy side going down, and the light side rising." In discussing this view Lyell writes ("Principles," Volume II. Edition X., page 229), "This hypothesis appears to me of very partial application, for active volcanoes, even such as are on the borders of continents, are rarely situated where great deltas have been forming, whether in Pliocene or post-Tertiary times. The number, also, of active volcanoes in oceanic islands is very great, not only in the Pacific, but equally in the Atlantic, where no load of coral matter...can cause a partial weighting and pressing down of a supposed flexible crust.") Would not the Atlantic and Antarctic volcanoes be the best examples for you, as there then can be no coral mud to depress the bottom? In my "Volcanic Islands," page 126, I just suggest that volcanoes may occur so frequently in the oceanic areas as the surface would be most likely to crack when first being elevated. I find one remark, page 128 (493/4. "Volcanic Islands," page 128: "The islands, moreover, of some of the small volcanic groups, which thus border continents, are placed in lines related to those along which the adjoining shores of the continents trend"),which seems to me worth consideration—viz. the parallelism of the lines of eruption in volcanic archipelagoes with the coast lines of the nearest continent, for this seems to indicate a mechanical rather than a chemical connection in both cases, i.e. the lines of disturbance and cracking. In my "South American Geology," page 185 (493/5. "Geological Observations on South America," London, 1846, page 185.), I allude to the remarkable absence at present of active volcanoes on the east side of the Cordillera in relation to the absence of the sea on this side. Yet I must own I have long felt a little sceptical on the proximity of water being the exciting cause. The one volcano in the interior of Asia is said, I think, to be near great lakes; but if lakes are so important, why are there not many other volcanoes within other continents? I have always felt rather inclined to look at the position of volcanoes on the borders of continents, as resulting from coast lines being the lines of separation between areas of elevation and subsidence. But it is useless in me troubling you with my old speculations.

LETTER 494. TO A.R. WALLACE. March 22nd {1869}.

(494/1. The following extract from a letter to Mr. Wallace refers to his "Malay Archipelago," 1869.)

I have only one criticism of a general nature, and I am not sure that other geologists would agree with me. You repeatedly speak as if the pouring out of lava, etc., from volcanoes actually caused the subsidence of an adjoining area. I quite agree that areas undergoing opposite movements are somehow connected; but volcanic outbursts must, I think, be looked at as mere accidents in the swelling up of a great dome or surface of plutonic rocks, and there seems no more reason to conclude that such swelling or elevation in mass is the cause of the subsidence, than that the subsidence is the cause of the elevation, which latter view is indeed held by some geologists. I have regretted to find so little about the habits of the many animals which you have seen.

LETTER 495. TO C. LYELL. Down, May 20th, 1869.

I have been much pleased to hear that you have been looking at my S. American book (495/1. "Geological Observations on South America," London, 1846.), which I thought was as completely dead and gone as any pre-Cambrian fossil. You are right in supposing that my memory about American geology has grown very hazy. I remember, however, a paper on the Cordillera by D. Forbes (495/2. "Geology of Bolivia and South Peru," by Forbes, "Quart. Journ. Geol. Soc." Volume XVII., pages 7-62, 1861. Forbes admits that there is "the fullest evidence of elevation of the Chile coast since the arrival of the Spaniards. North of Arica, if we accept the evidence of M. d'Orbigny and others, the proof of elevation is much more decided; and consequently it may be possible that here, as is the case about Lima, according to Darwin, the elevation may have taken place irregularly in places..." (loc. cit., page 11).), with splendid sections, which I saw in MS., but whether "referred" to me or lent to me I cannot remember. This would be well worth your looking to, as I think he both supports and criticises my views. In Ormerod's Index to the Journal (495/3. "Classified Index to the Transactions, Proceedings and Quarterly Journal of the Geological Society."), which I do not possess, you would, no doubt, find a reference; but I think the sections would be worth borrowing from Forbes. Domeyko (495/4. Reference is made by Forbes in his paper on Bolivia and Peru to the work of Ignacio Domeyko on the geology of Chili. Several papers by this author were published in the "Annales des Mines" between 1840 and 1869, also in the "Comptes Rendus" of 1861, 1864, etc.) has published in the "Comptes Rendus" papers on Chili, but not, as far as I can remember, on the structure of the mountains. Forbes, however, would know. What you say about the plications being steepest in the central and generally highest part of the range is conclusive to my mind that there has been the chief axis of disturbance. The lateral thrusting has always appeared to me fearfully perplexing. I remember formerly thinking that all lateral flexures probably occurred deep beneath the surface, and have been brought into view by an enormous superincumbent mass having been denuded. If a large and deep box were filled with layers of damp paper or clay, and a blunt wedge was slowly driven up from beneath, would not the layers above it and on both sides become greatly convoluted, whilst those towards the top would be only slightly arched? When I spoke of the Andes being comparatively recent, I suppose that I referred to the absence of the older formations. In looking to my volume, which I have not done for many years, I came upon a passage (page 232) which would be worth your looking at, if you have ever felt perplexed, as I often was, about the sources of volcanic rocks in mountain chains. You have stirred up old memories, and at the risk of being a bore I should like to call your attention to another point which formerly perplexed me much—viz. the presence of basaltic dikes in most great granitic areas. I cannot but think the explanation given at page 123 of my "Volcanic Islands" is the true one. (495/5. On page 123 of the "Geological Observations on the Volcanic Islands visited during the Voyage of H.M.S. 'Beagle,'" 1844, Darwin quotes several instances of greenstone and basaltic dikes intersecting granitic and allied metamorphic rocks. He suggests that these dikes "have been formed by fissures penetrating into partially cooled rocks of the granitic and metamorphic series, and by their more fluid parts, consisting chiefly of hornblende oozing out, and being sucked into such fissures.")

LETTER 496. TO VICTOR CARUS. Down, March 21st, 1876.

The very kind expressions in your letter have gratified me deeply.

I quite forget what I said about my geological works, but the papers referred to in your letter are the right ones. I enclose a list with those which are certainly not worth translating marked with a red line; but whether those which are not thus marked with a red line are worth translation you will have to decide. I think much more highly of my book on "Volcanic Islands" since Mr. Judd, by far the best judge on the subject in England, has, as I hear, learnt much from it.

I think the short paper on the "formation of mould" is worth translating, though, if I have time and strength, I hope to write another and longer paper on the subject.

I can assure you that the idea of any one translating my books better than you never even momentarily crossed my mind. I am glad that you can give a fairly good account of your health, or at least that it is not worse.

LETTER 497. TO T. MELLARD READE. London, December 9th, 1880.

I am sorry to say that I do not return home till the middle of next week, and as I order no pamphlets to be forwarded to me by post, I cannot return the "Geolog. Mag." until my return home, nor could my servants pick it out of the multitude which come by the post. (497/1. Article on "Oceanic Islands," by T. Mellard Reade, "Geol. Mag." Volume VIII., page 75, 1881.)

As I remarked in a letter to a friend, with whom I was discussing Wallace's last book (497/2. Wallace's "Island Life," 1880.), the subject to which you refer seems to me a most perplexing one. The fact which I pointed out many years ago, that all oceanic islands are volcanic (except St. Paul's, and now this is viewed by some as the nucleus of an ancient volcano), seems to me a strong argument that no continent ever occupied the great oceans. (497/3. "During my investigations on coral reefs I had occasion to consult the works of many voyagers, and I was invariably struck with the fact that, with rare exceptions, the innumerable islands scattered through the Pacific, Indian, and Atlantic Oceans were composed either of volcanic or of modern coral rocks" ("Geological Observations on Volcanic Islands, etc." Edition II., 1876, page 140).) Then there comes the statement from the "Challenger" that all sediment is deposited within one or two hundred miles from the shores, though I should have thought this rather doubtful with respect to great rivers like the Amazons.

The chalk formerly seemed to me the best case of an ocean having extended where a continent now stands; but it seems that some good judges deny that the chalk is an oceanic deposit. On the whole, I lean to the side that the continents have since Cambrian times occupied approximately their present positions. But, as I have said, the question seems a difficult one, and the more it is discussed the better.

LETTER 498. TO A. AGASSIZ. Down, January 1st, 1881.

I must write a line or two to thank you much for having written to me so long a letter on coral reefs at a time when you must have been so busy. Is it not difficult to avoid believing that the wonderful elevation in the West Indies must have been accompanied by much subsidence, notwithstanding the state of Florida? (498/1. The Florida reefs cannot be explained by subsidence. Alexander Agassiz, who has described these reefs in detail ("Three Cruises of the U.S. Coast and Geodetic Survey Steamer 'Blake,'" 2 volumes, London, 1888), shows that the southern extremity of the peninsula "is of comparatively recent growth, consisting of concentric barrier-reefs, which have been gradually converted into land by the accumulation of intervening mud-flats" (see also Appendix II., page 287, to Darwin's "Coral Reefs," by T.G. Bonney, Edition III., 1889.)) When reflecting in old days on the configuration of our continents, the position of mountain chains, and especially on the long-continued supply of sediment over the same areas, I used to think (as probably have many other persons) that areas of elevation and subsidence must as a general rule be separated by a single great line of fissure, or rather of several closely adjoining lines of fissure. I mention this because, when looking within more recent times at charts with the depths of the sea marked by different tints, there seems to be some connection between the profound depths of the ocean and the trends of the nearest, though distant, continents; and I have often wished that some one like yourself, to whom the subject was familiar, would speculate on it.

P.S.—I do hope that you will re-urge your views about the reappearance of old characters (498/2. See "Life and Letters," III., pages 245, 246.), for, as far as I can judge, the most important views are often neglected unless they are urged and re-urged.

I am greatly indebted to you for sending me very many most valuable works published at your institution.

2.IX.II. ICE-ACTION, 1841-1882. LETTER 499. TO C. LYELL. {1841.}

Your extract has set me puzzling very much, and as I find I am better at present for not going out, you must let me unload my mind on paper. I thought everything so beautifully clear about glaciers, but now your case and Agassiz's statement about the cavities in the rock formed by cascades in the glaciers, shows me I don't understand their structure at all. I wish out of pure curiosity I could make it out. (499/1. "Etudes sur les Glaciers," by Louis Agassiz, 1840, contains a description of cascades (page 343), and "des cavites interieures" (page 348).)

If the glacier travelled on (and it certainly does travel on), and the water kept cutting back over the edge of the ice, there would be a great slit in front of the cascade; if the water did not cut back, the whole hollow and cascade, as you say, must travel on; and do you suppose the next season it falls down some crevice higher up? In any case, how in the name of Heaven can it make a hollow in solid rock, which surely must be a work of many years? I must point out another fact which Agassiz does not, as it appears to me, leave very clear. He says all the blocks on the surface of the glaciers are angular, and those in the moraines rounded, yet he says the medial moraines whence the surface rocks come and are a part {of}, are only two lateral moraines united. Can he refer to terminal moraines alone when he says fragments in moraines are rounded? What a capital book Agassiz's is. In {reading} all the early part I gave up entirely the Jura blocks, and was heartily ashamed of my appendix (499/2. "M. Agassiz has lately written on the subject of the glaciers and boulders of the Alps. He clearly proves, as it appears to me, that the presence of the boulders on the Jura cannot be explained by any debacle, or by the power of ancient glaciers driving before them moraines...M. Agassiz also denies that they were transported by floating ice." ("Voyages of the 'Adventure' and 'Beagle,'" Volume III., 1839: "Journal and Remarks: Addenda," page 617.)) (and am so still of the manner in which I presumptuously speak of Agassiz), but it seems by his own confession that ordinary glaciers could not have transported the blocks there, and if an hypothesis is to be introduced the sea is much simpler; floating ice seems to me to account for everything as well as, and sometimes better than the solid glaciers. The hollows, however, formed by the ice-cascades appear to me the strongest hostile fact, though certainly, as you said, one sees hollow round cavities on present rock-beaches.

I am glad to observe that Agassiz does not pretend that direction of scratches is hostile to floating ice. By the way, how do you and Buckland account for the "tails" of diluvium in Scotland? (499/3. Mr. Darwin speaks of the tails of diluvium in Scotland extending from the protected side of a hill, of which the opposite side, facing the direction from which the ice came, is marked by grooves and striae (loc. cit., pages 622, 623).) I thought in my appendix this made out the strongest argument for rocks having been scratched by floating ice.

Some facts about boulders in Chiloe will, I think, in a very small degree elucidate some parts of Jura case. What a grand new feature all this ice work is in Geology! How old Hutton would have stared! (499/4. Sir Charles Lyell speaks of the Huttonian theory as being characterised by "the exclusion of all causes not supposed to belong to the present order of Nature" (Lyell's "Principles," Edition XII., volume I., page 76, 1875). Sir Archibald Geikie has recently edited the third volume of Hutton's "Theory of the Earth," printed by the Geological Society, 1899. See also "The Founders of Geology," by Sir Archibald Geikie; London, 1897.)

I ought to be ashamed of myself for scribbling on so. Talking of shame, I have sent a copy of my "Journal" (499/5. "Journal and Remarks," 1832-36. See note 2, page 148.) with very humble note to Agassiz, as an apology for the tone I used, though I say, I daresay he has never seen my appendix, or would care at all about it.

I did not suppose my note about Glen Roy could have been of any use to you—I merely scribbled what came uppermost. I made one great oversight, as you would perceive. I forgot the Glacier theory: if a glacier most gradually disappeared from mouth of Spean Valley {this} would account for buttresses of shingle below lowest shelf. The difficulty I put about the ice-barrier of the middle Glen Roy shelf keeping so long at exactly same level does certainly appear to me insuperable. (499/5. For a description of the shelves or parallel roads in Glen Roy see Darwin's "Observations on the Parallel Roads of Glen Roy, etc." "Phil. Trans. R. Soc." 1839, page 39; also Letter 517 et seq.)

What a wonderful fact this breakdown of old Niagara is. How it disturbs the calculations about lengths of time before the river would have reached the lakes.

I hope Mrs. Lyell will read this to you, then I shall trust for forgiveness for having scribbled so much. I should have sent back Agassiz sooner, but my servant has been very unwell. Emma is going on pretty well.

My paper on South American boulders and "till," which latter deposit is perfectly characterised in Tierra del Fuego, is progressing rapidly. (499/6. "On the Distribution of the Erratic Boulders and on the Contemporaneous Unstratified Deposits of South America," "Trans. Geol. Soc." Volume VI., page 415, 1842.)

I much like the term post-Pliocene, and will use it in my present paper several times.

P.S.—I should have thought that the most obvious objection to the marine-beach theory for Glen Roy would be the limited extension of the shelves. Though certainly this is not a valid one, after an intermediate one, only half a mile in length, and nowhere else appearing, even in the valley of Glen Roy itself, has been shown to exist.

LETTER 500. TO C. LYELL. 1842.

I had some talk with Murchison, who has been on a flying visit into Wales, and he can see no traces of glaciers, but only of the trickling of water and of the roots of the heath. It is enough to make an extraneous man think Geology from beginning to end a work of imagination, and not founded on observation. Lonsdale, I observe, pays Buckland and myself the compliment of thinking Murchison not seeing as worth nothing; but I confess I am astonished, so glaringly clear after two or three days did the evidence appear to me. Have you seen last "New Edin. Phil. Journ.", it is ice and glaciers almost from beginning to end. (500/1. "The Edinburgh New Philosophical Journal," Volume XXXIII. (April-October), 1842, contains papers by Sir G.S. Mackenzie, Prof. H.G. Brown, Jean de Charpentier, Roderick Murchison, Louis Agassiz, all dealing with glaciers or ice; also letters to the Editor relating to Prof. Forbes' account of his recent observations on Glaciers, and a paper by Charles Darwin entitled "Notes on the Effects produced by the Ancient Glaciers of Carnarvonshire, and on the Boulders transported by Floating Ice.") Agassiz says he saw (and has laid down) the two lowest terraces of Glen Roy in the valley of the Spean, opposite mouth of Glen Roy itself, where no one else has seen them. (500/2. "The Glacial Theory and its Recent Progress," by Louis Agassiz, loc. cit., page 216. Agassiz describes the parallel terraces on the flanks of Glen Roy and Glen Spean (page 236), and expresses himself convinced "that the Glacial theory alone satisfies all the exigencies of the phenomenon" of the parallel roads.) I carefully examined that spot, owing to the sheep tracks {being} nearly but not quite parallel to the terrace. So much, again, for difference of observation. I do not pretend to say who is right.

LETTER 501. TO J.D. HOOKER. Down, October 12th, 1849.

I was heartily glad to get your last letter; but on my life your thanks for my very few and very dull letters quite scalded me. I have been very indolent and selfish in not having oftener written to you and kept my ears open for news which would have interested you; but I have not forgotten you. Two days after receiving your letter, there was a short leading notice about you in the "Gardeners' Chronicle" (501/1. The "Gardeners' Chronicle," 1849, page 628.); in which it is said you have discovered a noble crimson rose and thirty rhododendrons. I must heartily congratulate you on these discoveries, which will interest the public; and I have no doubt that you will have made plenty of most interesting botanical observations. This last letter shall be put with all your others, which are now safe together. I am very glad that you have got minute details about the terraces in the valleys: your description sounds curiously like the terraces in the Cordillera of Chili; these latter, however, are single in each valley; but you will hereafter see a description of these terraces in my "Geology of S. America." (501/2. "Geological Observations," pages 10 et passim.) At the end of your letter you speak about giving up Geology, but you must not think of it; I am sure your observations will be very interesting. Your account of the great dam in the Yangma valley is most curious, and quite full; I find that I did not at all understand its wonderful structure in your former letter. Your notion of glaciers pushing detritus into deep fiords (and ice floating fragments on their channels), is in many respects new to me; but I cannot help believing your dam is a lateral moraine: I can hardly persuade myself that the remains of floating ice action, at a period so immensely remote as when the Himalaya stood at a low level in the sea, would now be distinguishable. (501/3. Hooker's "Himalayan Journals," Volume II., page 121, 1854. In describing certain deposits in the Lachoong valley, Hooker writes: "Glaciers might have forced immense beds of gravel into positions that would dam up lakes between the ice and the flanks of the valley" (page 121). In a footnote he adds: "We are still very ignorant of many details of ice action, and especially of the origin of many enormous deposits which are not true moraines." Such deposits are referred to as occurring in the Yangma valley.) Your not having found scored boulders and solid rocks is an objection both to glaciers and floating ice; for it is certain that both produce such. I believe no rocks escape scoring, polishing and mammillation in the Alps, though some lose it easily when exposed. Are you familiar with appearance of ice-action? If I understand rightly, you object to the great dam having been produced by a glacier, owing to the dryness of the lateral valley and general infrequency of glaciers in Himalaya; but pray observe that we may fairly (from what we see in Europe) assume that the climate was formerly colder in India, and when the land stood at a lower height more snow might have fallen. Oddly enough, I am now inclined to believe that I saw a gigantic moraine crossing a valley, and formerly causing a lake above it in one of the great valleys (Valle del Yeso) of the Cordillera: it is a mountain of detritus, which has puzzled me. If you have any further opportunities, do look for scores on steep faces of rock; and here and there remove turf or matted parts to have a look. Again I beg, do not give up Geology:—I wish you had Agassiz's work and plates on Glaciers. (501/4. "Etudes sur les Glaciers." L. Agassiz, Neuchatel, 1840.) I am extremely sorry that the Rajah, ill luck to him, has prevented your crossing to Thibet; but you seem to have seen most interesting country: one is astonished to hear of Fuegian climate in India. I heard from the Sabines that you were thinking of giving up Borneo; I hope that this report may prove true.

LETTER 502. TO C. LYELL. Down, May 8th {1855}.

The notion you refer to was published in the "Geological Journal" (502/1. "on the Transportal of Erratic Boulders from a lower to a higher Level." By C. Darwin.), Volume IV. (1848), page 315, with reference to all the cases which I could collect of boulders apparently higher than the parent rock.

The argument of probable proportion of rock dropped by sea ice compared to land glaciers is new to me. I have often thought of the idea of the viscosity and enormous momentum of great icebergs, and still think that the notion I pointed out in appendix to Ramsay's paper is probable, and can hardly help being applicable in some cases. (502/2. The paper by Ramsay has no appendix; probably, therefore Mr. Darwin's notes were published separately as a paper in the "Phil. Mag.") I wonder whether the "Phil. Journal {Magazine?.}" would publish it, if I could get it from Ramsay or the Geological Society. (502/3. "On the Power of Icebergs to make rectilinear, uniformly-directed grooves across a Submarine Undulatory Surface." By C. Darwin, "Phil. Mag." Volume X., page 96, 1855.) If you chance to meet Ramsay will you ask him whether he has it? I think it would perhaps be worth while just to call the N. American geologists' attention to the idea; but it is not worth any trouble. I am tremendously busy with all sorts of experiments. By the way, Hopkins at the Geological Society seemed to admit some truth in the idea of scoring by (viscid) icebergs. If the Geological Society takes so much {time} to judge of truth of notions, as you were telling me in regard to Ramsay's Permian glaciers (502/4. "On the Occurrence of angular, sub-angular, polished, and striated Fragments and Boulders in the Permian Breccia of Shropshire, Worcestershire, etc.; and on the Probable Existence of Glaciers and Icebergs in the Permian Epoch." By A.C. Ramsay, "Quart. Journ. Geol. Soc." Volume XI., page 185, 1855.), it will be as injurious to progress as the French Institut.

LETTER 503. TO J.D. HOOKER. Cliff Cottage, Bournemouth, {September} 21st {1862}.

I am especially obliged to you for sending me Haast's communications. (503/1. "Quart. Journ. Geol. Soc." Volume XXI., pages 130, 133, 1865; Volume XXIII., page 342, 1867.) They are very interesting and grand about glacial and drift or marine glacial. I see he alludes to the whole southern hemisphere. I wonder whether he has read the "Origin." Considering your facts on the Alpine plants of New Zealand and remarks, I am particularly glad to hear of the geological evidence of glacial action. I presume he is sure to collect and send over the mountain rat of which he speaks. I long to know what it is. A frog and rat together would, to my mind, prove former connection of New Zealand to some continent; for I can hardly suppose that the Polynesians introduced the rat as game, though so esteemed in the Friendly Islands. Ramsay sent me his paper (503/2.

"On the Glacial Origin of certain Lakes in Switzerland, etc." "Quart. Journ. Geol. Soc." Volume XVIII., page 185, 1862.) and asked my opinion on it. I agree with you and think highly of it. I cannot doubt that it is to a large extent true; my only doubt is, that in a much disturbed country, I should have thought that some depressions, and consequently lakes, would almost certainly have been left. I suggested a careful consideration of mountainous tropical countries such as Brazil, peninsula of India, etc.; if lakes are there, {they are} very rare. I should fully subscribe to Ramsay's views.

What presumption, as it seems to me, in the Council of Geological Society that it hesitated to publish the paper.

We return home on the 30th. I have made up {my} mind, if I can keep up my courage, to start on the Saturday for Cambridge, and stay the last few days of the {British} Association there. I do so hope that you may be there then.

LETTER 504. TO J.D. HOOKER. November 3rd {1864}.

When I wrote to you I had not read Ramsay. (504/1. "On the Erosion of Valleys and Lakes: a Reply to Sir Roderick Murchison's Anniversary Address to the Geographical Society." "Phil. Mag." Volume XXVIII., page 293, 1864) How capitally it is written! It seems that there is nothing for style like a man's dander being put up. I think I agree largely with you about denudation—but the rocky-lake-basin theory is the part which interests me at present. It seems impossible to know how much to attribute to ice, running water, and sea. I did not suppose that Ramsay would deny that mountains had been thrown up irregularly, and that the depressions would become valleys. The grandest valleys I ever saw were at Tahiti, and here I do not believe ice has done anything; anyhow there were no erratics. I said in my S. American Geology (504/2. "Finally, the conclusion at which I have arrived with respect to the relative powers of rain, and sea-water on the land is, that the latter is by far the most efficient agent, and that its chief tendency is to widen the valleys, whilst torrents and rivers tend to deepen them and to remove the wreck of the sea's destroying action" ("Geol. Observations," pages 66, 67).) that rivers deepen and the sea widens valleys, and I am inclined largely to stick to this, adding ice to water. I am sorry to hear that Tyndall has grown dogmatic. H. Wedgwood was saying the other day that T.'s writings and speaking gave him the idea of intense conceit. I hope it is not so, for he is a grand man of science.

...I have had a prospectus and letter from Andrew Murray (504/3. See Volume II., Letters 379, 384, etc.) asking me for suggestions. I think this almost shows he is not fit for the subject, as he gives me no idea what his book will be, excepting that the printed paper shows that all animals and all plants of all groups are to be treated of. Do you know anything of his knowledge?

In about a fortnight I shall have finished, except concluding chapter, my book on "Variation under Domestication"; (504/4. Published in 1868.) but then I have got to go over the whole again, and this will take me very many months. I am able to work about two hours daily.

LETTER 505. TO J.D. HOOKER. Down {July, 1865}.

I was glad to read your article on Glaciers, etc., in Yorkshire. You seem to have been struck with what most deeply impressed me at Glen Roy (wrong as I was on the whole subject)—viz. the marvellous manner in which every detail of surface of land had been preserved for an enormous period. This makes me a little sceptical whether Ramsay, Jukes, etc., are not a little overdoing sub-aerial denudation.

In the same "Reader" (505/1. Sir J.D. Hooker wrote to Darwin, July 13th, 1865, from High Force Inn, Middleton, Teesdale: "I am studying the moraines all day long with as much enthusiasm as I am capable of after lying in bed till nine, eating heavy breakfasts, and looking forward to dinner as the summum bonum of existence." The result of his work, under the title "Moraines of the Tees Valley," appeared in the "Reader" (July 15th, 1865, page 71), of which Huxley was one of the managers or committee-men, and Norman Lockyer was scientific editor ("Life and Letters of T.H. Huxley," I., page 211). Hooker describes the moraines and other evidence of glacial action in the upper part of the Tees valley, and speaks of the effect of glaciers in determining the present physical features of the country.) there was a striking article on English and Foreign Men of Science (505/2. "British and Foreign Science," "The Reader," loc. cit., page 61.

The writer of the article asserts the inferiority of English scientific workers.), and I think unjust to England except in pure Physiology; in biology Owen and R. Brown ought to save us, and in Geology we are most rich.

It is curious how we are reading the same books. We intend to read Lecky and certainly to re-read Buckle—which latter I admired greatly before. I am heartily glad you like Lubbock's book so much. It made me grieve his taking to politics, and though I grieve that he has lost his election, yet I suppose, now that he is once bitten, he will never give up politics, and science is done for. Many men can make fair M.P.'s; and how few can work in science like him!

I have been reading a pamphlet by Verlot on "Variation of Flowers," which seems to me very good; but I doubt whether it would be worth your reading. it was published originally in the "Journal d'Hort.," and so perhaps you have seen it. It is a very good plan this republishing separately for sake of foreigners buying, and I wish I had tried to get permission of Linn. Soc. for my Climbing paper, but it is now too late.

Do not forget that you have my paper on hybridism, by Max Wichura. (505/3. Wichura, M.E., "L'Hybridisation dans le regne vegetal etudiee sur les Saules," "Arch. Sci. Phys. Nat." XXIII., page 129, 1865.)

I hope you are returned to your work, refreshed like a giant by your huge breakfasts. How unlucky you are about contagious complaints with your children!

I keep very weak, and had much sickness yesterday, but am stronger this morning.

Can you remember how we ever first met? (505/4. See "Life and Letters," II., page 19.) It was in Park Street; but what brought us together? I have been re-reading a few old letters of yours, and my heart is very warm towards you.

LETTER 506. TO C. LYELL. Down, March 8th {1866}.

(506/1. In a letter from Sir Joseph Hooker to Mr. Darwin on February 21st, 1866, the following passage occurs: "I wish I could explain to you my crude notions as to the Glacial period and your position towards it. I suppose I hold this doctrine: that there was a Glacial period, but that it was not one of universal cold, because I think that the existing distribution of glaciers is sufficiently demonstrative of the proposition that by comparatively slight redispositions of sea and land, and perhaps axis of globe, you may account for all the leading palaeontological phenomena." This letter was sent by Mr. Darwin to Sir Charles Lyell, and the latter, writing on March 1st, 1866, expresses his belief that "the whole globe must at times have been superficially cooler. Still," he adds, "during extreme excentricity the sun would make great efforts to compensate in perihelion for the chill of a long winter in aphelion in one hemisphere, and a cool summer in the other. I think you will turn out to be right in regard to meridional lines of mountain-chains by which the migrations across the equator took place while there was contemporaneous tropical heat of certain lowlands, where plants requiring heat and moisture were saved from extinction by the heat of the earth's surface, which was stored up in perihelion, being prevented from radiating off freely into space by a blanket of aqueous vapour caused by the melting of ice and snow. But though I am inclined to profit by Croll's maximum excentricity for the glacial period, I consider it quite subordinate to geographical causes or the relative position of land and sea and the abnormal excess of land in polar regions." In another letter (March 5th, 1866) Lyell writes: "In the beginning of Hooker's letter to you he speaks hypothetically of a change in the earth's axis as having possibly co-operated with redistribution of land and sea in causing the cold of the Glacial period. Now, when we consider how extremely modern, zoologically and botanically, the Glacial period is proved to be, I am shocked at any one introducing, with what I may call so much levity, so organic a change as a deviation in the axis of the planet...' (see Lyell's "Principles," 1875, Chapter XIII.; also a letter to Sir Joseph Hooker printed in the "Life of Sir Charles Lyell," Volume II., page 410.))

Many thanks for your interesting letter. From the serene elevation of my old age I look down with amazement at your youth, vigour, and indomitable energy. With respect to Hooker and the axis of the earth, I suspect he is too much overworked to consider now any subject properly. His mind is so acute and critical that I always expect to hear a torrent of objections to anything proposed; but he is so candid that he often comes round in a year or two. I have never thought on the causes of the Glacial period, for I feel that the subject is beyond me; but though I hope you will own that I have generally been a good and docile pupil to you, yet I must confess that I cannot believe in change of land and water, being more than a subsidiary agent. (506/2. In Chapter XI. of the "Origin," Edition V., 1869, page 451, Darwin discusses Croll's theory, and is clearly inclined to trust in Croll's conclusion that "whenever the northern hemisphere passes through a cold period the temperature of the southern hemisphere is actually raised..." In Edition VI., page 336, he expresses his faith even more strongly. Mr. Darwin apparently sent his MS. on the climate question, which was no doubt prepared for a new edition of the "Origin," to Sir Charles. The arrival of the MS. is acknowledged in a letter from Lyell on March 10th, 1866 ("Life of Sir Charles Lyell," II., page 408), in which the writer says that he is "more than ever convinced that geographical changes...are the principal and not the subsidiary causes.") I have come to this conclusion from reflecting on the geographical distribution of the inhabitants of the sea on the opposite sides of our continents and of the inhabitants of the continents themselves.

LETTER 507. TO C. LYELL. Down, September 8th {1866}.

Many thanks for the pamphlet, which was returned this morning. I was very glad to read it, though chiefly as a psychological curiosity. I quite follow you in thinking Agassiz glacier-mad. (507/1. Agassiz's pamphlet, ("Geology of the Amazons") is referred to by Lyell in a letter written to Bunbury in September, 1866 ("Life of Sir Charles Lyell," II., page 409): "Agassiz has written an interesting paper on the 'Geology of the Amazons,' but, I regret to say, he has gone wild about glaciers, and has actually announced his opinion that the whole of the great valley, down to its mouth in latitude 0 deg., was filled by ice..." Agassiz published a paper, "Observations Geologiques faites dans la Vallee de l'Amazone," in the "Comptes

Rendus," Volume LXIV., page 1269, 1867. See also a letter addressed to M. Marcou, published in the "Bull. Soc. Geol. France," Volume XXIV., page 109, 1866.) His evidence reduces itself to supposed moraines, which would be difficult to trace in a forest-clad country; and with respect to boulders, these are not said to be angular, and their source cannot be known in a country so imperfectly explored. When I was at Rio, I was continually astonished at the depth (sometimes 100 feet) to which the granitic rocks were decomposed in situ, and this soft matter would easily give rise to great alluvial accumulations; I well remember finding it difficult to draw a line between the alluvial matter and the softened rock in situ. What a splendid imagination Agassiz has, and how energetic he is! What capital work he would have done, if he had sucked in your "Principles" with his mother's milk. It is wonderful that he should have written such wild nonsense about the valley of the Amazon; yet not so wonderful when one remembers that he once maintained before the British Association that the chalk was all deposited at once.

With respect to the insects of Chili, I knew only from Bates that the species of Carabus showed no special affinity to northern species; from the great difference of climate and vegetation I should not have expected that many insects would have shown such affinity. It is more remarkable that the birds on the broad and lofty Cordillera of Tropical S. America show no affinity with European species. The little power of diffusion with birds has often struck me as a most singular fact—even more singular than the great power of diffusion with plants. Remember that we hope to see you in the autumn.

P.S.—There is a capital paper in the September number of "Annals and Magazine," translated from Pictet and Humbert, on Fossil Fish of Lebanon, but you will, I daresay, have received the original. (507/2. "Recent Researches on the Fossil Fishes of Mount Lebanon," "Ann. Mag. Nat. Hist." Volume XVIII., page 237, 1866.) It is capital in relation to modification of species; I would not wish for more confirmatory facts, though there is no direct allusion to the modification of species. Hooker, by the way, gave an admirable lecture at Nottingham; I read it in MS., or rather, heard it. I am glad it will be published, for it was capital. (507/3. Sir Joseph Hooker delivered a lecture at the Nottingham meeting of the British

Association (1866) on "Insular Floras," published in the "Gardeners' Chronicle," 1867. See Letters 366-377, etc.)

Sunday morning.

P.S.—I have just received a letter from Asa Gray with the following passage, so that, according to this, I am the chief cause of Agassiz's absurd views:—

"Agassiz is back (I have not seen him), and he went at once down to the National Academy of Sciences, from which I sedulously keep away, and, I hear, proved to them that the Glacial period covered the whole continent of America with unbroken ice, and closed with a significant gesture and the remark: 'So here is the end of the Darwin theory.' How do you like that?

"I said last winter that Agassiz was bent on covering the whole continent with ice, and that the motive of the discovery he was sure to make was to make sure that there should be no coming down of any terrestrial life from Tertiary or post-Tertiary period to ours. You cannot deny that he has done his work effectually in a truly imperial way."

LETTER 508. TO C. LYELL. Down, July 14th, 1868.

Mr. Agassiz's book has been read aloud to me, and I am wonderfully perplexed what to think about his precise statements of the existence of glaciers in the Ceara Mountains, and about the drift formation near Rio. (508/1. "Sur la Geologie de l'Amazone," by MM. Agassiz and Continho, "Bull. Soc. Geol. France," Volume XXV., page 685, 1868. See also "A Journey in Brazil," by Professor and Mrs. Louis Agassiz, Boston, 1868.) There is a sad want of details. Thus he never mentions whether any of the blocks are angular, nor whether the embedded rounded boulders, which cannot all be disintegrated, are scored. Yet how can so experienced an observer as A. be deceived about lateral and terminal moraines? If there really were glaciers in the Ceara Mountains, it seems to me one of the most important facts in the history of the inorganic and organic world ever observed. Whether true or not, it will be widely believed, and until finally decided will greatly interfere with future progress on many points. I have made these remarks in the hope that you will coincide. If so, do you think it would be possible to persuade some known man, such as Ramsay, or, what would be far better, some two men, to go out for a summer trip, which would be in many respects delightful, for the sole object of observing these phenomena in the Ceara Mountains, and if possible also near Rio? I would gladly put my name down for 50 pounds in aid of the expense of travelling. Do turn this over in your mind. I am so very sorry not to have seen you this summer, but for the last three weeks I have been good for nothing, and have had to stop almost all work. I hope we may meet in the autumn.

LETTER 509. TO JAMES CROLL. Down, November 24th, 1868.

I have read with the greatest interest the last paper which you have kindly sent me. (509/1. Croll discussed the power of icebergs as grinding and striating agents in the latter part of a paper ("On Geological Time, and the probable Dates of the Glacial and the Upper Miocene Period") published in the "Philosophical Magazine," Volume XXXV., page 363, 1868, Volume XXXVI., pages 141, 362, 1868. His conclusion was that the advocates of the Iceberg theory had formed "too extravagant notions regarding the potency of floating ice as a striating agent.") If we are to admit that all the scored rocks throughout the more level parts of the United States result from true glacier action, it is a most wonderful conclusion, and you certainly make out a very strong case; so I suppose I must give up one more cherished belief. But my object in writing is to trespass on your kindness and ask a question, which I daresay I could answer for myself by reading more carefully, as I hope hereafter to do, all your papers; but I shall feel much more confidence in a brief reply from you. Am I right in supposing that you believe that the glacial periods have always occurred alternately in the northern and southern hemispheres, so that the erratic deposits which I have described in the southern parts of America, and the glacial work in New Zealand, could not have been simultaneous with our Glacial period? From the glacial deposits occurring all round the northern hemisphere, and from such deposits appearing in S. America to be as recent as in the north, and lastly, from there being some evidence of the former lower descent of glaciers all along the Cordilleras, I inferred that the whole world was at this period cooler. It did not appear to me justifiable without distinct evidence to suppose that the N. and S. glacial deposits belonged to distinct epochs, though it would have been an immense relief to my mind if I could have assumed that this had been the case. Secondly, do you believe that during the Glacial period in one hemisphere the opposite hemisphere actually becomes warmer, or does it merely retain the same temperature as before? I do not ask these questions out of mere curiosity; but I have to prepare a new edition of my "Origin of Species," and am anxious to say a few words on this subject on your authority. I hope that you will excuse my troubling you.

LETTER 510. TO J. CROLL. Down, January 31st, 1869.

To-morrow I will return registered your book, which I have kept so long. I am most sincerely obliged for its loan, and especially for the MS., without which I should have been afraid of making mistakes. If you require it, the MS. shall be returned. Your results have been of more use to me than, I think, any other set of papers which I can remember. Sir C. Lyell, who is staying here, is very unwilling to admit the greater warmth of the S. hemisphere during the Glacial period in the N.; but, as I have told him, this conclusion which you have arrived at from physical considerations, explains so well whole classes of facts in distribution, that I must joyfully accept it; indeed, I go so far as to think that your conclusion is strengthened by the facts in distribution. Your discussion on the flowing of the great ice-cap southward is most interesting. I suppose that you have read Mr. Moseley's recent discussion on the force of gravity being quite insufficient to account for the downward movement of glaciers (510/1. Canon Henry Moseley, "On the Mechanical Impossibility of the Descent of Glaciers by their Weight only." "Proc. R. Soc." Volume XVII., page 202, 1869; "Phil. Mag." Volume XXXVII., page 229, 1869.): if he is right, do you not think that the unknown force may make more intelligible the extension of the great northern ice-cap? Notwithstanding your excellent remarks on the work which can be effected within the million years (510/2. In his paper "On Geological Time, and the probable Date of the Glacial and the Upper Miocene Period" ("Phil. Mag." Volume XXXV., page 363, 1868), Croll endeavours to convey to the mind some idea of what a million years really is: "Take a narrow strip of paper, an inch broad or more, and 83 feet 4 inches in length, and stretch it along the wall of a large hall, or round the walls of an apartment somewhat over 20 feet square. Recall to memory the days of your boyhood, so as to get some adequate conception of what a period of a hundred years is. Then mark off from one of the ends of the strip one-tenth of an inch. The one-tenth of an inch will then represent a hundred years, and the entire length of the strip a million of years" (loc. cit., page 375).), I am greatly troubled at the short duration of the world according to Sir W. Thomson (510/3. In a paper communicated to the Royal Society of Edinburgh, Lord Kelvin (then Sir William Thomson) stated his belief that the age of our planet must be more than twenty millions of years, but not more than four hundred millions of years ("Trans. R. Soc. Edinb." Volume XXIII., page 157, 1861, "On the Secular Cooling of the Earth."). This subject has been recently dealt with by Sir Archibald Geikie in his address as President of the Geological Section of the British Association, 1899 ("Brit. Assoc. Report," Dover Meeting, 1899, page 718).), for I require for my theoretical views a very long period BEFORE the Cambrian formation. If it would not trouble you, I should like to hear what you think of Lyell's remark on the magnetic force which comes from the sun to the earth: might not this penetrate the crust of the earth and then be converted into heat? This would give a somewhat longer time during which the crust might have been solid; and this is the argument on which Sir W. Thomson seems chiefly to rest. You seem to argue chiefly on the expenditure of energy of all kinds by the sun, and in this respect Lyell's remark would have no bearing.

My new edition of the "Origin" (510/4. Fifth edition, May, 1869.) will be published, I suppose, in about two months, and for the chance of your liking to have a copy I will send one.

P.S.—I wish that you would turn your astronomical knowledge to the consideration whether the form of the globe does not become periodically slightly changed, so as to account for the many repeated ups and downs of the surface in all parts of the world. I have always thought that some cosmical cause would some day be discovered.

LETTER 511. TO C. LYELL. Down, July 12th {1872}.

I have been glad to see the enclosed and return it. It seems to me very cool in Agassiz to doubt the recent upheaval of Patagonia, without having visited any part; and he entirely misrepresents me in saying that I infer upheaval from the form of the land, as I trusted entirely to shells embedded and on the surface. It is simply monstrous to suppose that the terraces stretching on a dead level for leagues along the coast, and miles in breadth, and covered with beds of stratified gravel, 10 to 30 feet in thickness, are due to subaerial denudation.

As for the pond of salt-water twice or thrice the density of sea-water, and nearly dry, containing sea-shells in the same relative proportions as on the adjoining coast, it almost passes my belief. Could there have been a lively midshipman on board, who in the morning stocked the pool from the adjoining coast?

As for glaciation, I will not venture to express any opinion, for when in S. America I knew nothing about glaciers, and perhaps attributed much to icebergs which ought to be attributed to glaciers. On the other hand, Agassiz seems to me mad about glaciers, and apparently never thinks of drift ice.

I did see one clear case of former great extension of a glacier in T. del Fuego.

LETTER 512. TO J. GEIKIE.

(512/1. The following letter was in reply to a request from Prof. James Geikie for permission to publish Mr. Darwin's views, communicated in a previous letter (November 1876), on the vertical position of stones in gravelly drift near Southampton. Prof. Geikie wrote (July 15th, 1880): "You may remember that you attributed the peculiar position of those stones to differential movements in the drift itself arising from the slow melting of beds of frozen snow interstratified into the gravels...I have found this explanation of great service even in Scotland, and from what I have seen of the drift-gravels in various parts of southern England and northern France, I am inclined to think that it has a wide application.")

Down, July 19th, 1880.

Your letter has pleased me very much, and I truly feel it an honour that anything which I wrote on the drift, etc., should have been of the least use or interest to you. Pray make any use of my letter (512/2. Professor James Geikie quotes the letter in "Prehistoric Europe," London, 1881 (page 141). Practically the whole of it is given in the "Life and Letters," III., page 213.): I forget whether it was written carefully or clearly, so pray touch up any passages that you may think fit to quote.

All that I have seen since near Southampton and elsewhere has strengthened my notion. Here I live on a chalk platform gently sloping down from the edge of the escarptment to the south (512/3. Id est, sloping down from the escarpment which is to the south.) (which is about 800 feet in height) to beneath the Tertiary beds to the north. The (512/4. From here to the end of the paragraph is quoted by Prof. Geikie, loc. cit., page 142.) beds of the large and broad valleys (and only of these) are covered with an immense mass of closely packed broken and angular flints; in which mass the skull of the musk-ox {musk-sheep} and woolly elephant have been found. This great accumulation of unworn flints must therefore have been made when the climate was cold, and I believe it can be accounted for by the larger valleys having been filled up to a great depth during a large part of the year with drifted frozen snow, over which rubbish from the upper parts of the platforms was washed by the summer rains, sometimes along one line and sometimes along another, or in channels cut through the snow all along the main course of the broad valleys.

I suppose that I formerly mentioned to you the frequent upright position of elongated flints in the red clayey residue over the chalk, which residue gradually subsides into the troughs and pipes corroded in the solid chalk. This letter is very untidy, but I am tired.

P.S. Several palaeolithic celts have recently been found in the great angular gravel-bed near Southampton in several places.

LETTER 513. TO D. MACKINTOSH. Down, November 13th, 1880.

Your discovery is a very interesting one, and I congratulate you on it. (513/1. "On the Precise Mode of Accumulation and Derivation of the Moel-Tryfan Shelly Deposits; on the Discovery of Similar High-level Deposits along the Eastern Slopes of the Welsh Mountains; and on the Existence of Drift-Zones, showing probable Variations in the

Rate of Submergence." By D. Mackintosh, "Quart. Journ. Geol. Soc." Volume XXXVII., pages 351-69, 1881. {Read April 27th, 1881.}) I failed to find shells on Moel Tryfan, but was interested by finding ("Philosoph. Mag." 3rd series, Volume XXI., page 184) shattered rocks (513/2. In reviewing the work by previous writers on the Moel-Tryfan deposits, Mackintosh refers to Darwin's "very suggestive description of the Moel-Tryfan deposits...Under the drift he saw that the surface of the slate, TO A DEPTH OF SEVERAL FEET, HAD BEEN SHATTERED AND CONTORTED IN A VERY PECULIAR MANNER." The contortion of the slate, which Mackintosh regarded as "the most interesting of the Moel-Tryfan phenomena," had not previously been regarded as "sufficiently striking to arrest attention" by any geologist except Darwin. The Pleistocene gravel and sand containing marine shells on Moel-Tryfan, about five miles south-east of Caernarvon, have been the subject of considerable controversy. By some geologists the drift deposits have been regarded as evidence of a great submergence in post-Pliocene times, while others have explained their occurrence at a height of 1300 feet by assuming that the gravel and sand had been thrust uphill by an advancing ice-sheet. (See H.B. Woodward, "Geology of England and Wales," Edition II., 1887, pages 491, 492.) Darwin attributed the shattering and contorting of the slates below the drift to "icebergs grating over the surface.") and far-distant rounded boulders, which I attributed to the violent impact of icebergs or coast-ice. I can offer no opinion on whether the more recent changes of level in England were or were not accompanied by earthquakes. It does not seem to me a correct expression (which you use probably from haste in your note) to speak of elevations or depressions as caused by earthquakes: I suppose that every one admits that an earthquake is merely the vibration from the fractured crust when it yields to an upward or downward force. I must confess that of late years I have often begun to suspect (especially when I think of the step-like plains of Patagonia, the heights of which were measured by me) that many of the changes of level in the land are due to changes of level in the sea. (513/3. This view is an agreement with the theory recently put forward by Suess in his "Antlitz der Erde" (Prag and Leipzig, 1885). Suess believes that "the local invasions and transgressions of the continental areas by the sea" are due to "secular movements of the hydrosphere itself." (See J. Geikie, F.R.S., Presidential Address before Section E at the

Edinburgh Meeting of the British Association, "Annual Report," page 794.) I suppose that there can be no doubt that when there was much ice piled up in the Arctic regions the sea would be attracted to them, and the land on the temperate regions would thus appear to have risen. There would also be some lowering of the sea by evaporation and the fixing of the water as ice near the Pole.

I shall read your paper with much interest when published.

LETTER 514. TO J. GEIKIE. Down, December 13th, 1880.

You must allow me the pleasure of thanking you for the great interest with which I have read your "Prehistoric Europe." (514/1. "Prehistoric Europe: a Geological Sketch," London, 1881.) Nothing has struck me more than the accumulated evidence of interglacial periods, and assuredly the establishment of such periods is of paramount importance for understanding all the later changes of the earth's surface. Reading your book has brought vividly before my mind the state of knowledge, or rather ignorance, half a century ago, when all superficial matter was classed as diluvium, and not considered worthy of the attention of a geologist. If you can spare the time (though I ask out of mere idle curiosity) I should like to hear what you think of Mr. Mackintosh's paper, illustrated by a little map with lines showing the courses or sources of the erratic boulders over the midland counties of England. (514/2. "Results of a Systematic Survey, in 1878, of the Directions and Limits of Dispersion, Mode of Occurrence, and Relation to Drift-Deposits of the Erratic Blocks or Boulders of the West of England and East of Wales, including a Revision of Many Years' Previous Observations," D. Mackintosh, "Quart. Journ. Geol. Soc." Volume XXXV., page 425, 1879.) It is a little suspicious their ending rather abruptly near Wolverhampton, yet I must think that they were transported by floating ice. Fifty years ago I knew Shropshire well, and cannot remember anything like till, but abundance of gravel and sand beds, with recent marine shells. A great boulder (514/3. Mackintosh alludes (loc. cit., page 442) to felstone boulders around Ashley Heath, the highest ground between the Pennine and Welsh Hills north of the Wrekin; also to a boulder on the summit of the eminence (774 feet above sea-level), "probably the same as that noticed many years ago by Mr. Darwin." In a later paper, "On the

Correlation of the Drift-Deposits of the North-West of England with those of the Midland and Eastern Counties" ("Quart. Journ. Geol. Soc." Volume XXXVI., page 178, 1880) Mackintosh mentions a letter received from Darwin, "who was the first to elucidate the boulder-transporting agency of floating ice," containing an account of the great Ashley Heath boulder, which he was the first to discover and expose,...so as to find that the block rested on fragments of New Red Sandstone, one of which was split into two and deeply scored...The facts mentioned in the letter from Mr. Darwin would seem to show that the boulder must have fallen through water from floating ice with a force sufficient to split the underlying lump of sandstone, but not sufficient to crush it.") which I had undermined on the summit of Ashley Heath, 720 (?) feet above the sea, rested on clean blocks of the underlying red sandstone. I was also greatly interested by your long discussion on the Loss (514/4. For an account of the Loss of German geologists—"a fine-grained, more or less homogeneous, consistent, non-plastic loam, consisting of an intimate admixture of clay and carbonate of lime," see J. Geikie, loc. cit., page 144 et seq.); but I do not feel satisfied that all has been made out about it. I saw much brick-earth near Southampton in some manner connected with the angular gravel, but had not strength enough to make out relations. It might be worth your while to bear in mind the possibility of fine sediment washed over and interstratified with thick beds of frozen snow, and therefore ultimately dropped irrespective of the present contour of the country.

I remember as a boy that it was said that the floods of the Severn were more muddy when the floods were caused by melting snow than from the heaviest rains; but why this should be I cannot see.

Another subject has interested me much—viz. the sliding and travelling of angular debris. Ever since seeing the "streams of stones" at the Falkland Islands (514/5. "Geological Observations on South America" (1846), page 19 et seq.), I have felt uneasy in my mind on this subject. I wish Mr. Kerr's notion could be fully elucidated about frozen snow. Some one ought to observe the movements of the fields of snow which supply the glaciers in Switzerland.

Yours is a grand book, and I thank you heartily for the instruction and pleasure which it has given me.

For heaven's sake forgive the untidiness of this whole note.

LETTER 515. TO JOHN LUBBOCK {Lord Avebury}. Down, November 6th, 1881.

If I had written your Address (515/1. Address delivered by Lord Avebury as President of the British Association at York in 1881. Dr. Hicks is mentioned as having classed the pre-Cambrian strata in "four great groups of immense thickness and implying a great lapse of time" and giving no evidence of life. Hicks' third formation was named by him the Arvonian ("Quart. Journ. Geol. Soc." Volume XXXVII., 1881, Proc., page 55.) (but this requires a fearful stretch of imagination on my part) I should not alter what I had said about Hicks. You have the support of the President {of the} Geological Society (515/2. Robert Etheridge.), and I think that Hicks is more likely to be right than X. The latter seems to me to belong to the class of objectors general. If Hicks should be hereafter proved to be wrong about this third formation, it would signify very little to you.

I forget whether you go as far as to support Ramsay about lakes as large as the Italian ones: if so, I would myself modify the passage a little, for these great lakes have always made me tremble for Ramsay, yet some of the American geologists support him about the still larger N. American lakes. I have always believed in the main in Ramsay's views from the date of publication, and argued the point with Lyell, and am convinced that it is a very interesting step in Geology, and that you were quite right to allude to it. (515/3. "Glacial Origin of Lakes in Switzerland, Black Forest, etc." ("Quart. Journ. Geol. Soc." Volume XVIII., pages 185-204, 1862). Sir John Lubbock (Lord Avebury) gives a brief statement of Ramsay's views concerning the origin of lakes (Presidential Address, Brit. Assoc. 1881, page 22): "Prof. Ramsay divides lakes into three classes: (1) Those which are due to irregular accumulations of drift, and which are generally quite shallow; (2) those which are formed by moraines; and (3) those which occupy true basins scooped by glaciers out of the solid rocks. To the latter class belong, in his opinion, most of the great Swiss and Italian lakes...Professor Ramsay's theory seems, therefore, to account for a large number of interesting facts." Sir

Archibald Geikie has given a good summary of Ramsay's theory in his "Memoir of Sir Andrew Crombie Ramsay," page 361, London, 1895.)

LETTER 516. TO D. MACKINTOSH. Down, February 28th, 1882.

I have read professor Geikie's essay, and it certainly appears to me that he underrated the importance of floating ice. (516/1. "The Intercrossing of Erratics in Glacial Deposits," by James Geikie, "Scottish Naturalist," 1881.) Memory extending back for half a century is worth a little, but I can remember nothing in Shropshire like till or ground moraine, yet I can distinctly remember the appearance of many sand and gravel beds—in some of which I found marine shells. I think it would be well worth your while to insist (but perhaps you have done so) on the absence of till, if absent in the Western Counties, where you find many erratic boulders.

I was pleased to read the last sentence in Geikie's essay about the value of your work. (516/2. The concluding paragraph reads as follows: "I cannot conclude this paper without expressing my admiration for the long-continued and successful labours of the well-known geologist whose views I have been controverting. Although I entered my protest against his iceberg hypothesis, and have freely criticised his theoretical opinions, I most willingly admit that the results of his unwearied devotion to the study of those interesting phenomena with which he is so familiar have laid all his fellow-workers under a debt of gratitude." Mr. Darwin used to speak with admiration of Mackintosh's work, carried on as it was under considerable difficulties.)

With respect to the main purport of your note, I hardly know what to say. Though no evidence worth anything has as yet, in my opinion, been advanced in favour of a living being, being developed from inorganic matter, yet I cannot avoid believing the possibility of this will be proved some day in accordance with the law of continuity. I remember the time, above fifty years ago, when it was said that no substance found in a living plant or animal could be produced without the aid of vital forces. As far as external form is concerned, Eozoon shows how difficult it is to distinguish between organised and inorganised bodies. If it is ever found that life can originate on this world, the vital phenomena will come under some general law of nature. Whether the existence of a conscious God can be proved from the existence of the so-called laws of nature (i.e., fixed sequence of events) is a perplexing subject, on which I have often thought, but cannot see my way clearly. If you have not read W. Graham's "Creed of Science," (516/3. "The Creed of Science: Religious, Moral, and Social," London, 1881.), it would, I think, interest you, and he supports the view which you are inclined to uphold.

2.IX.III. THE PARALLEL ROADS OF GLEN ROY, 1841-1880.

(517/1. In the bare hilly country of Lochaber, in the Scotch Highlands, the slopes of the mountains overlooking the vale of Glen Roy are marked by narrow terraces or parallel roads, which sweep round the shoulders of the hills with "undeviating horizontality." These roads are described by Sir Archibald Geikie as having long been "a subject of wonderment and legendary story among the Highlanders, and for so many years a source of sore perplexity among men of science." (517/2. "The Scenery of Scotland," 1887, page 266.) In Glen Roy itself there are three distinct shelves or terraces, and the mountain sides of the valley of the Spean and other glens bear traces of these horizontal "roads."

The first important papers dealing with the origin of this striking physical feature were those of MacCulloch (517/3. "Trans. Geol. Soc." Volume IV., page 314, 1817.) and Sir Thomas Lauder Dick (517/4. "Trans. R. Soc. Edinb." Volume IX., page 1, 1823.), in which the writers concluded that the roads were the shore-lines of lakes which once filled the Lochaber valleys. Towards the end of June 1838 Mr. Darwin devoted "eight good days" (517/5. "Life and Letters," I., page 290.) to the examination of the Lochaber district, and in the following year he communicated a paper to the Royal Society of London, in which he attributed their origin to the action of the sea, and regarded them as old sea beaches which had been raised to their present level by a gradual elevation of the Lochaber district.

In 1840 Louis Agassiz and Buckland (517/6. "Edinb. New Phil. Journal," Volume XXXIII., page 236, 1842.) proposed the glacier-ice theory; they described the valleys as having been filled with lakes dammed back by glaciers which formed bars across the valleys of

Glen Roy, Glen Spean, and the other glens in which the hill-sides bear traces of old lake-margins. Agassiz wrote in 1842: "When I visited the parallel roads of Glen Roy with Dr. Buckland we were convinced that the glacial theory alone satisfied all the exigencies of the phenomenon." (517/7. Ibid., page 236.)

Mr. David Milne (afterwards Milne-Home) (517/8. "Trans. R. Soc. Edinb." Volume XVI., page 395, 1847.) in 1847 upheld the view that the ledges represent the shore-lines of lakes which were imprisoned in the valleys by dams of detrital material left in the glens during a submergence of 3,000 feet, at the close of the Glacial period. Chambers, in his "Ancient Sea Margins" (1848), expressed himself in agreement with Mr. Darwin's marine theory. The Agassiz-Buckland theory was supported by Mr. Jamieson (517/9. "Quart. Journ. Geol. Soc." Volume XIX., page 235, 1863.), who brought forward additional evidence in favour of the glacial barriers. Sir Charles Lyell at first (517/10. "Elements of Geology," Edition II., 1841.) accepted the explanation given by Mr. Darwin, but afterwards (517/11. "Antiquity of Man," 1863, pages 252 et seq.) came to the conclusion that the terrace-lines represent the beaches of glacial lakes. In a paper published in 1878 (517/12. "Phil. Trans. R. Soc." 1879, page 663.), Prof. Prestwich stated his acceptance of the lake theory of MacCulloch and Sir T. Lauder Dick and of the glacial theory of Agassiz, but differed from these authors in respect of the age of the lakes and the manner of formation of the roads.

The view that has now gained general acceptance is that the parallel roads of Glen Roy represent the shores of a lake "that came into being with the growth of the glaciers and vanished as these melted away." (517/13. Sir Archibald Geikie, loc. cit., page 269.)

Mr. Darwin became a convert to the glacier theory after the publication of Mr. Jamieson's paper. He speaks of his own paper as "a great failure"; he argued in favour of sea action as the cause of the terraces "because no other explanation was possible under our then state of knowledge." Convinced of his mistake, Darwin looked upon his error as "a good lesson never to trust in science to the principle of exclusion." (517/14. "Life and Letters," I., page 69.)

LETTER 517. TO C. LYELL. {March 9th, 1841.}

I have just received your note. It is the greatest pleasure to me to write or talk Geology with you...

I think I have thought over the whole case without prejudice, and remain firmly convinced they {the parallel roads} are marine beaches. My principal reason for doing so is what I have urged in my paper (517/15. "Observations on the Parallel Roads of Glen Roy, and of other parts of Lochaber in Scotland, with an attempt to prove that they are of Marine Origin." "Phil. Trans. R. Soc." 1839, page 39.), the buttress-like accumulations of stratified shingle on sides of valley, especially those just below the lowest shelf in Spean Valley.

2nd. I can hardly conceive the extension of the glaciers in front of the valley of Kilfinnin, where I found a new road—where the sides of Great Glen are not very lofty.

3rd. The flat watersheds which I describe in places where there are no roads, as well as those connected with "roads." These remain unexplained.

I might continue to add many other such reasons, all of which, however, I daresay would appear trifling to any one who had not visited the district. With respect to equable elevation, it cannot be a valid objection to any one who thinks of Scandinavia or the Pampas. With respect to the glacier theory, the greatest objection appears to me the following, though possibly not a sound one. The water has beyond doubt remained very long at the levels of each shelf—this is unequivocally shown by the depth of the notch or beach formed in many places in the hard mica-slate, and the large accumulations or buttresses of well-rounded pebbles at certain spots on the level of old beaches. (The time must have been immense, if formed by lakes without tides.) During the existence of the lakes their drainage must have been at the head of the valleys, and has given the flat appearance of the watersheds. All this is very clear for four of the shelves (viz., upper and lower in Glen Roy, the 800-foot one in Glen Spean, and the one in Kilfinnin), and explains the coincidence of "roads" with the watersheds more simply than my view, and as simply as the common lake theory. But how was the Glen Roy lake drained when the water stood at level of the middle "road"? It must (for there is no other exit whatever) have been drained over the glacier. Now this shelf is full as narrow in a vertical line and as deeply worn horizontally into the mountain side and with a large accumulation of shingle (I can give cases) as the other shelves. We must, therefore, on the glacier theory, suppose that the surface of the ice remained at exactly the same level, not being worn down by the running water, or the glacier moved by its own movement during the very long period absolutely necessary for a quiet lake to form such a beach as this shelf presents in its whole course. I do not know whether I have explained myself clearly. I should like to know what you think of this difficulty. I shall much like to talk over the Jura case with you. I am tired, so goodbye.

LETTER 518. TO L. HORNER. Down {1846}.

(518/1. It was agreed at the British Association meeting held at Southampton in 1846 "That application be made to Her Majesty's Government to direct that during the progress of the Ordnance Trigonometrical Surveys in the North of Scotland, the so-called Parallel Roads of Glen Roy and the adjoining country be accurately surveyed, with the view of determining whether they are truly parallel and horizontal, the intervening distances, and their elevations above the present sea-level" ("British Association Report," 1846, page xix). The survey was undertaken by the Government Ordnance Survey Office under Col. Sir Henry James, who published the results in 1874 ("Notes on the Parallel Roads of Glen Roy"); the map on which the details are given is sheet 63 (one-inch scale).)

In following your suggestion in drawing out something about Glen Roy for the Geological Committee, I have been completely puzzled how to do it. I have written down what I should say if I had to meet the head of the Survey and wished to persuade him to undertake the task; but as I have written it, it is too long, ill expressed, seems as if it came from nobody and was going to nobody, and therefore I send it to you in despair, and beg you to turn the subject in your mind. I feel a conviction if it goes through the Geological part of Ordnance Survey it will be swamped, and as it is a case for mere accurate measurements it might, I think without offence, go to the head of the real Surveyors.

If Agassiz or Buckland are on the Committee they will sneer at the whole thing and declare the beaches are those of a glacier-lake, than which I am sure I could convince you that there never was a more futile theory.

I look forward to Southampton (518/2. The British Association meeting (1846).) with much interest, and hope to hear to-morrow that the lodgings are secured to us. You cannot think how thoroughly I enjoyed our geological talks, and the pleasure of seeing Mrs. Horner and yourself here. (518/3. This letter is published in the privately printed "Memoir of Leonard Horner," II., page 103.)

{Here follows Darwin's Memorandum.}

The Parallel Roads of Glen Roy, in Scotland, have been the object of repeated examination, but they have never hitherto been levelled with sufficient accuracy. Sir T. Lauder Dick (518/4. "On the Parallel Roads of Lochaber" (with map and plates), by Sir Thomas Lauder Dick, "Trans. R. Soc. Edinb." Volume IX., page 1, 1823.) procured the assistance of an engineer for this purpose, but owing to the want of a true ground-plan it was impossible to ascertain their exact curvature, which, as far as could be estimated, appeared equal to that of the surface of the sea. Considering how very rarely the sea has left narrow and well-defined marks of its action at any considerable height on the land, and more especially considering the remarkable observations by M. Bravais (518/5. "On the Lines of Ancient Level of the Sea in Finmark," by M. A. Bravais, translated from "Voyages de la Commission Scientifique du Nord, etc."; "Quart. Journ. Geol. Soc." Volume I., page 534, 1845.) on the ancient sea-beaches of Scandinavia, showing the they are not strictly parallel to each other, and that the movement has been greater nearer the mountains than on the coast, it appears highly desirable that the roads of Glen Roy should be examined with the utmost care during the execution of the Ordnance Survey of Scotland. The best instruments and the most accurate measurements being necessary for this end almost precludes the hope of its being ever undertaken by private individuals; but by the means at the disposal of the Ordnance, measurements would be easily made even more accurate than those of M. Bravais. It would be desirable to take two lines of the greatest possible length in the district, and at nearly right angles to each other, and to level from the beach at one extremity to that at the other, so that it might be ascertained whether the curvature does exactly correspond with that of the globe, or, if not, what is the direction of the line of greatest elevation. Much attention would be requisite in fixing on either the upper or lower edge of the ancient beaches as the standard of measurement, and in rendering this line conspicuous. The heights of the three roads, one above the other and above the level of the sea, ought to be accurately ascertained. Mr. Darwin observed one short beach-line north of Glen Roy, and he has indicated, on the authority of Sir David Brewster, others in the valley of the Spey. If these could be accurately connected, by careful measurements of their absolute heights or by levelling, with those of Glen Roy, it would make a most valuable addition to our knowledge on this subject. Although the observations here specified would probably be laborious, yet, considering how rarely such evidence is afforded in any quarter of the world, it cannot be doubted that one of the most important problems in Geology—namely, the exact manner in which the crust of the earth rises in mass—would be much elucidated, and a great service done to geological science.

LETTER 519. R. CHAMBERS TO D. MILNE-HOME. St. Andrews, September 7th, 1847.

I have had a letter to-day from Mr. Charles Darwin, beseeching me to obtain for him a copy of your paper on Glen Roy. (519/1. No doubt Mr. Milne's paper "On the Parallel Roads of Lochaber," "Trans. R. Soc. Edinb." Volume XVI., page 395, 1849. {Read March 1st and April 5th, 1847.}) I am sure you will have pleasure in sending him one; his address is "Down, Farnborough, Kent." I have again read over your paper carefully, and feel assured that the careful collection and statement of facts which are found in it must redound to your credit with all candid persons. The suspicions, however, which I obtained some time ago as to land-straits and heights of country being connected with sea-margins and their ordinary memorials still possesses me, and I am looking forward to some means of further testing the Glen Roy mystery. If my suspicion turn out true, I shall at once be regretful on your account, and shall feel it as a great check and admonition to myself not to be too confident about anything in science till it has been proved over and over again. The ground hereabouts is now getting clear of the crops; perhaps when I am in town a few days hence we may be able to make some appointment for an examination of the beaches of the district, my list of which has been greatly enlarged during the last two months.

LETTER 520. TO R. CHAMBERS. September 11th, 1847.

I hope you will read the first part of my paper before you go {to Glen Roy}, and attend to the manner in which the lines end in Glen Collarig. I wish Mr. Milne had read it more carefully. He misunderstands me in several respects, but {I} suppose it is my own fault, for my paper is most tediously written. Mr. Milne fights me very pleasantly, and I plead guilty to his rebuke about "demonstration." (520/1. See Letter 521, note.) I do not know what you think; but Mr. Milne will think me as obstinate as a pig when I say that I think any barriers of detritus at the mouth of Glen Roy, Collarig and Glaster more utterly impossible than words can express. I abide by all that I have written on that head. Conceive such a mass of detritus having been removed, without great projections being left on each side, in the very close proximity to every little delta preserved on the lines of the shelves, even on the shelf 4, which now crosses with uniform breadth the spot where the barrier stood, with the shelves dying gradually out, etc. To my mind it is monstrous. Oddly enough, Mr. Milne's description of the mouth of Loch Treig (I do not believe that valley has been well examined in its upper end) leaves hardly a doubt that a glacier descended from it, and, if the roads were formed by a lake of any kind, I believe it must have been an ice-lake. I have given in detail to Lyell my several reasons for not thinking ice-lakes probable (520/2. Mr. Darwin gives some arguments against the glacier theory in the letter (517) to Sir Charles Lyell; but the letter alluded to is no doubt the one written to Lyell on "Wednesday, 8th" (Letter 522), in which the reasons are fully stated.); but to my mind they are incomparably more probable than detritus of rock-barriers. Have you ever attended to glacier action? After having seen N. Wales, I can no more doubt the former existence of gigantic glaciers than I can the sun in the heaven. I could distinguish in N. Wales to a certain extent icebergs from glacier action (Lyell has shown that icebergs at the present day score rocks), and I suspect that in Lochaber the two actions are united, and that the scored rock on the watersheds, when tideways, were rubbed and bumped by half-stranded icebergs. You will, no doubt, attend to Glen Glaster. Mr. Milne, I think, does not mention whether shelf 4 enters it, which I should like to know, and especially he does not state whether rocks worn on their upper faces are found on the whole 212 {feet} vertical course of this Glen down to near L. Loggan, or whether only in the upper part; nor does he state whether these rocks are scored, or polished, or moutonnees, or whether there are any "perched" boulders there or elsewhere. I suspect it would be difficult to distinguish between a river-bed and tidal channel. Mr. Milne's description of the Pass of Mukkul, expanding to a width of several hundred yards 21 feet deep in the shoalest part, and with a worn islet in the middle, sounds to me much more like a tidal channel than a river-bed. There must have been, on the latter view, plenty of fresh water in those days. With respect to the coincidence of the shelves with the now watersheds, Mr. Milne only gives half of my explanation. Please read page 65 of my paper. (520/3. "Observations on the Parallel Roads of Glen Roy, and of other Parts of Lochaber in Scotland, with an Attempt to Prove that they are of Marine Origin." "Phil. Trans. R. Soc." 1839, page 39. {Read February 7th, 1839.}) I allude only to the head of Glen Roy and Kilfinnin as silted up. I did not know Mukkul Pass; and Glen Roy was so much covered up that I did not search it well, as I was not able to walk very well. It has been an old conjectural belief of mine that a rising surface becomes stationary, not suddenly, but by the movement becoming very slow. Now, this would greatly aid the tidal currents cutting down the passes between the mountains just before, and to the level of, the stationary periods. The currents in the fiords in T. del Fuego in a narrow crooked part are often most violent; in other parts they seem to silt up.

Shall you do any levelling? I believe all the levelling has been {done} in Glen Roy, nearly parallel to the Great Glen of Scotland. For inequalities of elevation, the valley of the Spean, at right angles to the apparent axes of elevation, would be the one to examine. If you go to the head of Glen Roy, attend to the apparent shelf above the highest one in Glen Roy, lying on the south side of Loch Spey, and therefore beyond the watershed of Glen Roy. It would be a crucial case. I was too unwell on that day to examine it carefully, and I had no levelling instruments. Do these fragments coincide in level with Glen Gluoy shelf?

MacCulloch talks of one in Glen Turret above the shelf. I could not see it. These would be important discoveries. But I will write no more, and pray your forgiveness for this long, ill-written outpouring. I am very glad you keep to your subject of the terraces. I have lately observed that you have one great authority (C. Prevost), {not} that authority signifies a {farthing?} on your side respecting your heretical and damnable doctrine of the ocean falling. You see I am orthodox to the burning pitch.

LETTER 521. TO D. MILNE-HOME. Down, {September} 20th, {1847}.

I am much obliged by your note. I returned from London on Saturday, and I found then your memoir (521/1. "On the Parallel Roads of Lochaber, with Remarks on the Change of Relative Levels of Sea and Land in Scotland, and on the Detrital Deposits in that Country," "Trans. R. Soc. Edinb." Volume XVI., page 395, 1849. {Read March 1st and April 5th, 1847.}), which I had not then received, owing to the porter having been out when I last sent to the Geological Society. I have read your paper with the greatest interest, and have been much struck with the novelty and importance of many of your facts. I beg to thank you for the courteous manner in which you combat me, and I plead quite guilty to your rebuke about demonstration. (521/2. Mr. Milne quotes a passage from Mr. Darwin's paper ("Phil. Trans. R. Soc." 1839, page 56), in which the latter speaks of the marine origin of the parallel roads of Lochaber as appearing to him as having been demonstrated. Mr. Milne adds: "I regret that Mr. Darwin should have expressed himself in these very decided and confident terms, especially as his survey was incomplete; for I venture to think that it can be satisfactorily established that the parallel roads of Lochaber were formed by fresh-water lakes" (Milne, loc. cit., page 400).) You have misunderstood my paper on a few points, but I do not doubt that is owing to its being badly and tediously written. You will, I fear, think me very obstinate when I say that I am not in the least convinced about the barriers (521/3. Mr. Milne believed that the lower parts of the valleys were filled with detritus, which constituted barriers and thus dammed up the waters into lakes.): they remain to me as improbable as ever. But the oddest result of your paper on me (and I assure you, as far as I know myself, it is not perversity) is that I am very much staggered in favour of the ice-lake theory of Agassiz and Buckland (521/4. Agassiz and Buckland believed that the lakes which formed the "roads" were confined by glaciers or moraines. See "The Glacial Theory and its Recent Progress," by Louis Agassiz, "Edinb. New Phil. Journ." Volume XXXIII., page 217, 1842 (with map).): until I read your important discovery of the outlet in Glen Glaster I never thought this theory at all tenable. (521/5. Mr. Milne discovered that the middle shelf of Glen Roy, which Mr. Darwin stated was "not on a level with any watershed" (Darwin, loc. cit., page 43), exactly coincided with a watershed at the head of Glen Glaster (Milne, loc. cit., page 398).) Now it appears to me that a very good case can be made in its favour. I am not, however, as yet a believer in the ice-lake theory, but I tremble for the result. I have had a good deal of talk with Mr. Lyell on the subject, and from his advice I am going to send a letter to the "Scotsman," in which I give briefly my present impression (though there is not space to argue with you on such points as I think I could argue), and indicate what points strike me as requiring further investigation with respect, chiefly, to the ice-lake theory, so that you will not care about it...

P.S.—Some facts mentioned in my "Geology of S. America," page 24 (521/6. The creeks which penetrate the western shores of Tierra del Fuego are described as "almost invariably much shallower close to the open sea at their mouths than inland...This shoalness of the sea-channels near their entrances probably results from the quantity of sediment formed by the wear and tear of the outer rocks exposed to the full force of the open sea. I have no doubt that many lakes—for instance, in Scotland—which are very deep within, and are separated from the sea apparently only by a tract of detritus, were originally sea-channels, with banks of this nature near their mouths, which have since been upheaved" ("Geol. Obs. S. America," page 24, footnote.), with regard to the shoaling of the deep fiords of T. del Fuego near their mouths, and which I have remarked would tend, with a little elevation, to convert such fiords into lakes with a great mound-like barrier of detritus at their mouths, might, possibly, have been of use to you with regard to the lakes of Glen Roy.

LETTER 522. TO C. LYELL. Down, Wednesday, 8th.

Many thanks for your paper. (522/1. "On the Ancient Glaciers of Forfarshire." "Proc. Geol. Soc." Volume III., page 337, 1840.) I do admire your zeal on a subject on which you are not immediately at work. I will give my opinion as briefly as I can, and I have endeavoured my best to be honest. Poor Mrs. Lyell will have, I foresee, a long letter to read aloud, but I will try to write better than usual. Imprimis, it is provoking that Mr. Milne (522/2. "On the Parallel Roads of Lochaber, etc." "Trans. R. Soc. Edinb." Volume XVI., page 395, 1849. {Read March 1st and April 5th, 1847.}) has read my paper (522/3. "Observations on the Parallel Roads of Glen Roy, etc." "Phil. Trans. R. Soc." 1839, page 39. {Read February 7th, 1839.}.) with little attention, for he makes me say several things which I do not believe—as, that the water sunk suddenly! (page 10), that the Valley of Glen Roy, page 13, and Spean was filled up with detritus to level of the lower shelf, against which there is, I conceive, good evidence, etc., but I suppose it is the consequence of my paper being most tediously written. He gives me a just snub for talking of demonstration, and he fights me in a very pleasant manner. Now for business. I utterly disbelieve in the barriers (522/4. See note, Letter 521.) for his lakes, and think he has left that point exactly where it was in the time of MacCulloch (522/5. "On the Parallel Roads of Glen Roy." "Geol. Trans." Volume IV., page 314, 1817 (with several maps and sections).) and Dick. (522/6. "On the Parallel Roads of Lochaber." "Trans. R. Soc. Edinb." Volume IX., page 1, 1823.) Indeed, in showing that there is a passage at Glen Glaster at the level of the intermediate shelf, he makes the difficulty to my mind greater. (522/7. See Letter 521, note.) When I think of the gradual manner in which the two upper terraces die out at Glen Collarig and at the mouth of Glen Roy, the smooth rounded form of the hills there, and the lower shelf retaining its usual width where the immense barrier stood, I can deliberately repeat "that more convincing proofs of the non-existence of the imaginary Loch Roy could scarcely have been invented with full play given to the imagination," etc.: but I do not adhere to this remark with such strength when applied to the glacier-lake theory. Oddly, I was never at all staggered by this theory until now, having read Mr. Milne's argument against it. I now can hardly doubt that a great glacier did emerge from Loch Treig, and this by the ice itself (not moraine) might have blocked up the three outlets from Glen Roy. I do not, however, yet believe in the glacier theory, for reasons which I will presently give.

There are three chief hostile considerations in Mr. Milne's paper. First, the Glen {shelf?}, not coinciding in height with the upper one {outlet?}, from observations giving 12 feet, 15 feet, 29 feet, 23 feet: if the latter are correct the terrace must be quite independent, and the case is hostile; but Mr. Milne shows that there is one in Glen Roy 14 feet below the upper one, and a second one again (which I observed) beneath this, and then we come to the proper second shelf. Hence there is no great improbability in an independent shelf having been found in Glen Gluoy.

This leads me to Mr. Milne's second class of facts (obvious to every one), namely the non-extension of the three shelves beyond Glen Roy; but I abide by what I have written on that point, and repeat that if in Glen Roy, where circumstances have been so favourable for the preservation or formation of the terraces, a terrace could be formed quite plain for three-quarters of a mile with hardly a trace elsewhere, we cannot argue, from the non-existence of shelves, that water did not stand at the same levels in other valleys. Feeling absolutely convinced that there was no barrier of detritus at the mouth of Glen Roy, and pretty well convinced that there was none of ice, the manner in which the terraces die out when entering Glen Spean, which must have been a tideway, shows on what small circumstances the formation of these shelves depended. With respect to the non-existence of shelves in other parts of Scotland, Mr. Milne shows that many others do exist, and their heights above the sea have not yet been carefully measured, nor have even those of Glen Roy, which I suspect are all 100 feet too high. Moreover, according to Bravais (522/8. "On the Lines of Ancient Level of the Sea in Finmark." By A. Bravais, Member of the Scientific Commission of the North. "Quart. Journ. Geol. Soc." Volume I., page 534, 1845 (a translation).), we must not feel sure that either the absolute height or the intermediate heights between the terraces would be at all the same at distant points. In levelling the terraces in Lochaber, all, I believe, have been taken in Glen Roy, nearly N. and S. There should be levels taken at right angles to this line and to the Great Glen of Scotland or chief line of elevation.

Thirdly, the nature of the outlets from the supposed lakes. This appears to me the best and newest part of the paper. If Sir James Clark would like to attend to any particular points, direct his attention to this: especially to follow Glen Glaster from Glen Roy to L. Laggan. Mr. Milne describes this as an old and great river-course with a fall of 212 feet. He states that the rocks are smooth on upper face and rough on lower, but he does not mention whether this character prevails throughout the whole 212 vertical feet—a most important consideration; nor does he state whether these rocks are polished or scratched, as might have happened even to a considerable depth beneath the water (Mem. great icebergs in narrow fiords of T. del Fuego (522/9. In the "Voyage of the 'Beagle'" a description is given of the falling of great masses of ice from the icy cliffs of the glaciers with a crash that "reverberates like the broadside of a man-of-war, through the lonely channels" which intersect the coast-line of Tierra del Fuego. Loc. cit., page 246.)) by the action of icebergs, for that icebergs transported boulders on to terraces, I have no doubt. Mr. Milne's description of the outlets of his lake sound to me more like tidal channels, nor does he give any arguments how such are to be distinguished from old river-courses. I cannot believe in the body of fresh water which must, on the lake theory, have flowed out of them. At the Pass of Mukkul he states that the outlet is 70 feet wide and the rocky bottom 21 feet below the level of the shelf, and that the gorge expands to the eastwards into a broad channel of several hundred yards in width, divided in the middle by what has formerly been a rocky islet, against which the waters of this large river had chafed in issuing from the pass. We know the size of the river at the present day which would flow out through this pass, and it seems to me (and in the other given cases) to be as inadequate; the whole seems to me far easier explained by a tideway than by a formerly more humid climate.

With respect to the very remarkable coincidence between the shelves and the outlets (rendered more remarkable by Mr. Milne's discovery of the outlet to the intermediate shelf at Glen Glaster (522/10. See Letter 521, note.)), Mr. Milne gives only half of my explanation; he alludes to (and disputes) the smoothing and silting-up action, which I still believe in. I state: If we consider what must take place during the gradual rise of a group of islands, we shall have the currents endeavouring to cut down and deepen some shallow parts in the channels as they are successively brought near the surface, but tending from the opposition of tides to choke up others with littoral deposits. During a long interval of rest, from the length of time allowed to the above processes, the tendency would often prove effective, both in forming, by accumulation of matter, isthmuses, and in keeping open channels. Hence such isthmuses and channels just kept open would oftener be formed at the level which the waters held at the interval of rest, than at any other (page 65). I look at the Pass of Mukkul (21 feet deep, Milne) as a channel just kept open, and the head of Glen Roy (where there is a great bay silted up) and of Kilfinnin (at both which places there are level-topped mounds of detritus above the level of the terraces) as instances of channels filled up at the stationary levels. I have long thought it a probable conjecture that when a rising surface becomes stationary it becomes so, not at once, but by the movements first becoming very slow; this would greatly favour the cutting down many gaps in the mountains to the level of the stationary periods.

GLACIER THEORY.

If a glacialist admitted that the sea, before the formation of the terraces, covered the country (which would account for land-straits above level of terraces), and that the land gradually emerged, and if he supposed his lakes were banked by ice alone, he would make out, in my opinion, the best case against the marine origin of the terraces. From the scattered boulders and till, you and I must look at it as certain that the sea did cover the whole country, and I abide quite by my arguments from the buttresses, etc., that water of some kind receded slowly from the valleys of Lochaber (I presume Mr. Milne admits this). Now, I do not believe in the ice-lake theory, from the following weak but accumulating reasons: because, 1st, the receding water must have been that of a lake in Glen Spean, and of the sea in the other valleys of Scotland, where I saw similar buttresses at many levels; 2nd, because the outlets of the supposed lakes as already stated seem, from Mr. Milne's statements, too much worn and too large; 3rd, when the lake stood at the three-quarters of a mile shelf the water from it must have flowed over ice itself for a very long time, and kept at the same exact level: certainly this shelf required a long time for its formation; 4th, I cannot believe a glacier would have blocked up the short, very wide valley of Kilfinnin, the

Great Glen of Scotland also being very low there; 5th, the country at some places where Mr. Milne has described terraces is not mountainous, and the number of ice-lakes appears to me very improbable; 6th, I do not believe any lake could scoop the rocks so much as they are at the entrance to Loch Treig or cut them off at the head of Upper Glen Roy; 7th, the very gradual dying away of the terraces at the mouth of Glen Roy does not look like a barrier of any kind; 8th, I should have expected great terminal moraines across the mouth of Glen Roy, Glen Collarig, and Glaster, at least at the bottom of the valleys. Such, I feel pretty sure, do not exist.

I fear I must have wearied you with the length of this letter, which I have not had time to arrange properly. I could argue at great length against Mr. Milne's theory of barriers of detritus, though I could help him in one way—viz., by the soundings which occur at the entrances of the deepest fiords in T. del Fuego. I do not think he gives the smallest satisfaction with respect to the successive and comparatively sudden breakage of his many lakes.

Well, I enjoyed my trip to Glen Roy very much, but it was time thrown away. I heartily wish you would go there; it should be some one who knows glacier and iceberg action, and sea action well. I wish the Queen would command you. I had intended being in London to-morrow, but one of my principal plagues will, I believe, stop me; if I do I will assuredly call on you. I have not yet read Mr. Milne on Elevation (522/11. "On a Remarkable Oscillation of the Sea, observed at Various Places on the Coasts of Great Britain in the First Week of July, 1843." "Trans. R. Soc. Edinb." Volume XV., page 609, 1844.), so will keep his paper for a day or two.

P.S.—As you cannot want this letter, I wish you would return it to me, as it will serve as a memorandum for me. Possibly I shall write to Mr. Chambers, though I do not know whether he will care about what I think on the subject. This letter is too long and ill-written for Sir J. Clark.

LETTER 523. TO LADY LYELL. {October 4th, 1847.}

I enclose a letter from Chambers, which has pleased me very much (which please return), but I cannot feel quite so sure as he does. If the Lochaber and Tweed roads really turn out exactly on a level, the sea theory is proved. What a magnificent proof of equality of elevation, which does not surprise me much; but I fear I see cause of doubt, for as far as I remember there are numerous terraces, near Galashiels, with small intervals of height, so that the coincidence of height might be cooked. Chambers does not seem aware of one very striking coincidence, viz., that I made by careful measurement my Kilfinnin terrace 1202 feet above sea, and now Glen Gluoy is 1203 feet, according to the recent more careful measurements. Even Agassiz (523/1. "On the Glacial Theory," by Louis Agassiz, "Edinb. New Phil. Journ." Volume XXXIII., page 217, 1842. The parallel terraces are dealt with by Agassiz, pages 236 et seq.) would be puzzled to block up Glen Gluoy and Kilfinnin by the same glacier, and then, moreover, the lake would have two outlets. With respect to the middle terrace of Glen Roy—seen by Chambers in the Spean (figured by Agassiz, and seen by myself but not noticed, as I thought it might have been a sheep track)—it might yet have been formed on the ice-lake theory by two independent glaciers going across the Spean, but it is very improbable that two such immense ones should not have been united into one. Chambers, unfortunately, does not seem to have visited the head of the Spey, and I have written to propose joining funds and sending some young surveyor there. If my letter is published in the "Scotsman," how Buckland (523/2. Professor Buckland may be described as joint author, with Agassiz, of the Glacier theory.), as I have foreseen, will crow over me: he will tell me he always knew that I was wrong, but now I shall have rather ridiculously to say, "but I am all right again."

I have been a good deal interested in Miller (523/3. Hugh Miller's "First Impressions of England and its People," London, 1847.), but I find it not quick reading, and Emma has hardly begun it yet. I rather wish the scenic descriptions were shorter, and that there was a little less geologic eloquence.

Lyell's picture now hangs over my chimneypiece, and uncommonly glad I am to have it, and thank you for it.

LETTER 524. TO C. LYELL. Down, September 6th {1861}.

I think the enclosed is worth your reading. I am smashed to atoms about Glen Roy. My paper was one long gigantic blunder from beginning to end. Eheu! Eheu! (524/1. See "Life and Letters," I., pages 68, 69, also pages 290, 291.)

LETTER 525. TO C. LYELL. Down, September 22nd {1861}.

I have read Mr. Jamieson's last letter, like the former ones, with very great interest. (525/1. Mr. Jamieson visited Glen Roy in August 1861 and in July 1862. His paper "On the Parallel Roads of Glen Roy, and their Place in the History of the Glacial Period," was published in the "Quarterly Journal of the Geological Society" in 1863, Volume XIX., page 235. His latest contribution to this subject was published in the "Quarterly Journal," Volume XLVIII., page 5, 1892.) What a problem you have in hand! It beats manufacturing new species all to bits. It would be a great personal consolation to me if Mr. J. can admit the sloping Spean terrace to be marine, and would remove one of my greatest difficulties—viz. the vast contrast of Welsh and Lochaber valleys. But then, as far as I dare trust my observations, the sloping terraces ran far up the Roy valley, so as to reach not far below the lower shelf. If the sloping fringes are marine and the shelves lacustrine, all I can say is that nature has laid a shameful trap to catch an unwary wretch. I suppose that I have underrated the power of lakes in producing pebbles; this, I think, ought to be well looked to. I was much struck in Wales on carefully comparing the glacial scratches under a lake (formed by a moraine and which must have existed since the Glacial epoch) and above water, and I could perceive NO difference. I believe I saw many such beds of good pebbles on level of lower shelf, which at the time I could not believe could have been found on shores of lake. The land-straits and little cliffs above them, to which I referred, were quite above the highest shelf; they may be of much more ancient date than the shelves. Some terrace-like fringes at head of the Spey strike me as very suspicious. Mr. J. refers to absence of pebbles at considerable heights: he must remember that every storm, every deer, every hare which runs tends to roll pebbles down hill, and not one ever goes up again. I may mention that I particularly alluded to this on S. Ventanao (525/2. "Geolog. Obs. on South America," page 79. "On the flanks of the mountains, at a height of 300 or 400 feet above the plain, there were a few small patches of conglomerate and breccia, firmly cemented by ferruginous matter to the abrupt and battered face of the quartz—traces being thus exhibited of ancient sea-action.") in N. Patagonia, a great isolated rugged quartz-mountain 3,000 feet high, and I could find not one pebble except on one very small spot, where a ferruginous spring had firmly cemented a few to the face of mountain. If the Lochaber lakes had been formed by an ice-period posterior to the (marine?) sloping terraces in the Spean, would not Mr. J. have noticed gigantic moraines across the valley opposite the opening of Lake Treig? I go so far as not to like making the elevation of the land in Wales and Scotland considerably different with respect to the ice-period, and still more do I dislike it with respect to E. and W. Scotland. But I may be prejudiced by having been so long accustomed to the plains of Patagonia. But the equality of level (barring denudation) of even the Secondary formations in Britain, after so many ups and downs, always impresses my mind, that, except when the crust-cracks and mountains are formed, movements of elevation and subsidence are generally very equable.

But it is folly my scribbling thus. You have a grand problem, and heaven help you and Mr. Jamieson through it. It is out of my line nowadays, and above and beyond me.

LETTER 526. TO J.D. HOOKER. Down, September 28th {1861}.

It is, I believe, true that Glen Roy shelves (I remember your Indian letter) were formed by glacial lakes. I persuaded Mr. Jamieson, an excellent observer, to go and observe them; and this is his result. There are some great difficulties to be explained, but I presume this will ultimately be proved the truth...

LETTER 527. TO C. LYELL. Down, October 1st {1861}.

Thank you for the most interesting correspondence. What a wonderful case that of Bedford. (527/1. No doubt this refers to the discovery of flint implements in the Valley of the Ouse, near Bedford, in 1861 (see Lyell's "Antiquity of Man," pages 163 et seq., 1863.) I thought the problem sufficiently perplexing before, but now it beats anything I ever heard of. Far from being able to give any hypothesis for any part, I cannot get the facts into my mind. What a capital observer and reasoner Mr. Jamieson is. The only way that I can reconcile my memory of Lochaber with the state of the Welsh valleys is by imagining a great barrier, formed by a terminal moraine, at the mouth of the Spean, which the river had to cut slowly through, as it drained the lowest lake after the Glacial period. This would, I can suppose, account for the sloping terraces along the Spean. I further presume that sharp transverse moraines would not be formed under the waters of the lake, where the glacier came out of L. Treig and abutted against the opposite side of the valley. A nice mess I made of Glen Roy! I have no spare copy of my Welsh paper (527/2. "Notes on the Effects produced by the Ancient Glaciers of Caernarvonshire, and on the Boulders transported by Floating Ice," "Edinb. New Phil. Journ." Volume XXXIII., page 352, 1842.); it would do you no good to lend it. I suppose I thought that there must have been floating ice on Moel Tryfan. I think it cannot be disputed that the last event in N. Wales was land-glaciers. I could not decide where the action of land-glaciers ceased and marine glacial action commenced at the mouths of the valleys.

What a wonderful case the Bedford case. Does not the N. American view of warmer or more equable period, after great Glacial period, become much more probable in Europe?

But I am very poorly to-day, and very stupid, and hate everybody and everything. One lives only to make blunders. I am going to write a little book for Murray on Orchids (527/3. "On the Various Contrivances by which Orchids are Fertilised by Insects," London, 1862.), and to-day I hate them worse than everything. So farewell, in a sweet frame of mind.

LETTER 528. TO C. LYELL. Down, October 14th {1861}.

I return Jamieson's capital letter. I have no comments, except to say that he has removed all my difficulties, and that now and for evermore I give up and abominate Glen Roy and all its belongings. It certainly is a splendid case, and wonderful monument of the old Ice-period. You ought to give a woodcut. How many have blundered over those horrid shelves!

That was a capital paper by Jamieson in the last "Geol. Journal." (528/1. "On the Drift and Rolled Gravel of the North of Scotland," "Quart. Journ. Geol. Soc." Volume XVI., page 347, 1860.) I was never before fully convinced of the land glacialisation of Scotland before, though Chambers tried hard to convince me.

I must say I differ rather about Ramsay's paper; perhaps he pushes it too far. (528/2. "On the Glacial Origin of Certain Lakes, etc." "Quart. Journ. Geol. Soc." Volume XVIII., page 185. See Letter 503.) It struck me the more from remembering some years ago marvelling what could be the meaning of such a multitude of lakes in Friesland and other northern districts. Ramsay wrote to me, and I suggested that he ought to compare mountainous tropical regions with northern regions. I could not remember many lakes in any mountainous tropical country. When Tyndall talks of every valley in Switzerland being formed by glaciers, he seems to forget there are valleys in the tropics; and it is monstrous, in my opinion, the accounting for the Glacial period in the Alps by greater height of mountains, and their lessened height, if I understand, by glacial erosion. "Ne sutor ultra crepidam," I think, applies in this case to him. I am hard at work on "Variation under Domestication." (528/3. Published 1868.)

P.S.—I am rather overwhelmed with letters at present, and it has just occurred to me that perhaps you will forward my note to Mr. Jamieson; as it will show that I entirely yield. I do believe every word in my Glen Roy paper is false.

LETTER 529. TO C. LYELL. Down, October 20th {1861}.

Notwithstanding the orchids, I have been very glad to see Jamieson's letter; no doubt, as he says, certainty will soon be reached.

With respect to the minor points of Glen Roy, I cannot feel easy with a mere barrier of ice; there is so much sloping, stratified detritus in the valleys. I remember that you somewhere have stated that a running stream soon cuts deeply into a glacier. I have been hunting up all old references and pamphlets, etc., on shelves in Scotland, and will send them off to Mr. J., as they possibly may be of use to him if he continues the subject. The Eildon Hills ought to be specially examined. Amongst MS. I came across a very old letter from me to you, in which I say: "If a glacialist admitted that the sea, before the formation of the shelves, covered the country (which would account for the land-straits above the level of the shelves), and if he admitted that the land gradually emerged, and if he supposed that his lakes were banked up by ice alone, he would make out, in my opinion, the best case against the marine origin of the shelves." (529/1. See Letter 522.) This seems very much what you and Mr. J. have come to.

The whole glacial theory is really a magnificent subject.

LETTER 530. TO C. LYELL. Down, April 1st {1862}.

I am not quite sure that I understand your difficulty, so I must give what seems to me the explanation of the glacial lake theory at some little length. You know that there is a rocky outlet at the level of all the shelves. Please look at my map. (530/1. The map accompanying Mr. Darwin's paper in the "Phil. Trans. R. Soc." 1839.) I suppose whole valley of Glen Spean filled with ice; then water would escape from an outlet at Loch Spey, and the highest shelf would be first formed. Secondly, ice began to retreat, and water will flow for short time over its surface; but as soon as it retreated from behind the hill marked Craig Dhu, where the outlet on level of second shelf was discovered by Milne (530/2. See note, Letter 521.), the water would flow from it and the second shelf would be formed. This supposes that a vast barrier of ice still remains under Ben Nevis, along all the lower part of the Spean. Lastly, I suppose the ice disappeared everywhere along L. Loggan, L. Treig, and Glen Spean, except close under Ben Nevis, where it still formed a barrier, the water flowing out at level of lowest shelf by the Pass of Mukkul at head of L. Loggan. This seems to me to account for everything. It presupposes that the shelves were formed towards the close of the Glacial period. I come up to London to read on Thursday a short paper at the Linnean Society. Shall I call on Friday morning at 9.30 and sit half an hour with you? Pray have no scruple to send a line to Queen Anne Street to say "No" if it will take anything out of you. If I do not hear, I will come.

LETTER 531. TO J. PRESTWICH. Down, January 3rd, 1880.

You are perfectly right. (531/1. Prof. Prestwich's paper on Glen Roy was published in the "Phil. Trans. R. Soc." for 1879, page 663.) As soon as I read Mr. Jamieson's article on the parallel roads, I gave up the ghost with more sighs and groans than on almost any other occasion in my life.

2.IX.IV. CORAL REEFS, FOSSIL AND RECENT, 1841-1881.

LETTER 532. TO C. LYELL. Shrewsbury, Tuesday, 6th {July, 1841}.

Your letter was forwarded me here. I was the more glad to receive it, as I never dreamed of your being able to find time to write, now that you must be so very busy; and I had nothing to tell you about myself, else I should have written. I am pleased to hear how extensive and successful a trip you appear to have made. You must have worked hard, and got your Silurian subject well in your head, to have profited by so short an excursion. How I should have enjoyed to have followed you about the coral-limestone. I once was close to Wenlock (532/1. The Wenlock limestone (Silurian) contains an abundance of corals. "The rock seems indeed to have been formed in part by massive sheets and bunches of coral" (Geikie, "Text-book of Geology," 1882, page 678.), something such as you describe, and made a rough drawing, I remember, of the masses of coral. But the degree in which the whole mass was regularly stratified, and the quantity of mud, made me think that the reefs could never have been like those in the Pacific, but that they most resembled those on the east coast of Africa, which seem (from charts and descriptions) to confine extensive flats and mangrove swamps with mud, or like some imperfect ones about the West India Islands, within the reefs of which there are large swamps. All the reefs I have myself seen could be associated only with nearly pure calcareous rocks. I have received a description of a reef lying some way off the coast near Belize (terra firma), where a thick bed of mud seems to have invaded and covered a coral reef, leaving but very few islets yet free from it. But I can give you no precise information without my notes (even if then) on these heads...

Bermuda differs much from any other island I am acquainted with. At first sight of a chart it resembles an atoll; but it differs from this structure essentially in the gently shelving bottom of the sea all round to some distance; in the absence of the defined circular reefs, and, as a consequence, of the defined central pool or lagoon; and lastly, in the height of the land. Bermuda seems to be an irregular, circular, flat bank, encrusted with knolls and reefs of coral, with land formed on one side. This land seems once to have been more extensive, as on some parts of the bank farthest removed from the island there are little pinnacles of rock of the same nature as that of the high larger islands. I cannot pretend to form any precise notion how the foundation of so anomalous an island has been produced, but its whole history must be very different from that of the atolls of the Indian and Pacific oceans—though, as I have said, at first glance of the charts there is a considerable resemblance.

LETTER 533. TO C. LYELL. {1842.}

Considering the probability of subsidence in the middle of the great oceans being very slow; considering in how many spaces, both large ones and small ones (within areas favourable to the growth of corals), reefs are absent, which shows that their presence is determined by peculiar conditions; considering the possible chance of subsidence being more rapid than the upward growth of the reefs; considering that reefs not very rarely perish (as I cannot doubt) on part, or round the whole, of some encircled islands and atolls: considering these things, I admit as very improbable that the polypifers should continue living on and above the same reef during a subsidence of very many thousand feet; and therefore that they should form masses of enormous thickness, say at most above 5,000 feet. (533/1. "...As we know that some inorganic causes are highly injurious to the growth of coral, it cannot be expected that during the round of change to which earth, air, and water are exposed, the reef-building polypifers should keep alive for perpetuity in any one place; and still less can this be expected during the progressive subsidences...to which by our theory these reefs and islands have been subjected, and are liable" ("The Structure and Distribution of Coral Reefs," page 107: London, 1842).) This admission, I believe, is in no way fatal to the theory, though it is so to certain few passages in my book.

In the areas where the large groups of atolls stand, and where likewise a few scattered atolls stand between such groups, I always imagined that there must have been great tracts of land, and that on such large tracts there must have been mountains of immense altitudes. But not, it appears to me, that one is only justified in supposing that groups of islands stood there. There are (as I believe) many considerable islands and groups of islands (Galapagos Islands, Great Britain, Falkland Islands, Marianas, and, I believe, Viti groups), and likewise the majority of single scattered islands, all of which a subsidence between 4,000 and 5,000 feet would entirely submerge or would leave only one or two summits above water, and hence they would produce either groups of nothing but atolls, or of atolls with one or two encircled islands. I am far from wishing to say that the islands of the great oceans have not subsided, or may not continue to subside, any number of feet, but if the average duration (from all causes of destruction) of reefs on the same spot is limited, then after this limit has elapsed the reefs would perish, and if the subsidence continued they would be carried down; and if the group consisted only of atolls, only open ocean would be left; if it consisted partly or wholly of encircled islands, these would be left naked and reefless, but should the area again become favourable for growth of reefs, new barrier-reefs might be formed round them. As an illustration of this notion of a certain average duration of reefs on the same spot, compared with the average rate of subsidence, we may take the case of Tahiti, an island of 7,000 feet high. Now here the present barrier-reefs would never be continued upwards into an atoll, although, should the subsidence continue at a period long after the death of the present reefs, new ones might be formed high up round its sides and ultimately over it. The case resolves itself into: what is the ordinary height of groups of islands, of the size of existing groups of atolls (excepting as many of the highest islands as there now ordinarily occur encircling barrier-reefs in the existing groups of atolls)? and likewise what is the height of the single scattered islands standing between such groups of islands? Subsidence sufficient to bury all these islands (with the exception of as many of the highest as there are encircled islands in the present groups of atolls) my theory absolutely requires, but no more. To say what amount of subsidence would be required for this end, one ought to know the height of all existing islands, both single ones and those in groups, on the face of the globe—and, indeed, of half a dozen worlds like ours. The reefs may be of much greater {thickness} than that just sufficient on an average to bury groups of islands; and the probability of the thickness being greater seems to resolve itself into the average rate of subsidence allowing upward growth, and average duration of reefs on the same spot. Who will say what this rate and what this duration is? but till both are known, we cannot, I think, tell whether we ought to look for upraised coral formations (putting on one side denudation) above the unknown limit, say between 3,000 and 5,000 feet, necessary to submerge groups of common islands. How wretchedly involved do these speculations become.

LETTER 534. TO E. VON MOJSISOVICS. Down, January 29th, 1879.

I thank you cordially for the continuation of your fine work on the Tyrolese Dolomites (534/1. "Dolomitriffe Sudtirols und Venetiens": Wien, 1878.), with its striking engravings and the maps, which are quite wonderful from the amount of labour which they exhibit, and its extreme difficulty. I well remember more than forty years ago examining a section of Silurian limestone containing many corals, and thinking to myself that it would be for ever impossible to discover whether the ancient corals had formed atolls or barrier reefs; so you may well believe that your work will interest me greatly as soon as I can find time to read it. I am much obliged for your photograph, and from its appearance rejoice to see that much more good work may be expected from you.

I enclose my own photograph, in case you should like to possess a copy.

LETTER 535. TO A. AGASSIZ.

(535/1. Part of this letter is published in "Life and Letters," III., pages 183, 184.)

Down, May 5th, 1881.

It was very good of you to write to me from Tortugas, as I always feel much interested in hearing what you are about, and in reading your many discoveries. It is a surprising fact that the peninsula of Florida should have remained at the same level for the immense period requisite for the accumulation of so vast a pile of debris. (535/2. Alexander Agassiz published a paper on "The Tortugas and Florida Reefs" in the "Mem. Amer. Acad. Arts and Sci." XI., page 107, 1885. See also his "Three Cruises of the 'Blake,'" Volume I., 1888.)

You will have seen Mr. Murray's views on the formation of atolls and barrier reefs. (535/3. "On the Structure and Origin of Coral Reefs and Islands," "Proc. R. Soc. Edin." Volume X., page 505, 1880. Prof. Bonney has given a summary of Sir John Murray's views in Appendix II. of the third edition of Darwin's "Coral Reefs," 1889.) Before publishing my book, I thought long over the same view, but only as far as ordinary marine organisms are concerned, for at that time little was known of the multitude of minute oceanic organisms. I rejected this view, as from the few dredgings made in the 'Beagle' in the S. Temperate regions, I concluded that shells, the smaller corals, etc., etc., decayed and were dissolved when not protected by the deposition of sediment; and sediment could not accumulate in the open ocean. Certainly shells, etc., were in several cases completely rotten, and crumbled into mud between my fingers; but you will know well whether this is in any degree common. I have expressly said that a bank at the proper depth would give rise to an atoll, which could not be distinguished from one formed during subsidence. I can, however, hardly believe, in the former presence of as many banks (there having been no subsidence) as there are atolls in the great oceans, within a reasonable depth, on which minute oceanic organisms could have accumulated to the thickness of many hundred feet. I think that it has been shown that the oscillations from great waves extend down to a considerable depth, and if so the oscillating water would tend to lift up (according to an old doctrine propounded by Playfair) minute particles lying at the bottom, and allow them to be slowly drifted away from the submarine bank by the slightest current. Lastly, I cannot understand Mr. Murray, who admits that small calcareous organisms are dissolved by the carbonic acid in the water at great depths, and that coral reefs, etc., etc., are likewise dissolved near the surface, but that this does not occur at intermediate depths, where he believes that the minute oceanic calcareous organisms accumulate until the bank reaches within the reef-building depth. But I suppose that I must have misunderstood him.

Pray forgive me for troubling you at such a length, but it has occurred to me that you might be disposed to give, after your wide experience, your judgment. If I am wrong, the sooner I am knocked on the head and annihilated so much the better. It still seems to me a marvellous thing that there should not have been much and long-continued subsidence in the beds of the great oceans. I wish that some doubly rich millionaire would take it into his head to have borings made in some of the Pacific and Indian atolls, and bring home cores for slicing from a depth of 500 or 600 feet. (535/4. In 1891 a Committee of the British Association was formed for the investigation of an atoll by means of boring. The Royal Society took up the scheme, and an expedition was sent to Funafuti, with Prof. Sollas as leader. Another expedition left Sydney in 1897 under the direction of Prof. Edgeworth David, and a deeper boring was made. The Reports will be published in the "Philosophical Transactions," and will contain Prof. David's notes upon the boring and the island generally, Dr. Hinde's description of the microscopic structure of the cores and other examinations of them, carried on at the Royal College of Science, South Kensington. The boring reached a depth of 1114 feet; the cores were found to consist entirely of reef-forming corals in situ and in fragments, with foraminifera and calcareous algae; at the bottom there were no traces of any other kind of rock. It seems, therefore, to us, that unless it can be proved that reef-building corals began their work at depths of at least 180 fathoms—far below that hitherto assigned—the result gives the strongest support to Darwin's theory of subsidence; the test which Darwin wished to be applied has been fairly tried, and the verdict is entirely in his favour.)

2.IX.V. CLEAVAGE AND FOLIATION, 1846-1856. LETTER 536. TO D. SHARPE.

(536/1. The following eight letters were written at a time when the subjects of cleavage and foliation were already occupying the minds of several geologists, including Sharpe, Sorby, Rogers, Haughton, Phillips, and Tyndall. The paper by Sharpe referred to was published in 1847 ("Quart. Journ. Geol. Soc." Volume III.), and his ideas were amplified in two later papers (ibid., Volume V., 1849, and "Phil. Trans." 1852). Darwin's own views, based on his observations during the "Beagle" expedition, had appeared in Chapter XIII. of "South America" (1846) and in the "Manual of Scientific Enquiry" (1849), but are perhaps nowhere so clearly expressed as in this correspondence. His most important contribution to the question was in establishing the fact that foliation is often a part of the same process as cleavage, and is in nowise necessarily connected with planes of stratification. Herein he was opposed to Lyell and the other geologists of the day, but time has made good his position. The postscript to Letter 542 is especially interesting. We are indebted to Mr. Harker, of St. John's College, for this note.)

Down, August 23rd {1846?}.

I must just send one line to thank you for your note, and to say how heartily glad I am that you stick to the cleavage and foliation question. Nothing will ever convince me that it is not a noble subject of investigation, which will lead some day to great views. I think it quite extraordinary how little the subject seems to interest British geologists. You will, I think live to see the importance of your paper recognised. (536/2. Probably the paper "On Slaty Cleavage." "Quart. Journ. Geol. Soc." Volume III., page 74, 1847.) I had always thought that Studer was one of the few geologists who had taken a correct and enlarged view on the subject.

LETTER 537. TO D. SHARPE. Down {November 1846}.

I have been much interested with your letter, and am delighted that you have thought my few remarks worth attention. My observations on foliation are more deserving confidence than those on cleavage; for during my first year in clay-slate countries, I was quite unaware of there being any marked difference between cleavage and stratification; I well remember my astonishment at coming to the conclusion that they were totally different actions, and my delight at subsequently reading Sedgwick's views (537/1. "Remarks on the Structure of Large Mineral Masses, and especially on the Chemical Changes produced in the Aggregation of Stratified Rocks during different periods after their Deposition." "Trans. Geol. Soc." Volume III., page 461, 1835. In the section of this paper dealing with cleavage (page 469) Prof. Sedgwick lays stress on the fact that "the cleavage is in no instance parallel to the true beds."); hence at that time I was only just getting out of a mist with respect to cleavage-laminae dipping inwards on mountain flanks. I have certainly often observed it—so often that I thought myself justified in propounding it as usual. I might perhaps have been in some degree prejudiced by Von Buch's remarks, for which in those days I had a somewhat greater deference than I now have. The Mount at M. Video (page 146 of my book (537/2. "Geol. Obs. S. America."

page 146. The mount is described as consisting of hornblendic slate; "the laminae of the slate on the north and south side near the summit dip inwards.")) is certainly an instance of the cleavage-laminae of a hornblendic schist dipping inwards on both sides, for I examined this hill carefully with compass in hand and notebook. I entirely admit, however, that a conclusion drawn from striking a rough balance in one's mind is worth nothing compared with the evidence drawn from one continuous line of section. I read Studer's paper carefully, and drew the conclusion stated from it; but I may very likely be in an error. I only state that I have frequently seen cleavage-laminae dipping inwards on mountain sides; that I cannot give up, but I daresay a general extension of the rule (as might justly be inferred from the manner of my statement) would be quite erroneous. Von Buch's statement is in his "Travels in Norway" (537/3. "Travels through Norway and Lapland during the years 1806-8": London, 1813.); I have unfortunately lost the reference, and it is a high crime, I confess, even to refer to an opinion without a precise reference. If you never read these travels they might be worth skimming, chiefly as an amusement; and if you like and will send me a line by the general post of Monday or Tuesday, I will either send it up with Hopkins on Wednesday, or bring it myself to the Geological Society. I am very glad you are going to read Hopkins (537/4. "Researches in Physical Geology," by W. Hopkins. "Phil. Trans. R. Soc." 1839, page 381; ibid, 1842, page 43, etc.); his views appear to me eminently worth well comprehending; false views and language appear to me to be almost universally held by geologists on the formation of fissures, dikes and mountain chains. If you would have the patience, I should be glad if you would read in my "Volcanic Islands" from page 65, or even pages 54 to 72—viz., on the lamination of volcanic rocks; I may add that I sent the series of specimens there described to Professor Forbes of Edinburgh, and he thought they bore out my views.

There is a short extract from Prof. Rogers (537/5. "On Cleavage of Slate-strata." "Edinburgh New Phil. Journ." Volume XLI., page 422, 1846.) in the last "Edinburgh New Phil. Journal," well worth your attention, on the cleavage of the Appalachian chain, and which seems far more uniform in the direction of dip than in any case which I have met with; the Rogers doctrine of the ridge being thrown up by great waves I believe is monstrous; but the manner in which the ridges have been thrown over (as if by a lateral force acting on one side on a higher level than on the other) is very curious, and he now states that the cleavage is parallel to the axis-planes of these thrown-over ridges. Your case of the limestone beds to my mind is the greatest difficulty on any mechanical doctrine; though I did not expect ever to find actual displacement, as seems to be proved by your shell evidence. I am extremely glad you have taken up this most interesting subject in such a philosophical spirit; I have no doubt you will do much in it; Sedgwick let a fine opportunity slip away. I hope you will get out another section like that in your letter; these are the real things wanted.

LETTER 538. TO D. SHARPE. Down, {January 1847}.

I am very much obliged for the MS., which I return. I do not quite understand from your note whether you have struck out all on this point in your paper: I much hope not; if you have, allow me to urge on you to append a note, briefly stating the facts, and that you omitted them in your paper from the observations not being finished.

I am strongly tempted to suspect that the cleavage planes will be proved by you to have slided a little over each other, and to have been planes of incipient tearing, to use Forbes' expression in ice; it will in that case be beautifully analogical with my laminated lavas, and these in composition are intimately connected with the metamorphic schists.

The beds without cleavage between those with cleavage do not weigh quite so heavily on me as on you. You remember, of course, Sedgwick's facts of limestone, and mine of sandstone, breaking in the line of cleavage, transversely to the planes of deposition. If you look at cleavage as I do, as the result of chemical action or crystalline forces, super-induced in certain places by their mechanical state of tension, then it is not surprising that some rocks should yield more or less readily to the crystalline forces.

I think I shall write to Prof. Forbes (538/1. Prof. D. Forbes.) of Edinburgh, with whom I corresponded on my laminated volcanic rocks, to call his early attention to your paper.

LETTER 539. TO D. SHARPE. Down, October 16th {1851}.

I am very much obliged to you for telling me the results of your foliaceous tour, and I am glad you are drawing up an account for the Royal Society. (539/1. "On the Arrangement of the Foliation and Cleavage of the Rocks of the North of Scotland." "Phil. Trans. R. Soc." 1852, page 445, with Plates XXIII. and XXIV.) I hope you will have a good illustration or map of the waving line of junction of the slate and schist with uniformly directed cleavage and foliation. It strikes me as crucial. I remember longing for an opportunity to observe this point. All that I say is that when slate and the metamorphic schists occur in the same neighbourhood, the cleavage and foliation are uniform: of this I have seen many cases, but I have never observed slate overlying mica-slate. I have, however, observed many cases of glossy clay-slate included within mica-schist and gneiss. All your other observations on the order, etc., seem very interesting. From conversations with Lyell, etc., I recommend you to describe in a little detail the nature of the metamorphic schists; especially whether there are quasi-substrata of different varieties of mica-slate or gneiss, etc.; and whether you traced such quasi beds into the cleavage slate. I have not the least doubt of such facts occurring, from what I have seen (and described at M. Video) of portions of fine chloritic schists being entangled in the midst of a gneiss district. Have you had any opportunity of tracing a bed of marble? This, I think, from reasons given at page 166 of my "S. America," would be very interesting. (539/2. "I have never had an opportunity of tracing, for any distance, along the line both of strike and dip, the so-called beds in the metamorphic schists, but I strongly suspect that they would not be found to extend, with the same character, very far in the line either of their dip or strike. Hence I am led to believe that most of the so-called beds are of the nature of complex folia, and have not been separately deposited. Of course, this view cannot be extended to THICK masses included in the metamorphic series, which are of totally different composition from the adjoining schists, and which are far-extended, as is sometimes the case with quartz and marble; these must generally be of the nature of true strata" ("Geological Observations," page 166).) A suspicion has sometimes occurred to me (I remember more especially when tracing the clay-slate at the Cape of Good Hope turning into true gneiss) that possibly all the metamorphic schists necessarily once existed as clay-slate, and that the foliation did not arise or take its direction in the metamorphic schists, but resulted simply from the pre-existing cleavage. The so-called beds in the metamorphic schists, so unlike common cleavage laminae, seems the best, or at least one argument against such a suspicion. Yet I think it is a point deserving your notice. Have you thought at all over Rogers' Law, as he reiterates it, of cleavage being parallel to his axes-planes of elevation?

If you know beforehand, will you tell me when your paper is read, for the chance of my being able to attend? I very seldom leave home, as I find perfect quietude suits my health best.

(PLATE: CHARLES DARWIN, Cir. 1854. Maull & Fox, photo. Walker & Cockerell, ph. sc.)

LETTER 540. TO C. LYELL. Down, January 10th, 1855.

I received your letter yesterday, but was unable to answer it, as I had to go out at once on business of importance. I am very glad that you are reconsidering the subject of foliation; I have just read over what I have written on the subject, and admire it very much, and abide by it all. (540/1. "Geological Observations on South America," Chapter VI., 1846.) You will not readily believe how closely I attended to the subject, and in how many and wide areas I verified my remarks. I see I have put pretty strongly the mechanical view of origin; but I might even then, but was afraid, have put my belief stronger. Unfortunately I have not D. Sharpe's paper here to look over, but I think his chief points {are} (1) the foliation forming great symmetrical curves, and (2) the proof from effects of form of shell (540/2. This refers to the distortion of shells in cleaved rocks.) of the mechanical action in cleaved rocks. The great curvature would be, I think, a grand discovery of Sharpe's, but I confess there is some want of minuteness in the statement of Sharpe which makes me wish to see his facts confirmed. That the foliation and cleavage are parts of curves I am quite prepared, from what I have seen, to believe; but the simplicity and grandeur of Sharpe's curves rather stagger me. I feel deeply convinced that when (and I and Sharpe have seen several most striking and obvious examples) great neighbouring or alternating regions of true metamorphic schists and clay-slate have their foliations and cleavage parallel, there is no way of escaping the conclusion, that the layers of pure quartz, feldspar, mica, chlorite, etc., etc., are due not to original deposition, but to segregation; and this is I consider the point which I have established. This is very odd, but I suspect that great metamorphic areas are generally derived from the metamorphosis of clay-slate, and not from alternating layers of ordinary sedimentary matter. I think you have exactly put the chief difficulty in its strongest light—viz. what would be the result of pure or nearly pure layers of very different mineralogical composition being metamorphosed? I believe even such might be converted into an ordinary varying mass of metamorphic schists. I am certain of the correctness of my account of patches of chlorite schists enclosed in other schist, and of enormous quartzose veins of segregation being absolutely continuous and contemporaneous with the folia of quartz, and such, I think, might be the result of the folia crossing a true stratum of quartz. I think my description of the wonderful and beautiful laminated volcanic rocks at Ascension would be worth your looking at. (540/3. "Geological Observations on S. America," pages 166, 167; also "Geological Observations on the Volcanic Islands," Chapter III. (Ascension), 1844.)

LETTER 541. TO C. LYELL. Down, January 14th {1855}.

We were yesterday and the day before house-hunting, so I could not answer your letter. I hope we have succeeded in a house, after infinite trouble, but am not sure, in York Place, Baker Street.

I do not doubt that I either read or heard from Sharpe about the Grampians; otherwise from my own old suspicion I should not have inserted the passage in the manual.

The laminated rocks at Ascension are described at page 54. (541/1. "Volcanic Islands," page 54. "Singular laminated beds alternating with and passing into obsidian.")

As far as my experience has gone, I should speak only of clay-slate being associated with mica-slate, for when near the metamorphic schists I have found stratification so gone that I should not dare to speak of them as overlying them. With respect to the difficulty of beds of quartz and marble, this has for years startled me, and I have longed (since I have felt its force) to have some opportunity of testing this point, for without you are sure that the beds of quartz dip, as well as strike, parallel to the foliation, the case is only just like true strata of sandstone included in clay-slate and striking parallel to the cleavage of the clay-slate, but of course with different dip (excepting in those rare cases when cleavage and stratification are parallel). Having this difficulty before my eyes, I was much struck with MacCulloch's statement (page 166 of my "S. America") about marble in the metamorphic series not forming true strata.

Your expectation of the metamorphic schists sending veins into neighbouring rocks is quite new to me; but I much doubt whether you have any right to assume fluidity from almost any amount of molecular change. I have seen in fine volcanic sandstone clear evidence of all the calcareous matter travelling at least 4 1/2 feet in distance to concretions on either hand (page 113 of "S. America") (541/2. "Some of these concretions (flattened spherical concretions composed of hard calcareous sandstone, containing a few shells, occurring in a bed of sandstone) were 4 feet in diameter, and in a horizontal line 9 feet apart, showing that the calcareous matter must have been drawn to the centres of attraction from a distance of four feet and a half on both sides" ("Geological Observations on S. America," page 113).) I have not examined carefully, from not soon enough seeing all the difficulties; but I believe, from what I have seen, that the folia in the metamorphic schists (I do not here refer to the so-called beds) are not of great length, but thin out, and are succeeded by others; and the notion I have of the molecular movements is shown in the indistinct sketch herewith sent. The quartz of the strata might here move into the position of the folia without much more movement of molecules than in the formation of concretions. I further suspect in such cases as this, when there is a great original abundance of quartz, that great branching contemporaneous veins of segregation (as sometimes called) of quartz would be formed. I can only thus understand the relation which exists between the distorted foliation (not appearing due to injection) and the presence of such great veins.

I believe some gneiss, as the gneiss-granite of Humboldt, has been as fluid as granite, but I do not believe that this is usually the case, from the frequent alternations of glossy clay and chlorite slates, which we cannot suppose to have been melted.

I am far from wishing to doubt that true sedimentary strata have been converted into metamorphic schists: all I can say is, that in the three or four great regions, where I could ascertain the relations of the metamorphic schists to the neighbouring cleaved rocks, it was impossible (as it appeared to me) to admit that the foliation was due to aqueous deposition. Now that you intend agitating the subject, it will soon be cleared up.

LETTER 542. TO C. LYELL. 27, York Place, Baker Street {1855}.

I have received your letter from Down, and I have been studying my S. American book.

I ought to have stated {it} more clearly, but undoubtedly in W. Tierra del Fuego, where clay-slate passes by alternation into a grand district of mica-schist, and in the Chonos Islands and La Plata, where glossy slates occur within the metamorphic schists, the foliation is parallel to the cleavage—i.e. parallel in strike and dip; but here comes, I am sorry and ashamed to say, a great hiatus in my reasoning. I have assumed that the cleavage in these neighbouring or intercalated beds was (as in more distant parts) distinct from stratification. If you choose to say that here the cleavage was or might be parallel to true bedding, I cannot gainsay it, but can only appeal to apparent similarity to the great areas of uniformity of strike and high angle—all certainly unlike, as far as my experience goes, to true stratification. I have long known how easily I overlook flaws in my own reasoning, and this is a flagrant case. I have been amused to find, for I had quite forgotten, how distinctly I give a suspicion (top of page 155) to the idea, before Sharpe, of cleavage (not foliation) being due to the laminae forming parts of great curves. (542/1. "I suspect that the varying and opposite dips (of the cleavage-planes) may possibly be accounted for by the cleavage-laminae...being parts of large abrupt curves, with their summits cut off and worn down" ("Geological Observations on S. America," page 155). I well remember the fine section at the end of a region where the cleavage (certainly cleavage) had been most uniform in strike and most variable in dip.

I made with really great care (and in MS. in detail) observations on a case which I believe is new, and bears on your view of metamorphosis (page 149, at bottom). (Ibid., page 149.)

In a clay-slate porphyry region, where certain thin sedimentary layers of tuff had by self-attraction shortened themselves into little curling pieces, and then again into crystals of feldspar of large size, and which consequently were all strictly parallel, the series was perfect and beautiful. Apparently also the rounded grains of quartz had in other parts aggregated themselves into crystalline nodules of quartz.

I have not been able to get Sorby yet, but shall not probably have anything to write on it. I am delighted you have taken up the subject, even if I am utterly floored.

P.S.—I have a presentiment it will turn out that when clay-slate has been metamorphosed the foliation in the resultant schist has been due generally (if not, as I think, always) to the cleavage, and this to a certain degree will "save my bacon" (please look at my saving clause, page 167) (542/2. "As in some cases it appears that where a fissile rock has been exposed to partial metamorphic action (for instance, from the irruption of granite) the foliation has supervened on the already existing cleavage-planes; so, perhaps in some instances, the foliation of a rock may have been determined by the original planes of deposition or of oblique current laminae. I have, however, myself never seen such a case, and I must maintain that in most extensive metamorphic areas the foliation is the extreme result of that process, of which cleavage is the first effect" (Ibid., page 167).), but {with} other rocks than that, stratification has been the ruling agent, the strike, but not the dip, being in such cases parallel to any adjoining clay-slate. If this be so, pre-existing planes of division, we must suppose on my view of the cause, determining the lines of crystallisation and segregation, and not planes of division produced for the first time during the act of crystallisation, as in volcanic rocks. If this should ever be proved, I shall not look back with utter shame at my work.

LETTER 543. TO J.D. HOOKER. Down, September 8th {1856}.

I got your letter of the 1st this morning, and a real good man you have been to write. Of all the things I ever heard, Mrs. Hooker's pedestrian feats beat them. My brother is quite right in his comparison of "as strong as a woman," as a type of strength. Your letter, after what you have seen in the Himalayas, etc., gives me a wonderful idea of the beauty of the Alps. How I wish I was one-half or one-quarter as strong as Mrs. Hooker: but that is a vain hope. You must have had some very interesting work with glaciers, etc. When will the glacier structure and motion ever be settled! When reading Tyndall's paper it seemed to me that movement in the particles must come into play in his own doctrine of pressure; for he expressly states that if there be pressure on all sides, there is no lamination. I suppose I cannot have understood him, for I should have inferred from this that there must have been movement parallel to planes of pressure. (543/1. Prof. Tyndall had published papers "On Glaciers," and "On some Physical Properties of Ice" ("Proc. R. Inst." 1854-58) before the date of this letter. In 1856 he wrote a paper entitled "Observations on 'The Theory of the Origin of Slaty Cleavage,' by H.C. Sorby." "Phil. Mag." XII., 1856, page 129.)

Sorby read a paper to the Brit. Assoc., and he comes to the conclusion that gneiss, etc., may be metamorphosed cleavage or strata; and I think he admits much chemical segregation along the planes of division. (543/2. "On the Microscopical Structure of Mica-schist:" "Brit. Ass. Rep." 1856, page 78. See also Letters 540-542.) I quite subscribe to this view, and should have been sorry to have been so utterly wrong, as I should have been if foliation was identical with stratification.

I have been nowhere and seen no one, and really have no news of any kind to tell you. I have been working away as usual, floating plants in salt water inter alia, and confound them, they all sink pretty soon, but at very different rates. Working hard at pigeons, etc., etc. By the way, I have been astonished at the differences in the skeletons of domestic rabbits. I showed some of the points to Waterhouse, and asked him whether he could pretend that they were not as great as between species, and he answered, "They are a great deal more." How very odd that no zoologist should ever have thought it worth while to look to the real structure of varieties...

2.IX.VI. AGE OF THE WORLD, 1868-1877.

LETTER 544. TO J. CROLL. Down, September 19th, 1868.

I hope that you will allow me to thank you for sending me your papers in the "Phil. Magazine." (544/1. Croll published several papers in the "Philosophical Magazine" between 1864 and the date of this letter (1868).) I have never, I think, in my life been so deeply interested by any geological discussion. I now first begin to see what a million means, and I feel quite ashamed of myself at the silly way in which I have spoken of millions of years. I was formerly a great believer in the power of the sea in denudation, and this was perhaps natural, as most of my geological work was done near sea-coasts and on islands. But it is a consolation to me to reflect that as soon as I read Mr. Whitaker's paper (544/2. "On Subaerial Denudation," and "On Cliffs and Escarpments of the Chalk and Lower Tertiary Beds," "Geol. Mag." Volume IV., page 447, 1867.) on the escarpments of England, and Ramsay (544/3. "Quart. Journ. Geol. Soc." Volume XVIII., page 185, 1862. "On the Glacial Origin of certain Lakes in Switzerland, the Black Forest, Great Britain, Sweden, North America, and elsewhere.') and Jukes' papers (544/4. "Quart. Journ. Geol. Soc." Volume XVIII., page 378, 1862. "On the Mode of Formation of some River-Valleys in the South of Ireland."), I gave up in my own mind the case; but I never fully realised the truth until reading your papers just received. How often I have speculated in vain on the origin of the valleys in the chalk platform round this place, but now all is clear. I thank you cordially for having cleared so much mist from before my eyes.

LETTER 545. TO T. MELLARD READE. Down, February 9th, 1877.

I am much obliged for your kind note, and the present of your essay. I have read it with great interest, and the results are certainly most surprising. (545/1. Presidential Address delivered by T. Mellard Reade before the Liverpool Geological Society ("Proc. Liverpool Geol. Soc." Volume III., pt. iii., page 211, 1877). See also "Examination of a Calculation of the Age of the Earth, based upon the hypothesis of the Permanence of Oceans and Continents." "Geol. Mag." Volume X., page 309, 1883.) It appears to me almost monstrous that Professor Tait should say that the duration of the world has not exceeded ten million years. (545/2. "Lecture on Some Recent Advances in Physical Science," by P.G. Tait, London, 1876.) The argument which seems the most weighty in favour of the belief that no great number of millions of years have elapsed since the world was inhabited by living creatures is the rate at which the temperature of the crust increases, and I wish that I could see this argument answered.

LETTER 546. TO J. CROLL. Down, August 9th, 1877.

I am much obliged for your essay, which I have read with the greatest interest. With respect to the geological part, I have long wished to see the evidence collected on the time required for denudation, and you have done it admirably. (546/1. In a paper "On the Tidal Retardation Argument for the Age of the Earth" ("Brit. Assoc. Report," 1876, page 88), Croll reverts to the influence of subaerial denudation in altering the form of the earth as an objection to the argument from tidal retardation. He had previously dealt with this subject in "Climate and Time," Chapter XX., London, 1875.) I wish some one would in a like spirit compare the thickness of sedimentary rocks with the quickest estimated rate of deposition by a large river, and other such evidence. Your main argument with respect to the sun seems to me very striking.

My son George desires me to thank you for his copy, and to say how much he has been interested by it.

2.IX.VII. GEOLOGICAL ACTION OF EARTHWORMS, 1880-1882.

"My whole soul is absorbed with worms just at present." (From a letter to Sir W. Thistleton-Dyer, November 26th, 1880.)

LETTER 547. TO T.H. FARRER (Lord Farrer).

(547/1. The five following letters, written shortly before and after the publication of "The Formation of Vegetable Mould through the Action of Worms," 1881, deal with questions connected with Mr. Darwin's work on the habits and geological action of earthworms.)

Down, October 20th, 1880.

What a man you are to do thoroughly whatever you undertake to do! The supply of specimens has been magnificent, and I have worked at them for a day and a half. I find a very few well-rounded grains of brick in the castings from over the gravel walk, and plenty over the hole in the field, and over the Roman floor. (547/2. See "The Formation of Vegetable Mould," 1881, pages 178 et seq. The Roman remains formed part of a villa discovered at Abinger, Surrey. Excavations were carried out, under Lord Farrer's direction, in a field adjoining the ground in which the Roman villa was first found, and extended observations were made by Lord Farrer, which led Mr. Darwin to conclude that a large part of the fine vegetable mould covering the floor of the villa had been brought up from below by worms.) You have done me the greatest possible service by making me more cautious than I should otherwise have been—viz., by sending me the rubbish from the road itself; in this rubbish I find very many particles, rounded (I suppose) by having been crushed, angles knocked off, and somewhat rolled about. But not a few of the particles may have passed through the bodies of worms during the years since the road was laid down. I still think that the fragments are ground in the gizzards of worms, which always contain bits of stone; but I must try and get more evidence. I have to-day started a pot with worms in very fine soil, with sharp fragments of hard tiles laid on the surface, and hope to see in the course of time whether any of those become rounded. I do not think that more specimens from Abinger would aid me...

LETTER 548. TO G.J. ROMANES. Down, March 7th.

I was quite mistaken about the "Gardeners' Chronicle;" in my index there are only the few enclosed and quite insignificant references having any relation to the minds of animals. When I returned to my work, I found that I had nearly completed my statement of facts about worms plugging up their burrows with leaves (548/1. Chapter II., of "The Formation of Vegetable Mould through the Action of Worms," 1881, contains a discussion on the intelligence shown by worms in the manner of plugging up their burrows with leaves (pages 78 et seq.).), etc., etc., so I waited until I had naturally to draw up a few concluding remarks. I hope that it will not bore you to read the few accompanying pages, and in the middle you will find a few sentences with a sort of definition of, or rather discussion on, intelligence. I am altogether dissatisfied with it. I tried to observe what passed in my own mind when I did the work of a worm. If I come across a professed metaphysician, I will ask him to give me a more technical definition, with a few big words about the abstract, the concrete, the absolute, and the infinite; but seriously, I should be grateful for any suggestions, for it will hardly do to assume that every fool knows what "intelligent" means. (548/2. "Mr. Romanes, who has specially studied the minds of animals, believes that we can safely infer intelligence only when we see an individual profiting by its own experience...Now, if worms try to drag objects into their burrows, first in one way and then in another, until they at last succeed, they profit, at least in each particular instance, by experience" ("The Formation of Vegetable Mould," 1881, page 95).) You will understand that the MS. is only the first rough copy, and will need much correction. Please return it, for I have no other copy—only a few memoranda. When I think how it has bothered me to know what I mean by "intelligent," I am sorry for you in your great work on the minds of animals.

I daresay that I shall have to alter wholly the MS.

LETTER 549. TO FRANCIS GALTON. Down, March 8th {1881}.

Very many thanks for your note. I have been observing the {worm} tracks on my walks for several months, and they occur (or can be seen) only after heavy rain. As I know that worms which are going to die (generally from the parasitic larva of a fly) always come out of their burrows, I have looked out during these months, and have usually found in the morning only from one to three or four along the whole length of my walks. On the other hand, I remember having in former years seen scores or hundreds of dead worms after heavy rain. (549/1. "After heavy rain succeeding dry weather, an astonishing number of dead worms may sometimes be seen lying on the ground. Mr. Galton informs me that on one occasion (March, 1881), the dead worms averaged one for every two-and-a-half paces in length on a walk in Hyde Park, four paces in width" (loc. cit., page 14).) I cannot possibly believe that worms are drowned in the course of even three or four days' immersion; and I am inclined to conclude that the death of sickly (probably with parasites) worms is thus hastened. I will add a few words to what I have said about these tracks. Occasionally worms suffer from epidemics (of what nature I know not) and die by the million on the surface of the ground. Your ruby paper answers capitally, but I suspect that it is only for dimming the light, and I know not how to illuminate worms by the same intensity of light, and yet of a colour which permits the actinic rays to pass. I have tried drawing triangles of damp paper through a small cylindrical hole, as you suggested, and I can discover no source of error. (549/2. Triangles of paper were used in experiments to test the intelligence of worms (loc. cit., page 83).) Nevertheless, I am becoming more doubtful about the intelligence of worms. The worst job is that they will do their work in a slovenly manner when kept in pots (549/3. Loc. cit., page 75.), and I am beyond measure perplexed to judge how far such observations are trustworthy.

LETTER 550. TO E. RAY LANKESTER.

(550/1. Mr. Lankester had written October 11th, 1881, to thank Mr. Darwin for the present of the Earthworm book. He asks whether Darwin knows of "any experiments on the influence of sea-water on earthworms. I have assumed that it is fatal to them. But there is a littoral species (Pontodrilus of Perrier) found at Marseilles." Lankester adds, "It is a great pleasure and source of pride to me to see my drawing of the earthworm's alimentary canal figuring in your pages."

Down, October 13th {1881}.

I have been much pleased and interested by your note. I never actually tried sea-water, but I was very fond of angling when a boy, and as I could not bear to see the worms wriggling on the hook, I dipped them always first in salt water, and this killed them very quickly. I remember, though not very distinctly, seeing several earthworms dead on the beach close to where a little brook entered, and I assumed that they had been brought down by the brook, killed by the sea-water, and cast on shore. With your skill and great knowledge, I have no doubt that you will make out much new about the anatomy of worms, whenever you take up the subject again.

LETTER 551. TO J.H. GILBERT. Down, January, 12th, 1882.

I have been much interested by your letter, for which I thank you heartily. There was not the least cause for you to apologise for not having written sooner, for I attributed it to the right cause, i.e. your hands being full of work.

Your statement about the quantity of nitrogen in the collected castings is most curious, and much exceeds what I should have expected. In lately reading one of your and Mr. Lawes' great papers in the "Philosophical Transactions" (551/1. The first Report on "Agricultural, Botanical, and Chemical Results of Experiments on the Mixed Herbage of Permanent Grassland, conducted for many years in succession on the same land," was published in the "Philosophical Transactions of the Royal Society" in 1880, the second paper appeared in the "Phil. Trans." for 1882, and the third in the "Phil. Trans." of 1900, Volume 192, page 139.) (the value and importance of which cannot, in my opinion, be exaggerated) I was struck with the similarity of your soil with that near here; and anything observed here would apply to your land. Unfortunately I have never made deep sections in this neighbourhood, so as to see how deep the worms burrow, except in one spot, and here there had been left on the surface of the chalk a little very fine ferruginous sand, probably of Tertiary age; into this the worms had burrowed to a depth of 55 and 61 inches. I have never seen here red castings on the surface, but it seems possible (from what I have observed with reddish sand) that much of the red colour of the underlying clay would be discharged in passing through the intestinal canal.

Worms usually work near the surface, but I have noticed that at certain seasons pale-coloured earth is brought up from beneath the outlying blackish mould on my lawn; but from what depth I cannot say. That some must be brought up from a depth of four or five or six feet is certain, as the worms retire to this depth during very dry and very cold weather. As worms devour greedily raw flesh and dead worms, they could devour dead larvae, eggs, etc., etc., in the soil, and thus they might locally add to the amount of nitrogen in the soil, though not of course if the whole country is considered. I saw in your paper something about the difference in the amount of nitrogen at different depths in the superficial mould, and here worms may have played a part. I wish that the problem had been before me when observing, as possibly I might have thrown some little light on it, which would have pleased me greatly.

2.IX.VIII. MISCELLANEOUS, 1846-1878.

(552/1. The following four letters refer to questions connected with the origin of coal.)

LETTER 552. TO J.D. HOOKER. Down, May {1846}.

I am delighted that you are in the field, geologising or palaeontologising. I beg you to read the two Rogers' account of the Coal-fields of N. America; in my opinion they are eminently instructive and suggestive. (552/1. "On the Physical Structure of the Appalachian Chain," by W.B. and H.D. Rogers. Boston, 1843. See also "Geology of Pennsylvania," by H.D. Rogers. 4 volumes. London and Philadelphia, 1843.) I can lend you their resume of their own labours, and, indeed, I do not know that their work is yet published in full. L. Horner gives a capital balance of difficulties on the Coal-theory in his last Anniversary Address, which, if you have not read, will, I think, interest you. (552/2. "Quart. Journ. Geol. Soc." Volume II., 1846, page 170.) In a paper just read an author (552/3. "On the Remarkable Fossil Trees lately discovered near St. Helen's." By E.W. Binney. "Phil. Mag." Volume XXIV., page 165, 1844. On page 173 the author writes: "The Stigmaria or Sigillaria, whichever name is to be retained... was a tree that undoubtedly grew in water.") throws out the idea that the Sigillaria was an aquatic plant (552/4. See "Life and Letters," I., pages 356 et seq.)—I suppose a Cycad-Conifer with the habits of the mangrove. From simple geological reasoning I have for some time been led to suspect that the great (and great and difficult it is) problem of the Coal would be solved on the theory of the upright plants having been aquatic. But even on such, I presume improbable notion, there are, as it strikes me, immense difficulties, and none greater than the width of the coal-fields. On what kind of coast or land could the plants have lived? It is a grand problem, and I trust you will grapple with it. I shall like much to have some discussion with you. When will you come here again? I am very sorry to infer from your letter that your sister has been ill.

LETTER 553. TO J.D. HOOKER. {June 2nd, 1847.}

I received your letter the other day, full of curious facts, almost all new to me, on the coal-question. (553/1. Sir Joseph Hooker deals with the formation of coal in his classical paper "On the Vegetation of the Carboniferous Period, as compared with that of the Present Day." "Mem. Geol. Surv. Great Britain," Volume II., pt. ii., 1848.) I will bring your note to Oxford (553/2. The British Association met at Oxford in 1847.), and then we will talk it over. I feel pretty sure that some of your purely geological difficulties are easily solvable, and I can, I think, throw a very little light on the shell difficulty. Pray put no stress in your mind about the alternate, neatly divided, strata of sandstone and shale, etc. I feel the same sort of interest in the coal question as a man does watching two good players at play, he knowing little or nothing of the game. I confess your last letter (and this you will think very strange) has almost raised Binney's notion (an old, growing hobby-horse of mine) to the dignity of an hypothesis (553/3. Binney suggested that the Coal-plants grew in salt water. (See Letters 102, 552.) Recent investigations have shown that several of the plants of the Coal period possessed certain anatomical peculiarities, which indicate xerophytic characteristics, and lend support to the view that some at least of the plants grew in seashore swamps.), though very far yet below the promotion of being properly called a theory.

I will bring the remainder of my species-sketch to Oxford to go over your remarks. I have lately been getting a good many rich facts. I saw the poor old Dean of Manchester (553/4. Dean Herbert.) on Friday, and he received me very kindly. He looked dreadfully ill, and about an hour afterwards died! I am most sincerely sorry for it.

LETTER 554. TO J.D. HOOKER. {May 12th, 1847.}

I cannot resist thanking you for your most kind note. Pray do not think that I was annoyed by your letter. I perceived that you had been thinking with animation, and accordingly expressed yourself strongly, and so I understood it. Forefend me from a man who weighs every expression with Scotch prudence. I heartily wish you all success in your noble problem, and I shall be very curious to have some talk with you and hear your ultimatum. (554/1. The above paragraph was published in "Life and Letters," I., page 359.) I do really think, after Binney's pamphlet (554/2. "On the Origin of Coal," "Mem. Lit. Phil. Soc." Manchester Volume VIII., page 148, 1848.), it will be worth your while to array your facts and ideas against an aquatic origin of the coal, though I do not know whether you object to freshwater. I am sure I have read somewhere of the cones of Lepidodendron being found round the stump of a tree, or am I confusing something else? How interesting all rooted—better, it seems from what you say, than upright—specimens become.

I wish Ehrenberg would undertake a microscopical hunt for infusoria in the underclay and shales; it might reveal something. Would a comparison of the ashes of terrestrial peat and coal give any clue? (554/3. In an article by M. F. Rigaud on "La Formation de la Houille," published in the "Revue Scientifique," Volume II., page 385, 1894, the author lays stress on the absence of certain elements in the ash of coals, which ought to be present, on the assumption that the carbon has been derived from plant tissues. If coal consists of altered vegetable debris, we ought to find a certain amount of alkalies and phosphoric acid in its ash. Had such substances ever been present, it is difficult to understand how they could all have been removed by the solvent action of water. (Rigaud's views are given at greater length in an article on the "Structure and Formation of Coal," "Science Progress," Volume II., pages 355 and 431, 1895.)) Peat ashes are good manure, and coal ashes, except mechanically, I believe are of little use. Does this indicate that the soluble salts have been washed out? i.e., if they are NOT present. I go up to Geological Council to-day—so farewell.

(554/4. In a letter to Sir Joseph Hooker, October 6th, 1847, Mr. Darwin, in referring to the origin of Coal, wrote: "...I sometimes think it could not have been formed at all. Old Sir Anthony Carlisle once said to me gravely that he supposed Megatherium and such cattle were just sent down from heaven to see whether the earth would support them, and I suppose the coal was rained down to puzzle mortals. You must work the coal well in India.")

LETTER 555. TO J.D. HOOKER. Down, May 22nd, 1860.

Lyell tells me that Binney has published in Proceedings of Manchester Society a paper trying to show that Coal plants must have grown in very marine marshes. (555/1. "On the Origin of Coal," by E.W. Binney, "Mem. Lit. Phil. Soc. Manchester," Volume VIII., 1848, page 148. Binney examines the evidence on which dry land has been inferred to exist during the formation of the Coal Measures, and comes to the conclusion that the land was covered by water, confirming Brongniart's opinion that Sigillaria was an aquatic plant. He believes the Sigillaria "grew in water, on the deposits where it is now discovered, and that it is the plant which in a great measure contributed to the formation of our valuable beds of coal." (Loc. cit., page 193.)) Do you remember how savage you were long years ago at my broaching such a conjecture?

LETTER 556. TO L. HORNER. Down {1846?}.

I am truly pleased at your approval of my book (556/1. "Geological Observations on South America," London, 1846.): it was very kind of you taking the trouble to tell me so. I long hesitated whether I would publish it or not, and now that I have done so at a good cost of trouble, it is indeed highly satisfactory to think that my labour has not been quite thrown away.

I entirely acquiesce in your criticism on my calling the Pampean formation "recent" (556/2. "We must, therefore, conclude that the Pampean formation belongs, in the ordinary geological sense of the word, to the Recent Period." ("Geol. Obs." page 101).); Pleistocene would have been far better. I object, however, altogether on principle (whether I have always followed my principle is another question) to designate any epoch after man. It breaks through all principles of classification to take one mammifer as an epoch. And this is presupposing we know something of the introduction of man: how few years ago all beds earlier than the Pleistocene were characterised as being before the monkey epoch. It appears to me that it may often be convenient to speak of an Historical or Human deposit in the same way as we speak of an Elephant bed, but that to apply it to an epoch is unsound.

I have expressed myself very ill, and I am not very sure that my notions are very clear on this subject, except that I know that I have often been made wroth (even by Lyell) at the confidence with which people speak of the introduction of man, as if they had seen him walk on the stage, and as if, in a geological chronological sense, it was more important than the entry of any other mammifer.

You ask me to do a most puzzling thing, to point out what is newest in my volume, and I found myself incapable of doing almost the same for Lyell. My mind goes from point to point without deciding: what has interested oneself or given most trouble is, perhaps quite falsely, thought newest. The elevation of the land is perhaps more carefully treated than any other subject, but it cannot, of course, be called new. I have made out a sort of index, which will not take you a couple of minutes to skim over, and then you will perhaps judge what seems newest. The summary at the end of the book would also serve same purpose.

I do not know where E. de B. {Elie de Beaumont} has lately put forth on the recent elevation of the Cordillera. He "rapported" favourably on d'Orbigny, who in late times fires off a most Royal salute; every volcano bursting forth in the Andes at the same time with their elevation, the debacle thus caused depositing all the Pampean mud and all the Patagonian shingle! Is not this making Geology nice and simple for beginners?

We have been very sorry to hear of Bunbury's severe illness; I believe the measles are often dangerous to grown-up people. I am very glad that your last account was so much better.

I am astonished that you should have had the courage to go right through my book. It is quite obvious that most geologists find it far easier to write than to read a book.

Chapter I. and II.—Elevation of the land: equability on E. coast as shown by terraces, page 19; length on W. coast, page 53; height at Valparaiso, page 32; number of periods of rest at Coquimbo, page 49; elevation within Human period near Lima greater than elsewhere observed; the discussion (page 41) on non-horizontality of terraces perhaps one of newest features—on formation of terraces rather newish.

Chapter III., page 65.—Argument of horizontal elevation of Cordillera I believe new. I think the connection (page 54) between earthquake {shocks} and insensible rising important.

Chapter IV.—The strangeness of the (Eocene) mammifers, co-existing with recent shells.

Chapter V.—Curious pumiceous infusorial mudstone (page 118) of Patagonia; climate of old Tertiary period, page 134. The subject which has been most fertile in my mind is the discussion from page 135 to end of chapter on the accumulation of fossiliferous deposits. (556/3. The last section of Chapter V. treats of "the Absence of extensive modern Conchiferous Deposits in South America; and on the contemporaneousness of the older Tertiary Deposits at distant points being due to contemporaneous movements of subsidence." Darwin expresses the view that "the earth's surface oscillates up and down; and...during the elevatory movements there is but a small chance of durable fossiliferous deposits accumulating" (loc. cit., page 139).)

Chapter VI.—Perhaps some facts on metamorphism, but chiefly on the layers in mica-slate, etc., being analogous to cleavage.

Chapter VII.—The grand up-and-down movements (and vertical silicified trees) in the Cordillera: see summary, page 204 and page 240. Origin of the Claystone porphyry formation, page 170.

Chapter VIII., page 224.—Mixture of Cretaceous and Oolitic forms (page 226)—great subsidence. I think (page 232) there is some novelty in discussion on axes of eruption and injection. (page 247) Continuous volcanic action in the Cordillera. I think the concluding summary (page 237) would show what are the most salient features in the book.

LETTER 557. TO C. LYELL. Shrewsbury {August 10th, 1846}.

I was delighted to receive your letter, which was forwarded here to me. I am very glad to hear about the new edition of the "Principles," (557/1. The seventh edition of the "Principles of Geology" was published in 1847.), and I most heartily hope you may live to bring out half a dozen more editions. There would not have been such books as d'Orbigny's S. American Geology (557/2. "Voyage dans l'Amerique meridionale execute pendant les Annees 1826-37." 6 volumes, Paris, 1835-43.) published, if there had been seven editions of the "Principles" distributed in France. I am rather sorry about the small type; but the first edition, my old true love, which I never deserted for the later editions, was also in small type. I much fear I shall not be able to give any assistance to Book III. (557/3. This refers to Book III. of the "Principles"—"Changes of the Organic World now in Progress.") I think I formerly gave my few criticisms, but I will read it over again very soon (though I am striving to finish my S. American Geology (557/4. "Geological Observations on South America" was published in 1846.)) and see whether I can give you any references. I have been thinking over the subject, and can remember no one book of consequence, as all my materials (which are in an absolute chaos on separate bits of paper) have been picked out of books not directly treating of the subjects you have discussed, and which I hope some day to attempt; thus Hooker's "Antarctic Flora" I have found eminently useful (557/5. "Botany of the Antarctic Voyage of H.M.S. 'Erebus' and 'Terror' in the Years 1839-43." I., "Flora Antarctica." 2 volumes, London, 1844-47.), and yet I declare I do not know what precise facts I could refer you to. Bronn's "Geschichte" (557/6. "Naturgeschichte der drei Reiche." H.E. Bronn, Stuttgart, 1834-49.) which you once borrowed) is the only systematic book I have met with on such subjects; and there are no general views in such parts as I have read, but an immense accumulation of references, very useful to follow up, but not credible in themselves: thus he gives hybrids from ducks and fowls just as readily as between fowls and pheasants! You can have it again if you like. I have no doubt Forbes' essay, which is, I suppose, now fairly out, will be very good under geographical head. (557/7. "On the Connection between the Distribution of the existing Fauna and Flora of the British Isles, and the Geological Changes which have affected their Area, especially during the Epoch of the Northern Drift," by E. Forbes. "Memoirs of Geological Survey," Volume I., page 336, 1846.) Kolreuter's German book is excellent on hybrids, but it will cost you a good deal of time to work out any conclusion from his numerous details. (557/8. Joseph Gottlieb Kolreuter's "Vorlaufige Nachricht von eininigen das Geschlecht der Pflanzen betreffenden Versuchen und Beobachtungen." Leipzig, 1761.) With respect to variation I have found nothing—but minute details scattered over scores of volumes. But I will look over Book III. again. What a quantity of work you have in hand! I almost wish you could have finished America, and thus have allowed yourself rather more time for the old "Principles"; and I am quite surprised that you could possibly have worked your own new matter in within six weeks. Your intention of being in Southampton will much strengthen mine, and I shall be very glad to hear some of your American Geology news.

LETTER 558. TO L. HORNER. Down, Sunday {January 1847}.

Your most agreeable praise of my book is enough to turn my head; I am really surprised at it, but shall swallow it with very much gusto... (558/1. "Geological Observations in S. America," London, 1846.)

E. de Beaumont measured the inclination with a sextant and artificial horizon, just as you take the height of the sun for latitude.

With respect to my Journal, I think the sketches in the second edition (558/2. "Journal of Researches into the Natural History and Geology of the Countries visited during the Voyage of H.M.S. 'Beagle.'" Edition II. London, 1845.) are pretty accurate; but in the first they are not so, for I foolishly trusted to my memory, and was much annoyed to find how hasty and inaccurate many of my remarks were, when I went over my huge pile of descriptions of each locality.

If ever you meet anyone circumstanced as I was, advise him not, on any account, to give any sketches until his materials are fully worked out.

What labour you must be undergoing now; I have wondered at your patience in having written to me two such long notes. How glad Mrs. Horner will be when your address is completed. (558/3. Anniversary Address of the President ("Quart. Journ. Geol. Soc." Volume III., page xxii, 1847).) I must say that I am much pleased that you will notice my volume in your address, for former Presidents took no notice of my two former volumes.

I am exceedingly glad that Bunbury is going on well.

LETTER 559. TO C. LYELL. Down, July 3rd {1849}.

I don't know when I have read a book so interesting (559/1. "A Second Visit to the United States of North America." 2 volumes, London, 1849.); some of your stories are very rich. You ought to be made Minister of Public Education—not but what I should think even that beneath the author of the old "Principles." Your book must, I should think, do a great deal of good and set people thinking. I quite agree with the "Athenaeum" that you have shown how a man of science can bring his powers of observation to social subjects. (559/2. "Sir Charles Lyell, besides the feelings of a gentleman, seems to carry with him the best habits of scientific observation into other strata than those of clay, into other 'formations' than those of rock or river-margin." "The Athenaeum," June 23rd, 1849, page 640.) You have made H. Wedgwood, heart and soul, an American; he wishes the States would annex us, and was all day marvelling how anyone who could pay his passage money was so foolish as to remain here.

LETTER 560. TO C. LYELL. Down, {December, 1849}.

(560/1. In this letter Darwin criticises Dana's statements in his volume on "Geology," forming Volume X. of the "Wilkes Exploring Expedition," 1849.)

...Dana is dreadfully hypothetical in many parts, and often as "d—d cocked sure" as Macaulay. He writes however so lucidly that he is very persuasive. I am more struck with his remarks on denudation than you seem to be. I came to exactly the same conclusion in Tahiti, that the wonderful valleys there (on the opposite extreme of the scale of wonder {to} the valleys of New South Wales) were formed exclusively by fresh water. He underrates the power of sea, no doubt, but read his remarks on valleys in the Sandwich group. I came to the conclusion in S. America (page 67) that the main effect of fresh water is to deepen valleys, and sea to widen them; I now rather doubt whether in a valley or fiord...the sea would deepen the rock at its head during the elevation of the land. I should like to tour on the W. coast of Scotland, and attend to this. I forget how far generally the shores of fiords (not straits) are cliff-formed. It is a most interesting subject.

I return once again to Coral. I find he does not differ so much in detail with me regarding areas of subsidence; his map is coloured on some quite unintelligible principle, and he deduces subsidence from the vaguest grounds, such as that the N. Marianne Islands must have subsided because they are small, though long in volcanic action: and that the Marquesas subsided because they are penetrated by deep bays, etc., etc. I utterly disbelieve his statements that most of the atolls have been lately raised a foot or two. He does not condescend to notice my explanation for such appearances. He misrepresents me also when he states that I deduce, without restriction, elevation from all fringing reefs, and even from islands without any reefs! If his facts are true, it is very curious that the atolls decrease in size in approaching the vast open ocean S. of the Sandwich Islands. Dana puts me in a passion several times by disputing my conclusions without condescending to allude to my reasons; thus, regarding S. Lorenzo elevation, he is pleased to speak of my "characteristic accuracy" (560/2. Dana's "Geology" (Wilkes expedition), page 590.), and then gives difficulties (as if his own) when they are stated by me, and I believe explained by me—whereas he only alludes to a few of the facts. So in Australian valleys, he does not allude to my several reasons. But I am forgetting myself and running on about what can only interest myself. He strikes me as a very clever fellow; I wish he was not quite so grand a generaliser. I see little of interest except on volcanic action and denudation, and here and there scattered remarks; some of the later chapters are very bald.

LETTER 561. TO J.D. DANA. Down, December 5th, 1849.

I have not for some years been so much pleased as I have just been by reading your most able discussion on coral reefs. I thank you most sincerely for the very honourable mention you make of me. (561/1. "United States Exploring Expedition during the Years 1839-42 under the Command of Charles Wilkes, U.S.N." Volume X., "Geology," by J.D. Dana, 1849.) This day I heard that the atlas has arrived, and this completes your munificent present to me. I have not yet come to the chapter on subsidence, and in that I fancy we shall disagree, but in the descriptive part our agreement has been eminently satisfactory to me, and far more than I ever ventured to anticipate. I consider that now the subsidence theory is established. I have read about half through the descriptive part of the "Volcanic Geology" (561/2. Part of Dana's "Geology" is devoted to volcanic action.) (last night I ascended the peaks of Tahiti with you, and what I saw in my short excursion was most vividly brought before me by your descriptions), and have been most deeply interested by it. Your observations on the Sandwich craters strike me as the most important and original of any that I have read for a long time. Now that I have read yours, I believe I saw at the Galapagos, at a distance, instances of those most curious fissures of eruption. There are many points of resemblance between the Galapagos and Sandwich Islands (even to the shape of the mound-like hills)—viz., in the liquidity of the lavas, absence of scoriae, and tuff-craters. Many of your scattered remarks on denudation have particularly interested me; but I see that you attribute less to sea and more to running water than I have been accustomed to do. After your remarks in your last very kind letter I could not help skipping on to the Australian valleys (561/3. Ibid., pages 526 et seq.: "The Formation of Valleys, etc., in New South Wales."), on which your remarks strike me as exceedingly ingenious and novel, but they have not converted me. I cannot conceive how the great lateral bays could have been scooped out, and their sides rendered precipitous by running water. I shall go on and read every word of your excellent volume.

If you look over my "Geological Instructions" you will be amused to see that I urge attention to several points which you have elaborately discussed. (561/4. "A Manual of Scientific Enquiry, prepared for the use of Her Majesty's Navy, and adapted for Travellers in General." Edited by Sir John F.W. Herschel, Bart. London, 1849 (Section VI., "Geology." By Charles Darwin).) I lately read a paper of yours on Chambers' book, and was interested by it. I really believe the facts of the order described by Chambers, in S. America, which I have described in my Geolog. volume. This leads me to ask you (as I cannot doubt that you will have much geological weight in N. America) to look to a discussion at page 135 in that volume on the importance of subsidence to the formation of deposits, which are to last to a distant age. This view strikes me as of some importance.

When I meet a very good-natured man I have that degree of badness of disposition in me that I always endeavour to take advantage of him; therefore I am going to mention some desiderata, which if you can supply I shall be very grateful, but if not no answer will be required.

Thank you for your "Conspectus Crust.," but I am sorry to say I am not worthy of it, though I have always thought the Crustacea a beautiful subject. (561/5. "Conspectus Crustaceorum in orbis terrarum circumnavigatione, C. Wilkes duce, collectorum." Cambridge (U.S.A.), 1847.)

LETTER 562. TO C. LYELL. {Down, March 9th, 1850.}

I am uncommonly much obliged to you for your address, which I had not expected to see so soon, and which I have read with great interest. (562/1. Anniversary Address of the President, "Quart. Journ. Geol. Soc." Volume VI., page 32, 1850.) I do not know whether you spent much time over it, but it strikes me as extra well arranged and written—done in the most artistic manner, to use an expression which I particularly hate. Though I am necessarily pretty well familiar with your ideas from your conversation and books, yet the whole had an original freshness to me. I am glad that you broke through the routine of the President's addresses, but I should be sorry if others did. Your criticisms on Murchison were to me, and I think would be to many, particularly acceptable. (562/2. In a paper "On the Geological Structure of the Alps, etc." ("Quart. Journ. Geol. Soc." Volume V., page 157, 1849) Murchison expressed his belief that the apparent inversion of certain Tertiary strata along the flanks of the Alps afforded "a clear demonstration of a sudden operation or catastrophe." It is this view of paroxysmal energy that Lyell criticises in the address.) Capital, that metaphor of the clock. (562/3. "In a word, the movement of the inorganic world is obvious and palpable, and might be likened to the minute-hand of a clock, the progress of which can be seen and heard, whereas the fluctuations of the living creation are nearly invisible, and resemble the motion of the hour-hand of a timepiece" (loc. cit., page xlvi).) I shall next February be much interested by seeing your hour-hand of the organic world going.

Many thanks for your kindness in taking the trouble to tell me of the anniversary dinner. What a compliment that was which Lord Mahon paid me! I never had so great a one. He must be as charming a man as his wife is a woman, though I was formerly blind to his merit. Bunsen's speech must have been very interesting and very useful, if any orthodox clergyman were present. Your metaphor of the pebbles of pre-existing languages reminds me that I heard Sir J. Herschel at the Cape say how he wished some one would treat language as you had Geology, and study the existing causes of change, and apply the deduction to old languages.

We are all pretty flourishing here, though I have been retrograding a little, and I think I stand excitement and fatigue hardly better than in old days, and this keeps me from coming to London. My cirripedial task is an eternal one; I make no perceptible progress. I am sure that they belong to the hour-hand, and I groan under my task.

LETTER 563. C. LYELL TO CHARLES DARWIN. April 23rd, 1855.

I have seen a good deal of French geologists and palaeontologists lately, and there are many whom I should like to put on the R.S. Foreign List, such as D'Archiac, Prevost, and others. But the man who has made the greatest sacrifices and produced the greatest results, who has, in fact, added a new period to the calendar, is Barrande.

The importance of his discoveries as they stand before the public fully justify your choice of him; but what is unpublished, and which I have seen, is, if possible, still more surprising. Thirty genera of gasteropods (150 species) and 150 species of lamellibranchiate bivalves in the Silurian! All obtained by quarries opened solely by him for fossils. A man of very moderate fortune spending nearly all his capital on geology, and with success.

E. Forbes' polarity doctrines are nearly overturned by the unpublished discoveries of Barrande. (563/1. See note, Letter 41, Volume I.)

I have called Barrande's new period Cambrian (see "Manual," 5th edition), and you will see why. I could not name it Protozoic, but had Barrande called it Bohemian, I must have adopted that name. All the French will rejoice if you confer an honour on Barrande. Dana is well worthy of being a foreign member.

Should you succeed in making Barrande F.R.S., send me word.

LETTER 564. TO J.D. HOOKER. June 5th {1857}.

(564/1. The following, which bears on the subject of medals, forms part of the long letter printed in the "Life and Letters," II., page 100.)

I do not quite agree with your estimate of Richardson's merits. Do, I beg you (whenever you quietly see), talk with Lyell on Prestwich: if he agrees with Hopkins, I am silenced; but as yet I must look at the correlation of the Tertiaries as one of the highest and most frightfully difficult tasks a man could set himself, and excellent work, as I believe, P. has done. (564/2. Prof. Prestwich had published numerous papers dealing with Tertiary Geology before 1857. The contributions referred to are probably those "On the Correlation of the Lower Tertiaries of England with those of France and Belgium," "Quart. Journ. Geol. Soc." Volume X., 1854, page 454; and "On the Correlation of the Middle Eocene Tertiaries of England, France, and Belgium," ibid., XII., 1856, page 390.) I confess I do not value Hopkins' opinion on such a point. I confess I have never thought, as you show ought to be done, on the future. I quite agree, under all circumstances, with the propriety of Lindley. How strange no new geologists are coming forward! Are there not lots of good young chemists and astronomers or physicists? Fitton is the only old geologist left who has done good work, except Sedgwick. Have you thought of him? He would be a brilliant companion for Lindley. Only it would never do to give Lyell a Copley and Sedgwick a Royal in the same year. It seems wrong that there should be three Natural Science medals in the same year. Lindley, Sedgwick, and Bunsen sounds well, and Lyell next year for the Copley. (564/3. In 1857 a Royal medal was awarded to John Lindley; Lyell received the Copley in 1858, and Bunsen in 1860.) You will see that I am speculating as a mere idle amateur.

LETTER 565. TO S.P. WOODWARD. Down, May 27th {1856}.

I am very much obliged to you for having taken the trouble to answer my query so fully. I can now be at rest, for from what you say and from what little I remember Forbes said, my point is unanswerable. The case of Terebratula is to the point as far as it goes, and is negative. I have already attempted to get a solution through geographical distribution by Dr. Hooker's means, and he finds that the same genera which have very variable species in

Europe have other very variable species elsewhere. This seems the general rule, but with some few exceptions. I see from the several reasons which you assign, that there is no hope of comparing the same genus at two different periods, and seeing whether the tendency to vary is greater at one period in such genus than at another period. The variability of certain genera or groups of species strikes me as a very odd fact. (565/1. The late Dr. Neumayr has dealt, to some extent, with this subject in "Die Stamme des Thierreichs," Volume I., Wien, 1889.)

I shall have no points, as far as I can remember, to suggest for your reconsideration, but only some on which I shall have to beg for a little further information. However, I feel inclined very much to dispute your doctrine of islands being generally ancient in comparison, I presume, with continents. I imagine you think that islands are generally remnants of old continents, a doctrine which I feel strongly disposed to doubt. I believe them generally rising points; you, it seems, think them sinking points.

LETTER 566. TO T.H. HUXLEY. Down, April 14th {1860}.

Many thanks for your kind and pleasant letter. I have been much interested by "Deep-sea Soundings,", and will return it by this post, or as soon as I have copied a few sentences. (566/1. Specimens of the mud dredged by H.M.S. "Cyclops" were sent to Huxley for examination, who gave a brief account of them in Appendix A of Capt. Dayman's Report, 1858, under the title "Deep-sea Soundings in the North Atlantic.") I think you said that some one was investigating the soundings. I earnestly hope that you will ask the some one to carefully observe whether any considerable number of the calcareous organisms are more or less friable, or corroded, or scaling; so that one might form some crude notion whether the deposition is so rapid that the foraminifera are preserved from decay and thus are forming strata at this profound depth. This is a subject which seems to me to have been much neglected in examining soundings.

Bronn has sent me two copies of his Morphologische Studien uber die Gestaltungsgesetze." (H.G. Bronn, "Morphologische Studien uber die Gestaltungsgesetze der Naturkorper uberhaupt und der organischen insbesondere": Leipzig, 1858.) It looks elementary. If you will write you shall have the copy; if not I will give it to the Linnean Library.

I quite agree with the letter from Lyell that your extinguished theologians lying about the cradle of each new science, etc., etc., is splendid. (566/2. "Darwiniana, Collected Essays," Volume II., page 52.)

LETTER 567. TO T.H. HUXLEY. May 10th {1862 or later}.

I have been in London, which has prevented my writing sooner. I am very sorry to hear that you have been ill: if influenza, I can believe in any degree of prostration of strength; if from over-work, for God's sake do not be rash and foolish. You ask for criticisms; I have none to give, only impressions. I fully agree with your "skimming-of-pot theory," and very well you have put it. With respect {to} contemporaneity I nearly agree with you, and if you will look to the d—d book, 3rd edition, page 349 you will find nearly similar remarks. (567/1. "When the marine forms are spoken of as having changed simultaneously throughout the world, it must not be supposed that this expression relates to the same year, or to the same century, or even that it has a very strict geological sense; for if all the marine animals now living in Europe, and all those that lived in Europe during the Pleistocene period (a very remote period as measured by years, including the whole Glacial epoch), were compared with those now existing in South America or in Australia, the most skilful naturalist would hardly be able to say whether the present or the Pleistocene inhabitants of Europe resembled most closely those of the Southern hemisphere." "Origin," Edition VI., page 298. The passage in Edition III., page 350, is substantially the same.) But at page 22 of your Address, in my opinion you put your ideas too far. (567/2. Anniversary Address to the Geological Society of London ("Quart. Journ. Geol. Soc." Volume XVIII., page xl, 1862). As an illustration of the misleading use of the term "contemporaneous" as employed by geologists, Huxley gives the following illustration: "Now suppose that, a million or two of years hence, when Britain has made another dip beneath the sea and has come up again, some geologist applies this doctrine {i.e., the doctrine of the Contemporaneity of the European and of the North American Silurians: proof of contemporaneity is considered to be established by the occurrence of 60 per cent. of species in common}, in comparing the strata laid bare by the upheaval of the bottom, say, of St. George's Channel with what may then remain of the Suffolk Crag. Reasoning in the same way, he will at once decide the Suffolk Crag and the St. George's Channel beds to be contemporaneous; although we happen to know that a vast period...of time...separates the two" (loc. cit., page xlv). This address is republished in the "Collected Essays," Volume VIII.; the above passage is at page 284.) I cannot think that future geologists would rank the Suffolk and St. George's strata as contemporaneous, but as successive sub-stages; they rank N. America and British stages as contemporaneous, notwithstanding a percentage of different species (which they, I presume, would account for by geographical difference) owing to the parallel succession of the forms in both countries. For terrestrial productions I grant that great errors may creep in (567/3. Darwin supposes that terrestrial productions have probably not changed to the same extent as marine organisms. "If the Megatherium, Mylodon...had been brought to Europe from La Plata, without any information in regard to their geological position, no one would have suspected that they had co-existed with sea shells all still living" ("Origin," Edition VI., page 298).); but I should require strong evidence before believing that, in countries at all well-known, so-called Silurian, Devonian, and Carboniferous strata could be contemporaneous. You seem to me on the third point, viz., on non-advancement of organisation, to have made a very strong case. I have not knowledge or presumption enough to criticise what you say. I have said what I could at page 363 of "Origin." It seems to me that the whole case may be looked at from several points of view. I can add only one miserable little special case of advancement in cirripedes. The suspicion crosses me that if you endeavoured your best you would say more on the other side. Do you know well Bronn in his last Entwickelung (or some such word) on this subject? it seemed to me very well done. (567/4. Probably "Untersuchungen uber die Entwickelungsgesetze der organischen Welt wahrend der Bildungszeit unserer Erdoberflache," Stuttgart, 1858. Translated by W.S. Dallas in the "Ann. and Mag. Nat. Hist." Volume IV., page 81.) I hope before you publish again you will read him again, to consider the case as if you were a judge in a court of appeal; it is a very important subject. I can say nothing against your side, but I have an "inner consciousness" (a highly philosophical style of arguing!) that something could be said against you; for I cannot help hoping that you are not quite as right as you seem to be. Finally, I cannot tell why, but when I finished your Address I felt convinced that many would infer that you were dead against change of species, but I clearly saw that you were not. I am not very well, so good-night, and excuse this horrid letter.

LETTER 568. TO J.D. HOOKER. Down, June 30th {1866}.

I have heard from Sulivan (who, poor fellow, gives a very bad account of his own health) about the fossils (568/1. In a letter to Huxley (June 4th, 1866) Darwin wrote: "Admiral Sulivan several years ago discovered an astonishingly rich accumulation of fossil bones not far from the Straits {of Magellan}...During many years it has seemed to me extremely desirable that these should be collected; and here is an excellent opportunity.")... The place is Gallegos, on the S. coast of Patagonia. Sulivan says that in the course of two or three days all the boats in the ship could be filled twice over; but to get good specimens out of the hardish rock two or three weeks would be requisite. It would be a grand haul for Palaeontology. I have been thinking over your lecture. (568/2. A lecture on "Insular Floras" given at the British Association meeting at Nottingham, August 27th, 1866, published in the "Gard. Chron." 1867.) Will it not be possible to give enlarged drawings of some leading forms of trees? You will, of course, have a large map, and George tells me that he saw at Sir H. James', at Southampton, a map of the world on a new principle, as seen from within, so that almost 4/5ths of the globe was shown at once on a large scale. Would it not be worth while to borrow one of these from Sir H. James as a curiosity to hang up?

Remember you are to come here before Nottingham. I have almost finished the last number of H. Spencer, and am astonished at its prodigality of original thought. But the reflection constantly recurred to me that each suggestion, to be of real value to science, would require years of work. It is also very unsatisfactory, the impossibility of conjecturing where direct action of external circumstances begins and ends—as he candidly owns in discussing the production of woody tissue in the trunks of trees on the one hand, and on the other in spines and the shells of nuts. I shall like to hear what you think of this number when we meet.

LETTER 569. TO A. GAUDRY. Down, November 17th, 1868.

On my return home after a short absence I found your note of Nov. 9th, and your magnificent work on the fossil animals of Attica. (569/1. The "Geologie de l'Attique," 2 volumes 4to, 1862-7, is the only work of Gaudry's of this date in Mr. Darwin's library.) I assure you that I feel very grateful for your generosity, and for the honour which you have thus conferred on me. I know well, from what I have already read of extracts, that I shall find your work a perfect mine of wealth. One long passage which Sir C. Lyell quotes from you in the 10th and last edition of the "Principles of Geology" is one of the most striking which I have ever read on the affiliation of species. (569/2. The quotation in Lyell's "Principles," Edition X., Volume II., page 484, is from M. Gaudry's "Animaux Fossiles de Pikermi," 1866, page 34:—

"In how different a light does the question of the nature of species now present itself to us from that in which it appeared only twenty years ago, before we had studied the fossil remains of Greece and the allied forms of other countries. How clearly do these fossil relics point to the idea that species, genera, families, and orders now so distinct have had common ancestors. The more we advance and fill up the gaps, the more we feel persuaded that the remaining voids exist rather in our knowledge than in nature. A few blows of the pickaxe at the foot of the Pyrenees, of the Himalaya, of Mount Pentelicus in Greece, a few diggings in the sandpits of Eppelsheim, or in the Mauvaises Terres of Nebraska, have revealed to us the closest connecting links between forms which seemed before so widely separated. How much closer will these links be drawn when Palaeontology shall have escaped from its cradle!")

LETTER 570. A. SEDGWICK TO CHARLES DARWIN.

(570/1. In May, 1870, Darwin "went to the Bull Hotel, Cambridge, to see the boys, and for a little rest and enjoyment." (570/2. See "Life and Letters," III., 125.) The following letter was received after his return to Down.)

Trinity College, Cambridge, May 30th, 1870.

My dear Darwin,

Your very kind letter surprised me. Not that I was surprised at the pleasant and very welcome feeling with which it was written. But I could not make out what I had done to deserve the praise of "extraordinary kindness to yourself and family." I would most willingly have done my best to promote the objects of your visit, but you gave me no opportunity of doing so. I was truly grieved to find that my joy at seeing you again was almost too robust for your state of nerves, and that my society, after a little while, became oppressive to you. But I do trust that your Cambridge visit has done you no constitutional harm; nay, rather that it has done you some good. I only speak honest truth when I say that I was overflowing with joy when I saw you, and saw you in the midst of a dear family party, and solaced at every turn by the loving care of a dear wife and daughters. How different from my position—that of a very old man, living in cheerless solitude! May god help and cheer you all with the comfort of hopeful hearts—you and your wife, and your sons and daughters!

You were talking about my style of writing,—I send you my last specimen, and it will probably continue to be my last. It is the continuation of a former pamphlet of which I have not one spare copy. I do not ask you to read it. It is addressed to the old people in my native Dale of Dent, on the outskirts of Westmorland. While standing at the door of the old vicarage, I can see down the valley the Lake mountains—Hill Bell at the head of Windermere, about twenty miles off. On Thursday next (D.V.) I am to start for Dent, which I have not visited for full two years. Two years ago I could walk three or four miles with comfort. Now, alas! I can only hobble about on my stick.

I remain your true-hearted old friend A. Sedgwick.

LETTER 571. TO C. LYELL. Down, September 3rd {1874}.

Many thanks for your very kind and interesting letter. I was glad to hear at Southampton from Miss Heathcote a good account of your health and strength.

With respect to the great subject to which you refer in your P.S., I always try to banish it from my mind as insoluble; but if I were circumstanced as you are, no doubt it would recur in the dead of the night with painful force. Many persons seem to make themselves quite easy about immortality (571/1. See "Life and Letters," I., page 312.) and the existence of a personal God, by intuition; and I suppose that I must differ from such persons, for I do not feel any innate conviction on any such points.

We returned home about ten days ago from Southampton, and I enjoyed my holiday, which did me much good. But already I am much fatigued by microscope and experimental work with insect-eating plants.

When at Southampton I was greatly interested by looking at the odd gravel deposits near at hand, and speculating about their formation. You once told me something about them, but I forget what; and I think that Prestwich has written on the superficial deposits on the south coasts, and I must find out his paper and read it. (571/2. Prof. Prestwich contributed several papers to the Geological Society on the Superficial Deposits of the South of England.)

From what I have seen of Mr. Judd's papers I have thought that he would rank amongst the few leading British geologists.

LETTER 572. TO J.D. HOOKER.

(572/1. The following letter was written before Mr. Darwin knew that Sir Charles Lyell was to be buried in Westminster Abbey, a memorial which thoroughly satisfied him. See "Life and Letters," III., 197.)

Down, February 23rd, 1875.

I have just heard from Miss Buckley of Lyell's death. I have long felt opposed to the present rage for testimonials; but when I think how Lyell revolutionised Geology, and aided in the progress of so many other branches of science, I wish that something could be done in his honour. On the other hand it seems to me that a poor testimonial would be worse than none; and testimonials seem to succeed only when a man has been known and loved by many persons, as in the case of Falconer and Forbes. Now, I doubt whether of late years any large number of scientific men did feel much attachment towards Lyell; but on this head I am very ill fitted to judge. I should like to hear some time what you think, and if anything is proposed I should particularly wish to join in it. We have both lost as good and as true a friend as ever lived.

LETTER 573. TO J.D. HOOKER.

(573/1. This letter shows the difficulty which the inscription for Sir Charles Lyell's memorial gave his friends. The existing inscription is, "Charles Lyell...Author of 'The Principles of Geology'...Throughout a long and laborious life he sought the means of deciphering the fragmentary records of the Earth's history in the patient investigation of the present order of Nature, enlarging the boundaries of knowledge, and leaving on Scientific thought an enduring influence...")

Down, June 21st {1876}.

I am sorry for you about the inscription, which has almost burst me. We think there are too many plurals in yours, and when read aloud it hisses like a goose. I think the omission of some words makes it much stronger. "World" (573/2. The suggested sentence runs: "he gave to the world the results of his labour, etc.") is much stronger and truer than "public." As Lyell wrote various other books and memoirs, I have some little doubt about the "Principles of Geology." People here do not like your "enduring value": it sounds almost an anticlimax. They do not much like my "last (or endure) as long as science lasts." If one reads a sentence often enough, it always becomes odious.

God help you.

LETTER 574. TO OSWALD HEER. Down, March 8th {1875}.

I thank you for your very kind and deeply interesting letter of March 1st, received yesterday, and for the present of your work, which no doubt I shall soon receive from Dr. Hooker. (574/1. "Flora Fossilis Arctica," Volume III., 1874, sent by Prof. Heer through Sir

Joseph Hooker.) The sudden appearance of so many Dicotyledons in the Upper Chalk appears to me a most perplexing phenomenon to all who believe in any form of evolution, especially to those who believe in extremely gradual evolution, to which view I know that you are strongly opposed. (574/2. The volume referred to contains a paper on the Cretaceous Flora of the Arctic Zone (Spitzbergen and Greenland), in which several dicotyledonous plants are described. In a letter written by Heer to Darwin the author speaks of a species of poplar which he describes as the oldest Dicotyledon so far recorded.) The presence of even one true Angiosperm in the Lower Chalk makes me inclined to conjecture that plants of this great division must have been largely developed in some isolated area, whence owing to geographical changes, they at last succeeded in escaping, and spread quickly over the world. (574/3. No satisfactory evidence has so far been brought forward of the occurrence of fossil Angiosperms in pre-Cretaceous rocks. The origin of the Monocotyledons and Dicotyledons remains one of the most difficult and attractive problems of Palaeobotany.) (574/4. See Letters 395, 398.) But I fully admit that this case is a great difficulty in the views which I hold. Many as have been the wonderful discoveries in Geology during the last half-century, I think none have exceeded in interest your results with respect to the plants which formerly existed in the Arctic regions. How I wish that similar collections could be made in the Southern hemisphere, for instance in Kerguelen's Land.

The death of Sir C. Lyell is a great loss to science, but I do not think to himself, for it was scarcely possible that he could have retained his mental powers, and he would have suffered dreadfully from their loss. The last time I saw him he was speaking with the most lively interest about his last visit to you, and I was grieved to hear from him a very poor account of your health. I have been working for some time on a special subject, namely insectivorous plants. I do not know whether the subject will interest you, but when my book is published I will have the pleasure of sending you a copy.

I am very much obliged for your photograph, and enclose one of myself.

LETTER 574. TO S.B.J. SKERTCHLY. March 2nd, 1878.

It is the greatest possible satisfaction to a man nearly at the close of his career to believe that he has aided or stimulated an able and energetic fellow-worker in the noble cause of science. Therefore your letter has deeply gratified me. I am writing this away from home, as my health failed, and I was forced to rest; and this will account for the delay in answering your letter. No doubt on my return home I shall find the memoir which you have kindly sent me. I shall read it with much interest, as I have heard something of your work from Prof. Geikie, and have read his admirable "Ice Age." (574/5. "The Great Ice Age and its Relation to the Antiquity of Man": London, 1874. By James Geikie.) I have noticed the criticisms on your work, but such opposition must be expected by every one who draws fine grand conclusions, and such assuredly are yours as abstracted in your letter. (574/6. Mr. S.B.J. Skertchly recorded "the discovery of palaeolithic flint implements, mammalian bones, and fresh-water shells in brick-earths below the Boulder-clay of East Anglia," in a letter published in the "Geol. Mag." Volume III., page 476, 1876. (See also "The Fenland, Past and Present." S.H. Miller and S.B.J. Skertchly, London, 1878.) The conclusions of Mr. Skertchly as to the pre-Glacial age of the flint implements were not accepted by some authorities. (See correspondence in "Nature," Volume XV., 1877, pages 141, 142.) We are indebted to Mr. Marr for calling our attention to Mr. Skertchly's discovery.) What magnificent progress Geology has made within my lifetime!

I shall have very great pleasure in sending you any of my books with my autograph, but I really do not know which to send. It will cost you only the trouble of a postcard to tell me which you would like, and it shall soon be sent. Forgive this untidy note, as it is rather an effort to write.

With all good wishes for your continued success in science and for your happiness...

CHAPTER 2.X.—BOTANY, 1843-1871.

2.X.I. Miscellaneous.—2.X.II. Melastomaceae.—2.X.III. Correspondence with John Scott.

2.X.I. MISCELLANEOUS, 1843-1862.

(PLATE: SIR JOSEPH HOOKER, 1897. From a Photograph by W.J. Hawker Wimborne. Walker & Cockerell, ph. sc.)

LETTER 575. TO WILLIAM JACKSON HOOKER. Down, March 12th {1843}.

...When you next write to your son, will you please remember me kindly to him and give him my best thanks for his note? I had the pleasure yesterday of reading a letter from him to Mr. Lyell of Kinnordy, full of the most interesting details and descriptions, and written (if I may be permitted to make such a criticism) in a particularly agreeable style. It leads me anxiously to hope, even more than I did before, that he will publish some separate natural history journal, and not allow (if it can be avoided) his materials to be merged in another work. I am very glad to hear you talk of inducing your son to publish an Antarctic Flora. I have long felt much curiosity for some discussion on the general character of the flora of Tierra del Fuego, that part of the globe farthest removed in latitude from us. How interesting will be a strict comparison between the plants of these regions and of Scotland and Shetland. I am sure I may speak on the part of Prof. Henslow that all my collection (which gives a fair representation of the Alpine flora of Tierra del Fuego and of Southern Patagonia) will be joyfully laid at his disposal.

LETTER 576. TO JOHN LINDLEY. Down, Saturday {April 8th, 1843}.

I take the liberty, at the suggestion of Dr. Royle, of forwarding to you a few seeds, which have been found under very singular circumstances. They have been sent to me by Mr. W. Kemp, of Galashiels, a (partially educated) man, of whose acuteness and accuracy of observation, from several communications on geological subjects, I have a VERY HIGH opinion. He found them in a layer under twenty-five feet thickness of white sand, which seems to have been deposited on the margins of an anciently existing lake. These seeds are not known to the provincial botanists of the district. He states that some of them germinated in eight days after being planted, and are now alive. Knowing the interest you took in some raspberry seeds, mentioned, I remember, in one of your works, I hope you will not think me troublesome in asking you to have these seeds carefully planted, and in begging you so far to oblige me as to take the trouble to inform me of the result. Dr. Daubeny has started for Spain, otherwise I would have sent him some. Mr. Kemp is anxious to publish an account of his discovery himself, so perhaps you will be so kind as to communicate the result to me, and not to any periodical. The chance, though appearing so impossible, of recovering a plant lost to any country if not to the world, appears to me so very interesting, that I hope you will think it worth while to have these seeds planted, and not returned to me.

LETTER 577. TO C. LYELL. {September, 1843.}

An interesting fact has lately, as it were, passed through my hands. A Mr. Kemp (almost a working man), who has written on "parallel roads," and has corresponded with me (577/1. In a letter to Henslow, Darwin wrote: "If he {Mr. Kemp} had not shown himself a most careful and ingenious observer, I should have thought nothing of the case."), sent me in the spring some seeds, with an account of the spot where they were found, namely, in a layer at the bottom of a deep sand pit, near Melrose, above the level of the river, and which sand pit he thinks must have been accumulated in a lake, when the whole features of the valleys were different, ages ago; since which whole barriers of rock, it appears, must have been worn down. These seeds germinated freely, and I sent some to the Horticultural Society, and Lindley writes to me that they turn out to be a common Rumex and a species of Atriplex, which neither he nor Henslow (as I have since heard) have ever seen, and certainly not a British plant! Does this not look like a vivification of a fossil seed? It is not surprising, I think, that seeds should last ten or twenty thousand {years}, as they have lasted two or three {thousand years} in the Druidical mounds, and have germinated.

When not building, I have been working at my volume on the volcanic islands which we visited; it is almost ready for press...I hope you will read my volume, for, if you don't, I cannot think of anyone else who will! We have at last got our house and place tolerably comfortable, and I am well satisfied with our anchorage for life. What an autumn we have had: completely Chilian; here we have had not a drop of rain or a cloudy day for a month. I am positively tired of the fine weather, and long for the sight of mud almost as much as I did when in Peru.

(577/2. The vitality of seeds was a subject in which Darwin continued to take an interest. In July, 1855 ("Life and Letters," II., page 65), he wrote to Hooker: "A man told me the other day of, as I thought, a splendid instance—and splendid it was, for according to his evidence the seed came up alive out of the lower part of the London Clay! I disgusted him by telling him that palms ought to have come up."

In the "Gardeners' Chronicle," 1855, page 758, appeared a notice (half a column in length) by Darwin on the "Vitality of Seeds." The facts related refer to the "Sand-walk" at Down; the wood was planted in 1846 on a piece of pasture land laid down as grass in 1840. In 1855, on the soil being dug in several places, Charlock (Brassica sinapistrum) sprang up freely. The subject continued to interest him, and we find a note dated July 2nd, 1874, in which Darwin recorded that forty-six plants of Charlock sprang up in that year over a space (14 x 7 feet) which had been dug to a considerable depth. In the course of the article in the "Gardeners' Chronicle," Darwin remarks: "The power in seeds of retaining their vitality when buried in damp soil may well be an element in preserving the species, and therefore seeds may be specially endowed with this capacity; whereas the power of retaining vitality in a dry artificial condition must be an indirect, and in one sense accidental, quality in seeds of little or no use to the species."

The point of view expressed in the letter to Lyell above given is of interest in connection with the research of Horace Brown and F. Escombe (577/3. "Proc. Roy. Soc." Volume LXII., page 160.) on the remarkable power possessed by dry seeds of resistance to the temperature of liquid air. The point of the experiment is that life continues at a temperature "below that at which ordinary chemical reactions take place." A still more striking demonstration of the fact has been made by Thiselton-Dyer and Dewar who employed liquid hydrogen as a refrigerant. (577/4. Read before the British Association (Dover), 1899, and published in the "Comptes rendus," 1899, and in the "Proc. R. Soc." LXV., page 361, 1899.) The connection between these facts and the dormancy of buried seeds is only indirect; but inasmuch as the experiment proves the possibility of life surviving a period in which no ordinary chemical change occurs, it is clear that they help one to believe in greatly prolonged dormancy in conditions which tend to check metabolism. For a discussion of the bearing of their results on the life-problem, and for the literature of the subject, reference should be made to the paper by Brown and Escombe. See also C. de Candolle "On Latent Life in Seeds," "Brit. Assoc. Report," 1896, page 1023 and F. Escombe, "Science Progress," Volume I., N.S., page 585, 1897.)

LETTER 578. TO J.S. HENSLOW. Down, Saturday {November 5th, 1843}.

I sent that weariful Atriplex to Babington, as I said I would, and he tells me that he has reared a facsimile by sowing the seeds of A. angustifolia in rich soil. He says he knows the A. hastata, and that it is very different. Until your last note I had not heard that Mr. Kemp's seeds had produced two Polygonums. He informs me he saw each plant bring up the husk of the individual seed which he planted. I believe myself in his accuracy, but I have written to advise him not to publish, for as he collected only two kinds of seeds—and from them two Polygomuns, two species or varieties of Atriplex and a Rumex have come up, any one would say (as you suggested) that more probably all the seeds were in the soil, than that seeds, which must have been buried for tens of thousands of years, should retain their vitality. If the Atriplex had turned out new, the evidence would indeed have been good. I regret this result of poor Mr. Kemp's seeds, especially as I believed, from his statements and the appearance of the seeds, that they did germinate, and I further have no doubt that their antiquity must be immense. I am sorry also for the trouble you have had. I heard the other day through a circuitous course how you are astonishing all the clodhoppers in your whole part of the county: and {what is} far more wonderful, as it was remarked to me, that you had not, in doing this, aroused the envy of all the good surrounding sleeping parsons. What good you must do to the present and all succeeding generations. (578/1. For an account of Professor Henslow's management of his parish of Hitcham see "Memoir of the Rev. John Stevens Henslow, M.A." by the Rev. Leonard Jenyns: 8vo, London, 1862.)

LETTER 579. TO J.D. HOOKER. Down, November 14th {1855}.

You well know how credulous I am, and therefore you will not be surprised at my believing the Raspberry story (579/1. This probably refers to Lindley's story of the germination of raspberry seeds taken from a barrow 1600 years old.): a very similar case is on record in Germany—viz., seeds from a barrow; I have hardly zeal to translate it for the "Gardeners' Chronicle." (579/2. "Vitality of Seeds," "Gardeners' Chronicle," November 17th, 1855, page 758.) I do not go the whole hog—viz., that sixty and two thousand years are all the same, for I should imagine that some slight chemical change was always going on in a seed. Is this not so? The discussions have stirred me up to send my very small case of the charlock; but as it required some space to give all details, perhaps Lindley will not insert; and if he does, you, you worse than an unbelieving dog, will not, I know, believe. The reason I do not care to try Mr. Bentham's plan is that I think it would be very troublesome, and it would not, if I did not find seed, convince me myself that none were in the earth, for I have found in my salting experiments that the earth clings to the seeds, and the seeds are very difficult to find. Whether washing would do I know not; a gold-washer would succeed, I daresay.

LETTER 580. TO W.J. HOOKER.

Testimonial from Charles Darwin, Esq., M.A., F.R.S. and G.S., late Naturalist to Captain Fitz-Roy's Voyage.

Down House, Farnborough, August 25th, 1845.

I have heard with much interest that your son, Dr. Hooker, is a candidate for the Botanical Chair at Edinburgh. From my former attendance at that University, I am aware how important a post it is for the advancement of science, and I am therefore the more anxious for your son's success, from my firm belief that no one will fulfil its duties with greater zeal or ability. Since his return from the famous Antarctic expedition, I have had, as you are aware, much communication with him, with respect to the collections brought home by myself, and on other scientific subjects; and I cannot express too strongly my admiration at the accuracy of his varied knowledge, and at his powers of generalisation. From Dr. Hooker's disposition, no one, in my opinion, is more fitted to communicate to beginners a strong taste for those pursuits to which he is himself so ardently devoted. For the sake of the advancement of Botany in all its branches, your son has my warmest wishes for his success.

LETTER 581. TO J.D. HOOKER. Down, Thursday {June 11th, 1847}.

Many thanks for your kindness about the lodgings—it will be of great use to me. (581/1. The British Association met at Oxford in 1847.) Please let me know the address if Mr. Jacobson succeeds, for I think I shall go on the 22nd and write previously to my lodgings. I have since had a tempting invitation from Daubeny to meet Henslow, etc., but upon the whole, I believe, lodgings will answer best, for then I shall have a secure solitary retreat to rest in.

I am extremely glad I sent the Laburnum (581/2. This refers to the celebrated form known as Cytisus Adami, of which a full account is given in "Variation of Animals and Plants," Volume I., Edition II., page 413. It has been supposed to be a seminal hybrid or graft-hybrid between C. laburnum and C. purpureus. It is remarkable for bearing "on the same tree tufts of dingy red, bright yellow, and purple flowers, borne on branches having widely different leaves and manner of growth." In a paper by Camuzet in the "Annales de la Societe d'Horticulture de Paris, XIII., 1833, page 196, the author tries to show that Cytisus Adami is a seminal hybrid between C. alpinus and C. laburnum. Fuchs ("Sitz. k. Akad. Wien," Bd. 107) and Beijerinck ("K. Akad. Amsterdam," 1900) have spoken on Cytisus Adami, but throw no light on the origin of the hybrid. See letters to Jenner Weir in the present volume.): the raceme grew in centre of tree, and had a most minute tuft of leaves, which presented no unusual appearance: there is now on one raceme a terminal bilateral {i.e., half yellow, half purple} flower, and on other raceme a single terminal pure yellow and one adjoining bilateral flower. If you would like them I will send them; otherwise I would keep them to see whether the bilateral flowers will seed, for Herbert (581/3. Dean Herbert.) says the yellow ones will. Herbert is wrong in thinking there are no somewhat analogous facts: I can tell you some, when we meet. I know not whether botanists consider each petal and stamen an individual; if so, there seems to me no especial difficulty in the case, but if a flower-bud is a unit, are not their flowers very strange?

I have seen Dillwyn in the "Gardeners' Chronicle," and was disgusted at it, for I thought my bilateral flowers would have been a novelty for you.

(581/4. In a letter to Hooker, dated June 2nd, 1847, Darwin makes a bold suggestion as to floral symmetry:—)

I send you a tuft of the quasi-hybrid Laburnum, with two kinds of flowers on same stalk, and with what strikes {me} as very curious (though I know it has been observed before), namely, a flower bilaterally different: one other, I observe, has half its calyx purple. Is this not very curious, and opposed to the morphological idea that a flower is a condensed continuous spire of leaves? Does it not look as if flowers were normally bilateral; just in the same way as we now know that the radiating star-fish, etc., are bilateral? The case reminds me of those insects with exactly half having secondary male characters and the other half female.

(581/5. It is interesting to note his change of view in later years. In an undated letter written to Mr. Spencer, probably in 1873, he says: "With respect to asymmetry in the flowers themselves, I remain contented, from all that I have seen, with adaptation to visits of insects. There is, however, another factor which it is likely enough may have come into play—viz., the protection of the anthers and pollen from the injurious effects of rain. I think so because several flowers inhabiting rainy countries, as A. Kerner has lately shown, bend their heads down in rainy weather.")

LETTER 582. TO J.D. HOOKER. June {1855}.

(582/1. This is an early example of Darwin's interest in the movements of plants. Sleeping plants, as is well-known, may acquire a rhythmic movement differing from their natural period, but the precise experiment here described has not, as far as known, been carried out. See Pfeffer, "Periodische Bewegungen," 1875, page 32.)

I thank you much for Hedysarum: I do hope it is not very precious, for, as I told you, it is for probably a most foolish purpose. I read somewhere that no plant closes its leaves so promptly in darkness, and I want to cover it up daily for half an hour, and see if I can TEACH IT to close by itself, or more easily than at first in darkness. I am rather puzzled about its transmission, from not knowing how tender it is...

LETTER 583. TO J.D. HOOKER. Down, July 19th, 1856.

I thank you warmly for the very kind manner with which you have taken my request. It will, in truth, be a most important service to me; for it is absolutely necessary that I should discuss single and double creations, as a very crucial point on the general origin of species, and I must confess, with the aid of all sorts of visionary hypotheses, a very hostile one. I am delighted that you will take up possibility of crossing, no botanist has done so, which I have long regretted, and I am glad to see that it was one of A. De Candolle's desiderata. By the way, he is curiously contradictory on subject. I am far from expecting that no cases of apparent impossibility will be found; but certainly I expect that ultimately they will disappear; for instance, Campanulaceae seems a strong case, but now it is pretty clear that they must be liable to crossing. Sweet-peas (583/1. In Lathyrus odoratus the absence of the proper insect has been supposed to prevent crossing. See "Variation under Domestication," Edition II., Volume II., page 68; but the explanation there given for Pisum may probably apply to Lathyrus.), bee-orchis, and perhaps hollyhocks are, at present, my greatest difficulties; and I find I cannot experimentise by castrating sweet-peas, without doing fatal injury. Formerly I felt most interest on this point as one chief means of eliminating varieties; but I feel interest now in other ways. One general fact {that} makes me believe in my doctrine (583/2. The doctrine which has been epitomised as "Nature abhors perpetual self-fertilisation," and is generally known as Knight's Law or the Knight-Darwin Law, is discussed by Francis Darwin in "Nature," 1898. References are there given to the chief passages in the "Origin of Species," etc., bearing on the question. See Letter 19, Volume I.), is that NO terrestrial animal in which semen is liquid is hermaphrodite except with mutual copulation; in terrestrial plants in which the semen is dry there are many hermaphrodites. Indeed, I do wish I lived at Kew, or at least so that I could see you oftener. To return again to subject of crossing: I have been inclined to speculate so far, as to think (my!?) notion (I say MY notion, but I think others have put forward nearly or quite similar ideas) perhaps explains the frequent separation of the sexes in trees, which I think I have heard remarked (and in looking over the mono- and dioecious Linnean classes in Persoon seems true) are very apt to have sexes separated; for {in} a tree having a vast number of flowers on the same individual, or at least the same stock, each flower, if only hermaphrodite on the common plan, would generally get its own pollen or only pollen from another flower on same stock,—whereas if the sexes were separate there would be a better chance of occasional pollen from another distinct stock. I have thought of testing this in your New Zealand Flora, but I have no standard of comparison, and I found myself bothered by bushes. I should propound that some unknown causes had favoured development of trees and bushes in New Zealand, and consequent on this there had been a development of separation of sexes to prevent too much intermarriage. I do not, of course, suppose the prevention of too much intermarriage the only good of separation of sexes. But such wild notions are not worth troubling you with the reading of.

LETTER 584. TO J.D. HOOKER. Moor Park {May 2nd, 1857}.

The most striking case, which I have stumbled on, on apparent, but false relation of structure of plants to climate, seems to be Meyer and Doege's remark that there is not one single, even moderately-sized, family at the Cape of Good Hope which has not one or several species with heath-like foliage; and when we consider this together with the number of true heaths, any one would have been justified, had it not been for our own British heaths (584/1. It is well known that plants with xerophytic characteristics are not confined to dry climates; it is only necessary to mention halophytes, alpine plants and certain epiphytes. The heaths of Northern Europe are placed among the xerophytes by Warming ("Lehrbuch der okologischen Pflanzengeographie," page 234, Berlin, 1896).), in saying that heath-like foliage must stand in direct relation to a dry and moderately warm climate. Does this not strike you as a good case of false relation? I am so pleased with this place and the people here, that I am greatly tempted to bring Etty here, for she has not, on the whole, derived any benefit from Hastings. With thanks for your never failing assistance to me...

I remember that you were surprised at number of seeds germinating in pond mud. I tried a fourth pond, and took about as much mud (rather more than in former case) as would fill a very large breakfast cup, and before I had left home 118 plants had come up; how many more will be up on my return I know not. This bears on chance of birds by their muddy feet transporting fresh-water plants.

This would not be a bad dodge for a collector in country when plants were not in seed, to collect and dry mud from ponds.

LETTER 585. TO ASA GRAY. Down {1857}.

I am very glad to hear that you think of discussing the relative ranges of the identical and allied U. States and European species, when you have time. Now this leads me to make a very audacious remark in opposition to what I imagine Hooker has been writing (585/1. See Letter 338, Volume I.), and to your own scientific conscience. I presume he has been urging you to finish your great "Flora" before you do anything else. Now I would say it is your duty to generalise as far as you safely can from your as yet completed work. Undoubtedly careful discrimination of species is the foundation of all good work; but I must look at such papers as yours in Silliman as the fruit. As careful observation is far harder work than generalisation, and still harder than speculation, do you not think it very possible that it may be overvalued? It ought never to be forgotten that the observer can generalise his own observations incomparably better than any one else. How many astronomers have laboured their whole lives on observations, and have not drawn a single conclusion; I think it is Herschel who has remarked how much better it would be if they had paused in their devoted work and seen what they could have deduced from their work. So do pray look at this side of the question, and let us have another paper or two like the last admirable ones. There, am I not an audacious dog!

You ask about my doctrine which led me to expect that trees would tend to have separate sexes. I am inclined to believe that no organic being exists which perpetually self-fertilises itself. This will appear very wild, but I can venture to say that if you were to read my observations on this subject you would agree it is not so wild as it will at first appear to you, from flowers said to be always fertilised in bud, etc. It is a long subject, which I have attended to for eighteen years. Now, it occurred to me that in a large tree with hermaphrodite flowers, we will say it would be ten to one that it would be fertilised by the pollen of its own flower, and a thousand or ten thousand to one that if crossed it would be crossed only with pollen from another flower of same tree, which would be opposed to my doctrine. Therefore, on the great principle of "Nature not lying," I fully expected that trees would be apt to be dioecious or monoecious (which, as pollen has to be carried from flower to flower every time, would favour a cross from another individual of the same species), and so it seems to be in Britain and New Zealand. Nor can the fact be explained by certain families having this structure and chancing to be trees, for the rule seems to hold both in genera and families, as well as in species.

I give you full permission to laugh your fill at this wild speculation; and I do not pretend but what it may be chance which, in this case, has led me apparently right. But I repeat that I feel sure that my doctrine has more probability than at first it appears to have. If you had not asked, I should not have written at such length, though I cannot give any of my reasons.

The Leguminosae are my greatest opposers: yet if I were to trust to observations on insects made during many years, I should fully expect crosses to take place in them; but I cannot find that our garden varieties ever cross each other. I do NOT ask you to take any trouble about it, but if you should by chance come across any intelligent nurseryman, I wish you would enquire whether they take any pains in raising the varieties of papilionaceous plants apart to prevent crossing. (I have seen a statement of naturally formed crossed Phaseoli near N. York.) The worst is that nurserymen are apt to attribute all varieties to crossing.

Finally I incline to believe that every living being requires an occasional cross with a distinct individual; and as trees from the mere multitude of flowers offer an obstacle to this, I suspect this obstacle is counteracted by tendency to have sexes separated. But I have forgotten to say that my maximum difficulty is trees having papilionaceous flowers: some of them, I know, have their keel-petals expanded when ready for fertilisation; but Bentham does not believe that this is general: nevertheless, on principle of nature not lying, I suspect that this will turn out so, or that they are eminently sought by bees dusted with pollen. Again I do NOT ask you to take trouble, but if strolling under your Robinias when in full flower, just look at stamens and pistils whether protruded and whether bees visit them. I must just mention a fact mentioned to me the other day by Sir W. Macarthur, a clever Australian gardener: viz., how odd it was that his Erythrinas in N.S. Wales would not set a seed, without he imitated the movements of the petals which bees cause. Well, as long as you live, you will never, after this fearfully long note, ask me why I believe this or that.

LETTER 586. TO ASA GRAY. June 18th {1857}.

It has been extremely kind of you telling me about the trees: now with your facts, and those from Britain, N. Zealand, and Tasmania I shall have fair materials for judging. I am writing this away from home, but I think your fraction of 95/132 is as large as in other cases, and is at least a striking coincidence.

I thank you much for your remarks about my crossing notions, to which, I may add, I was led by exactly the same idea as yours, viz., that crossing must be one means of eliminating variation, and then I wished to make out how far in animals and vegetables this was possible. Papilionaceous flowers are almost dead floorers to me, and I cannot experimentise, as castration alone often produces sterility. I am surprised at what you say about Compositae and Gramineae. From what I have seen of latter they seemed to me (and I have watched wheat, owing to what L. de Longchamps has said on their fertilisation in bud) favourable for crossing; and from Cassini's observations and Kolreuter's on the adhesive pollen, and C.C. Sprengel's, I had concluded that the Compositae were eminently likely (I am aware of the pistil brushing out pollen) to be crossed. (586/1. This is an instance of the curious ignorance of the essential principles of floral mechanism which was to be found even among learned and accomplished botanists such as Gray, before the publication of the "Fertilisation of Orchids." Even in 1863 we find Darwin explaining the meaning of dichogamy in a letter to Gray.) If in some months' time you can find time to tell me whether you have made any observations on the early fertilisation of plants in these two orders, I should be very glad to hear, as it would save me from great blunder. In several published remarks on this subject in various genera it has seemed to me that the early fertilisation has been inferred from the early shedding of the pollen, which I think is clearly a false inference. Another cause, I should think, of the belief of fertilisation in the bud, is the not-rare, abnormal, early maturity of the pistil as described by Gartner. I have hitherto failed in meeting with detailed accounts of regular and normal impregnation in the bud. Podostemon and Subularia under water (and Leguminosae) seem and are strongest cases against me, as far as I as yet know. I am so sorry that you are so overwhelmed with work; it makes your VERY GREAT kindness to me the more striking.

It is really pretty to see how effectual insects are. A short time ago I found a female holly sixty measured yards from any other holly, and I cut off some twigs and took by chance twenty stigmas, cut off their tops, and put them under the microscope: there was pollen on every one, and in profusion on most! weather cloudy and stormy and unfavourable, wind in wrong direction to have brought any.

LETTER 587. TO J.D. HOOKER. Down, January 12th {1858}.

I want to ask a question which will take you only few words to answer. It bears on my former belief (and Asa Gray strongly expressed opinion) that Papilionaceous flowers were fatal to my notion of there being no eternal hermaphrodites. First let me say how evidence goes. You will remember my facts going to show that kidney-beans require visits of bees to be fertilised. This has been positively stated to be the case with Lathyrus grandiflorus, and has been very partially verified by me. Sir W. Macarthur tells me that Erythrina will hardly seed in Australia without the petals are moved as if by bee. I have just met the statement that, with common bean, when the humble-bees bite holes at the base of the flower, and therefore cease visiting the mouth of the corolla, "hardly a bean will set." But now comes a much more curious statement, that {in} 1842-43, "since bees were established at Wellington (New Zealand), clover seeds all over the settlement, WHICH IT DID NOT BEFORE." (587/1. See Letter 362, Volume I.) The writer evidently has no idea what the connection can be. Now I cannot help at once connecting this statement (and all the foregoing statements in some degree support each other, as all have been advanced without any sort of theory) with the remarkable absence of Papilionaceous plants in N. Zealand. I see in your list Clianthus, Carmichaelia (four species), a new genus, a shrub, and Edwardsia (is latter Papilionaceous?). Now what I want to know is whether any of these have flowers as small as clover; for if they have large flowers they may be visited by humble-bees, which I think I remember do exist in New Zealand; and which humble-bees would not visit the smaller clover. Even the very minute little yellow clover in England has every flower visited and revisited by hive-bees, as I know by experience. Would it not be a curious case of correlation if it could be shown to be probable that herbaceous and small Leguminosae do not exist because when {their} seeds {are} washed ashore (!!!) no small bees exist there. Though this latter fact must be ascertained. I may not prove anything, but does it not seem odd that so many quite independent facts, or rather statements, should point all in one direction, viz., that bees are necessary to the fertilisation of Papilionaceous flowers?

LETTER 588. TO JOHN LUBBOCK (Lord Avebury). Sunday {1859}.

Do you remember calling my attention to certain flowers in the truss of Pelargoniums not being true, or not having the dark shade on the two upper petals? I believe it was Lady Lubbock's observation. I find, as I expected, it is always the central or sub-central flower; but what is far more curious, the nectary, which is blended with the peduncle of the flowers, gradually lessens and quite disappears (588/1. This fact is mentioned in Maxwell Masters' "Vegetable Teratology" (Ray Society's Publications), 1869, page 221.), as the dark shade on the two upper petals disappears. Compare the stalk in the two enclosed parcels, in each of which there is a perfect flower.

Now, if your gardener will not be outrageous, do look over your geraniums and send me a few trusses, if you can find any, having the flowers without the marks, sending me some perfect flowers on same truss. The case seems to me rather a pretty one of correlation of growth; for the calyx also becomes slightly modified in the flowers without marks.

LETTER 589. TO MAXWELL MASTERS. Down, April 7th {1860}.

I hope that you will excuse the liberty which I take in writing to you and begging a favour. I have been very much interested by the abstract (too brief) of your lecture at the Royal Institution. Many of the facts alluded to are full of interest for me. But on one point I should be infinitely obliged if you could procure me any information: namely, with respect to sweet-peas. I am a great believer in the natural crossing of individuals of the same species. But I have been assured by Mr. Cattell (589/1. The nurseryman he generally dealt with.), of Westerham, that the several varieties of sweet-pea can be raised close together for a number of years without intercrossing. But on the other hand he stated that they go over the beds, and pull up any false plant, which they very naturally attribute to wrong seeds getting mixed in the lot. After many failures, I succeeded in artificially crossing two varieties, and the offspring out of the same pod, instead of being intermediate, was very nearly like the two pure parents; yet in one, there was a trace of the cross, and these crossed peas in the next generation showed still more plainly their mongrel origin. Now, what I want to know is, whether there is much variation in sweet-peas which might be owing to natural crosses. What I should expect would be that they would keep true for many years, but that occasionally, perhaps at long intervals, there would be a considerable amount of crossing of the varieties grown close together. Can you give, or obtain from your father, any information on this head, and allow me to quote your authority? It would really be a very great favour and kindness.

LETTER 590. TO J.D. HOOKER.

(590/1. The genera Scaevola and Leschenaultia, to which the following letter refers, belong to the Goodeniaceae (Goodenovieae, Bentham & Hooker), an order allied to the Lobeliaceae, although the mechanism of fertilisation resembles rather more nearly that of Campanula. The characteristic feature of the flower in this order is the indusium, or, as Delpino (590/2. Delpino's observations on Dichogamy, summarised by Hildebrand in "Bot. Zeitung," 1870, page 634.) calls it, the "collecting cup": this cuplike organ is a development of the style, and serves the same function as the hairs on the style of Campanula, namely, that of taking the pollen from the anthers and presenting it to the visiting insect. During this stage the immature stigma is at the bottom of the cup, and though surrounded by pollen is incapable of being pollinated. In most genera of the order the pollen is pushed out of the indusium by the growth of the style or stigma, very much as occurs in Lobelia or the Compositae. Finally the style emerges from the indusium (590/3. According to Hamilton ("Proc. Linn. Soc. N. S. Wales," X., 1895, page 361) the stigma rarely grows beyond the indusium in Dampiera. In the same journal (1885-6, page 157, and IX., 1894, page 201) Hamilton has given a number of interesting observations on Goodenia, Scaevola, Selliera, Brunonia. There seem to be mechanisms for cross- and also for self-fertilisation.), the stigmas open out and are pollinated from younger flowers. The mechanism of fertilisation has been described by F. Muller (590/4. In a letter to Hildebrand published in the "Bot. Zeitung," 1868, page 113.), and more completely by Delpino (loc. cit.).

Mr. Bentham wrote a paper (590/5. "Linn. Soc. Journal," 1869, page 203.) on the style and stigma in the Goodenovieae, where he speaks of Mr. Darwin's belief that fertilisation takes place outside the indusium. This statement, which we imagine Mr. Bentham must have had from an unpublished source, was incomprehensible to him as long as he confined his work to such genera as Goodenia, Scaevola, Velleia, Coelogyne, in which the mechanism is much as above described; but on examining Leschenaultia the meaning became clear. Bentham writes of this genus:—"The indusium is usually described as broadly two-lipped, without any distinct stigma. The fact appears to be that the upper less prominent lip is stigmatic all over, inside and out, with a transverse band of short glandular hairs at its base outside, while the lower more prominent lip is smooth and glabrous, or with a tuft of rigid hairs. Perhaps this lower lip and the upper band of hairs are all that correspond to the indusium of other genera; and the so-called upper lip, outside of which impregnation may well take place, as observed by Mr. Darwin, must be regarded as the true stigma."

Darwin's interest in the Goodeniaceae was due to the mechanism being apparently fitted for self-fertilisation. In 1871 a writer signing himself F.W.B. made a communication to the "Gardeners' Chronicle" (590/6. 1871, page 1103.), in which he expresses himself as "agreeably surprised" to find Leschenaultia adapted for self-fertilisation, or at least for self-pollinisation. This led Darwin to publish a short note in the same journal, in which he describes the penetration of pollen-tubes into the viscid surface on the outside of the indusium. (590/7. 1871, page 1166. He had previously written in the "Journal of Horticulture and Cottage Gardener," May 28th, 1861, page 151:—"Leschenaultia formosa has apparently the most effective contrivance to prevent the stigma of one flower ever receiving a grain of pollen from another flower; for the pollen is shed in the early bud, and is there shut up round the stigma within a cup or indusium. But some observations led me to suspect that nevertheless insect agency here comes into play; for I found by holding a camel-hair pencil parallel to the pistil, and moving it as if it were a bee going to suck the nectar, the straggling hairs of the brush opened the lip of the indusium, entered it, stirred up the pollen, and brought out some grains. I did this to five flowers, and marked them. These five flowers all set pods; whereas only two other pods set on the whole plant, though covered with innumerable flowers...I wrote to Mr. James Drummond, at Swan River in Australia,...and he soon wrote to me that he had seen a bee cleverly opening the indusium and extracting pollen.") He also describes how a brush, pushed into the flower in imitation of an insect, presses "against the slightly projecting lower lip of the indusium, opens it, and some of the hairs enter and become smeared with pollen." The yield of pollen is therefore differently arranged in Leschenaultia; for in the more typical genera it depends on the growth of the style inside the indusium. Delpino, however (see Hildebrand's version, loc. cit.), describes a similar opening of the cup produced by pressure on the hairs in some genera of the order.)

Down, June 7th {1860}.

Best and most beloved of men, I supplicate and entreat you to observe one point for me. Remember that the Goodeniaceae have weighed like an incubus for years on my soul. It relates to Scaevola microcarpa. I find that in bud the indusium collects all the pollen splendidly, but, differently from Leschenaultia, cannot be afterwards easily opened. Further, I find that at an early stage, when the flower first opens, a boat-shaped stigma lies at the bottom of the indusium, and further that this stigma, after the flower has some time expanded, grows very rapidly, when the plant is kept hot, and pushes out of the indusium a mass of pollen; and at same time two horns project at the corners of the indusium. Now the appearance of these horns makes me suppose that these are the stigmatic surfaces. Will you look to this? for if they be by the relative position of the parts (with indusium and stigma bent at right angles to style) {I am led to think} that an insect entering a flower could not fail to have {its} whole back (at the period when, as I have seen, a whole mass of pollen is pushed out) covered with pollen, which would almost certainly get rubbed on the two horns. Indeed, I doubt whether, without this aid, pollen would get on to the horns. What interests me in the case is the analogy in result with the Lobelia, but by very different means. In Lobelia the stigma, before it is mature, pushes by its circular brush of hairs the pollen out of the conjoined anthers; here the indusium collects pollen, and then the growth of the stigma pushes it out. In the course of about 1 1/2 hour, I found an indusium with hairs on the outer edge perfectly clogged with pollen, and horns protruded, which before the 1 1/2 hour had not one grain of pollen outside the indusium, and no trace of protruding horns. So you will see how I wish to know whether the horns are the true stigmatic surfaces. I would try the case experimentally by putting pollen on the horns, but my greenhouse is so cold, and my plant so small, and in such a little pot, that I suppose it would not seed...

The little length of stigmatic horns at the moment when pollen is forced out of the indusium, compared to what they ultimately attain, makes me fancy that they are not then mature or ready, and if so, as in Lobelia, each flower must be fertilised by pollen from another and earlier flower.

How curious that the indusium should first so cleverly collect pollen and then afterwards push it out! Yet how closely analogous to Campanula brushing pollen out of the anther and retaining it on hairs till the stigma is ready. I am going to try whether Campanula sets seed without insect agency.

LETTER 591. TO J.D. HOOKER.

(591/1. The following letters are given here rather than in chronological order, as bearing on the Leschenaultia problem. The latter part of Letter 591 refers to the cleistogamic flowers of Viola.)

Down, May 1st {1862}.

If you can screw out time, do look at the stigma of the blue Leschenaultia biloba. I have just examined a large bud with the indusium not yet closed, and it seems to me certain that there is no stigma within. The case would be very important for me, and I do not like to trust solely to myself. I have been impregnating flowers, but it is rather difficult...

I have just looked again at Viola canina. The case is odder: only 2 stamens which embrace the stigma have pollen; the 3 other stamens have no anther-cells and no pollen. These 2 fertile anthers are of different shape from the 3 sterile others, and the scale representing the lower lip is larger and differently shaped from the 4 other scales representing 4 other petals.

In V. odorata (single flower) all five stamens produce pollen. But I daresay all this is known.

LETTER 592. TO J.D. HOOKER. November 3rd {1862}.

Do you remember the scarlet Leschenaultia formosa with the sticky margin outside the indusium? Well, this is the stigma—at least, I find the pollen-tubes here penetrate and nowhere else. What a joke it would be if the stigma is always exterior, and this by far the greatest difficulty in my crossing notions should turn out a case eminently requiring insect aid, and consequently almost inevitably ensuring crossing. By the way, have you any other Goodeniaceae which you could lend me, besides Leschenaultia and Scaevola, of which I have seen enough?

I had a long letter the other day from Crocker of Chichester; he has the real spirit of an experimentalist, but has not done much this summer.

LETTER 593. TO F. MULLER. Down, April 9th and 15th {1866}.

I am very much obliged by your letter of February 13th, abounding with so many highly interesting facts. Your account of the Rubiaceous plant is one of the most extraordinary that I have ever read, and I am glad you are going to publish it. I have long wished some one to observe the fertilisation of Scaevola, and you must permit me to tell you what I have observed. First, for the allied genus of Leschenaultia: utterly disbelieving that it fertilises itself, I introduced a camel-hair brush into the flower in the same way as a bee would enter, and I found that the flowers were thus fertilised, which never otherwise happens; I then searched for the stigma, and found it outside the indusium with the pollen-tubes penetrating it; and I convinced Dr. Hooker that botanists were quite wrong in supposing that the stigma lay inside the indusium. In Scaevola microcarpa the structure is very different, for the immature stigma lies at the base within the indusium, and as the stigma grows it pushes the pollen out of the indusium, and it then clings to the hairs which fringe the tips of the indusium; and when an insect enters the flower, the pollen (as I have seen) is swept from these long hairs on to the insect's back. The stigma continues to grow, but is not apparently ready for impregnation until it is developed into two long protruding horns, at which period all the pollen has been pushed out of the indusium. But my observations are here at fault, for I did not observe the penetration of the pollen-tubes. The case is almost parallel with that of Lobelia. Now, I hope you will get two plants of Scaevola, and protect one from insects, leaving the other uncovered, and observe the results, both in the number of capsules produced, and in the average number of seeds in each. It would be well to fertilise half a dozen flowers under the net, to prove that the cover is not injurious to fertility.

With respect to your case of Aristolochia, I think further observation would convince you that it is not fertilised only by larvae, for in a nearly parallel case of an Arum and a Aristolochia, I found that insects flew from flower to flower. I would suggest to you to observe any cases of flowers which catch insects by their probosces, as occurs with some of the Apocyneae (593/1. Probably Asclepiadeae. See H. Muller, "Fertilisation of Flowers," page 396.); I have never been able to conceive for what purpose (if any) this is effected; at the same time, if I tempt you to neglect your zoological work for these miscellaneous observations I shall be guilty of a great crime.

To return for a moment to the indusium: how curious it is that the pollen should be thus collected in a special receptacle, afterwards to be swept out by insects' agency!

I am surprised at what you tell me about the fewness of the flowers of your native orchids which produce seed-capsules. What a contrast with our temperate European species, with the exception of some species of Ophrys!—I now know of three or four cases of self-fertilising orchids, but all these are provided with means for an occasional cross.

I am sorry to say Dr. Cruger is dead from a fever.

I received yesterday your paper in the "Botanische Zeitung" on the wood of climbing plants. (593/2. Fritz Muller, "Ueber das Holz einiger um Desterro wachsenden Kletterpflanzen." "Botanische Zeitung," 1866, pages 57, 65.) I have read as yet only your very interesting and curious remarks on the subject as bearing on the change of species; you have pleased me by the very high compliments which you pay to my paper. I have been at work since March 1st on a new English edition (593/3. The 4th Edition.) of my "Origin," of which when published I will send you a copy. I have much regretted the time it has cost me, as it has stopped my other work. On the other hand, it will be useful for a new third German edition, which is now wanted. I have corrected it largely, and added some discussions, but not nearly so much as I wished to do, for, being able to work only two hours daily, I feared I should never get it finished. I have taken some facts and views from your work "Fur Darwin"; but not one quarter of what I should like to have quoted.

LETTER 594. TO A.G. MORE. Down, June 24th, 1860.

I hope that you will forgive the liberty which I take in writing to you and requesting a favour. Mr. H.C. Watson has given me your address, and has told me that he thought that you would be willing to oblige me. Will you please to read the enclosed, and then you will understand what I wish observed with respect to the bee-orchis. (594/1. Ophrys apifera.) What I especially wish, from information which I have received since publishing the enclosed, is that the state of the pollen-masses should be noted in flowers just beginning to wither, in a district where the bee-orchis is extremely common. I have been assured that in parts of Isle of Wight, viz., Freshwater Gate, numbers occur almost crowded together: whether anything of this kind occurs in your vicinity I know not; but, if in your power, I should be infinitely obliged for any information. As I am writing, I will venture to mention another wish which I have: namely, to examine fresh flowers and buds of the Aceras, Spiranthes, marsh Epipactis, and any other rare orchis. The point which I wish to examine is really very curious, but it would take too long space to explain. Could you oblige me by taking the great trouble to send me in an old tin canister any of these orchids, permitting me, of course, to repay postage? It would be a great kindness, but perhaps I am unreasonable to make such a request. If you will inform me whether you have leisure so far to oblige me, I would tell you my movements, for on account of my own health and that of my daughter, I shall be on the move for the next two or three weeks.

I am sure I have much cause to apologise for the liberty which I have taken...

LETTER 595. TO A.G. MORE. Down, August 3rd, 1860.

I thank you most sincerely for sending me the Epipactis {palustris}. You can hardly imagine what an interesting morning's work you have given me, as the rostellum exhibited a quite new modification of structure. It has been extremely kind of you to take so very much trouble for me. Have you looked at the pollen-masses of the bee-Ophrys? I do not know whether the Epipactis grows near to your house: if it does, and any object takes you to the place (pray do not for a moment think me so very unreasonable as to ask you to go on purpose), would you be so kind {as} to watch the flowers for a quarter of an hour, and mark whether any insects (and what?) visit these flowers.

I should suppose they would crawl in by depressing the terminal portion of the labellum; and that when within the flower this terminal portion would resume its former position; and lastly, that the insect in crawling out would not depress the labellum, but would crawl out at back of flower. (595/1. The observations of Mr. William Darwin on Epipactis palustris given in the "Fertilisation of Orchids," Edition II., 1877, page 99, bear on this point. The chief fertilisers are hive-bees, which are too big to crawl into the flower. They cling to the labellum, and by depressing it open up the entrance to the flower. Owing to the elasticity of the labellum and its consequent tendency to spring up when released, the bees, "as they left the flower, seemed to fly rather upwards." This agrees with Darwin's conception of the mechanism of the flower as given in the first edition of the Orchid book, 1862, page 100, although at that time he imagined that the fertilising insect crawled into the flower. The extreme flexibility and elasticity of the labellum was first observed by Mr. More (see first edition, page 99). The description of the flower given in the above letter to Mr. More is not quite clear; the reader is referred to the "Fertilisation of Orchids," loc. cit.) An insect crawling out of a recently opened flower would, I believe, have parts of the pollen-masses adhering to the back or shoulder. I have seen this in Listera. How I should like to watch the Epipactis.

If you can it any time send me Spiranthes or Aceras or O. ustulata, you would complete your work of kindness.

P.S.—If you should visit the Epipactis again, would you gather a few of the lower flowers which have been opened for some time and have begun to wither a little, and observe whether pollen is well cleared out of anther-case. I have been struck with surprise that in nearly all the lower flowers sent by you, though much of the pollen has been removed, yet a good deal of pollen is left wasted within the anthers. I observed something of this kind in Cephalanthera grandiflora. But I fear that you will think me an intolerable bore.

LETTER 596. TO A.G. MORE. Down, August 5th, 1860.

I am infinitely obliged for your most clearly stated observations on the bee-orchis. It is now perfectly clear that something removes the pollen-masses far more with you than in this neighbourhood. But I am utterly puzzled about the foot-stalk being so often cut through. I should suspect snails. I yesterday found thirty-nine flowers, and of them only one pollen-mass in three flowers had been removed, and as these were extremely much-withered flowers I am not quite sure of the truth of this. The wind again is a new element of doubt. Your observations will aid me extremely in coming to some conclusion. (596/1. Mr. More's observations on the percentage of flowers in which the pollinia were absent are quoted in "Fertilisation of Orchids," Edition I., page 68.) I hope in a day or two to receive some day-moths, on the probosces of which I am assured the pollen-masses of the bee-orchis still adhere (596/2. He was doomed to disappointment. On July 17th, 1861, he wrote to Mr. More:—"I found the other day a lot of bee-Ophrys with the glands of the pollinia all in their pouches. All facts point clearly to eternal self-fertilisation in this species; yet I cannot swallow the bitter pill. Have you looked at any this year?")...

I wrote yesterday to thank you for the Epipactis. For the chance of your liking to look at what I have found: take a recently opened flower, drag gently up the stigmatic surface almost any object (the side of a hooked needle), and you will find the cap of the hemispherical rostellum comes off with a touch, and being viscid on under-surface, clings to needle, and as pollen-masses are already attached to the back of rostellum, the needle drags out much pollen. But to do this, the curiously projecting and fleshy summits of anther-cases must at some time be pushed back slightly. Now when an insect's head gets into the flower, when the flap of the labellum has closed by its elasticity, the insect would naturally creep out by the back-side of the flower. And mark when the insect flies to another flower with the pollen-masses adhering to it, if the flap of labellum did not easily open and allow free ingress to the insect, it would surely rub off the pollen on the upper petals, and so not leave it on stigma. It is to know whether I have rightly interpreted the structure of this whole flower that I am so curious to see how insects act. Small insects, I daresay, would crawl in and out and do nothing. I hope that I shall not have wearied you with these details.

If you would like to see a pretty and curious little sight, look to Orchis pyramidalis, and you will see that the sticky glands are congenitally united into a saddle-shaped organ. Remove this under microscope by pincers applied to foot-stalk of pollen-mass, and look quickly at the spontaneous movement of the saddle-shaped organs and see how beautifully adapted to seize proboscis of moth.

LETTER 597. TO J.D. HOOKER December 4th {1860}.

Many thanks about Apocynum and Meyen.

The latter I want about some strange movements in cells of Drosera, which Meyen alone seems to have observed. (597/1. No observations of Meyen are mentioned in "Insectivorous Plants.") It is very curious, but Trecul disbelieves that Drosera really clasps flies! I should very much wish to talk over Drosera with you. I did chloroform it, and the leaves which were already expanded did not recover thirty seconds of exposure for three days. I used the expression weight for the bit of hair which caused movement and weighed 1/78000 of a grain; but I do not believe it is weight, and what it is, I cannot after many experiments conjecture. (597/2. The doubt here expressed as to whether the result is due to actual weight is interesting in connection with Pfeffer's remarkable discovery that a smooth object in contact with the gland produces no effect if the plant is protected from all vibration; on an ordinary table the slight shaking which reaches the plant is sufficient to make the body resting on the gland tremble, and thus produce a series of varying pressures—under these circumstances the gland is irritated, and the tentacle moves. See Pfeffer, "Untersuchungen aus d. bot. Institut zu Tubingen," Volume I., 1885, page 483; also "Insectivorous Plants," Edition II., page 22.) The movement in this case does not depend on the chemical nature of substance. Latterly I have tried experiments on single glands, and a microscopical atom of raw meat causes such rapid movement that I could see it move like hand of clock. In this case it is the nature of the object. It is wonderful the rapidity of the absorption: in ten seconds weak solution of carbonate of ammonia changes not the colour, but the state of contents within the glands. In two minutes thirty seconds juice of meat has been absorbed by gland and passed from cell to cell all down the pedicel (or hair) of the gland, and caused the sap to pass from the cells on the upper side of the pedicel to the lower side, and this causes the curvature of the pedicel. I shall work away next summer when Drosera opens again, for I am much interested in subject. After the glandular hairs have curved, the oddest changes take place—viz., a segregation of the homogeneous pink fluid and necessary slow movements in the thicker matter. By Jove, I sometimes think Drosera is a disguised animal! You know that I always so like telling you what I do, that you must forgive me scribbling on my beloved Drosera. Farewell. I am so very glad that you are going to reform your ways; I am sure that you would have injured your health seriously. There is poor Dana has done actually nothing—cannot even write a letter—for a year, and it is hoped that in another YEAR he may quite recover.

After this homily, good night, my dear friend. Good heavens, I ought not to scold you, but thank you, for writing so long and interesting a letter.

LETTER 598. TO E. CRESY. Down, December 12th {1860?}.

After writing out the greater part of my paper on Drosera, I thought of so many points to try, and I wished to re-test the basis of one large set of experiments, namely, to feel still more sure than I am, that a drop of plain water never produces any effect, that I have resolved to publish nothing this year. For I found in the record of my daily experiments one suspicious case. I must wait till next summer. It will be difficult to try any solid substances containing nitrogen, such as ivory; for two quite distinct causes excite the movement, namely, mechanical irritation and presence of nitrogen. When a solid substance is placed on leaf it becomes clasped, but is released sooner than when a nitrogenous solid is clasped; yet it is difficult (except with raw meat and flies) to be sure of the result, owing to differences in vigour of different plants. The last experiments which I tried before my plants became too languid are very curious, and were tried by putting microscopical atoms on the gland itself of single hairs; and it is perfectly evident that an atom of human hair, 1/76000 of a grain (as ascertained by weighing a length of hair) in weight, causes conspicuous movement. I do not believe (for atoms of cotton thread acted) it is the chemical nature; and some reasons make me doubt whether it is actual weight; it is not the shadow; and I am at present, after many experiments, confounded to know what the cause is. That these atoms did really act and alter the state of the contents of all the cells in the glandular hair, which moved, was perfectly clear. But I hope next summer to make out a good deal more...

LETTER 599. TO J.D. HOOKER. Down, May 14th {1861}.

I have been putting off writing from day to day, as I did not wish to trouble you, till my wish for a little news will not let me rest...

I have no news to tell you, for I have had no interesting letters for some time, and have not seen a soul. I have been going through the "Cottage Gardener" of last year, on account chiefly of Beaton's articles (599/1. Beaton was a regular contributor to the "Cottage Gardener," and wrote various articles on cross breeding, etc., in 1861. One of these was in reply to a letter published in the "Cottage Gardener," May 14th, 1861, page 112, in which Darwin asked for information as to the Compositae and the hollyhock being crossed by insect visitors. In the number for June 8th, 1861, page 211, Darwin wrote on the variability of the central flower of the carrot and the peloria of the central flower in Pelargonium. An extract from a letter by Darwin on Leschenaultia, "Cottage Gardener," May 28th, 1861, page 151, is given in Letter 590, note.); he strikes me as a clever but d—d cock-sure man (as Lord Melbourne said), and I have some doubts whether to be much trusted. I suspect he has never recorded his experiment at the time with care. He has made me indignant by the way he speaks of Gartner, evidently knowing nothing of his work. I mean to try and pump him in the "Cottage Gardener," and shall perhaps defend Gartner. He alludes to me occasionally, and I cannot tell with what spirit. He speaks of "this Mr. Darwin" in one place as if I were a very noxious animal.

Let me have a line about poor Henslow pretty soon.

(599/2. In a letter of May 18th, 1861, Darwin wrote again:—)

By the way, thanks about Beaton. I have now read more of his writings, and one answer to me in "Cottage Gardener." I can plainly see that he is not to be trusted. He does not well know his own subject of crossing.

LETTER 600. TO J.D. HOOKER.

(600/1. Part of this letter has been published in "Life and Letters," III., page 265.)

2, Hesketh Crescent, Torquay {1861}.

...The beauty of the adaptation of parts seems to me unparalleled. I should think or guess {that} waxy pollen was most differentiated. In Cypripedium, which seems least modified, and a much exterminated group, the grains are single. In all others, as far as I have seen, they are in packets of four; and these packets cohere into many wedge-formed masses in Orchis, into eight, four, and finally two. It seems curious that a flower should exist which could, at most, fertilise only two other flowers, seeing how abundant pollen generally is; this fact I look at as explaining the perfection of the contrivance by which the pollen, so important from its fewness, is carried from flower to flower. By the way, Cephalanthera has single pollen-grains, but this seems to be a case of degradation, for the rostellum is utterly aborted. Oddly, the columns of pollen are here kept in place by very early penetration of pollen-tubes into the edge of the stigma; nevertheless, it receives more pollen by insect agency. Epithecia {Dichaea} has done me one good little turn. I often speculated how the caudicle of Orchis had been formed. (600/2. The gradation here suggested is thoroughly worked out in the "Fertilisation of Orchids," Edition I., page 323, Edition II., page 257.) I had noticed slight clouds in the substance half way down; I have now dissected them out, and I find they are pollen-grains fairly embedded and useless. If you suppose the pollen-grains to abort in the lower half of the pollinia of Epipactis, but the parallel elastic threads to remain and cohere, you have the caudicle of Orchis, and can understand the few embedded and functionless pollen-grains. I must not look at any more exotic orchids: hearty thanks for your offer. But if you would make one single observation for me on Cypripedium, I should be glad. Asa Gray writes to me that the outside of the pollen-masses is sticky in this genus; I find that the whole mass consists of pollen-grains immersed in a sticky brownish thick fluid. You could tell by a mere lens and penknife. If it is, as I find it, pollen could not get on the stigma without insect aid. Cypripedium confounds me much. I conjecture that drops of nectar are secreted by the surface of the labellum beneath the anthers and in front of the stigma, and that the shield over the anthers and the form of labellum is to compel insects to insert their proboscis all round both organs. (600/3. This view was afterwards given up.) It would be troublesome for you to look at this, as it is always bothersome to catch the nectar secreting, and the cup of the labellum gets filled with water by gardener's watering.

I have examined Listera ovata, cordata, and Neottia nidus avis: the pollen is uniform; I suspect you must have seen some observation founded on a mistake from the penetration and hardening of sticky fluid from the rostellum, which does penetrate the pollen a little.

It is mere virtue which makes me not wish to examine more orchids; for I like it far better than writing about varieties of cocks and hens and ducks. Nevertheless, I have just been looking at Lindley's list in the "Vegetable Kingdom," and I cannot resist one or two of his great division of Arethuseae, which includes Vanilla. And as I know so well the Ophreae, I should like (God forgive me) any one of the Satyriadae, Disidae and Corycidae.

I fear my long lucubrations will have wearied you, but it has amused me to write, so forgive me.

LETTER 601. TO J.D. HOOKER.

(601/1. Part of the following letter is published in the "Life and Letters," the remainder, with the omission of part bearing on the Glen Roy problem, is now given as an example of the varied botanical assistance Darwin received from Sir Joseph Hooker. For the part relating to Verbascum see the "Variation of Animals and Plants," Edition II., 1875, Volume II., page 83. The point is that the white and yellow flowered plants which occur in two species of Verbascum are undoubted varieties, yet "the sterility which results from the crossing of the differently coloured varieties of the same species is fully as great as that which occurs in many cases when distinct species are crossed."

The sterility of the long-styled form (B) of Linum grandiflorum, with its own pollen is described in "Forms of Flowers," Edition II., page 87: his conclusions on the short-styled form (A) differ from those in the present letter.)

September 28th {1861}.

I am going to beg for help, and I will explain why I want it.

You offer Cypripedium; I should be very glad of a specimen, and of any good-sized Vandeae, or indeed any orchids, for this reason: I never thought of publishing separately, and therefore did not keep specimens in spirits, and now I should be very glad of a few woodcuts to illustrate my few remarks on exotic orchids. If you can send me any, send them by post in a tin canister on middle of day of Saturday, October 5th, for Sowerby will be here.

Secondly: Have you any white and yellow varieties of Verbascum which you could give me, or propagate for me, or LEND me for a year? I have resolved to try Gartner's wonderful and repeated statement, that pollen of white and yellow varieties, whether used on the varieties or on DISTINCT species, has different potency. I do not think any experiment can be more important on the origin of species; for if he is correct we certainly have what Huxley calls new physiological species arising. I should require several species of Verbascum besides the white and yellow varieties of the same species. It will be tiresome work, but if I can anyhow get the plants, it shall be tried.

Thirdly: Can you give me seeds of any Rubiaceae of the sub-order Cinchoneae, as Spermacoce, Diodia, Mitchella, Oldenlandia? Asa Gray says they present two forms like Primula. I am sure that this subject is well worth working out. I have just almost proved a very curious case in Linum grandiflorum which presents two forms, A and B. Pollen of A is perfectly fertile on stigma of A. But pollen of B is absolutely barren on its own stigma; you might as well put so much flour on it. It astounded me to see the stigmas of B purple with its own pollen; and then put a few grains of similar-looking pollen of A on them, and the germen immediately and always swelled; those not thus treated never swelling.

Fourthly: Can you give me any very hairy Saxifraga (for their functions) {i.e. the functions of the hairs}?

I send you a resume of my requests, to save you trouble. Nor would I ask for so much aid if I did not think all these points well worth trying to investigate.

My dear old friend, a letter from you always does me a world of good. And, the Lord have mercy on me, what a return I make.

LETTER 602. TO J.D. HOOKER. Down, October 4th {1861}.

Will you have the kindness to read the enclosed, and look at the diagram. Six words will answer my question. It is not an important point, but there is to me an irresistible charm in trying to make out homologies. (602/1. In 1880 he wrote to Mr. Bentham: "It was very kind of you to write to me about the Orchideae, for it has pleased me to an extreme degree that I could have been of the least use to you about the nature of the parts."—"Life and Letters," III., page 264.) You know the membranous cup or clinandrum, in many orchids, behind the stigma and rostellum: it is formed of a membrane which unites the filament of the normal dorsal anther with the edges of the pistil. The clinandrum is largely developed in Malaxis, and is of considerable importance in retaining the pollinia, which as soon as the flower opens are quite loose.

The appearance and similarity of the tissues, etc., at once gives suspicion that the lateral membranes of the clinandrum are the two other and rudimentary anthers, which in Orchis and Cephalanthera, etc., exist as mere papillae, here developed and utilised.

Now for my question. Exactly in the middle of the filament of the normal anther, and exactly in the middle of the lateral membrane of the clinandrum, and running up to the same height, are quite similar bundles of spiral vessels; ending upwards almost suddenly. Now is not this structure a good argument that I interpret the homologies of the sides of clinandrum rightly? (602/2. Though Robert Brown made use of the spiral vessels of orchids, yet according to Eichler, "Bluthendiagramme," 1875, Volume I., page 184, Darwin was the first to make substantial additions to the conclusions deducible from the course of the vessels in relation to the problem of the morphology of these plants. Eichler gives Darwin's diagram side by side with that of Van Tieghem without attempting to decide between the differences in detail by which they are characterised.)

I find that the great Bauer does not draw very correctly! (602/3. F. Bauer, whom Pritzel calls "der grosste Pflanzenmaler." The reference is to his "Illustrations of Orchidaceous Plants, with Notes and Prefatory Remarks by John Lindley," London, 1830-38, Folio. See "Fertilisation of Orchids," Edition II., page 82.) And, good Heavens, what a jumble he makes on functions.

LETTER 603. TO J.D. HOOKER. Down, October 22nd. {1861}.

Acropera is a beast,—stigma does not open, everything seems contrived that it shall NOT be anyhow fertilised. There is something very odd about it, which could only be made out by incessant watching on several individual plants.

I never saw the very curious flower of Canna; I should say the pollen was deposited where it is to prevent inevitable self-fertilisation. You have no time to try the smallest experiment, else it would be worth while to put pollen on some stigmas (supposing that it does not seed freely with you). Anyhow, insects would probably carry pollen from flower to flower, for Kurr states the tube formed by pistil, stamen and "nectarblatt" secretes (I presume internally) much nectar. Thanks for sending me the curious flower.

Now I want much some wisdom; though I must write at considerable length, your answer may be very brief.

In R. Brown's admirable paper in the "Linnean Transacts." (603/4. Volume XVI., page 685.) he suggests (and Lindley cautiously agrees) that the flower of orchids consists of five whorls, the inner whorl of the two whorls of anthers being all rudimentary, and when the labellum presents ridges, two or three of the anthers of both whorls {are} combined with it. In the ovarium there are six bundles of vessels: R. Brown judged by transverse sections. It occurred to me, after what you said, to trace the vessels longitudinally, and I have succeeded well. Look at my diagram (which please return, for I am transported with admiration at it), which shows the vessels which I have traced, one bundle to each of fifteen theoretical organs, and no more. You will see the result is nothing new, but it seems to confirm strongly R. Brown, for I have succeeded (perhaps he did, but he does not say so) in tracing the vessels belonging to each organ in front of each other to the same bundle in the ovarium: thus the vessels going to the lower sepal, to the side of the labellum, and to one stigma (when there are two) all distinctly branch from one ovarian bundle. So in other cases, but I have not completely traced (only seen) that going to the rostellum. But here comes my only point of novelty: in all orchids as yet looked at (even one with so simple a labellum as Gymnadenia and Malaxis) the vessels on the two sides of the labellum are derived from the bundle which goes to the lower sepal, as in the diagram. This leads me to conclude that the labellum is always a compound organ. Now I want to know whether it is conceivable that the vessels coming from one main bundle should penetrate an organ (the labellum) which receives its vessels from another main bundle? Does it not imply that all that part of the labellum which is supplied by vessels coming from a lateral bundle must be part of a primordially distinct organ, however closely the two may have become united? It is curious in Gymnadenia to trace the middle anterior bundle in the ovarium: when it comes to the orifice of the nectary it turns and runs right down it, then comes up the opposite side and runs to the apex of the labellum, whence each side of the nectary is supplied by vessels from the bundles, coming from the lower sepals. Hence even the thin nectary is essentially, I infer, tripartite; hence its tendency to bifurcation at its top. This view of the labellum always consisting of three organs (I believe four when thick, as in Mormodes, at base) seems to me to explain its great size and tripartite form, compared with the other petals. Certainly, if I may trust the vessels, the simple labellum of Gymnadenia consists of three organs soldered together. Forgive me for writing at such length; a very brief answer will suffice. I am desperately interested in the subject: the destiny of the whole human race is as nothing to the course of vessels of orchids...

What plant has the most complex single stigma and pistil? The most complex I, in my ignorance, can think of is in Iris. I want to know whether anything beats in modification the rostellum of Catasetum. To-morrow I mean to be at Catasetum. Hurrah! What species is it? It is wonderfully different from that which Veitch sent me, which was C. saccatum.

According to the vessels, an orchid flower consists of three sepals and two petals free; and of a compound organ (its labellum), consisting of one petal and of two (or three) modified anthers; and of a second compound body consisting of three pistils, one normal anther, and two modified anthers often forming the sides of the clinandrum.

LETTER 604. TO JOHN LINDLEY.

(604/1. It was in the autumn of 1861 that Darwin made up his mind to publish his Orchid work as a book, rather than as a paper in the Linnean Society's "Journal." (604/2. See "Life and Letters," III., page 266.) The following letter shows that the new arrangement served as an incitement to fresh work.)

Down, October 25th {1861?}

Mr. James Veitch has been most generous. I did not know that you had spoken to him. If you see him pray say I am truly grateful; I dare not write to a live Bishop or a Lady, but if I knew the address of "Rucker"? and might use your name as introduction, I might write. I am half mad on the subject. Hooker has sent me many exotics, but I stopped him, for I thought I should make a fool of myself; but since I have determined to publish I much regret it.

(605/1. The three upper curved outlines, two of which passing through the words "upper sepal," "upper petal," "lower sepal," were in red in the original; for explanation see text.)

LETTER 605. TO J.D. HOOKER.

(605/2. The following letter is of interest because it relates to one of the two chief difficulties Darwin met with in working out the morphology of the orchid flower. In the orchid book (605/3. Edition I., page 303.) he wrote, "This anomaly {in Habenaria} is so far of importance, as it throws some doubt on the view which I have taken of the labellum being always an organ compounded of one petal and two petaloid stamens." That is to say, it leaves it open for a critic to assert that the vessels which enter the sides of the labellum are lateral vessels of the petal and do not necessarily represent petaloid stamens. In the sequel he gives a satisfactory answer to the supposed objector.)

Down, November 10th, {1861}.

For the love of God help me. I believe all my work (about a fortnight) is useless. Look at this accursed diagram of the butterfly-orchis {Habenaria}, which I examined after writing to you yesterday, when I thought all my work done. Some of the ducts of the upper sepal (605/4. These would be described by modern morphologists as lower, not upper, sepals, etc. Darwin was aware that he used these terms incorrectly.) and upper petal run to the wrong bundles on the column. I have seen no such case.

This case apparently shows that not the least reliance can be placed on the course of ducts. I am sure of my facts.

There is great adhesion and extreme displacement of parts where the organs spring from the top of the ovarium. Asa Gray says ducts are very early developed, and it seems to me wonderful that they should pursue this course. It may be said that the lateral ducts in the labellum running into the antero-lateral ovarian bundle is no argument that the labellum consists of three organs blended together.

In desperation (and from the curious way the base of upper petals are soldered at basal edges) I fancied the real form of upper sepal, upper petal and lower sepal might be as represented by red lines, and that there had been an incredible amount of splitting of sepals and petals and subsequent fusion.

This seems a monstrous notion, but I have just looked at Bauer's drawing of allied Bonatea, and there is a degree of lobing of petals and sepals which would account for anything.

Now could you spare me a dry flower out of your Herbarium of Bonatea speciosa (605/5. See "Fertilisation of Orchids," Edition I., page 304 (note), where the resemblances between the anomalous vessels of Bonatea and Habenaria are described. On November 14th, 1861, he wrote to Sir Joseph: "You are a true friend in need. I can hardly bear to let the Bonatea soak long enough."), that I might soak and look for ducts. If I cannot explain the case of Habenaria all my work is smashed. I was a fool ever to touch orchids.

LETTER 606. TO J.D. HOOKER. Down, November 17th {1861}.

What two very interesting and useful letters you have sent me. You rather astound me with respect to value of grounds of generalisation in the morphology of plants. It reminds me that years ago I sent you a grass to name, and your answer was, "It is certainly Festuca (so-and-so), but it agrees as badly with the description as most plants do." I have often laughed over this answer of a great botanist...Lindley, from whom I asked for an orchid with a simple labellum, has most kindly sent me a lot of what he marks "rare" and "rarissima" of peloric orchids, etc., but as they are dried I know not whether they will be of use. He has been most kind, and has suggested my writing to Lady D. Nevill, who has responded in a wonderfully kind manner, and has sent a lot of treasures. But I must stop; otherwise, by Jove, I shall be transformed into a botanist. I wish I had been one; this morphology is surprisingly interesting. Looking to your note, I may add that certainly the fifteen alternating bundles of spiral vessels (mingled with odd beadlike vessels in some cases) are present in many orchids. The inner whorl of anther ducts are oftenest aborted. I must keep clear of Apostasia, though I have cast many a longing look at it in Bauer. (606/1. Apostasia has two fertile anthers like Cypripedium. It is placed by Engler and Prantl in the Apostasieae or Apostasiinae, among the Orchideae, by others in a distinct but closely allied group.)

I hope I may be well enough to read my own paper on Thursday, but I have been very seedy lately. (606/2. "On the two Forms, or Dimorphic Condition, in the Species of the Genus Primula," "Linn. Soc. Journ." 1862. He did read the paper, but it cost him the next day in bed. "Life and Letters," III., page 299.) I see there is a paper at the Royal on the same night, which will more concern you, on fossil plants of Bovey (606/3. Oswald Heer, "The Fossil Flora of Bovey Tracey," "Phil. Trans. R. Soc." 1862, page 1039.), so that I suppose I shall not have you; but you must read my paper when published, as I shall very much like to hear what you think. It seems to me a large field for experiment. I shall make use of my Orchid little volume in illustrating modification of species doctrine, but I keep very, very doubtful whether I am not doing a foolish action in publishing. How I wish you would keep to your old intention and write a book on plants. (606/4. Possibly a book similar to that described in Letter 696.)

LETTER 607. TO G. BENTHAM. Down, November 26th {1861}.

Our notes have crossed on the road. I know it is an honour to have a paper in the "Transactions," and I am much obliged to you for proposing it, but I should greatly prefer to publish in the "Journal." Nor does this apply exclusively to myself, for in old days at the Geological Society I always protested against an abstract appearing when the paper itself might appear. I abominate also the waste of time (and it would take me a day) in making an abstract. If the referee on my paper should recommend it to appear in the "Transactions," will you be so kind as to lay my earnest request before the Council that it may be permitted to appear in the "Journal?"

You must be very busy with your change of residence; but when you are settled and have some leisure, perhaps you will be so kind as to give me some cases of dimorphism, like that of Primula. Should you object to my adding them to those given me by A. Gray? By the way, I heard from A. Gray this morning, and he gives me two very curious cases in Boragineae.

LETTER 608. TO JOHN LINDLEY.

(608/1. In the following fragment occurs the earliest mention of Darwin's work on the three sexual forms of Catasetum tridentatum. Sir R. Schomburgk (608/2. "Trans. Linn. Soc." XVII., page 522.) described Catasetum tridentatum, Monacanthus viridis and Myanthus barbatus occurring on a single plant, but it remained for Darwin to make out that they are the male, female and hermaphrodite forms of a single species. (608/3. "Fertilisation of Orchids," Edition I., page 236; Edition II., page 196.)

With regard to the species of Acropera (Gongora) (608/4. Acropera Loddigesii = Gongora galeata: A. luteola = G. fusca

("Index Kewensis").) he was wrong in his surmise. The apparent sterility seems to be explicable by Hildebrand's discovery (608/5. "Bot. Zeitung," 1863 and 1865.) that in some orchids the ovules are not developed until pollinisation has occurred. (608/6. "Fertilisation of Orchids," Edition II., page 172. See Letter 633.))

Down, December 15th {1861}.

I am so nearly ready for press that I will not ask for anything more; unless, indeed, you stumbled on Mormodes in flower. As I am writing I will just mention that I am convinced from the rudimentary state of the ovules, and from the state of the stigma, that the whole plant of Acropera luteola (and I believe A. Loddigesii) is male. Have you ever seen any form from the same countries which could be the females? Of course no answer is expected unless you have ever observed anything to bear on this. I may add {judging from the} state of the ovules and of the pollen {that}:—

Catasetum tridentatum is male (and never seeds, according to Schomburgk, whom you have accidentally misquoted in the "Vegetable Kingdom"). Monacanthus viridis is female. Myanthus barbatus is the hermaphrodite form of same species.

LETTER 609. TO J.D. HOOKER. Down, December 18th {1861}.

Thanks for your note. I have not written for a long time, for I always fancy, busy as you are, that my letters must be a bore; though I like writing, and always enjoy your notes. I can sympathise with you about fear of scarlet fever: to the day of my death I shall never forget all the sickening fear about the other children, after our poor little baby died of it. The "Genera Plantarum" must be a tremendous work, and no doubt very valuable (such a book, odd as it may appear, would be very useful even to me), but I cannot help being rather sorry at the length of time it must take, because I cannot enter on and understand your work. Will you not be puzzled when you come to the orchids? It seems to me orchids alone would be work for a man's lifetime; I cannot somehow feel satisfied with Lindley's classification; the Malaxeae and Epidendreae seem to me very artificially separated. (609/1. Pfitzer (in the "Pflanzenfamilien") places Epidendrum in the Laeliinae-Cattleyeae, Malaxis in the

Liparidinae. He states that Bentham united the Malaxideae and Epidendreae.) Not that I have seen enough to form an opinion worth anything.

Your African plant seems to be a vegetable Ornithorhynchus, and indeed much more than that. (609/2. See Sir J.D. Hooker, "On Welwitschia, a new genus of Gnetaceae." "Linn. Soc. Trans." XXIV., 1862-3.) The more I read about plants the more I get to feel that all phanerogams seem comparable with one class, as lepidoptera, rather than with one kingdom, as the whole insecta. (609/3. He wrote to Hooker (December 28th, 1861): "I wrote carelessly about the value of phanerogams; what I was thinking of was that the sub-groups seemed to blend so much more one into another than with most classes of animals. I suspect crustacea would show more difference in the extreme forms than phanerogams, but, as you say, it is wild speculation. Yet it is very strange what difficulty botanists seem to find in grouping the families together into masses.")

Thanks for your comforting sentence about the accursed ducts (accursed though they be, I should like nothing better than to work at them in the allied orders, if I had time). I shall be ready for press in three or four weeks, and have got all my woodcuts drawn. I fear much that publishing separately will prove a foolish job, but I do not care much, and the work has greatly amused me. The Catasetum has not flowered yet!

In writing to Lindley about an orchid which he sent me, I told him a little about Acropera, and in answer he suggests that Gongora may be its female. He seems dreadfully busy, and I feel that I have more right to kill you than to kill him; so can you send me one or at most two dried flowers of Gongora? if you know the habitat of Acropera luteola, a Gongora from the same country would be the best, but any true Gongora would do; if its pollen should prove as rudimentary as that of Monacanthus relatively to Catasetum, I think I could easily perceive it even in dried specimens when well soaked.

I have picked a little out of Lecoq, but it is awful tedious hunting.

Bates is getting on with his natural history travels in one volume. (609/4. H.W. Bates, the "Naturalist on the Amazons," 1863. See Volume I., Letters 123, 148, also "Life and Letters," Volume II., page

381.) I have read the first chapter in MS., and I think it will be an excellent book and very well written; he argues, in a good and new way to me, that tropical climate has very little direct relation to the gorgeous colouring of insects (though of course he admits the tropics have a far greater number of beautiful insects) by taking all the few genera common to Britain and Amazonia, and he finds that the species proper to the latter are not at all more beautiful. I wonder how this is in species of the same restricted genera of plants.

If you can remember it, thank Bentham for getting my Primula paper printed so quickly. I do enjoy getting a subject off one's hands completely.

I have now got dimorphism in structure in eight natural orders just like Primula. Asa Gray sent me dried flowers of a capital case in Amsinkia spectabilis, one of the Boragineae. I suppose you do not chance to have the plant alive at Kew.

LETTER 610. TO A.G. MORE. Down, June 7th, 1862.

If you are well and have leisure, will you kindly give me one bit of information: Does Ophrys arachnites occur in the Isle of Wight? or do the intermediate forms, which are said to connect abroad this species and the bee-orchis, ever there occur?

Some facts have led me to suspect that it might just be possible, though improbable in the highest degree, that the bee {orchis} might be the self-fertilising form of O. arachnites, which requires insects' aid, something {in the same way} as we have self-fertilising flowers of the violet and others requiring insects. I know the case is widely different, as the bee is borne on a separate plant and is incomparably commoner. This would remove the great anomaly of the bee being a perpetual self-fertiliser. Certain Malpighiaceae for years produce only one of the two forms. What has set my head going on this is receiving to-day a bee having one alone of the best marked characters of O. arachnites. (610/1. Ophrys arachnites is probably more nearly allied to O. aranifera than to O. apifera. For a case somewhat analogous to that suggested see the description of O. scolopax in "Fertilisation of Orchids," Edition II., page 52.) Pray forgive me troubling you.

LETTER 611. TO G. BENTHAM. Down, June 22nd {1862?}.

Here is a piece of presumption! I must think that you are mistaken in ranking Hab{enaria} chlorantha (611/1. In Hooker's "Students' Flora," 1884, page 395, H. chlorantha is given as a subspecies of H. bifolia. Sir J.D. Hooker adds that they are "according to Darwin, distinct, and require different species of moths to fertilise them. They vary in the position and distances of their anther-cells, but intermediates occur." See "Fertilisation of Orchids," Edition II., page 73.) as a variety of H. bifolia; the pollen-masses and stigma differ more than in most of the best species of Orchis. When I first examined them I remember telling Hooker that moths would, I felt sure, fertilise them in a different manner; and I have just had proof of this in a moth sent me with the pollinia (which can be easily recognised) of H. chlorantha attached to its proboscis, instead of to the sides of its face, as an H. bifolia.

Forgive me scribbling this way; but when a man gets on his hobby-horse he always is run away with. Anyhow, nothing here requires any answer.

LETTER 612. TO J.D. HOOKER. Down, {September} 14th {1862}.

Your letter is a mine of wealth, but first I must scold you: I cannot abide to hear you abuse yourself, even in joke, and call yourself a stupid dog. You, in fact, thus abuse me, because for long years I have looked up to you as the man whose opinion I have valued more on any scientific subject than any one else in the world. I continually marvel at what you know, and at what you do. I have been looking at the "Genera" (612/1. "Genera Plantarum," by Bentham and Hooker, Volume I., Part I., 1862.), and of course cannot judge at all of its real value, but I can judge of the amount of condensed facts under each family and genus.

I am glad you know my feeling of not being able to judge about one's own work; but I suspect that you have been overworking. I should think you could not give too much time to Wellwitchia (I spell it different every time I write it) (612/2. "On Welwitschia," "Linn. Soc. Trans." {1862}, XXIV., 1863.); at least I am sure in the animal kingdom monographs cannot be too long on the osculant groups.

Hereafter I shall be excessively glad to read a paper about Aldrovanda (612/3. See "Insectivorous Plants," page 321.), and am very much obliged for reference. It is pretty to see how the caught flies support Drosera; nothing else can live.

Thanks about plants with two kinds of anthers. I presume (if an included flower was a Cassia) (612/4. Todd has described a species of Cassia with an arrangement of stamens like the Melastomads. See Chapter 2.X.II.) that Cassia is like lupines, but with some stamens still more rudimentary. If I hear I will return the three Melastomads; I do not want them, and, indeed, have cuttings. I am very low about them, and have wasted enormous labour over them, and cannot yet get a glimpse of the meaning of the parts. I wish I knew any botanical collector to whom I could apply for seeds in their native land of any Heterocentron or Monochoetum; I have raised plenty of seedlings from your plants, but I find in other cases that from a homomorphic union one generally gets solely the parent form. Do you chance to know of any botanical collector in Mexico or Peru? I must not now indulge myself with looking after vessels and homologies. Some future time I will indulge myself. By the way, some time I want to talk over the alternation of organs in flowers with you, for I think I must have quite misunderstood you that it was not explicable.

I found out the Verbascum case by pure accident, having transplanted one for experiment, and finding it to my astonishment utterly sterile. I formerly thought with you about rarity of natural hybrids, but I am beginning to change: viz., oxlips (not quite proven), Verbascum, Cistus (not quite proven), Aegilops triticoides (beautifully shown by Godron), Weddell's and your orchids (612/5. For Verbascum see "Animals and Plants," Edition II., Volume I., page 356; for Cistus, Ibid., Edition II., Volume I., page 356, Volume II., page 122; for Aegilops, Ibid., Edition II., Volume I., page 330, note.), and I daresay many others recorded. Your letters are one of my greatest pleasures in life, but I earnestly beg you never to write unless you feel somewhat inclined, for I know how hard you work, as I work only in the morning it is different with me, and is only a pleasant relaxation. You will never know how much I owe to you for your constant kindness and encouragement.

LETTER 613. TO JOHN LUBBOCK (Lord Avebury). Cliff Cottage, Bournemouth, Hants, September 2nd {1862}.

Hearty thanks for your note. I am so glad that your tour answered so splendidly. My poor patients (613/1. Mrs. Darwin and one of her sons, both recovering from scarlet fever.) got here yesterday, and are doing well, and we have a second house for the well ones. I write now in great haste to beg you to look (though I know how busy you are, but I cannot think of any other naturalist who would be careful) at any field of common red clover (if such a field is near you) and watch the hive-bees: probably (if not too late) you will see some sucking at the mouth of the little flowers and some few sucking at the base of the flowers, at holes bitten through the corollas. All that you will see is that the bees put their heads deep into the {flower} head and rout about. Now, if you see this, do for Heaven's sake catch me some of each and put in spirits and keep them separate. I am almost certain that they belong to two castes, with long and short proboscids. This is so curious a point that it seems worth making out. I cannot hear of a clover field near here.

LETTER 614. TO JOHN LUBBOCK (Lord Avebury). Cliff Cottage, Bournemouth, Wednesday, September 3rd {1862}.

I beg a million pardons. Abuse me to any degree, but forgive me: it is all an illusion (but almost excusable) about the bees. (614/1. H. Muller, "Fertilisation of Flowers," page 186, describes hive-bees visiting Trifolium pratense for the sake of the pollen. Darwin may perhaps have supposed that these were the variety of bees whose proboscis was long enough to reach the nectar. In "Cross and Self Fertilisation," page 361, Darwin describes hive-bees apparently searching for a secretion on the calyx. In the same passage in "Cross and Self Fertilisation" he quotes Muller as stating that hive-bees obtain nectar from red clover by breaking apart the petals. This seems to us a misinterpretation of the "Befruchtung der Blumen," page 224.) I do so hope that you have not wasted any time from my stupid blunder. I hate myself, I hate clover, and I hate bees.

Laid flat open, showing by dotted lines the course of spiral vessels in all the organs; sepals and petals shown on one side alone, with the stamens on one side above with course of vessels indicated, but not prolonged. Near side of pistil with one spiral vessel cut away.)

LETTER 615. TO J.D. HOOKER. Cliff Cottage, Bournemouth, September 11th, 1862.

You once told me that Cruciferous flowers were anomalous in alternation of parts, and had given rise to some theory of dedoublement.

Having nothing on earth to do here, I have dissected all the spiral vessels in a flower, and instead of burning my diagrams, I send them to you, you miserable man. But mind, I do not want you to send me a discussion, but just some time to say whether my notions are rubbish, and then burn the diagrams. It seems to me that all parts alternate beautifully by fours, on the hypothesis that two short stamens of outer whorl are aborted (615/1. The view given by Darwin is (according to Eichler) that previously held by Knuth, Wydler, Chatin, and others. Eichler himself believes that the flower is dimerous, the four longer stamens being produced by the doubling or splitting of the upper (i.e. antero-posterior) pair of stamens. If this view is correct, and there are good reasons for it, it throws much suspicion on the evidence afforded by the course of vessels, for there is no trace of the common origin of the longer stamens in the diagram. Again, if Eichler is right, the four vessels shown in the section of the ovary are misleading. Darwin afterwards gave a doubtful explanation of this, and concluded that the ovary is dimerous. See Letter 616.); and this view is perhaps supported by their being so few, only two sub-bundles in the two lateral main bundles, where I imagine two short stamens have aborted, but I suppose there is some valid objection against this notion. The course of the side vessels in the sepals is curious, just like my difficulty in Habenaria. (615/2. See Letter 605.) I am surprised at the four vessels in the ovarium. Can this indicate four confluent pistils? anyhow, they are in the right alternating position. The nectary within the base of the shorter stamens seems to cause the end sepals apparently, but not really, to arise beneath the lateral sepals.

I think you will understand my diagrams in five minutes, so forgive me for bothering you. My writing this to you reminds me of a letter which I received yesterday from Claparede, who helped the French translatress of the "Origin" (615/3. The late Mlle. Royer.), and he tells me he had difficulty in preventing her (who never looked at a bee's cell) from altering my whole description, because she affirmed that an hexagonal prism must have an hexagonal base! Almost everywhere in the "Origin," when I express great doubt, she appends a note explaining the difficulty, or saying that there is none whatever!! (615/4. See "Life and Letters," II., page 387.) It is really curious to know what conceited people there are in the world (people, for instance, after looking at one Cruciferous flower, explain their homologies).

This is a nice, but most barren country, and I can find nothing to look at. Even the brooks and ponds produce nothing. The country is like Patagonia. my wife is almost well, thank God, and Leonard is wonderfully improved ...Good God, what an illness scarlet fever is! The doctor feared rheumatic fever for my wife, but she does not know her risk. It is now all over.

LETTER 616. TO J.D. HOOKER. Cliff Cottage, Bournemouth, Thursday Evening {September 18th, 1862}.

Thanks for your pleasant note, which told me much news, and upon the whole good, of yourselves. You will be awfully busy for a time, but I write now to say that if you think it really worth while to send me a few Dielytra, or other Fumariaceous plant (which I have already tried in vain to find here) in a little tin box, I will try and trace the vessels; but please observe, I do not know that I shall have time, for I have just become wonderfully interested in experimenting on Drosera with poisons, etc. If you send any Fumariaceous plant, send if you can, also two or three single balsams. After writing to you, I looked at vessels of ovary of a sweet-pea, and from this and other cases I believe that in the ovary the midrib vessel alone gives homologies, and that the vessels on the edge of the carpel leaf often run into the wrong bundle, just like those on the sides of the sepals. Hence I {suppose} in Crucifers that the ovarium consists of two pistils; AA being the midrib vessels, and BB being those formed of the vessels on edges of the two carpels, run together, and going to wrong bundles. I came to this conclusion before receiving your letter.

I wonder why Asa Gray will not believe in the quaternary arrangement; I had fancied that you saw some great difficulty in the case, and that made me think that my notion must be wrong.

LETTER 617. TO J.D. HOOKER. Down, September 27th {1862}.

Masdevallia turns out nothing wonderful (617/1. This may refer to the homologies of the parts. He was unable to understand the mechanism of the flower.—"Fertilisation of Orchids," Edition II., page 136.); I was merely stupid about it; I am not the less obliged for its loan, for if I had lived till 100 years old I should have been uneasy about it. It shall be returned the first day I send to Bromley. I have steamed the other plants, and made the sensitive plant very sensitive, and shall soon try some experiments on it. But after all it will only be amusement. Nevertheless, if not causing too much trouble, I should be very glad of a few young plants of this and Hedysarum in summer (617/2. Hedysarum or Desmodium gyrans, the telegraph-plant.), for this kind of work takes no time and amuses me much. Have you seeds of Oxalis sensitiva, which I see mentioned in books? By the way, what a fault it is in Henslow's "Botany" that he gives hardly any references; he alludes to great series of experiments on absorption of poison by roots, but where to find them I cannot guess. Possibly the all-knowing Oliver may know. I can plainly see that the glands of Drosera, from rapid power (almost instantaneous) of absorption and power of movement, give enormous advantage for such experiments. And some day I will enjoy myself with a good set to work; but it will be a great advantage if I can get some preliminary notion on other sensitive plants and on roots.

Oliver said he would speak about some seeds of Lythrum hyssopifolium being preserved for me. By the way, I am rather disgusted to find I cannot publish this year on Lythrum salicaria; I must make 126 additional crosses. All that I expected is true, but I have plain indication of much higher complexity. There are three pistils of different structure and functional power, and I strongly suspect altogether five kinds of pollen all different in this one species! (617/3. See "Forms of Flowers," Edition II., page 138.)

By any chance have you at Kew any odd varieties of the common potato? I want to grow a few plants of every variety, to compare flowers, leaves, fruit, etc., as I have done with peas, etc. (617/4. "Animals and Plants," Edition II., Volume I., page 346. Compare also the similar facts with regard to cabbages, loc. cit., page 342. Some of the original specimens are in the Botanical Museum at Cambridge.)

LETTER 618. J.D. HOOKER TO CHARLES DARWIN.

(618/1. The following is part of Letter 144, Volume I. It refers to reviews of "Fertilisation of Orchids" in the "Gardeners' Chronicle," 1862, pages 789, 863, 910, and in the "Natural History Review," October, 1862, page 371.)

November 7th, 1862.

Dear old Darwin,

I assure you it was not my fault! I worried Lindley over and over again to notice your orchid book in the "Chronicle" by the very broadest hints man could give. (618/2. See "Life and Letters," III., page 273.) At last he said, "really I cannot, you must do it for me," and so I did—volontiers. Lindley felt that he ought to have done it himself, and my main effort was to write it "a la Lindley," and in this alone I have succeeded—that people all think it is exactly Lindley's style!!! which diverts me vastly. The fact is, between ourselves, I fear that poor L. is breaking up—he said that he could not fix his mind on your book. He works himself beyond his mental or physical powers.

And now, my dear Darwin, I may as well make a clean breast of it, and tell you that I wrote the "Nat. Hist. Review" notice too—to me a very difficult task, and one I fancied I failed in, comparatively. Of this you are no judge, and can be none; you told me to tell Oliver it pleased you, and so I am content and happy.

LETTER 619. TO W.E. DARWIN. Down, 4th {about 1862-3?}

I have been looking at the fertilisation of wheat, and I think possibly you might find something curious. I observed in almost every one of the pollen-grains, which had become empty and adhered to (I suppose the viscid) branching hairs of the stigma, that the pollen-tube was always (?) emitted on opposite side of grain to that in contact with the branch of the stigma. This seems very odd. The branches of the stigma are very thin, formed apparently of three rows of cells of hardly greater diameter than pollen-tube. I am astonished that the tubes should be able to penetrate the walls. The specimens examined (not carefully by me) had pollen only during few hours on stigma; and the mere SUSPICION has crossed me that the pollen-tubes crawl down these branches to the base and then penetrate the stigmatic tissue. (619/1. See Strasburger's "Neue Untersuchungen uber den Befruchtungsvorgang bei den Phanerogamen," 1884. In Alopecurus pratensis he describes the pollen as adhering to the end of a projection from the stigma where it germinates; the tube crawls along or spirally round this projection until it reaches the angle where the stigmatic branch is given off; here it makes an entrance and travels in the middle lamella between two cells.) The paleae open for a short period for stigma to be dusted, and then close again, and such travelling down would take place under protection. High powers and good adjustment are necessary. Ears expel anthers when kept in water in room; but the paleae apparently do not open and expose stigma; but the stigma could easily be artificially impregnated.

If I were you I would keep memoranda of points worth attending to.

2.X.II. MELASTOMACEAE, 1862-1881.

(620/1. The following series of letters (620-630) refers to the Melastomaceae and certain other flowers of analogous form. In 1862 Darwin attempted to explain the existence of two very different sets of stamens in these plants as a case of dimorphism, somewhat analogous to the state of things in Primula. In this view he was probably wrong, but this does not diminish the interest of the crossing experiments described in the letters. The persistence of his interest in this part of the subject is shown in the following passage from his Preface to the English translation of H. Muller's "Befruchtung der Blumen"; the passage is dated February, 1882, but was not published until the following year.

"There exist also some few plants the flowers of which include two sets of stamens, differing in the shape of the anthers and in the colour of the pollen; and at present no one knows whether this difference has any functional significance, and this is a point which ought to be determined."

It is not obvious why he spoke of the problem as if no light had been thrown on it, since in 1881 Fritz Muller had privately (see Letter 629) offered an explanation which Darwin was strongly inclined to accept. (620/2. H. Muller published ("Nature," August 4th, 1881) a letter from his brother Fritz giving the theory in question for Heeria. Todd ("American Naturalist," April 1882), described a similar state of things in Solanum rostratum and in Cassia: and H.O. Forbes ("Nature," August 1882, page 386) has done the same for Melastoma. In Rhexia virginica Mr. W.H. Leggett ("Bulletin Torrey Bot. Club, New York," VIII., 1881, page 102) describes the curious structure of the anther, which consists of two inflated portions and a tubular part connecting the two. By pressing with a blunt instrument on one of the ends, the pollen is forced out in a jet through a fine pore in the other inflated end. Mr. Leggett has seen bees treading on the anthers, but could not get near enough to see the pollen expelled. In the same journal, Volume IX., page 11, Mr. Bailey describes how in Heterocentron roseum, "upon pressing the bellows-like anther with a blunt pencil, the pollen was ejected to a full inch in distance." On Lagerstroemia as comparable with the Melastomads see Letter 689.) Fritz Muller's theory with regard to the Melastomads and a number of analogous cases in other genera are discussed in H. Muller's article in "Kosmos" (620/3. "Kosmos," XIII., 1883, page 241.), where the literature is given. F. Muller's theory is that in Heeria the yellow anthers serve merely as a means of attracting pollen-collecting bees, while the longer stamens with purple or crimson anthers supply pollen for fertilising purposes. If Muller is right the pollen from the yellow anthers would not normally reach the stigma. The increased vigour observed in the seedlings from the yellow anthers would seem to resemble the good effect of a cross between different individuals of the same species as worked out in "Cross and Self Fertilisation," for it is difficult to believe that the pollen of the purple anthers has become, by adaptation, less effective than that of the yellow anthers. In the letters here given there is some contradiction between the statements as to the position of the two sets of stamens in relation to the sepals. According to Eichler ("Bluthendiagramme, II., page 482) the longer stamens may be either epipetalous or episepalous in this family.

The work on the Melastomads is of such intrinsic importance that we have thought it right to give the correspondence in considerable detail; we have done so in spite of the fact that Darwin arrived at no definite conclusion, and in spite of an element of confusion and unsatisfactoriness in the series of letters. This applies also to Letter 629, written after Darwin had learned Fritz Muller's theory, which is obscured by some errors or slips of the pen.)

LETTER 620. TO G. BENTHAM. Down, February 3rd {1862?}

As you so kindly helped me before on dimorphism, will you forgive me begging for a little further information, if in your power to give it? The case is that of the Melastomads with eight stamens, on which I have been experimenting. I am perplexed by opposed statements: Lindley says the stamens which face the petals are sterile; Wallich says in Oxyspora paniculata that the stamens which face the sepals are destitute of pollen; I find plenty of apparently good pollen in both sets of stamens in Heterocentron {Heeria}, Monochoetum, and Centradenia. Can you throw any light on this? But there is another point on which I am more anxious for information. Please look at the enclosed miserable diagram. I find that the pollen of the yellow petal-facing stamens produce more than twice as much seed as the pollen of the purple sepal-facing stamens. This is exactly opposed to Lindley's statement—viz., that the petal-facing stamens are sterile. But I cannot at present believe that the case has any relation to abortion; it is hardly possible to believe that the longer and very curious stamens, which face the sepals in this Heterocentron, are tending to be rudimentary, though their pollen applied to their own flowers produces so much less seed. It is conformable with what we see in Primula that the {purple} sepal-facing anthers, which in the plant seen by me stood quite close on each side of the stigma, should have been rendered less fitted to fertilise the stigma than the stamens on the opposite side of the flower. Hence the suspicion has crossed me that if many plants of the Heterocentron roseum were examined, half would be found with the pistil nearly upright, instead of being rectangularly bent down, as shown in the diagram (620/4. According to Willis, "Flowering Plants and Ferns," 1897, Volume II., page 252, the style in Monochoetum, "at first bent downwards, moves slowly up till horizontal."); or, if the position of pistil is fixed, that in half the plants the petal-facing stamens would bend down, and in the other half of the plants the sepal-facing stamens would bend down as in the diagram. I suspect the former case, as in Centradenia I find the pistil nearly straight. Can you tell me? (620/5. No reply by Mr. Bentham to this or the following queries has been found.) Can the name Heterocentron have any reference to such diversity? Would it be asking too great a favour to ask you to look at dried specimens of Heterocentron roseum (which would be best), or of Monochoetum, or any eight-stamened Melastomad, of which you have specimens from several localities (as this would ensure specimens having been taken from distinct plants), and observe whether the pistil bends differently or stamens differently in different plants? You will at once see that, if such were the fact, it would be a new form of dimorphism, and would open up a large field of inquiry with respect to the potency of the pollen in all plants which have two sets of stamens—viz., longer and shorter. Can you forgive me for troubling you at such unreasonable length? But it is such waste of time to experiment without some guiding light. I do not know whether you have attended particularly to Melastoma; if you have not, perhaps Hooker or Oliver may have done so. I should be very grateful for any information, as it will guide future experiments.

P.S.—Do you happen to know, when there are only four stamens, whether it is the petal or sepal-facers which are preserved? and whether in the four-stamened forms the pistil is rectangularly bent or is straight?

LETTER 621. TO ASA GRAY. Down, February 16th {1862?}.

I have been trying a few experiments on Melastomads; and they seem to indicate that the pollen of the two curious sets of anthers (i.e. the petal-facers and the sepal-facers) have very different powers; and it does not seem that the difference is connected with any tendency to abortion in the one set. Now I think I can understand the structure of the flower and means of fertilisation, if there be two forms,—one with the pistil bent rectangularly out of the flower, and the other with it nearly straight.

Our hot-house and green-house plants have probably all descended by cuttings from a single plant of each species; so I can make out nothing from them. I applied in vain to Bentham and

Hooker; but Oliver picked out some sentences from Naudin, which seem to indicate differences in the position of the pistil.

I see that Rhexia grows in Massachusetts; and I suppose has two different sets of stamens. Now, if in your power, would you observe the position of the pistil in different plants, in lately opened flowers of the same age? (I specify this because in Monochaetum I find great changes of position in the pistils and stamens, as flower gets old). Supposing that my prophecy should turn out right, please observe whether in both forms the passage into the flower is not {on} the upper side of the pistil, owing to the basal part of the pistil lying close to the ring of filaments on the under side of the flower. Also I should like to know the colour of the two sets of anthers. This would take you only a few minutes, and is the only way I see that I can find out whether these plants are dimorphic in this peculiar way—i.e., only in the position of the pistil (621/1. In Exacum and in Saintpaulia the flowers are dimorphic in this sense: the style projects to either the right or the left side of the corolla, from which it follows that a right-handed flower would fertilise a left-handed one, and vice versa. See Willis, "Flowering Plants and Ferns," 1897, Volume I., page 73.) and in its relation to the two kinds of pollen. I am anxious about this, because if it should prove so, it will show that all plants with longer and shorter or otherwise different anthers will have to be examined for dimorphism.

LETTER 622. TO ASA GRAY. March 15th {1862}.

...I wrote some little time ago about Rhexia; since then I have been carefully watching and experimenting on another genus, Monochaetum; and I find that the pistil is first bent rectangularly (as in the sketch sent), and then in a few days becomes straight: the stamens also move. If there be not two forms of Rhexia, will you compare the position of the part in young and old flowers? I have a suspicion (perhaps it will be proved wrong when the seed-capsules are ripe) that one set of anthers are adapted to the pistil in early state, and the other set for it in its later state. If bees visit the Rhexia, for Heaven's sake watch exactly how the anther and stigma strike them, both in old and young flowers, and give me a sketch.

Again I say, do not hate me.

LETTER 623. TO J.D. HOOKER. Leith Hill Place, Dorking, Thursday, 15th {May 1862}.

You stated at the Linnean Society that different sets of seedling Cinchona (623/1. Cinchona is apparently heterostyled: see "Forms of Flowers," Edition II., page 134.) grew at very different rate, and from my Primula case you attributed it probably to two sorts of pollen. I confess I thought you rash, but I now believe you were quite right. I find the yellow and crimson anthers of the same flower in the Melastomatous Heterocentron roseum have different powers; the yellow producing on the same plant thrice as many seeds as the crimson anthers. I got my neighbour's most skilful gardener to sow both kinds of seeds, and yesterday he came to me and said it is a most extraordinary thing that though both lots have been treated exactly alike, one lot all remain dwarfs and the other lot are all rising high up. The dwarfs were produced by the pollen of the crimson anthers. In Monochaetum ensiferum the facts are more complex and still more strange; as the age and position of the pistils comes into play, in relation to the two kinds of pollen. These facts seem to me so curious that I do not scruple to ask you to see whether you can lend me any Melastomad just before flowering, with a not very small flower, and which will endure for a short time a greenhouse or sitting-room; when fertilised and watered I could send it to Mr. Turnbull's to a cool stove to mature seed. I fully believe the case is worth investigation.

P.S.—You will not have time at present to read my orchid book; I never before felt half so doubtful about anything which I published. When you read it, do not fear "punishing" me if I deserve it.

Adios. I am come here to rest, which I much want.

Whenever you have occasion to write, pray tell me whether you have Rhododendron Boothii from Bhootan, with a smallish yellow flower, and pistil bent the wrong way; if so, I would ask Oliver to look for nectary, for it is an abominable error of Nature that must be corrected. I could hardly believe my eyes when I saw the pistil.

LETTER 624. TO ASA GRAY. January 19th {1863}.

I have been at those confounded Melastomads again; throwing good money (i.e. time) after bad. Do you remember telling me you could see no nectar in your Rhexia? well, I can find none in Monochaetum, and Bates tells me that the flowers are in the most marked manner neglected by bees and lepidoptera in Amazonia. Now the curious projections or horns to the stamens of Monochaetum are full of fluid, and the suspicion occurs to me that diptera or small hymenoptera may puncture these horns like they puncture (proved since my orchid book was published) the dry nectaries of true Orchis. I forget whether Rhexia is common; but I very much wish you would next summer watch on a warm day a group of flowers, and see whether they are visited by small insects, and what they do.

LETTER 625. TO I.A. HENRY. Down, January 20th {1863}.

...You must kindly permit me to mention any point on which I want information. If you are so inclined, I am curious to know from systematic experiments whether Mr. D. Beaton's statement that the pollen of two shortest anthers of scarlet Pelargonium produce dwarf plants (625/1. See "Animals and Plants," Edition II., Volume II., page 150, for a brief account of Darwin's experiments on this genus. Also loc. cit., page 338 (note), for a suggested experiment.), in comparison with plants produced from the same mother-plant by the pollen of longer stamens from the same flower. It would aid me much in some laborious experiments on Melastomads. I confess I feel a little doubtful; at least, I feel pretty nearly sure that I know the meaning of short stamens in most plants. This summer (for another object) I crossed Queen of Scarlet Pelargonium with pollen of long and short stamens of multiflora alba, and it so turns out that plants from short stamens are the tallest; but I believe this to have been mere chance. My few crosses in Pelargonium were made to get seed from the central peloric or regular flower (I have got one from peloric flower by pollen of peloric), and this leads me to suggest that it would be very interesting to test fertility of peloric flowers in three ways,—own peloric pollen on peloric stigma, common pollen on peloric stigma, peloric pollen on common stigma of same species. My object is to discover whether with change of structure of flower there is any change in fertility of pollen or of female organs. This might also be tested by trying peloric and common pollen on stigma of a distinct species, and conversely. I believe there is a peloric and common variety of Tropaeolum, and a peloric or upright and common variation of some species of Gloxinia, and the medial peloric flowers of Pelargonium, and probably others unknown to me.

LETTER 626. TO I.A. HENRY. Hartfield, May 2nd {1863}.

In scarlet dwarf Pelargonium, you will find occasionally an additional and abnormal stamen on opposite and lower side of flower. Now the pollen of this one occasional short stamen, I think, very likely would produce dwarf plants. If you experiment on Pelargonium I would suggest your looking out for this single stamen.

I observed fluctuations in length of pistil in Phloxes, but thought it was mere variability.

If you could raise a bed of seedling Phloxes of any species except P. Drummondii, it would be highly desirable to see if two forms are presented, and I should be very grateful for information and flowers for inspection. I cannot remember, but I know that I had some reason to look after Phloxes. (626/1. See "Forms of Flowers," Edition II., page 119, where the conjecture is hazarded that Phlox subulata shows traces of a former heterostyled condition.)

I do not know whether you have used microscopes much yet. It adds immensely to interest of all such work as ours, and is indeed indispensable for much work. Experience, however, has fully convinced me that the use of the compound without the simple microscope is absolutely injurious to progress of N{atural} History (excepting, of course, with Infusoria). I have, as yet, found no exception to the rule, that when a man has told me he works with the compound alone his work is valueless.

LETTER 627. TO ASA GRAY. March 20th {1863}.

I wrote to him {Dr. H. Cruger, of Trinidad} to ask him to observe what the insects did in the flowers of Melastomaceae: he says not proper season yet, but that on one species a small bee seemed busy about the horn-like appendages to the anthers. It will be too good luck if my study of the flowers in the greenhouse has led me to right interpretation of these appendages.

LETTER 628. TO J.D. HOOKER. Down, November 28th {1871}.

If you had come here on Sunday I should have asked you whether you could give me seed or seedlings of any Melastomad which would flower soon to experiment on! I wrote also to J. Scott to ask if he could give me seed.

Several years ago I raised a lot of seedlings of a Melastomad greenhouse bush (Monochaetus or some such name) (628/1. Monochaetum.) from stigmas fertilised separately by the two kinds of pollen, and the seedlings differed remarkably in size, and whilst young, in appearance; and I never knew what to think of the case (so you must not use it), and have always wished to try again, but they are troublesome beasts to fertilise.

On the other hand I could detect no difference in the product from the two coloured anthers of Clarkia. (628/2. Clarkia has eight stamens divided into two groups which differ in the colour of the anthers.) If you want to know further particulars of my experiments on Monochaetum (?) and Clarkia, I will hunt for my notes. You ask about difference in pollen in the same species. All dimorphic and trimorphic plants present such difference in function and in size. Lythrum and the trimorphic Oxalis are the most wonderful cases. The pollen of the closed imperfect cleistogamic flowers differ in the transparency of the integument, and I think in size. The latter point I could ascertain from my notes. The pollen or female organs must differ in almost every individual in some manner; otherwise the pollen of varieties and even distinct individuals of same varieties would not be so prepotent over the individual plant's own pollen. Here follows a case of individual differences in function of pollen or ovules or both. Some few individuals of Reseda odorata and R. lutea cannot be fertilised, or only very rarely, by pollen of the same plant, but can by pollen of any other individual. I chanced to have two plants of R. odorata in this state; so I crossed them and raised five seedlings, all of which were self sterile and all perfectly fertile with pollen of any other individual mignonette. So I made a self sterile race! I do not know whether these are the kinds of facts which you require.

Think whether you can help me to seed or better seedlings (not cuttings) of any Melastomad.

LETTER 629. TO F. MULLER. Down, March 20th, 1881.

I have received the seeds and your most interesting letter of February 7th. The seeds shall be sown, and I shall like to see the plants sleeping; but I doubt whether I shall make any more detailed observations on this subject, as, now that I feel very old, I require the stimulus of some novelty to make me work. This stimulus you have amply given me in your remarkable view of the meaning of the two-coloured stamens in many flowers. I was so much struck with this fact with Lythrum, that I began experimenting on some Melastomaceae, which have two sets of extremely differently coloured anthers. After reading your letter I turned to my notes (made 20 years ago!) to see whether they would support or contradict your suggestion. I cannot tell yet, but I have come across one very remarkable result, that seedlings from the crimson anthers were not 11/20ths of the size of seedlings from the yellow anthers of the same flowers. Fewer good seeds were produced by the crimson pollen. I concluded that the shorter stamens were aborting, and that the pollen was not good. (629/1. "Shorter stamens" seems to be a slip of the pen for "longer,"—unless the observations were made on some genus in which the structure is unusual.) The mature pollen is incoherent, and must be {word illegible} against the visiting insect's body. I remembered this, and I find it said in my EARLY notes that bees would never visit the flowers for pollen. This made me afterwards write to the late Dr. Cruger in the West Indies, and he observed for me the flowers, and saw bees pressing the anthers with their mandibles from the base upwards, and this forced a worm-like thread of pollen from the terminal pore, and this pollen the bees collected with their hind legs. So that the Melastomads are not opposed to your views.

I am now working on the habits of worms, and it tires me much to change my subject; so I will lay on one side your letter and my notes, until I have a week's leisure, and will then see whether my facts bear on your views. I will then send a letter to "Nature" or to the Linn. Soc., with the extract of your letter (and this ought to appear in any case), with my own observations, if they appear worth publishing. The subject had gone out of my mind, but I now remember thinking that the imperfect action of the crimson stamens might throw light on hybridism. If this pollen is developed, according to your view, for the sake of attracting insects, it might act imperfectly, as well as if the stamens were becoming rudimentary. (629/2. As far as it is possible to understand the earlier letters it seems that the pollen of the shorter stamens, which are adapted for attracting insects, is the most effective.) I do not know whether I have made myself intelligible.

LETTER 630. TO W. THISELTON-DYER. Down, March 21st {1881}.

I have had a letter from Fritz Muller suggesting a novel and very curious explanation of certain plants producing two sets of anthers of different colour. This has set me on fire to renew the laborious experiments which I made on this subject, now 20 years ago. Now, will you be so kind as to turn in your much worked and much holding head, whether you can think of any plants, especially annuals, producing 2 such sets of anthers. I believe that this is the case with Clarkia elegans, and I have just written to Thompson for seeds. The Lythraceae must be excluded, as these are heterostyled.

I have got seeds from Dr. King of some Melastomaceae, and will write to Veitch to see if I can get the Melastomaceous genera Monochaetum and Heterocentron or some such name, on which I before experimented. Now, if you can aid me, I know that you will; but if you cannot, do not write and trouble yourself.

2.X.III. CORRESPONDENCE WITH JOHN SCOTT, 1862-1871.

"If he had leisure he would make a wonderful observer, to my judgment; I have come across no one like him."—Letter to J.D. Hooker, May 29th {1863}.

(631/1. The following group of letters to John Scott, of whom some account is given elsewhere (Volume I., Letters 150 and 151, and Index.) deal chiefly with experimental work in the fertilisation of flowers. In addition to their scientific importance, several of the letters are of special interest as illustrating the encouragement and friendly assistance which Darwin gave to his correspondent.)

LETTER 631. JOHN SCOTT TO CHARLES DARWIN. Edinburgh Botanic Gardens, November 11th, 1862.

I take the liberty of addressing you for the purpose of directing your attention to an error in one of your ingenious explanations of the structural adaptations of the Orchidaceae in your late work. This occurs in the genus Acropera, two species of which you assume to be unisexual, and so far as known represented by male individuals only. Theoretically you have no doubt assigned good grounds for this view; nevertheless, experimental observations that I am now making have already convinced me of its fallacy. And I thus hurriedly, and as you may think prematurely, direct your attention to it, before I have seen the final result of my own experiment, that you might have the longer time for reconsidering the structure of this genus for another edition of your interesting book, if indeed it be not already called for. I am furthermore induced to communicate the results of my yet imperfect experiments in the belief that the actuating principle of your late work is the elicitation of truth, and that you will gladly avail yourself of this even at the sacrifice of much ingenious theoretical argumentation.

Since I have had an opportunity of perusing your work on orchid fertilisation, my attention has been particularly directed to the curiously constructed floral organs of Acropera. I unfortunately have as yet had only a few flowers for experimental enquiry, otherwise my remarks might have been clearer and more satisfactory. Such as they are, however, I respectfully lay {them} before you, with a full assurance of their veracity, and I sincerely trust that as such you will receive them.

Your observations seem to have been chiefly directed to the A. luteola, mine to the A. Loddigesii, which, however, as you remark, is in a very similar constructural condition with the former; having the same narrow stigmatic chamber, abnormally developed placenta, etc. In regard to the former point—contraction of stigmatic chamber—I may remark that it does not appear to be absolutely necessary that the pollen-masses penetrate this chamber for effecting fecundation. Thus a raceme was produced upon a plant of

A. Loddigesii in the Botanic Gardens here lately; upon this I left only six flowers. These I attempted to fertilise, but with two only of the six have I been successful: I succeeded in forcing a single pollen-mass into the stigmatic chamber of one of the latter, but I failed to do this on the other; however, by inserting a portion of a pedicel with a pollinium attached, I caused the latter to adhere, with a gentle press, to the mouth of the stigmatic chamber. Both of these, as I have already remarked, are nevertheless fertilised; one of them I have cut off for examination, and its condition I will presently describe; the other is still upon the plant, and promises fair to attain maturity. In regard to the other four flowers, I may remark that though similarly fertilised—part having pollinia inserted, others merely attached—they all withered and dropped off without the least swelling of the ovary. Can it be, then, that this is really an {andro-monoecious} species?—part of the flowers male, others truly hermaphrodite.

In making longitudinal sections of the fertilised ovary before mentioned, I found the basal portion entirely destitute of ovules, their place being substituted by transparent cellular ramification of the placentae. As I traced the placentae upwards, the ovules appeared, becoming gradually more abundant towards its apex. A transverse section near the apex of the ovary, however, still exhibited a more than ordinary placental development—i.e. {congenitally?} considered—each end giving off two branches, which meet each other in the centre of the ovary, the ovules being irregularly and sparingly disposed upon their surfaces.

In regard to the mere question of fertilisation, then, I am perfectly satisfied, but there are other points which require further elucidation. Among these I may particularly refer to the contracted stigmatic chamber, and the slight viscidity of its disk. The latter, however, may be a consequence of uncongenial conditions—as you do not mention particularly its examination by any author in its natural habitat. If such be the case, the contracted stigmatic chamber will offer no real difficulty, should the viscous exudations be only sufficient to render the mouth adhesive. For, as I have already shown, the pollen-tubes may be emitted in this condition, and effect fecundation without being in actual contact with the stigmatic surface, as occurs pretty regularly in the fertilisation of the Stapelias, for example. But, indeed, your own discovery of the independent germinative capabilities of the pollen-grains of certain Orchidaceae is sufficiently illustrative of this.

I may also refer to the peculiar abnormal condition that many at least of the ovaries present in a comparative examination of the placentae, and of which I beg to suggest the following explanation, though it is as yet founded on limited observations. In examining certain young ovaries of A. Loddigesii, I found some of them filled with the transparent membranous fringes of more or less distinctly cellular matter, which, from your description of the ovaries of luteola, appears to differ simply in the greater development in the former species. Again, in others I found small mammillary bodies, which appeared to be true ovules, though I could not perfectly satisfy myself as to the existence of the micropyle or nucleus. I unfortunately neglected to apply any chemical test. The fact, however, that in certain of the examined ovaries few or none of the latter bodies occurred—the placenta alone being developed in an irregular membranous form, taken in conjunction with the results of my experiments—before alluded to—on their fertilisation, leads me to infer that two sexual conditions are presented by the flowers of this plant. In short, that many of the ovaries are now normally abortive, though Nature occasionally makes futile efforts for their perfect development, in the production of ovuloid bodies; these then I regard as the male flowers. The others that are still capable of fertilisation, and likewise possessing male organs, are hermaphrodite, and must, I think, from the results of your comparative examinations, present a somewhat different condition; as it can scarcely be supposed that ovules in the condition you describe could ever be fertilised.

This is at least the most plausible explanation I can offer for the different results in my experiments on the fertilisation of apparently similar morphologically constructed flowers; others may, however, occur to you. Here there is not, as in the Catasetum, any external change visible in the respective unisexual and bisexual flowers. And yet it would appear from your researches that the ovules of Acropera are in a more highly atrophied condition than occurs in Catasetum, though, as you likewise remark, M. Neumann has never succeeded in fertilising C. tridentatum. If there be not, then, an arrangement of the reproductive structures, such as I have indicated, how can the different results in M. Neumann's experiments and mine be accounted for? However, as you have examined many flowers of both A. luteola and Loddigesii, such a difference in the ovulary or placental structures could scarcely have escaped your observation. But, be this as it may, the—to me at least—demonstrated fact still remains, that certain flowers of A. Loddigesii are capable of fertilisation, and that, though there are good grounds for supposing that important physiological changes are going on in the sexual phenomena of this species, there is no evidence whatever for supposing that external morphological changes have so masked certain individuals as to prevent their recognition.

I would now, sir, in conclusion beg you to excuse me for this infringement upon your valuable time, as I have been induced to write you in the belief that you have had negative results from other experimenters, before you ventured to propose your theoretical explanation, and consequently that you have been unknowingly led into error. I will continue, as opportunities present themselves, to examine the many peculiarities you have pointed out in this as well as others of the Orchid family; and at present I am looking forward with anxiety for the maturation of the ovary of A. Loddigesii, which will bear testimony to the veracity of the remarks I have ventured to lay before you.

LETTER 632. TO J.D. HOOKER. Down, 18th {November 1862}.

Strange to say, I have only one little bother for you to-day, and that is to let me know about what month flowers appear in Acropera Loddigesii and luteola; for I want extremely to beg a few more flowers, and if I knew the time I would keep a memorandum to remind you. Why I want these flowers is (and I am much alarmed) that Mr. J. Scott, of Bot. Garden of Edinburgh (do you know anything of him?) has written me a very long and clever letter, in which he confirms most of my observations; but tells me that with much difficulty he managed to get pollen into orifice, or as far as mouth of orifice, of six flowers of A. Loddigesii (the ovarium of which I did not examine), and two pods set; one he gathered, and saw a very few ovules, as he thinks, on the large and mostly rudimentary placenta. I shall be most curious to hear whether the other pod produces a good lot of seed. He says he regrets that he did not test the ovules with chemical agents: does he mean tincture of iodine? He suggests that in a state of nature the viscid matter may come to the very surface of stigmatic chamber, and so pollen-masses need not be inserted. This is possible, but I should think improbable. Altogether the case is very odd, and I am very uneasy, for I cannot hope that A. Loddigesii is hermaphrodite and A. luteola the male of the same species. Whenever I can get Acropera would be a very good time for me to look at Vanda in spirits, which you so kindly preserved for me.

LETTER 633. TO J. SCOTT.

(633/1. The following is Darwin's reply to the above letter from Scott. In the first edition of "Fertilisation of Orchids" (page 209) he assumed that the sexes in Acropera, as in Catasetum, were separate. In the second edition (page 172) he writes: "I was, however, soon convinced of my error by Mr. Scott, who succeeded in artificially fertilising the flowers with their own pollen. A remarkable discovery by Hildebrand (633/2. "Bot. Zeitung," 1863 and 1865.), namely, that in many orchids the ovules are not developed unless the stigma is penetrated by the pollen-tubes...explains the state of the ovarium in Acropera, as observed by me." In regard to this subject see Letter 608.)

Down, November 12th, 1862.

I thank you most sincerely for your kindness in writing to me, and for {your} very interesting letter. Your fact has surprised me greatly, and has alarmed me not a little, for if I am in error about Acropera I may be in error about Catasetum. Yet when I call to mind the state of the placentae in A. luteola, I am astonished that they should produce ovules. You will see in my book that I state that I did not look at the ovarium of A. Loddigesii. Would you have the kindness to send me word which end of the ovarium is meant by apex (that nearest the flower?), for I must try and get this species from Kew and look at its ovarium. I shall be extremely curious to hear whether the fruit, which is now maturing, produces a large number of good and plump seed; perhaps you may have seen the ripe capsules of other Vandeae, and may be able to form some conjecture what it ought to produce. In the young, unfertilised ovaria of many Vandeae there seemed an infinitude of ovules. In desperation it occurs to me as just possible, as almost everything in nature goes by gradation, that a properly male flower might occasionally produce a few seeds, in the same manner as female plants sometimes produce a little pollen. All your remarks seem to me excellent and very interesting, and I again thank you for your kindness in writing to me. I am pleased to observe that my description of the structure of Acropera seems to agree pretty well with what you have observed. Does it not strike you as very difficult to understand how insects remove the pollinia and carry them to the stigmas? Your suggestion that the mouth of the stigmatic cavity may become charged with viscid matter and thus secure the pollinia, and that the pollen-tubes may then protrude, seems very ingenious and new to me; but it would be very anomalous in orchids, i.e. as far as I have seen. No doubt, however, though I tried my best, I shall be proved wrong in many points. Botany is a new subject to me. With respect to the protrusion of pollen-tubes, you might like to hear (if you do not already know the fact) that, as I saw this summer, in the little imperfect flowers of Viola and Oxalis, which never open, the pollen-tubes always come out of the pollen-grain, whilst still in the anthers, and direct themselves in a beautiful manner to the stigma seated at some little distance. I hope that you will continue your very interesting observations.

LETTER 634. TO J. SCOTT. Down, November 19th {1862}.

I am much obliged for your letter, which is full of interesting matter. I shall be very glad to look at the capsule of the Acropera when ripe, and pray present my thanks to Mr. MacNab. (634/1. See Letter 608 (Lindley, December 15th, 1861). Also "Fertilisation of Orchids," Edition II., page 172, for an account of the observations on Acropera which were corrected by Scott.) I should like to keep it till I could get a capsule of some other member of the Vandeae for comparison, but ultimately all the seeds shall be returned, in case you would like to write any notice on the subject. It was, as I said (634/2. Letter 633.), only "in desperation" that I suggested that the flower might be a male and occasionally capable of producing a few seeds. I had forgotten Gartner's remark; in fact, I know only odds and ends of Botany, and you know far more. One point makes the above view more probable in Acropera than in other cases, viz. the presence of rudimentary placentae or testae, for I cannot hear that these have been observed in the male plants. They do not occur in male Lychnis dioica, but next spring I will look to male holly flowers. I fully admit the difficulty of similarity of stigmatic chamber in the two Acroperas. As far as I remember, the blunt end of pollen-mass would not easily even stick in the orifice of the chamber. Your view may be correct about abundance of viscid matter, but seems rather improbable. Your facts about female flowers occurring where males alone ought to occur is new to me; if I do not hear that you object, I will quote the Zea case on your authority in what I am now writing on the varieties of the maize. (634/3. See "Animals and Plants," Edition II., Volume I., page 339: "Mr. Scott has lately observed the rarer case of female flowers on a true male panicle, and likewise hermaphrodite flowers." Scott's paper on the subject is in "Trans. Bot. Soc. Edinburgh," Volume VIII. See Letter 151, Volume I.) I am glad to hear that you are now working on the most curious subject of parthenogenesis. I formerly fancied that I observed female Lychnis dioica seeded without pollen. I send by this post a paper on Primula, which may interest you. (634/4. "Linn. Soc. Journal," 1862.) I am working on the subject, and if you should ever observe any analogous case I should be glad to hear. I have added another very clever pamphlet by Prof. Asa Gray. Have you a copy of my Orchis book? If you have not, and would like one, I should be pleased to send one. I plainly see that you have the true spirit of an experimentalist and good observer. Therefore, I ask whether you have ever made any trials on relative fertility of varieties of plants (like those I quote from Gartner on the varieties of Verbascum). I much want information on this head, and on those marvellous cases (as some Lobelias and Crinum passiflora) in which a plant can be more easily fertilised by the pollen of another species than by its own good pollen. I am compelled to write in haste. With many thanks for your kindness.

LETTER 635. TO J. SCOTT. Down, 20th {1862?}.

What a magnificent capsule, and good Heavens, what a number of seeds! I never before opened pods of larger orchids. It did not signify a few seed being lost, as it would be hopeless to estimate number in comparison with other species. If you sow any, had you not better sow a good many? so I enclose small packet. I have looked at the seeds; I never saw in the British orchids nearly so many empty testae; but this goes for nothing, as unnatural conditions would account for it. I suspect, however, from the variable size and transparency, that a good many of the seeds when dry (and I have put the capsule on my chimney-piece) will shrivel up. So I will wait a month or two till I get the capsule of some large Vandeae for comparison. It is more likely that I have made some dreadful blunder about Acropera than that it should be male yet not a perfect male. May there be some sexual relation between A. Loddigesii and luteola; they seem very close? I should very much like to examine the capsule of the unimpregnated flower of A. Loddigesii. I have got both species from Kew, but whether we shall have skill to flower them I know not. One conjectures that it is imperfect male; I still should incline to think it would produce by seed both sexes. But you are right about Primula (and a very acute thought it was): the long-styled P. sinensis, homomorphically fertilised with own-form pollen, has produced during two successive homomorphic generations only long-styled plants. (635/1. In "Forms of Flowers," Edition II., page 216, a summary of the transmission of forms in the "homomorphic" unions of P. sinensis is given. Darwin afterwards used "illegitimate" for homomorphic, and "legitimate" for "heteromorphic" ("Forms of Flowers," Edition i., page 24).) The short-styled the same, i.e. produced short-styled for two generations with the exception of a single plant. I cannot say about cowslips yet. I should like to hear your case of the Primula: is it certainly propagated by seed?

LETTER 636. TO J. SCOTT. Down, December 3rd, {1862?}.

What a capital observer you are! and how well you have worked the primulas. All your facts are new to me. It is likely that I overrate the interest of the subject; but it seems to me that you ought to publish a paper on the subject. It would, however, greatly add to the value if you were to cover up any of the forms having pistil and anther of the same height, and prove that they were fully self-fertile. The occurrence of dimorphic and non-dimorphic species in the same genus is quite the same as I find in Linum. (636/1. Darwin finished his paper on Linum in December 1862, and it was published in the "Linn. Soc. Journal" in 1863.) Have any of the forms of Primula, which are non-dimorphic, been propagated for some little time by seed in garden? I suppose not. I ask because I find in P. sinensis a third rather fluctuating form, apparently due to culture, with stigma and anthers of same height. I have been working successive generations homomorphically of this Primula, and think I am getting curious results; I shall probably publish next autumn; and if you do not (but I hope you will) publish yourself previously, I should be glad to quote in abstract some of your facts. But I repeat that I hope you will yourself publish. Hottonia is dimorphic, with pollen of very different sizes in the two forms. I think you are mistaken about Siphocampylus, but I feel rather doubtful in saying this to so good an observer. In Lobelia the closed pistil grows rapidly, and pushes out the pollen and then the stigma expands, and the flower in function is monoecious; from appearance I believe this is the case with your plant. I hope it is so, for this plant can hardly require a cross, being in function monoecious; so that dimorphism in such a case would be a heavy blow to understanding its nature or good in all other cases. I see few periodicals: when have you published on Clivia? I suppose that you did not actually count the seeds in the hybrids in comparison with those of the parent-forms; but this is almost necessary after Gartner's observations. I very much hope you will make a good series of comparative trials on the same plant of Tacsonia. (636/2. See Scott in "Linn. Soc. Journal," VIII.) I have raised 700-800 seedlings from cowslips, artificially fertilised with care; and they presented not a hair's-breadth approach to oxlips. I have now seed in pots of cowslip fertilised by pollen of primrose, and I hope they will grow; I have also got fine seedlings from seed of wild oxlips; so I hope to make out the case. You speak of difficulties on Natural Selection: there are indeed plenty; if ever you have spare time (which is not likely, as I am sure you must be a hard worker) I should be very glad to hear difficulties from one who has observed so much as you have. The majority of criticisms on the "Origin" are, in my opinion, not worth the paper they are printed on. Sir C. Lyell is coming out with what, I expect, will prove really good remarks. (636/3. Lyell's "Antiquity of Man" was published in the spring of 1863. In the "Life and Letters," Volume III., pages 8, 11, Darwin's correspondence shows his deep disappointment at what he thought Lyell's half-heartedness in regard to evolution. See Letter 164, Volume I.) Pray do not think me intrusive; but if you would like to have any book I have published, such as my "Journal of Researches" or the "Origin," I should esteem it a compliment to be allowed to send it. Will you permit me to suggest one experiment, which I should much like to see tried, and which I now wish the more from an extraordinary observation by Asa Gray on Gymnadenia tridentata (in number just out of Silliman's N. American Journal) (636/4. In Gymnadenia tridentata, according to Asa Gray, the anther opens in the bud, and the pollen being somewhat coherent falls on the stigma and on the rostellum which latter is penetrated by the pollen-tubes. "Fertilisation of Orchids," Edition II., page 68. Asa Gray's papers are in "American Journal of Science," Volume XXXIV., 1862, and XXXVI., 1863.); namely, to split the labellum of a Cattleya, or of some allied orchis, remove caudicle from pollen-mass (so that no loose grains are about) and put it carefully into the large tongue-like rostellum, and see if pollen-tubes will penetrate, or better, see if capsule will swell. Similar pollen-masses ought to be put on true stigmas of two or three other flowers of same plants for comparison. It is to discover whether rostellum yet retains some of its primordial function of being penetrated by pollen-tubes. You will be sorry that you ever entered into correspondence with me. But do not answer till at leisure, and as briefly as you like. My handwriting, I know, is dreadfully bad. Excuse this scribbling paper, as I can write faster on it, and I have a rather large correspondence to keep up.

LETTER 637. TO J. SCOTT. Down, January 21st, 1863.

I thank you for your very interesting letter; I must answer as briefly as I can, for I have a heap of other letters to answer. I strongly advise you to follow up and publish your observations on the pollen-tubes of orchids; they promise to be very interesting. If you could prove what I only conjectured (from state of utriculi in rostellum and in stigma of Catasetum and Acropera) that the utriculi somehow induce, or are correlated with, penetration of pollen-tubes you will make an important physiological discovery. I will mention, as worth your attention (and what I have anxiously wished to observe, if time had permitted, and still hope to do)—viz., the state of tissues or cells of stigma in an utterly sterile hybrid, in comparison with the same in fertile parent species; to test these cells, immerse stigmas for 48 hours in spirits of wine. I should expect in hybrids that the cells would not show coagulated contents. It would be an interesting discovery to show difference in female organs of hybrids and pure species. Anyhow, it is worth trial, and I recommend you to make it, and publish if you do. The pollen-tubes directing themselves to stigma is also very curious, though not quite so new, but well worth investigation when you get Cattleya, etc., in flower. I say not so new, for remember small flowers of Viola and Oxalis; or better, see Bibliography in "Natural History Review," No. VIII., page 419 (October, 1862) for quotation from M. Baillon on pollen-tubes finding way from anthers to stigma in Helianthemum. I should doubt gum getting solid from {i.e. because of} continued secretion. Why not sprinkle fresh plaster of Paris and make impenetrable crust? (637/1. The suggestion that the stigma should be covered with a crust of plaster of Paris, pierced by a hole to allow the pollen-tubes to enter, bears a resemblance to Miyoshi's experiments with germinating pollen and fungal spores. See "Pringsheim's Jahrbucher," 1895; "Flora," 1894.) You might modify experiment by making little hole in one lower corner, and see if tubes find it out. See in my future paper on Linum pollen and stigma recognising each other. If you will tell me that pollen smells the stigma I will try and believe you; but I will not believe the Frenchman (I forget who) who says that stigma of Vanilla actually attracts mechanically, by some unknown force, the solid pollen-masses to it! Read Asa Gray in 2nd Review of my Orchis book on pollen of Gymnadenia penetrating rostellum. I can, if you like, lend you these Reviews; but they must be returned. R. Brown, I remember, says pollen-tubes separate from grains before the lower ends of tubes reach ovules. I saw, and was interested by, abstract of your Drosera paper (637/2. A short note on the irritability of Drosera in the "Trans. Bot. Soc. Edin." Volume VII.); we have been at very much the same work.

LETTER 638. TO J. SCOTT. Down, February 16th {1863}.

Absence from home has prevented me from answering you sooner. I should think that the capsule of Acropera had better be left till it shows some signs of opening, as our object is to judge whether the seeds are good; but I should prefer trusting to your better judgment. I am interested about the Gongora, which I hope hereafter to try myself, as I have just built a small hot-house.

Asa Gray's observations on the rostellum of Gymnadenia are very imperfect, yet worth looking at. Your case of Imatophyllum is most interesting (638/1. A sucker of Imatophyllum minatum threw up a shoot in which the leaves were "two-ranked instead of four-ranked," and showed other differences from the normal.—"Animals and Plants," Edition II., Volume I., page 411.); even if the sport does not flower it will be worth my giving. I did not understand, or I had forgotten, that a single frond on a fern will vary; I now see that the case does come under bud-variation, and must be given by me. I had thought of it only as proof {of} inheritance in cryptogams; I am much obliged for your correction, and will consult again your paper and Mr. Bridgeman's. (638/2. The facts are given in "Animals and Plants," Edition II., Volume I., page 408.) I enclose varieties of maize from Asa Gray. Pray do not thank me for trusting you; the thanks ought to go the other way. I felt a conviction after your first letter that you were a real lover of Natural History.

If you can advance good evidence showing that bisexual plants are more variable than unisexual, it will be interesting. I shall be very glad to read the discussion which you are preparing. I admit as fully as any one can do that cross-impregnation is the great check to endless variability; but I am not sure that I understand your view. I do not believe that the structure of Primula has any necessary relation to a tendency to a dioecious structure, but seeing the difference in the fertility of the two forms, I felt bound unwillingly to admit that they might be a step towards dioeciousness; I allude to this subject in my Linum paper. (638/3. "Linn. Soc. Journal," 1863.) Thanks for your answers to my other queries. I forgot to say that I was at Kew the other day, and I find that they can give me capsules of several Vandeae.

LETTER 639. TO J. SCOTT. Down, March 24th {1863}.

Your letter, as every one you have written, has greatly interested me. If you can show that certain individual Passifloras, under certain known or unknown conditions of life, have stigmas capable of fertilisation by pollen from another species, or from another individual of its own species, yet not by its own individual pollen (its own individual pollen being proved to be good by its action on some other species), you will add a case of great interest to me; and which in my opinion would be quite worth your publication. (639/1. Cases nearly similar to those observed by Scott were recorded by Gartner and Kolreuter, but in these instances only certain individuals were self-impotent. In "Animals and Plants," Edition II., Volume II., page 114, where the phenomenon is fully discussed, Scott's observations ("Trans. Bot. Soc. Edin." 1863) are given as the earliest, except for one case recorded by Lecoq ("Fecondation," 1862). Interesting work was afterwards done by Hildebrand and Fritz Muller, as illustrated in many of the letters addressed to the latter.) I always imagined that such recorded cases must be due to unnatural conditions of life; and think I said so in the "Origin." (639/2. See "Origin of Species," Edition I., page 251, for Herbert's observations on self-impotence in Hippeastrum. In spite of the uniformness of the results obtained in many successive years, Darwin inferred that the plants must have been in an "unnatural state.") I am not sure that I understand your result, {nor} whether it means what I have above obscurely expressed. If you can prove the above, do publish; but if you will not publish I earnestly beg you to let me have the facts in detail; but you ought to publish, for I may not use the facts for years. I have been much interested by what you say on the rostellum exciting pollen to protrude tubes; but are you sure that the rostellum does excite them? Would not tubes protrude if placed on parts of column or base of petals, etc., near to the stigma? Please look at the "Cottage Gardener" (or "Journal of Horticulture") (639/3. "Journal of Horticulture" and "Cottage Gardener," March 31st, 1863. A short note describing Cruger's discovery of self-fertilisation in Cattleya, Epidendrum, etc., and referring to the work of "an excellent observer, Mr. J. Scott." Darwin adds that he is convinced that he has underrated the power of tropical orchids occasionally to produce seeds without the aid of insects.) to be published to-morrow week for letter of mine, in which I venture to quote you, and in which you will see a curious fact about unopened orchid flowers setting seed in West Indies. Dr. Cruger attributes protrusion of tubes to ants carrying stigmatic secretion to pollen (639/4. In Cruger's paper ("Linn. Soc. Journ." VIII., 1865; read March 3rd 1864) he speaks of the pollen-masses in situ being acted on by the stigmatic secretion, but no mention is made of the agency of ants. He describes the pollen-tubes descending "from the {pollen} masses still in situ down into the ovarian canal."); but this is mere hypothesis. Remember, pollen-tubes protrude within anther in Neottia nidus-avis. I did think it possible or probable that perfect fertilisation might have been effected through rostellum. What a curious case your Gongora must be: could you spare me one of the largest capsules? I want to estimate the number of seed, and try my hand if I can make them grow. This, however, is a foolish attempt, for Dr. Hooker, who was here a day or two ago, says they cannot at Calcutta, and yet imported species have seeded and have naturally spread on to the adjoining trees! Dr. Cruger thinks I am wrong about Catasetum: but I cannot understand his letter. He admits there are three forms in two species; and he speaks as if the sexes were separate in some and that others were hermaphrodites (639/5. Cruger ("Linn. Soc. Journal," VIII., page 127) says that the apparently hermaphrodite form is always sterile in Trinidad. Darwin modified his account in the second edition of the orchid book.); but I cannot understand what he means. He has seen lots of great humble-bees buzzing about the flowers with the pollinia sticking to their backs! Happy man!! I have the promise, but not yet surety, of some curious results with my homomorphic seedling cowslips: these have not followed the rule of Chinese Primula; homomorphic seedlings from short-styled parent have presented both forms, which disgusts me.

You will see that I am better; but still I greatly fear that I must have a compulsory holiday. With sincere thanks and hearty admiration at your powers of observation...

My poor P. scotica looks very sick which you so kindly sent me. (639/6. Sent by Scott, January 6th, 1863.)

LETTER 640. TO J. SCOTT. April 12th {1863}.

I really hardly know how to thank you enough for your very interesting letter. I shall certainly use all the facts which you have given me (in a condensed form) on the sterility of orchids in the work which I am now slowly preparing for publication. But why do you not publish these facts in a separate little paper? (640/1. See Letter 642, note, for reference to Scott's paper.) They seem to me well worth it, and you really ought to get your name known. I could equally well use them in my book. I earnestly hope that you will experiment on Passiflora, and let me give your results. Dr. A. Gray's observations were made loosely; he said in a letter he would attend this summer further to the case, which clearly surprised him much. I will say nothing about the rostellum, stigmatic utriculi, fertility of Acropera and Catasetum, for I am completely bewildered: it will rest with you to settle these points by your excellent observations and experiments. I must own I never could help doubting Dr. Hooker's case of the poppy. You may like to hear what I have seen this morning: I found (640/2. See Letter 658.) a primrose plant with flowers having three pistils, which when pulled asunder, without any tearing, allowed pollen to be placed on ovules. This I did with three flowers—pollen-tubes did not protrude after several days. But this day, the sixteenth (N.B.—primulas seem naturally slowly fertilised), I found many tubes protruded, and, what is very odd, they certainly seemed to have penetrated the coats of the ovules, but in no one instance the foramen of the ovule!! I mention this because it directly bears on your explanation of Dr. Cruger's case. (640/3. Cruger's case here referred to is doubtless the cleistogamic fertilisation of Epidendrum, etc. Scott discusses the question of self-fertilisation at great length in a letter to Darwin dated April, and obviously written in 1863. In Epidendrum he observed a viscid matter extending from the stigmatic chamber to the anther: pollen-tubes had protruded from the anther not only where it was in contact with the viscid matter, but also from the central part, and these spread "over the anterior surface of the rostellum downward into the stigma." Cruger believed the viscid matter reaching the anther was a necessary condition for the germination of the pollen-grains. Scott points out that the viscid matter is produced in large quantity only after the pollen-grains have penetrated the stigma, and that it is, in fact, a consequence, not a preliminary to fertilisation. He finally explains Cruger's case thus: "The greater humidity and equability of temperature consequent on such conditions {i.e. on the flowers being closed} is, I believe, the probable cause of these abnormally conditioned flowers so frequently fertilising themselves." Scott also calls attention to the danger of being deceived by fungal hyphae in observations on germination of pollen.) I believe that your explanation is right; I should never have thought of it; yet this was stupid of me, for I remember thinking that the almost closed imperfect flowers of Viola and Oxalis were related to the protrusion of the pollen-tubes. My case of the Aceras with the aborted labellum squeezed against stigma supports your view. (640/4. See "Fertilisation of Orchids," Edition II., page 258: the pollen germinated within the anther of a monstrous flower.) Dr. Cruger's notion about the ants was a simple conjecture. About cryptogamic filaments, remember Dr. C. says that the unopened flowers habitually set fruit. I think that you will change your views on the imperfect flowers of Viola and Oxalis...

LETTER 641. (?)

LETTER 642. TO J. SCOTT. May 2nd {1863}.

I have left home for a fortnight to see if I can, with little hope, improve my health. The parcel of orchid pods, which you have so kindly sent me, has followed me. I am sure you will forgive the liberty which I take in returning you the postage stamps. I never heard of such a scheme as that you were compelled to practise to fertilise the Gongora! (642/1. See "Fertilisation of Orchids," Edition, II., page 169. "Mr. Scott tried repeatedly, but in vain, to force the pollen-masses into the stigma of Gongora atro-purpurea and truncata; but he readily fertilised them by cutting off the clinandrum and placing pollen-masses on the now exposed stigma.") It is a most curious problem what plan Nature follows in this genus and Acropera. (642/2. In the "Fertilisation of Orchids," Edition II., page 169, Darwin speculates as to the possible fertilisation of Acropera by an insect with pollen-masses adhering to the extremity of its abdomen. It would appear that this guess (which does not occur in the first edition) was made before he heard of Cruger's observation on the allied genus Gongora, which is visited by a bee with a long tongue, which projects, when not in use, beyond and above the tip of the abdomen. Cruger believes that this tongue is the pollinating agent. Cruger's account is in the "Journal of the Linn. Soc." VIII., 1865, page 130.) Some day I will try and estimate how many seeds there are in Gongora. I suppose and hope you have kept notes on all your observations on orchids, for, with my broken health and many other subjects, I do not know whether I shall ever have time to publish again; though I have a large collection of notes and facts ready. I think you show your wisdom in not wishing to publish too soon; a young author who publishes every trifle gets, sometimes unjustly, to be disregarded. I do not pretend to be much of a judge; but I can conscientiously say that I have never written one word to you on the merit of your letters that I do not fully believe in. Please remember that I should very much wish for a copy of your paper on sterility of individual orchids (642/3. "On the Individual Sterility and Cross-Impregnation of Certain Species of Oncidium." {Read June 2nd, 1864.} "Linn. Soc. Journal," VIII., 1865. This paper gives a full account of the self-sterility of Oncidium in cases where the pollen was efficient in fertilising other individuals of the same species and of distinct species. Some of the facts were given in Scott's paper, "Experiments on the Fertilisation of Orchids in the Royal Botanic Garden of Edinburgh," published in the "Proc. Bot. Soc. Edinb." 1863. It is probably to the latter paper that Darwin refers.) and on Drosera. (642/4. "Trans. Bot. Soc. Edinburgh," Volume VII.) Thanks for {note} about Campanula perfoliata. I have asked Asa Gray for seeds, to whom I have mentioned your observations on rostellum, and asked him to look closer to the case of Gymnadenia. (642/5. See "Fertilisation of Orchids," Edition II., page 68.) Let me hear about the sporting Imatophyllum if it flowers. Perhaps I have blundered about Primula; but certainly not about mere protrusion of pollen-tubes. I have been idly watching bees of several genera and diptera fertilising O. morio at this place, and it is a very pretty sight. I have confirmed in several ways the entire truth of my statement that there is no vestige of nectar in the spur; but the insects perforate the inner coat. This seems to me a curious little fact, which none of my reviewers have noticed.

LETTER 643. TO J.D. HOOKER. Down, May 23rd {1863}.

You can confer a real service on a good man, John Scott, the writer of the enclosed letter, by reading it and giving me your opinion. I assure {you} John Scott is a truly remarkable man. The part struck out is merely that he is not comfortable under Mr. McNab, and this part must be considered as private. Now the question is, what think you of the offer? Is expense of living high at Darjeeling? May I say it is healthy? Will he find the opportunity for experimental observations, which are a passion with him? It seems to me rather low pay. Will you advise me for him? I shall say that as far as experiments in hand at the Botanical Garden in Edinburgh are concerned, it would be a pity to hesitate to accept the offer.

J. Scott is head of the propagating department. I know you will not grudge aiding by your advice a good man. I shall tell him that I have not the slightest power to aid him in any way for the appointment. I should think voyage out and home ought to be paid for?

LETTER 644. TO JOHN SCOTT. Down, May 25th, 1863.

Now for a few words on science. I do not think I could be mistaken about the stigma of Bolbophyllum (644/1. Bolbophyllum is remarkable for the closure of the stigmatic cavity which comes on after the flower has been open a little while, instead of after fertilisation, as in other genera. Darwin connects the fact with the "exposed condition of the whole flower."—"Fertilisation of Orchids," Edition II., page 137.); I had the plant alive from Kew, and watched many flowers. That is a most remarkable observation on foreign pollen emitting tubes, but not causing orifice to close (644/2. See Scott, "Bot. Soc. Edin." 1863, page 546, note. He applied pollinia from Cypripedium and Asclepias to flowers of Tricopilia tortilis; and though the pollen germinated, the stigmatic chamber remained open, yet it invariably closes eighteen hours after the application of its own pollen.); it would have been interesting to have observed how close an alliance of form would have acted on the orifice of the stigma. It will probably be so many years, if ever, {before} I work up my observations on Drosera, that I will not trouble you to send your paper, for I could not now find time to read it. If you have spare copy of your Orchid paper, please send it, but do not get a copy of the journal, for I can get one, and you must often want to buy books. Let me know when it is published. I have been glad to hear about Mercurialis, but I will not accept your offer of seed on account of time, time, time, and weak health. For the same reason I must give up Primula mollis. What a wonderful, indefatigable worker you are! You seem to have made a famous lot of interesting experiments. D. Beaton once wrote that no man could cross any species of Primula. You have apparently proved the contrary with a vengeance. Your numerous experiments seem very well selected, and you will exhaust the subject. Now when you have completed your work you should draw up a paper, well worth publishing, and give a list of all the dimorphic and non-dimorphic forms. I can give you, on the authority of Prof. Treviranus in "Bot. Zeitung," case of P. longiflora non-dimorphic. I am surprised at your cowslips in this state. Is it a common yellow cowslip? I have seen oxlips (which from some experiments I now look at as certainly natural hybrids) in same state. If you think the Botanical Society of Edinburgh would not do justice and publish your paper, send it to me to be communicated to the Linnean Society. I will delay my paper on successive dimorphic generations in Primula (644/3. Published in the "Journ. Linn. Soc." X., 1869 {1868}.) till yours appears, so as in no way to interfere with your paper. Possibly my results may be hardly worth publishing, but I think they will; the seedlings from two successive homomorphic generations seem excessively sterile. I will keep this letter till I hear from Dr. Hooker. I shall be very glad if you try Passiflora. Your experiments on Primula seem so well chosen that whatever the result is they will be of value. But always remember that not one naturalist out of a dozen cares for really philosophical experiments.

LETTER 645. TO J. SCOTT. Down, May 31st {1863}.

I am unwell, and must write briefly. I am very much obliged for the "Courant." (645/1. The Edinburgh "Evening Courant" used to publish notices of the papers read at the Botanical Society of Edinburgh. The paper referred to here was Scott's on Oncidium.) The facts will be of highest use to me. I feel convinced that your paper will have permanent value. Your case seems excellently and carefully worked out. I agree that the alteration of title was unfortunate, but, after all, title does not signify very much. So few have attended to such points that I do not expect any criticism; but if so, I should think you had much better reply, but I could if you wished it much. I quite understand about the cases being individual sterility; so Gartner states it was with him. Would it be worth while to send a corrected copy of the "Courant" to the "Gardeners' Chronicle?" (645/2. An account of Scott's work appeared in the "Gardeners' Chronicle," June 13th, 1863, which is, at least partly, a reprint of the "Courant," since it contains the awkward sentence criticised by Darwin and referred to below. The title is "On the Fertilisation of Orchids," which was no doubt considered unfortunate as not suggesting the subject of the paper, and as being the same as that of Darwin's book.) I did not know that you had tried Lobelia fulgens: can you give me any particulars on the number of plants and kinds used, etc., that I may quote, as in a few days I shall be writing on this whole subject? No one will ever convince me that it is not a very important subject to philosophical naturalists. The Hibiscus seems a very curious case, and I agree with your remarks. You say that you are glad of criticisms (by the way avoid "former and latter," the reader is always forced to go back to look). I think you would have made the case more striking if you had first showed that the pollen of Oncidium sphacelatum was good; secondly, that the ovule was capable of fertilisation; and lastly, shown that the plant was impotent with its own pollen. "Impotence of organs capable of elimination"—capable here strictly refers to organs; you mean to impotence. To eliminate impotence is a curious expression; it is removing a non-existent quality. But style is a trifle compared with facts, and you are capable of writing well. I find it a good rule to imagine that I want to explain the case in as few and simple words as possible to one who knows nothing of the subject. (645/3. See Letter 151, Volume I.) I am tired. In my opinion you are an excellent observer.

LETTER 646. TO J. SCOTT. Down, June 6th, 1863.

I fear that you think that I have done more than I have with respect to Dr. Hooker. I did not feel that I had any right to ask him to remember you for a colonial appointment: all that I have done is to speak most highly of your scientific merits. Of course this may hereafter fructify. I really think you cannot go on better, for educational purposes, than you are now doing,—observing, thinking, and some reading beat, in my opinion, all systematic education. Do not despair about your style; your letters are excellently written, your scientific style is a little too ambitious. I never study style; all that I do is to try to get the subject as clear as I can in my own head, and express it in the commonest language which occurs to me. But I generally have to think a good deal before the simplest arrangement and words occur to me. Even with most of our best English writers, writing is slow work; it is a great evil, but there is no help for it. I am sure you have no cause to despair. I hope and suppose your sending a paper to the Linnean Society will not offend your Edinburgh friends; you might truly say that you sent the paper to me, and that (if it turns out so) I thought it worth communicating to the Linnean Society. I shall feel great interest in studying all your facts on Primula, when they are worked out and the seed counted. Size of capsules is often very deceptive. I am astonished how you can find time to make so many experiments. If you like to send me your paper tolerably well written, I would look it over and suggest any criticisms; but then this would cause you extra copying. Remember, however, that Lord Brougham habitually wrote everything important three times over. The cases of the Primulae which lose by variation their dimorphic characters seem to me very interesting. I find that the mid-styled (by variation) P. sinensis is more fertile with own pollen, even, than a heteromorphic union! If you have time it will be very good to experiment on Linum Lewisii. I wrote formerly to Asa Gray begging for seed. If you have time, I think experiments on any peloric flowers would be useful. I shall be sorry (and I am certain it is a mistake on the part of the Society) if your orchid paper is not printed in extenso. I am now at work compiling all such cases, and shall give a very full abstract of all your observations. I hope to add in autumn some from you on Passiflora. I would suggest to you the advantage, at present, of being very sparing in introducing theory in your papers (I formerly erred much in Geology in that way): LET THEORY GUIDE YOUR OBSERVATIONS, but till your reputation is well established be sparing in publishing theory. It makes persons doubt your observations. How rarely R. Brown ever indulged in theory: too seldom perhaps! Do not work too hard, and do not be discouraged because your work is not appreciated by the majority.

LETTER 647. TO J. SCOTT. July 2nd {1863?}

Many thanks for capsules. I would give table of the Auricula (647/1. In Scott's paper ("Linn. Soc. Journ." VIII.) many experiments on the Auricula are recorded.), especially owing to enclosed extract, which you can quote. Your facts about varying fertility of the primulas will be appreciated by but very few botanists; but I feel sure that the day will come when they will be valued. By no means modify even in the slightest degree any result. Accuracy is the soul of Natural History. It is hard to become accurate; he who modifies a hair's breadth will never be accurate. It is a golden rule, which I try to follow, to put every fact which is opposed to one's preconceived opinion in the strongest light. Absolute accuracy is the hardest merit to attain, and the highest merit. Any deviation is ruin. Sincere thanks for all your laborious trials on Passiflora. I am very busy, and have got two of my sons ill—I very much fear with scarlet fever; if so, no more work for me for some days or weeks. I feel greatly interested about your Primula cases. I think it much better to count seed than to weigh. I wish I had never weighed; counting is more accurate, though so troublesome.

LETTER 648. TO J. SCOTT. Down, 25th {1863?}

From what you say I looked again at "Bot. Zeitung." (648/1. "Ueber Dichogamie," "Bot. Zeit." January 1863.) Treviranus speaks of P. longiflora as short-styled, but this is evidently a slip of the pen, for further on, I see, he says the stigma always projects beyond anthers. Your experiments on coloured primroses will be most valuable if proved true. (648/2. The reference seems to be to Scott's observation that the variety rubra of the primrose was sterile when crossed with pollen from the common primrose. Darwin's caution to Scott was in some measure justified, for in his experiments on seedlings raised by self-fertilisation of the Edinburgh plants, he failed to confirm Scott's result. See "Forms of Flowers," Edition II., page 225. Scott's facts are in the "Journal Linn. Soc." VIII., page 97 (read February 4th, 1864).) I will advise to best of my power when I see MS. If evidence is not good I would recommend you, for your reputation's sake, to try them again. It is not likely that you will be anticipated, and it is a great thing to fully establish what in future time will be considered an important discovery (or rediscovery, for no one has noticed Gartner's facts). I will procure coloured primroses for next spring, but you may rely I will not publish before you. Do not work too hard to injure your health. I made some crosses between primrose and cowslip, and I send the results, which you may use if you like. But remember that I am not quite certain that I well castrated the short-styled primrose; I believe any castration would be superfluous, as I find all {these} plants sterile when insects are excluded. Be sure and save seed of the crossed differently coloured primroses or cowslips which produced least seed, to test the fertility of the quasi-hybrid seedlings. Gartner found the common primrose and cowslip very difficult to cross, but he knew nothing on dimorphism. I am sorry about delay {of} your orchid paper; I should be glad of abstract of your new observations of self-sterility in orchids, as I should probably use the new facts. There will be an important paper in September in "Annals and Magazine of Natural History," on ovules of orchids being formed after application of pollen, by Dr. F. Hildebrand of Bonn. (648/3.

"Ann. Mag. Nat. Hist." XII., 1863, page 169. The paper was afterwards published in the "Bot. Zeitung," 1863.)

LETTER 649. TO J. SCOTT. Down, November 7th {1863}.

Every day that I could do anything, I have read a few pages of your paper, and have now finished it, and return it registered. (649/1. This refers to the MS. of Scott's paper on the Primulaceae, "Linn. Soc. Journ." VIII. {February 4th, 1864} 1865.) It has interested me deeply, and is, I am sure, an excellent memoir. It is well arranged, and in most parts well written. In the proof sheets you can correct a little with advantage. I have suggested a few alterations in pencil for your consideration, and have put in here and there a slip of paper. There will be no occasion to rewrite the paper—only, if you agree with me, to alter a few pages. When finished, return it to me, and I will with the highest satisfaction communicate it to the Linnean Society. I should be proud to be the author of the paper. I shall not have caused much delay, as the first meeting of the Society was on November 5th. When your Primula paper is finished, if you are so inclined, I should like to hear briefly about your Verbascum and Passiflora experiments. I tried Verbascum, and have got the pods, but do not know when I shall be able to see to the results. This subject might make another paper for you. I may add that Acropera luteola was fertilised by me, and had produced two fine pods. I congratulate you on your excellent paper.

P.S.—In the summary to Primula paper can you conjecture what is the typical or parental form, i.e. equal, long or short styled?

LETTER 650. TO J.D. HOOKER. Down, {January 24th, 1864}.

(650/1. Darwin's interest in Scott's Primula work is shown by the following extracts from a letter to Hooker of January 24th, 1864, written, therefore, before the paper was read, and also by the subsequent correspondence with Hooker and Asa Gray. The first part of this letter illustrates Darwin's condition during a period of especially bad health.)

As I do nothing all day I often get fidgety, and I now fancy that Charlie or some of your family {are} ill. When you have time let me have a short note to say how you all are. I have had some fearful sickness; but what a strange mechanism one's body is; yesterday, suddenly, I had a slight attack of rheumatism in my back, and I instantly became almost well, and so wonderfully strong that I walked to the hot-houses, which must be more than a hundred yards. I have sent Scott's paper to the Linnean Society; I feel sure it is really valuable, but I fear few will care about it. Remember my URGENT wish to be able to send the poor fellow a word of praise from any one. I have had work to get him to allow me to send the paper to the Linnean Society, even after it was written out.

LETTER 651. TO J. SCOTT. Down, February 9th, 1864.

(651/1. Scott's paper on Primulaceae was read at the Linnean Society on February 4th, 1864.)

The President, Mr. Bentham, I presume, was so much struck by your paper that he sent me a message to know whether you would like to be elected an associate. As only one is elected annually, this is a decided honour. The enclosed list shows what respectable men are associates. I enclose the rules of admission. I feel sure that the rule that if no communication is received within three years the associate is considered to have voluntarily withdrawn, is by no means rigorously adhered to. Therefore, I advise you to accept; but of course the choice is quite free. You will see there is no payment. You had better write to me on this subject, as Dr. Hooker or I will propose you.

LETTER 652. TO J.D. HOOKER. September 13th, 1864.

I have been greatly interested by Scott's paper. I probably overrate it from caring for the subject, but it certainly seems to me one of the very most remarkable memoirs on such subjects which I have ever read. From the subject being complex, and the style in parts obscure, I suppose very few will read it. I think it ought to be noticed in the "Natural History Review," otherwise the more remarkable facts will never be known. Try and persuade Oliver to do it; with the summary it would not be troublesome. I would offer, but I have sworn to myself I will do nothing till my volume on "Variation under Domestication" is complete. I know you will not have time to read Scott, and therefore I will just point out the new and, as they seem to me, important points.

Firstly, the red cowslip, losing its dimorphic structure and changing so extraordinarily in its great production of seed with its own pollen, especially being nearly sterile when fertilised by, or fertilising, the common cowslip. The analogous facts with red and white primrose. Secondly, the utter dissimilarity of action of the pollen of long- and short-styled form of one species in crossing with a distinct species. And many other points. Will you suggest to Oliver to review this paper? if he does so, and if it would be of any service to him, I would (as I have attended so much to these subjects) just indicate, with pages, leading and new points. I could send him, if he wishes, a separate and spare copy marked with pencil.

LETTER 653. TO ASA GRAY. September 13th {1864}.

(653/1. In September, 1864, Darwin wrote to Asa Gray describing Scott's work on the Primulaceae as:—)

A paper which has interested me greatly by a gardener, John Scott; it seems to me a most remarkable production, though written rather obscurely in parts, but worth the labour of studying. I have just bethought me that for the chance of your noticing it in the "Journal," I will point out the new and very remarkable facts. I have paid the poor fellow's passage out to India, where I hope he will succeed, as he is a most laborious and able man, with the manners almost of a gentleman.

(653/2. The following is an abstract of the paper which was enclosed in the letter to Asa Gray.)

Pages 106-8. Red cowslip by variation has become non-dimorphic, and with this change of structure has become much more productive of seed than even the heteromorphic union of the common cowslip. Pages 91-2, similar case with Auricula; on the other hand a non-dimorphic variety of P. farinosa (page 115) is less fertile. These changes, or variations, in the generative system seem to me very remarkable. But far more remarkable is the fact that the red cowslip (pages 106-8) is very sterile when fertilising, or fertilised by the common cowslip. Here we have a new "physiological species." Analogous facts given (page 98) on the crossing of red and white primroses with common primroses. It is very curious that the two forms of the same species (pages 93, 94, 95, and 117) hybridise with extremely different degrees of facility with distinct species.

He shows (page 94) that sometimes a cross with a quite distinct species yields more seed than a homomorphic union with own pollen. He shows (page 111) that of the two homomorphic unions possible with each dimorphic species the short-styled (as I stated) is the most sterile, and that my explanation is probably true. There is a good summary to the paper.

LETTER 654. TO J.D. HOOKER.

(654/1. The following letters to Hooker, April 1st, April 5th and May 22nd, refer to Darwin's scheme of employing Scott as an assistant at Down, and to Scott's appointment to the Botanic Garden at Calcutta.)

Down, April 1st, 1864.

I shall not at present allude to your very interesting letter (which as yet has been read to me only twice!), for I am full of a project which I much want you to consider.

You will have seen Scott's note. He tells me he has no plans for the future. Thinking over all his letters, I believe he is a truly remarkable man. He is willing to follow suggestions, but has much originality in varying his experiments. I believe years may pass before another man appears fitted to investigate certain difficult and tedious points—viz. relative fertility of varieties of plants, including peloric and other monsters (already Scott has done excellent work on this head); and, secondly, whether a plant's own pollen is less effective than that of another individual. Now, if Scott is moderate in his wishes, I would pay him for a year or two to work and publish on these or other such subjects which might arise. But I dare not have him here, for it would quite overwork me. There would not be plants sufficient for his work, and it would probably be an injury to himself, as it would put him out of the way of getting a good situation. Now, I believe you have gardeners at Kew who work and learn there without pay. What do you think of having Scott there for a year or two to work and experiment? I can see enormous difficulties. In the first place you will not perhaps think the points indicated so highly important as I do. Secondly, he would require ground in some out-of-the-way place where the plants could be covered by a net, which would be unsightly. On the other hand, I presume you would like a series of memoirs published on work done at Kew, which I am fully convinced would have permanent value. It would, of course I conceive, be absolutely necessary that Scott should be under the regular orders of the superintendent. The only way I can fancy that it could be done would be to explain to the superintendent that I temporarily supported Scott solely for the sake of science, and appeal to his kindness to assist him. If you approved of having him (which I can see is improbable), and you simply ordered the superintendent to assist him, I believe everything would go to loggerheads. As for Scott himself, it would be of course an advantage to him to study the cultivation at Kew. You would get to know him, and if he really is a good man you could perhaps be able to recommend him to some situation at home or abroad. Pray turn this {over} in your mind. I have no idea whether Scott would like the place, but I can see that he has a burning zeal for science. He told me that his parents were in better circumstances, and that he chose a gardener's life solely as the best way of following science. I may just add that in his last letter he gives me the results of many experiments on different individuals of the same species of orchid, showing the most remarkable diversity in their sexual condition. It seems to me a grievous loss that such a man should have all his work cut short. Please remember that I know nothing of him excepting from his letters: these show remarkable talent, astonishing perseverance, much modesty, and what I admire, determined difference from me on many points.

What will Sir William say?

LETTER 655. TO J.D. HOOKER. Down, April 5th {1864}.

I see my scheme for Scott has invincible difficulties, and I am very much obliged to you for explaining them at such length. If ever I get decently well, and Scott is free and willing, I will have him here for a couple of years to work out several problems, which otherwise would never be done. I cannot see what will become of the poor fellow. I enclose a little pamphlet from him, which I suppose is not of much scientific value, but is surprising as the work of a gardener.

If you have time do just glance over it. I never heard anything so extraordinary as what you say about poisoning plants, etc.

...The post has just come in. Your interest about Scott is extraordinarily kind, and I thank you cordially. It seems absurd to say so, but I suspect that X is prejudiced against Scott because he partially supports my views. (655/1. In a letter to Scott (dated June 11th) Darwin warns him to keep his views "pretty quiet," and quotes Hooker's opinion that "if it is known that you agree at all with my views on species it is enough to make you unpopular in Edinburgh.")

You must not trust my former letter about Clematis. I worked on too old a plant, and blundered. I have now gone over the work again. It is really curious that the stiff peduncles are acted upon by a bit of thread weighing .062 of a grain.

Clematis glandulosa was a valuable present to me. My gardener showed it to me and said, "This is what they call a Clematis," evidently disbelieving it. So I put a little twig to the peduncle, and the next day my gardener said, "You see it is a Clematis, for it feels." That's the way we make out plants at Down.

My dear old friend, God bless you!

LETTER 656. TO J.D. HOOKER. {May 22nd, 1864}.

What a good kind heart you have got. You cannot tell how your letter has pleased me. I will write to Scott and ask him if he chooses to go out and risk engagement. If he will not he must want all energy. He says himself he wants stoicism, and is too sensitive. I hope he may not want courage. I feel sure he is a remarkable man, with much good in him, but no doubt many errors and blemishes. I can vouch for his high intellect (in my judgment he is the best observer I ever came across); for his modesty, at least in correspondence; and there is something high-minded in his determination not to receive money from me. I shall ask him whether he can get a good character for probity and sobriety, and whether he can get aid from his relations for his voyage out. I will help, and, if necessary, pay the whole voyage, and give him enough to support him for some weeks at Calcutta. I will write when I hear from him. God bless you; you, who are so overworked, are most generous to take so much trouble about a man you have had nothing to do with.

(656/1. Scott had left the Botanic Gardens at Edinburgh in March 1864, chagrined at what, justly or unjustly, he considered discouragement and slight. The Indian offer was most gladly and gratefully accepted.)

LETTER 657. TO J. SCOTT. Down, November 1st, 1871.

Dr. Hooker has forwarded to me your letter as the best and simplest plan of explaining affairs. I am sincerely grieved to hear of the pecuniary problem which you have undergone, but now fortunately passed. I assure you that I have never entertained any feelings in regard to you which you suppose. Please to remember that I distinctly stated that I did not consider the sum which I advanced as a loan, but as a gift; and surely there is nothing discreditable to you, under the circumstances, in receiving a gift from a rich man, as I am. Therefore I earnestly beg you to banish the whole subject from your mind, and begin laying up something for yourself in the future. I really cannot break my word and accept payment. Pray do not rob me of my small share in the credit of aiding to put the right man in the right place. You have done good work, and I am sure will do more; so let us never mention the subject again.

I am, after many interruptions, at work again on my essay on Expression, which was written out once many months ago. I have found your remarks the best of all which have been sent me, and so I state.

CHAPTER 2.XI.—BOTANY, 1863-1881.

2.XI.I. Miscellaneous, 1863-1866.—2.XI.II. Correspondence with Fritz Muller, 1865-1881.—2.XI.III. Miscellaneous, 1868-1881.

2.XI.I. MISCELLANEOUS, 1863-1866.

LETTER 658. TO D. OLIVER. Down {April, 1863}.

(658/1. The following letter illustrates the truth of Sir W. Thiselton-Dyer's remark that Darwin was never "afraid of his facts." (658/2. "Charles Darwin" (Nature Series), 1882, page 43.) The entrance of pollen-tubes into the nucellus by the chalaza, instead of through the micropyle, was first fully demonstrated by Treub in his paper "Sur les Casuarinees et leur place dans le Systeme naturel," published in the "Ann. Jard. Bot. Buitenzorg," X., 1891. Two years later Miss Benson gave an account of a similar phenomenon in certain Amentiferae ("Trans. Linn. Soc." 1888-94, page 409). This chalazogamic method of fertilisation has since been recognised in other flowering plants, but not, so far as we are aware, in the genus Primula.)

It is a shame to trouble {you}, but will you tell me whether the ovule of Primula is "anatropal," nearly as figured by Gray, page 123, "Lessons in Botany," or rather more tending to "amphitropal"? I never looked at such a point before. Why I am curious to know is because I put pollen into the ovarium of monstrous primroses, and now, after sixteen days, and not before (the length of time agrees with slowness of natural impregnation), I find abundance of pollen-tubes emitted, which cling firmly to the ovules, and, I think I may confidently state, penetrate the ovule. But here is an odd thing: they never once enter at (what I suppose to be) the "orifice," but generally at the chalaza...Do you know how pollen-tubes go naturally in Primula? Do they run down walls of ovarium, and then turn up the placenta, and so debouch near the "orifices" of the ovules?

If you thought it worth while to examine ovules, I would see if there are more monstrous flowers, and put pollen into the ovarium, and send you the flowers in fourteen or fifteen days afterwards. But it is rather troublesome. I would not do it unless you cared to examine the ovules. Like a foolish and idle man, I have wasted a whole morning over them...

In two ovules there was an odd appearance, as if the outer coat of ovule at the chalaza end (if I understand the ovule) had naturally opened or withered where most of the pollen-tubes seemed to penetrate, which made me at first think this was a widely open foramen. I wonder whether the ovules could be thus fertilised?

LETTER 659. TO D. OLIVER. Down {April, 1863}.

Many thanks about the Primula. I see that I was pretty right about the ovules. I have been thinking that the apparent opening at the chalaza end must have been withering or perhaps gnawing by some very minute insects, as the ovarium is open at the upper end. If I have time I will have another look at pollen-tubes, as, from what you say, they ought to find their way to the micropyle. But ovules to me are far more troublesome to dissect than animal tissue; they are so soft, and muddy the water.

LETTER 660. TO MAXWELL MASTERS. Down, April 6th {1863}.

I have been very glad to read your paper on Peloria. (660/1. "On the Existence of Two Forms of Peloria." "Natural History Review," April, 1863, page 258.) For the mere chance of the following case being new I send it. A plant which I purchased as Corydalis tuberosa has, as you know, one nectary—short, white, and without nectar; the pistil is bowed towards the true nectary; and the hood formed by the inner petals slips off towards the opposite side (all adaptations to insect agency, like many other pretty ones in this family). Now on my plants there are several flowers (the fertility of which I will observe) with both nectaries equal and purple and secreting nectar; the pistil is straight, and the hood slips off either way. In short, these flowers have the exact structure of Dielytra and Adlumia. Seeing this, I must look at the case as one of reversion; though it is one of the spreading of irregularity to two sides.

As columbine {Aquilegia} has all petals, etc., irregular, and as monkshood {Aconitum} has two petals irregular, may not the case given by Seringe, and referred to {by} you (660/2. "Seringe describes and figures a flower {of Aconitum} wherein all the sepals were helmet-shaped," and the petals similarly affected. Maxwell Masters, op. cit., page 260.), by you be looked at as reversion to the columbine state? Would it be too bold to suppose that some ancient Linaria, or allied form, and some ancient Viola, had all petals spur-shaped, and that all cases of "irregular peloria" in these genera are reversions to such imaginary ancient form? (660/3. "'Regular or Congenital Peloria' would include those flowers which, contrary to their usual habit, retain throughout the whole of their growth their primordial regularity of form and equality of proportion. 'Irregular or Acquired Peloria,' on the other hand, would include those flowers in which the irregularity of growth that ordinarily characterises some portions of the corolla is manifested in all of them." Maxwell Masters, loc. cit.)

It seems to me, in my ignorance, that it would be advantageous to consider the two forms of Peloria WHEN OCCURRING IN THE VERY SAME SPECIES as probably due to the same general law—viz., one as reversion to very early state, and the other as reversion to a later state when all the petals were irregularly formed. This seems at least to me a priori a more probable view than to look at one form of Peloria as due to reversion and the other as something distinct. (660/4. See Maxwell Masters, "Vegetable Teratology," 1869, page 235; "Variation of Animals and Plants," Edition II., Volume II., page 33.)

What do you think of this notion?

LETTER 661. TO P.H. GOSSE.

(661/1. The following was written in reply to Mr. Gosse's letter of May 30th asking for a solution of his difficulties in fertilising Stanhopea. It is reprinted by the kind permission of Mr. Edmund Gosse from his delightful book, the "Life of Philip Henry Gosse," London, 1890, page 299.)

Down, June 2nd, 1863.

It would give me real pleasure to resolve your doubts, but I cannot. I can give only suspicions and my grounds for them. I should think the non-viscidity of the stigmatic hollow was due to the plant not living under its natural conditions. Please see what I have said on Acropera. An excellent observer, Mr. J. Scott, of the Botanical Gardens, Edinburgh, finds all that I say accurate, but, nothing daunted, he with the knife enlarged the orifice and forced in pollen-masses; or he simply stuck them into the contracted orifice without coming into contact with the stigmatic surface, which is hardly at all viscid, when, lo and behold, pollen-tubes were emitted and fine seed capsules obtained. This was effected with Acropera Loddigesii; but I have no doubt that I have blundered badly about A. luteola. I mention all this because, as Mr. Scott remarks, as the plant is in our hot-houses, it is quite incredible it ever could be fertilised in its native land. The whole case is an utter enigma to me. Probably you are aware that there are cases (and it is one of the oddest facts in Physiology) of plants which, under culture, have their sexual functions in so strange a condition, that though their pollen and ovules are in a sound state and can fertilise and be fertilised by distinct but allied species, they cannot fertilise themselves. Now, Mr. Scott has found this the case with certain orchids, which again shows sexual disturbance. He had read a paper at the Botanical Society of Edinburgh, and I daresay an abstract which I have seen will appear in the "Gardeners' Chronicle"; but blunders have crept in in copying, and parts are barely intelligible. How insects act with your Stanhopea I will not pretend to conjecture. In many cases I believe the acutest man could not conjecture without seeing the insect at work. I could name common English plants in this predicament. But the musk-orchis {Herminium monorchis} is a case in point. Since publishing, my son and myself have watched the plant and seen the pollinia removed, and where do you think they invariably adhere in dozens of specimens?—always to the joint of the femur with the trochanter of the first pair of legs, and nowhere else. When one sees such adaptation as this, it would be hopeless to conjecture on the Stanhopea till we know what insect visits it. I have fully proved that my strong suspicion was correct that with many of our English orchids no nectar is excreted, but that insects penetrate the tissues for it. So I expect it must be with many foreign species. I forgot to say that if you find that you cannot fertilise any of your exotics, take pollen from some allied form, and it is quite probable that will succeed. Will you have the kindness to look occasionally at your bee-Ophrys near Torquay, and see whether pollinia are ever removed? It is my greatest puzzle. Please read what I have said on it, and on O. arachnites. I have since proved that the account of the latter is correct. I wish I could have given you better information.

P.S.—If the Flowers of the Stanhopea are not too old, remove pollen-masses from their pedicels, and stick them with a little liquid pure gum to the stigmatic cavity. After the case of the Acropera, no one can dare positively say that they would not act.

LETTER 662. TO J.D. HOOKER. Down, Saturday, 5th {December 1863}.

I am very glad that this will reach you at Kew. You will then get rest, and I do hope some lull in anxiety and fear. Nothing is so dreadful in this life as fear; it still sickens me when I cannot help remembering some of the many illnesses our children have endured. My father, who was a sceptical man, was convinced that he had distinctly traced several cases of scarlet fever to handling letters from convalescents.

The vases (662/1. Probably Wedgwood ware.) did come from my sister Susan. She is recovering, and was much pleased to hear that you liked them; I have now sent one of your notes to her, in which you speak of them as "enchanting," etc. I have had a bad spell—vomiting, every day for eleven days, and some days many times after every meal. It is astonishing the degree to which I keep up some strength. Dr. Brinton was here two days ago, and says he sees no reason {why} I may not recover my former degree of health. I should like to live to do a little more work, and often I feel sure I shall, and then again I feel that my tether is run out.

Your Hastings note, my dear old fellow, was a Copley Medal to me and more than a Copley Medal: not but what I know well that you overrate what I have been able to do. (662/2. The proposal to give the medal to Darwin failed in 1863, but his friends were successful in 1864: see "Life and Letters," III., page 28.) Now that I am disabled, I feel more than ever what a pleasure observing and making out little difficulties is. By the way, here is a very little fact which may interest you. A partridge foot is described in "Proc. Zoolog. Soc." with a huge ball of earth attached to it as hard as rock. (662/3. "Proc. Zool. Soc." 1863, page 127, by Prof. Newton, who sent the foot to Darwin: see "Origin," Edition VI., page 328.) Bird killed in

1860. Leg has been sent me, and I find it diseased, and no doubt the exudation caused earth to accumulate; now already thirty-two plants have come up from this ball of earth.

By Jove! I must write no more. Good-bye, my best of friends.

There is an Italian edition of the "Origin" preparing. This makes the fifth foreign edition—i.e. in five foreign countries. Owen will not be right in telling Longmans that the book would be utterly forgotten in ten years. Hurrah!

LETTER 663. TO D. OLIVER. Down, February 17th {1864}.

Many thanks for the Epacrids, which I have kept, as they will interest me when able to look through the microscope.

Dr. Cruger has sent me the enclosed paper, with power to do what I think fit with it. He would evidently prefer it to appear in the "Nat. Hist. Review." Please read it, and let me have your decision pretty soon. Some germanisms must be corrected; whether woodcuts are necessary I have not been able to pay attention enough to decide. If you refuse, please send it to the Linnean Society as communicated by me. (663/1. H. Cruger's "A Few Notes on the Fecundation of Orchids, etc." {Read March, 1864.} "Linn. Soc. Journ." VIII., 1864-5, page 127.) The paper has interested me extremely, and I shall have no peace till I have a good boast. The sexes are separate in Catasetum, which is a wonderful relief to me, as I have had two or three letters saying that the male C. tridentatum seeds. (663/2. See footnote Letter 608 on the sexual relation between the three forms known as Catasetum tridentatum, Monacanthus viridis, and Myanthus barbatus. For further details see Darwin, "Linn. Soc. Journ." VI., 1862, page 151, and "Fertilisation of Orchids," Edition II., page 196.) It is pretty clear to me that two or three forms are confounded under this name. Observe how curiously nearly perfect the pollen of the female is, according to Cruger,—certainly more perfect than the pollen from the Guyana species described by me. I was right in the manner in which the pollen adheres to the hairy back of the humble-bee, and hence the force of the ejection of the pollina. (663/3. This view was given in "Fertilisation of Orchids," Edition I., 1862, page 230.) I am still more pleased that I was right about insects gnawing the fleshy labellum. This is important, as it explains all the astounding projections on the labellum of Oncidium, Phalaenopsis, etc.

Excuse all my boasting. It is the best medicine for my stomach. Tell me whether you mean to take up orchids, as Hooker said you were thinking of doing. Do you know Coryanthes, with its wonderful basket of water? See what Cruger says about it. It beats everything in orchids. (663/4. For Coryanthes see "Fertilisation of Orchids," Edition II., page 173.)

LETTER 664. TO J.D. HOOKER. Down {September 13th, 1864}.

Thanks for your note of the 5th. You think much and greatly too much of me and my doings; but this is pleasant, for you have represented for many years the whole great public to me.

I have read with interest Bentham's address on hybridism. I am glad that he is cautious about Naudin's view, for I cannot think that it will hold. (664/1. C. Naudin's "Nouvelles Recherches sur l'Hydridite dans les Vegetaux." The complete paper, with coloured plates, was presented to the Academy in 1861, and published in full in the "Nouvelles Archives de Museum d'Hist. Nat." Volume I., 1865, page 25. The second part only appeared in the "Ann. Sci. Nat." XIX., 1863. Mr. Bentham's address dealing with hybridism is in "Proc. Linn. Soc." VIII., 1864, page ix. A review of Naudin is given in the "Natural History Review," 1864, page 50. Naudin's paper is of much interest, as containing a mechanical theory of reproduction of the same general character as that of pangenesis. In the "Variation of Animals and Plants," Edition II., Volume II., page 395, Darwin states that in his treatment of hybridism in terms of gemmules he is practically following Naudin's treatment of the same theme in terms of "essences." Naudin, however, does not clearly distinguish between hybrid and pure gemmules, and makes the assumption that the hybrid or mixed essences tend constantly to dissociate into pure parental essences, and thus lead to reversion. It is to this view that Darwin refers when he says that Naudin's view throws no light on the reversion to long-lost characters. His own attempt at explaining this fact occurs in "Variation under Domestication," II., Edition II., page 395. Mr. Bateson ("Mendel's Principle of Heredity," Cambridge, 1902, page 38) says: "Naudin clearly enuntiated what we shall henceforth know as the Mendelian conception of the dissociation of characters of cross-breds in the formation of the germ-cells, though apparently he never developed this conception." It is remarkable that, as far as we know, Darwin never in any way came across Mendel's work. One of Darwin's correspondents, however, the late Mr. T. Laxton, of Stamford, was close on the trail of Mendelian principle. Mr. Bateson writes (op. cit., page 181): "Had he {Laxton} with his other gifts combined this penetration which detects a great principle hidden in the thin mist of 'exceptions,' we should have been able to claim for him that honour which must ever be Mendel's in the history of discovery.") The tendency of hybrids to revert to either parent is part of a wider law (which I am fully convinced that I can show experimentally), namely, that crossing races as well as species tends to bring back characters which existed in progenitors hundreds and thousands of generations ago. Why this should be so, God knows. But Naudin's view throws no light, that I can see, on this reversion of long-lost characters. I wish the Ray Society would translate Gartner's "Bastarderzeugung"; it contains more valuable matter than all other writers put together, and would do great service if better known. (664/2. "Versuche uber die Bastarderzeugung im Pflanzenreich": Stuttgart, 1849.)

LETTER 665. TO T.H. HUXLEY.

(665/1. Mr. Huxley had doubted the accuracy of observations on Catasetum published in the "Fertilisation of Orchids." In what formed the postscript to the following letter, Darwin wrote: "I have had more Catasetums,—all right, you audacious 'caviller.'")

Down, October 31st {1862}.

In a little book, just published, called the "Three Barriers" (a theological hash of old abuse of me), Owen gives to the author a new resume of his brain doctrine; and I thought you would like to hear of this. He ends with a delightful sentence. "No science affords more scope or easier ground for the caviller and controversialist; and these do good by preventing scholars from giving more force to generalisations than the master propounding them does, or meant his readers or hearers to give."

You will blush with pleasure to hear that you are of some use to the master.

LETTER 666. TO J.D. HOOKER. {February, 1864?}

I shall write again. I write now merely to ask, if you have Naravelia (666/1. Ranunculaceae.) (the Clematis-like plant told me by Oliver), to try and propagate me a plant at once. Have you Clematis cirrhosa? It will amuse me to tell you why Clematis interests me, and why I should so very much like to have Naravelia. The leaves of Clematis have no spontaneous movement, nor have the internodes; but when by growth the peduncles of leaves are brought into contact with any object, they bend and catch hold. The slightest stimulus suffices, even a bit of cotton thread a few inches long; but the stimulus must be applied during six or twelve hours, and when the peduncles once bend, though the touching object be removed, they never get straight again. Now mark the difference in another leaf-climber—viz., Tropaeolum: here the young internodes revolve day and night, and the peduncles of the leaves are thus brought into contact with an object, and the slightest momentary touch causes them to bend in any direction and catch the object, but as the axis revolves they must be often dragged away without catching, and then the peduncles straighten themselves again, and are again ready to catch. So that the nervous system of Clematis feels only a prolonged touch—that of Tropaeolum a momentary touch: the peduncles of the latter recover their original position, but Clematis, as it comes into contact by growth with fixed objects, has no occasion to recover its position, and cannot do so. You did send me Flagellaria, but most unfortunately young plants do not have tendrils, and I fear my plant will not get them for another year, and this I much regret, as these leaf-tendrils seem very curious, and in Gloriosa I could not make out the action, but I have now a young plant of Gloriosa growing up (as yet with simple leaves) which I hope to make out. Thank Oliver for decisive answer about tendrils of vines. It is very strange that tendrils formed of modified leaves and branches should agree in all their four highly remarkable properties. I can show a beautiful gradation by which LEAVES produce tendrils, but how the axis passes into a tendril utterly puzzles me. I would give a guinea if vine-tednrils could be found to be leaves.

(666/2. It is an interesting fact that Darwin's work on climbing plants was well advanced before he discovered the existence of the works of Palm, Mohl, and Dutrochet on this subject. On March 22nd, 1864, he wrote to Hooker:—"You quite overrate my tendril work, and there is no occasion to plague myself about priority." In June he speaks of having read "two German books, and all, I believe, that has been written on climbers, and it has stirred me up to find that I have a good deal of new matter.")

LETTER 667. TO J.D. HOOKER. Down, June 2nd {1864}.

You once offered me a Combretum. (667/1. The two forms of shoot in C. argenteum are described in "Climbing Plants," page 41.) I having C. purpureum, out of modesty like an ass refused. Can you now send me a plant? I have a sudden access of furor about climbers. Do you grow Adlumia cirrhosa? Your seed did not germinate with me. Could you have a seedling dug up and potted? I want it fearfully, for it is a leaf-climber, and therefore sacred.

I have some hopes of getting Adlumia, for I used to grow the plant, and seedlings have often come up, and we are now potting all minute reddish-coloured weeds. (667/2. We believe that the Adlumia which came up year by year in flower boxes in the Down verandah grew from seed supplied by Asa Gray.) I have just got a plant with sensitive axis, quite a new case; and tell Oliver I now do not care at all how many tendrils he makes axial, which at one time was a cruel torture to me.

LETTER 668. TO J.D. HOOKER. Down, November 3rd {1864}.

Many thanks for your splendid long letter. But first for business. Please look carefully at the enclosed specimen of Dicentra thalictriformis, and throw away. (668/1. Dicentra thalictrifolia, a Himalayan species of Fumariaceae, with leaf-tendrils.) When the plant was young I concluded certainly that the tendrils were axial, or modified branches, which Mohl says is the case with some Fumariaceae. (668/2. "Ueber den Bau und das Winden der Ranken und Schlingpflanzen. Eine gekronte Preisschrift," 4to, Tubingen, 1827. At page 43 Mohl describes the tips of the branches of Fumaria {Corydalis} clavicualta as being developed into tendrils, as well as the leaves. For this reason Darwin placed the plant among the tendril-bearers rather than among the true leaf-climbers: see "Climbing Plants," Edition II., 1875, page 121.) You looked at them here and agreed. But now the plant is old, what I thought was a branch with two leaves and ending in a tendril looks like a gigantic leaf with two compound leaflets, and the terminal part converted into a tendril. For I see buds in the fork between supposed branch and main stem. Pray look carefully—you know I am profoundly ignorant—and save me from a horrid mistake.

LETTER 669. TO J.D. HOOKER.

(669/1. The following is interesting, as containing a foreshadowing of the chemotaxis of antherozoids which was shown to exist by Pfeffer in 1881: see "Untersuchungen aus dem botanischen Institut zu Tubingen," Volume I., page 363. There are several papers by H.J. Carter on the reproduction of the lower organisms in the "Annals and Magazine of Natural History" between 1855 and 1865.)

Down, Sunday, 22nd, and Saturday, 28th {October, 1865}.

I have been wading through the "Annals and Mag. of N. History." for last ten years, and have been interested by several papers, chiefly, however, translations; but none have interested me more than Carter's on lower vegetables, infusoria, and protozoa. Is he as good a workman as he appears? for if so he would deserve a Royal medal. I know it is not new; but how wonderful his account of the spermatozoa of some dioecious alga or conferva, swimming and finding the minute micropyle in a distinct plant, and forcing its way in! Why, these zoospores must possess some sort of organ of sense to guide their locomotive powers to the small micropyle; and does not this necessarily imply something like a nervous system, in the same way as complemental male cirripedes have organs of sense and locomotion, and nothing else but a sack of spermatozoa?

LETTER 670. TO F. HILDEBRAND. May 16th, 1866.

Since writing to you before, I have read your admirable memoir on Salvia (670/1. "Pringsheim's Jahrbucher," Volume IV., 1866.), and it has interested me almost as much as when I first investigated the structure of orchids. Your paper illustrates several points in my "Origin of Species," especially the transition of organs. Knowing only two or three species in the genus, I had often marvelled how one cell of the anther could have been transformed into the moveable plate or spoon; and how well you show the gradations. But I am surprised that you did not more strongly insist on this point.

I shall be still more surprised if you do not ultimately come to the same belief with me, as shown by so many beautiful contrivances,—that all plants require, from some unknown cause, to be occasionally fertilised by pollen from a distinct individual.

(PLATE: FRITZ MULLER.) 2.XI.II. CORRESPONDENCE WITH FRITZ MULLER, 1865-1881.

(671/1. The letters from Darwin to Muller are given as a separate group, instead of in chronological sequence with the other botanical letters, as better illustrating the uninterrupted friendship and scientific comradeship of the two naturalists.)

LETTER 671. TO F. MULLER. Down, October 17th {1865}.

I received about a fortnight ago your second letter on climbing plants, dated August 31st. It has greatly interested me, and it corrects and fills up a great hiatus in my paper. As I thought you could not object, I am having your letter copied, and will send the paper to the Linnean Society. (671/2. "Notes on some of the Climbing Plants near Desterro" {1865}, "Linn. Soc. Journ." IX., 1867.) I have slightly modified the arrangement of some parts and altered only a few words, as you write as good English as an Englishman. I do not quite understand your account of the arrangement of the leaves of Strychnos, and I think you use the word "bracteae" differently to what English authors do; therefore I will get Dr. Hooker to look over your paper.

I cannot, of course, say whether the Linnean Society will publish your paper; but I am sure it ought to do so. As the Society is rather poor, I fear that it will give only a few woodcuts from your truly admirable sketches.

LETTER 672. TO F. MULLER.

(672/1. In Darwin's book on Climbing Plants, 1875 (672/2. First given as a paper before the Linnean Society, and published in the "Linn. Soc. Journ." Volume IX.,), he wrote (page 205): "The conclusion is forced on our minds that the capacity of revolving, on which most climbing plants depend, is inherent, though undeveloped, in almost every plant in the vegetable Kingdom"—a conclusion which was verified in the "Power of Movement in Plants." The present letter is interesting in referring to Fritz Muller's observations on the "revolving nutation," or circumnutation of Alisma macrophylla and Linum usitatissimum, the latter fact having been discovered by F. Muller's daughter Rosa. This was probably the earliest observation on the circumnutation of a non-climbing plant, and Muller, in a paper dated 1868, and published in Volume V. of the "Jenaische Zeitschrift," page 133, calls attention to its importance in relation to the evolution of the habit of climbing. The present letter was probably written in 1865, since it refers to Muller's paper read before the Linnean Soc. on December 7th, 1865. If so, the facts on circumnutation must have been communicated to Darwin some years before their publication in the "Jenaische Zeitschrift.")

Down, December 9th {1865}.

I have received your interesting letter of October 10th, with its new facts on branch-tendrils. If the Linnean Society publishes your paper (672/3. Ibid., 1867, page 344.), as I am sure it ought to do, I will append a note with some of these new facts.

I forwarded immediately your MS. to Professor Max Schultze, but I did not read it, for German handwriting utterly puzzles me, and I am so weak, I am capable of no exertion. I took the liberty, however, of asking him to send me a copy, if separate ones are printed, and I reminded him about the Sponge paper.

You will have received before this my book on orchids, and I wish I had known that you would have preferred the English edition. Should the German edition fail to reach you, I will send an English one. That is a curious observation of your daughter about the movement of the apex of the stem of Linum, and would, I think, be worth following out. (672/4. F. Muller, "Jenaische Zeitschrift," Bd.

V., page 137. Here, also, are described the movements of Alisma.) I suspect many plants move a little, following the sun; but all do not, for I have watched some pretty carefully.

I can give you no zoological news, for I live the life of the most secluded hermit.

I occasionally hear from Ernest Hackel, who seems as determined as you are to work out the subject of the change of species. You will have seen his curious paper on certain medusae reproducing themselves by seminal generation at two periods of growth.

(672/5. On April 3rd, 1868, Darwin wrote to F. Muller: "Your diagram of the movements of the flower-peduncle of the Alisma is extremely curious. I suppose the movement is of no service to the plant, but shows how easily the species might be converted into a climber. Does it bend through irritability when rubbed?"

LETTER 673. TO F. MULLER. Down, September 25th {1866}.

I have just received your letter of August 2nd, and am, as usual, astonished at the number of interesting points which you observe. It is quite curious how, by coincidence, you have been observing the same subjects that have lately interested me.

Your case of the Notylia is quite new to me (673/1. See F. Muller, "Bot. Zeitung," 1868, page 630; "Fertilisation of Orchids," Edition II., page 171.); but it seems analogous with that of Acropera, about the sexes of which I blundered greatly in my book. I have got an Acropera now in flower, and have no doubt that some insect, with a tuft of hairs on its tail, removes by the tuft, the pollinia, and inserts the little viscid cap and the long pedicel into the narrow stigmatic cavity, and leaves it there with the pollen-masses in close contact with, but not inserted into, the stigmatic cavity. I find I can thus fertilise the flowers, and so I can with Stanhopea, and I suspect that this is the case with your Notylia. But I have lately had an orchis in flower—viz. Acineta, which I could not anyhow fertilise. Dr. Hildebrand lately wrote a paper (673/2. "Bot. Zeitung," 1863, 1865.) showing that with some orchids the ovules are not mature and are not fertilised until months after the pollen-tubes have penetrated the column, and you have independently observed the same fact, which

I never suspected in the case of Acropera. The column of such orchids must act almost like the spermatheca of insects. Your orchis with two leaf-like stigmas is new to me; but I feel guilty at your wasting your valuable time in making such beautiful drawings for my amusement.

Your observations on those plants being sterile which grow separately, or flower earlier than others, are very interesting to me: they would be worth experimenting on with other individuals. I shall give in my next book several cases of individual plants being sterile with their own pollen. I have actually got on my list Eschscholtzia (673/3. See "Animals and Plants," II., Edition II., page 118.) for fertilising with its own pollen, though I did not suspect it would prove sterile, and I will try next summer. My object is to compare the rate of growth of plants raised from seed fertilised by pollen from the same flower and by pollen from a distinct plant, and I think from what I have seen I shall arrive at interesting results. Dr. Hildebrand has lately described a curious case of Corydalis cava which is quite sterile with its own pollen, but fertile with pollen of any other individual plant of the species. (673/4. "International Horticultural Congress," London, 1866, quoted in "Variation of Animals and Plants," Edition II., Volume II., page 113.) What I meant in my paper on Linum about plants being dimorphic in function alone, was that they should be divided into two equal bodies functionally but not structurally different. I have been much interested by what you say on seeds which adhere to the valves being rendered conspicuous. You will see in the new edition of the "Origin" (673/5. "Origin of Species," Edition IV., 1866, page 238. A discussion on the origin of beauty, including the bright colours of flowers and fruits.) why I have alluded to the beauty and bright colours of fruit; after writing this it troubled me that I remembered to have seen brilliantly coloured seed, and your view occurred to me. There is a species of peony in which the inside of the pod is crimson and the seeds dark purple. I had asked a friend to send me some of these seeds, to see if they were covered with anything which could prove attractive to birds. I received some seeds the day after receiving your letter, and I must own that the fleshy covering is so thin that I can hardly believe it would lead birds to devour them; and so it was in an analogous case with Passiflora gracilis. How is this in the cases mentioned by you? The whole case seems to me rather a striking one.

I wish I had heard of Mikania being a leaf-climber before your paper was printed (673/6. See "Climbing Plants (3rd thousand, 1882), page 116. Mikania and Mutisia both belong to the Compositae. Mikania scandens is a twining plant: it is another species which, by its leaf-climbing habit, supplies a transition to the tendril-climber Mutisia. F. Muller's paper is in "Linn. Soc. Journ." IX., page 344.), for we thus get a good gradation from M. scandens to Mutisia, with its little modified, leaf-like tendrils.

I am glad to hear that you can confirm (but render still more wonderful) Hackel's most interesting case of Linope. Huxley told me that he thought the case would somehow be explained away.

LETTER 674. TO F. MULLER. Down {Received January 24th, 1867}.

I have so much to thank you for that I hardly know how to begin. I have received the bulbils of Oxalis, and your most interesting letter of October 1st. I planted half the bulbs, and will plant the other half in the spring. The case seems to me very curious, and until trying some experiments in crossing I can form no conjecture what the abortion of the stamens in so irregular a manner can signify. But I fear from what you say the plant will prove sterile, like so many others which increase largely by buds of various kinds. Since I asked you about Oxalis, Dr. Hildebrand has published a paper showing that a great number of species are trimorphic, like Lythrum, but he has tried hardly any experiments. (674/1. Hildebrand's work, published in the "Monatsb. d. Akad. d. Wiss. Berlin," 1866, was chiefly on herbarium specimens. His experimental work was published in the "Bot. Zeitung," 1871.)

I am particularly obliged for the information and specimens of Cordia (674/2. Cordiaceae: probably dimorphic.), and shall be most grateful for seed. I have not heard of any dimorphic species in this family. Hardly anything in your letter interested me so much as your account and drawing of the valves of the pod of one of the Mimoseae with the really beautiful seeds. I will send some of these seeds to Kew to be planted. But these seeds seem to me to offer a very great difficulty. They do not seem hard enough to resist the triturating power of the gizzard of a gallinaceous bird, though they must resist that of some other birds; for the skin is as hard as ivory. I presume that these seeds cannot be covered with any attractive pulp? I soaked one of the seeds for ten hours in warm water, which became only very slightly mucilaginous. I think I will try whether they will pass through a fowl uninjured. (674/3. The seeds proved to be those of Adenanthera pavonina. The solution of the difficulty is given in the following extract from a letter to Muller, March 2nd, 1867: "I wrote to India on the subject, and I hear from Mr. J. Scott that parrots are eager for the seeds, and, wonderful as the fact is, can split them open with their beaks; they first collect a large number in their beaks, and then settle themselves to split them, and in doing so drop many; thus I have no doubt they are disseminated, on the same principle that the acorns of our oaks are most widely disseminated." Possibly a similar explanation may hold good for the brightly coloured seeds of Abrus precatorius.) I hope you will observe whether any bird devours them; and could you get any young man to shoot some and observe whether the seeds are found low down in the intestines? It would be well worth while to plant such seeds with undigested seeds for comparison. An opponent of ours might make a capital case against us by saying that here beautiful pods and seeds have been formed not for the good of the plant, but for the good of birds alone. These seeds would make a beautiful bracelet for one of my daughters, if I had enough. I may just mention that Euonymus europoeus is a case in point: the seeds are coated by a thin orange layer, which I find is sufficient to cause them to be devoured by birds.

I have received your paper on Martha {Posoqueria (674/4. "Bot. Zeitung," 1866.)}; it is as wonderful as the most wonderful orchis; Ernst Hackel brought me the paper and stayed a day with me. I have seldom seen a more pleasant, cordial, and frank man. He is now in Madeira, where he is going to work chiefly on the Medusae. His great work is now published, and I have a copy; but the german is so difficult I can make out but little of it, and I fear it is too large a work to be translated. Your fact about the number of seeds in the capsule of the Maxillaria (674/5. See "Animals and Plants," Edition II., Volume II., page 115.) came just at the right time, as I wished to give one or two such facts. Does this orchid produce many capsules?

I cannot answer your question about the aerial roots of Catasetum. I hope you have received the new edition of the "Origin." Your paper on climbing plants (674/6. "Linn. Soc. Journal," IX., 1867, page 344.) is printed, and I expect in a day or two to receive the spare copies, and I will send off three copies as before stated, and will retain some in case you should wish me to send them to any one in Europe, and will transmit the remainder to yourself.

LETTER 675. TO F. MULLER. Down {received February 24th, 1867}.

Your letter of November 2nd contained an extraordinary amount of interesting matter. What a number of dimorphic plants South Brazil produces: you observed in one day as many or more dimorphic genera than all the botanists in Europe have ever observed. When my present book is finished I shall write a final paper upon these plants, so that I am extremely glad to hear of your observations and to see the dried flowers; nevertheless, I should regret MUCH if I prevented you from publishing on the subject. Plumbago (675/1. Plumbago has not been shown to be dimorphic.) is quite new to me, though I had suspected it. It is curious how dimorphism prevails by groups throughout the world, showing, as I suppose, that it is an ancient character; thus Hedyotis is dimorphic in India (675/2. Hedyotis was sent to Darwin by F. Muller; it seems possible, therefore, that Hedyotis was written by mistake for some other Rubiaceous plant, perhaps Oldenlandia, which John Scott sent him from India.); the two other genera in the same sub-family with Villarsia are dimorphic in Europe and Ceylon; a sub-genus of Erythroxylon (675/3. No doubt Sethia.) is dimorphic in Ceylon, and Oxalis with you and at the Cape of Good Hope. If you can find a dimorphic Oxalis it will be a new point, for all known species are trimorphic or monomorphic. The case of Convolvulus will be new, if proved. I am doubtful about Gesneria (675/4. Neither Convolvulus nor Gesneria have been shown to be dimorphic.), and have been often myself deceived by varying length of pistil. A difference in the size of the pollen-grains would be conclusive evidence; but in some cases experiments by fertilisation can alone decide the point. As yet I know of no case of dimorphism in flowers which are very irregular; such flowers being apparently always sufficiently visited and crossed by insects.

LETTER 676. TO F. MULLER. Down, April 22nd {1867}.

I am very sorry your papers on climbing plants never reached you. They must be lost, but I put the stamps on myself and I am sure they were right. I despatched on the 20th all the remaining copies, except one for myself. Your letter of March 4th contained much interesting matter, but I have to say this of all your letters. I am particularly glad to hear that Oncidium flexuosum (676/1. See "Animals and Plants," Edition II., Volume II., page 114. Observations on Oncidium were made by John Scott, and in Brazil by F. Muller, who "fertilised above one hundred flowers of the above-mentioned Oncidium flexuosum, which is there endemic, with its own pollen, and with that taken from distinct plants: all the former were sterile, whilst those fertilised by pollen from any OTHER PLANT of the same species were fertile.') is endemic, for I always thought that the cases of self-sterility with orchids in hot-houses might have been caused by their unnatural conditions. I am glad, also, to hear of the other analogous cases, all of which I will give briefly in my book that is now printing. The lessened number of good seeds in the self-fertilising Epidendrums is to a certain extent a new case. You suggest the comparison of the growth of plants produced from self-fertilised and crossed seeds. I began this work last autumn, and the result, in some cases, has been very striking; but only, as far as I can yet judge, with exotic plants which do not get freely crossed by insects in this country. In some of these cases it is really a wonderful physiological fact to see the difference of growth in the plants produced from self-fertilised and crossed seeds, both produced by the same parent-plant; the pollen which has been used for the cross having been taken from a distinct plant that grew in the same flower-pot. Many thanks for the dimorphic Rubiaceous plant. Three of your Plumbagos have germinated, but not as yet any of the Lobelias. Have you ever thought of publishing a work which might contain miscellaneous observations on all branches of Natural History, with a short description of the country and of any excursions which you might take? I feel certain that you might make a very valuable and interesting book, for every one of your letters is so full of good observations. Such books, for instance Bates' "Travels on the Amazons," are very popular in England. I will give your obliging offer about Brazilian plants to Dr. Hooker, who was to have come here to-day, but has failed. He is an excellent good fellow, as well as naturalist. He has lately published a pamphlet, which I think you would like to read; and I will try and get a copy and send you. (676/2. Sir J.D. Hooker's lecture on Insular Floras, given before the British Association in August, 1866, is doubtless referred to. It appeared in the "Gardeners' Chronicle," and was published as a pamphlet in January, 1867. This fact helps to fix the date of the present letter.)

LETTER 677. TO F. MULLER.

(677/1. The following refers to the curious case of Eschscholtzia described in "Cross and Self-Fertilisation," pages 343-4. The offspring of English plants after growing for two generations in Brazil became self-sterile, while the offspring of Brazilian plants became partly self-fertile in England.)

January 30th {1868}.

...The flowers of Eschscholtzia when crossed with pollen from a distinct plant produced 91 per cent. of capsules; when self-fertilised the flowers produced only 66 per cent. of capsules. An equal number of crossed and self-fertilised capsules contained seed by weight in the proportion of 100 to 71. Nevertheless, the self-fertilised flowers produced an abundance of seed. I enclose a few crossed seeds in hopes that you will raise a plant, cover it with a net, and observe whether it is self-fertile; at the same time allowing several uncovered plants to produce capsules, for the sterility formerly observed by you seems to me very curious.

LETTER 678. TO F. MULLER. Down, November 28th {1868}.

You end your letter of September 9th by saying that it is a very dull one; indeed, you make a very great mistake, for it abounds with interesting facts and thoughts. Your account of the tameness of the birds which apparently have wandered from the interior, is very curious. But I must begin on another subject: there has been a great and very vexatious, but unavoidable delay in the publication of your book. (678/1. "Facts and Arguments for Darwin," 1869, a translation by the late Mr. Dallas of F. Muller's "Fur Darwin," 1864: see Volume I., Letter 227.) Prof. Huxley agrees with me that Mr. Dallas is by far the best translator, but he is much overworked and had not quite finished the translation about a fortnight ago. He has charge of the Museum at York, and is now trying to get the situation of Assistant Secretary at the Geological Society; and all the canvassing, etc., and his removal, if he gets the place, will, I fear, cause more than a month's delay in the completion of the translation; and this I very much regret.

I am particularly glad to hear that you intend to repeat my experiments on illegitimate offspring, for no one's observations can be trusted until repeated. You will find the work very troublesome, owing to the death of plants and accidents of all kinds. Some dimorphic plant will probably prove too sterile for you to raise offspring; and others too fertile for much sterility to be expected in their offspring. Primula is bad on account of the difficulty of deciding which seeds may be considered as good. I have earnestly wished that some one would repeat these experiments, but I feared that years would elapse before any one would take the trouble. I received your paper on Bignonia in "Bot. Zeit." and it interested me much. (678/2. See "Variation of Animals and Plants," Edition II., Volume II., page 117. Fritz Muller's paper, "Befruchtungsversuche an Cipo alho (Bignonia)," "Botanische Zeitung," September 25th, 1868, page 625, contains an interesting foreshadowing of the generalisation arrived at in "Cross and Self-Fertilisation." Muller wrote: "Are the three which grow near each other seedlings from the same mother-plant or perhaps from seeds of the same capsule? Or have they, from growing in the same place and under the same conditions, become so like each other that the pollen of one has hardly any more effect on the others than their own pollen? Or, on the contrary, were the plants originally one—i.e., are they suckers from a single stock, which have gained a slight degree of mutual fertility in the course of an independent life? Or, lastly, is the result 'ein neckische Zufall,'" (The above is a free translation of Muller's words.)) I am convinced that if you can prove that a plant growing in a distant place under different conditions is more effective in fertilisation than one growing close by, you will make a great step in the essence of sexual reproduction.

Prof. Asa Gray and Dr. Hooker have been staying here, and, oddly enough, they knew nothing of your paper on Martha (678/3. F. Muller has described ("Bot. Zeitung," 1866, page 129) the explosive mechanism by which the pollen is distributed in Martha (Posoqueria) fragrans. He also gives an account of the remarkable arrangement for ensuring cross-fertilisation. See "Forms of Flowers," Edition II., page 131.), though the former was aware of the curious movements of the stamens, but so little understood the structure of the plant that he thought it was probably a dimorphic species. Accordingly, I showed them your drawings and gave them a little lecture, and they were perfectly charmed with your account. Hildebrand (678/4. See Letter 206, Volume I.) has repeated his experiments on potatoes, and so have I, but this summer with no result.

LETTER 679. TO F. MULLER. Down, March 14th {1869}.

I received some time ago a very interesting letter from you with many facts about Oxalis, and about the non-seeding and spreading of one species. I may mention that our common O. acetosella varies much in length of pistils and stamens, so that I at first thought it was certainly dimorphic, but proved it by experiment not to be so. Boiseria (679/1. This perhaps refers to Boissiera (Ladizabala).) has after all seeded well with me when crossed by opposite form, but very sparingly when self-fertilised. Your case of Faramea astonishes me. (679/2. See "Forms of Flowers," Edition II., page 129. Faramea is placed among the dimorphic species.) Are you sure there is no mistake? The difference in size of flower and wonderful difference in size and structure of pollen-grains naturally make me rather sceptical. I never fail to admire and to be surprised at the number of points to which you attend. I go on slowly at my next book, and though I never am idle, I make but slow progress; for I am often interrupted by being unwell, and my subject of sexual selection has grown into a very large one. I have also had to correct a new edition of my "Origin," (679/3. The 5th edition.), and this has taken me six weeks, for science progresses at railroad speed. I cannot tell you how rejoiced I am that your book is at last out; for whether it sells largely or not, I am certain it will produce a great effect on all capable judges, though these are few in number.

P.S.—I have just received your letter of January 12th. I am greatly interested by what you say on Eschscholtzia; I wish your plants had succeeded better. It seems pretty clear that the species is much more self-sterile under the climate of Brazil than here, and this seems to me an important result. (679/4. See Letter 677.) I have no spare seeds at present, but will send for some from the nurseryman, which, though not so good for our purpose, will be worth trying. I can send some of my own in the autumn. You could simply cover up separately two or three single plants, and see if they will seed without aid,—mine did abundantly. Very many thanks for seeds of Oxalis: how I wish I had more strength and time to carry on these experiments, but when I write in the morning, I have hardly heart to do anything in the afternoon. Your grass is most wonderful. You ought to send account to the "Bot. Zeitung." Could you not ascertain whether the barbs are sensitive, and how soon they become spiral in the bud? Your bird is, I have no doubt, the Molothrus mentioned in my "Journal of Travels," page 52, as representing a North American species, both with cuckoo-like habits. I know that seeds from same spike transmitted to a certain extent their proper qualities; but as far as I know, no one has hitherto shown how far this holds good, and the fact is very interesting. The experiment would be well worth trying with flowers bearing different numbers of petals. Your explanation agrees beautifully with the hypothesis of pangenesis, and delights me. If you try other cases, do draw up a paper on the subject of inheritance of separate flowers for the "Bot. Zeitung" or some journal. Most men, as far as my experience goes, are too ready to publish, but you seem to enjoy making most interesting observations and discoveries, and are sadly too slow in publishing.

LETTER 680. TO F. MULLER. Barmouth, July 18th, 1869.

I received your last letter shortly before leaving home for this place. Owing to this cause and to having been more unwell than usual I have been very dilatory in writing to you. When I last heard, about six or eight weeks ago, from Mr. Murray, one hundred copies of your book had been sold, and I daresay five hundred may now be sold. (680/1. "Facts and Arguments for Darwin," 1869: see Volume I., Letter 227.) This will quite repay me, if not all the money; for I am sure that your book will have got into the hands of a good many men capable of understanding it: indeed, I know that it has. But it is too deep for the general public. I sent you two or three reviews—one of which, in the "Athenaeum," was unfavourable; but this journal has abused me, and all who think with me, for many years.

(680/2. "Athenaeum," 1869, page 431.) I enclose two more notices, not that they are worth sending: some other brief notices have appeared. The case of the Abitulon sterile with some individuals is remarkable (680/3. "Bestaubungsversuche an Abutilon-Arten." "Jenaische Zeitschr." VII., 1873, page 22.): I believe that I had one plant of Reseda odorata which was fertile with own pollen, but all that I have tried since were sterile except with pollen from some other individual. I planted the seeds of the Abitulon, but I fear that they were crushed in the letter. Your Eschscholtzia plants were growing well when I left home, to which place we shall return by the end of this month, and I will observe whether they are self-sterile. I sent your curious account of the monstrous Begonia to the Linnean Society, and I suppose it will be published in the "Journal." (680/4. "On the Modification of the Stamens in a Species of Begonia." "Journ. Linn. Soc." XI., 1871, page 472.) I sent the extract about grafted orange trees to the "Gardeners' Chronicle," where it appeared. I have lately drawn up some notes for a French translation of my Orchis book: I took out your letters to make an abstract of your numerous discussions, but I found I had not strength or time to do so, and this caused me great regret. I have {in the French edition} alluded to your work, which will also be published in English, as you will see in my paper, and which I will send you. (680/5. "Notes on the Fertilisation of Orchids." "Ann. Mag. Nat. Hist." 1869, Volume IV., page 141. The paper gives an English version of the notes prepared for the French edition of the Orchid book.)

P.S.—By an odd chance, since I wrote the beginning of this letter, I have received one from Dr. Hooker, who has been reading "Fur Darwin": he finds that he has not knowledge enough for the first part; but says that Chapters X. and XI. "strike me as remarkably good." He is also particularly struck with one of your highly suggestive remarks in the note to page 119. Assuredly all who read your book will greatly profit by it, and I rejoice that it has appeared in English.

LETTER 681. TO F. MULLER. Down, December 1st {1869}.

I am much obliged for your letter of October 18th, with the curious account of Abutilon, and for the seeds. A friend of mine, Mr.

Farrer, has lately been studying the fertilisation of Passiflora (681/1. See Letters 701 and 704.), and concluded from the curiously crooked passage into the nectary that it could not be fertilised by humming-birds; but that Tacsonia was thus fertilised. Therefore I sent him the passage from your letter, and I enclose a copy of his answer. If you are inclined to gratify him by making a few observations on this subject I shall be much obliged, and will send them on to him. I enclose a copy of my rough notes on your Eschscholtzia, as you might like to see them. Somebody has sent me from Germany two papers by you, one with a most curious account of Alisma (681/2. See Letter 672.), and the other on crustaceans. Your observations on the branchiae and heart have interested me extremely.

Alex. Agassiz has just paid me a visit with his wife. He has been in England two or three months, and is now going to tour over the Continent to see all the zoologists. We liked him very much. He is a great admirer of yours, and he tells me that your correspondence and book first made him believe in evolution. This must have been a great blow to his father, who, as he tells me, is very well, and so vigorous that he can work twice as long as he (the son) can.

Dr. Meyer has sent me his translation of Wallace's "Malay Archipelago," which is a valuable work; and as I have no use for the translation, I will this day forward it to you by post, but, to save postage, via England.

LETTER 682. TO F. MULLER. Down, May 12th {1870}.

I thank you for your two letters of December 15th and March 29th, both abounding with curious facts. I have been particularly glad to hear in your last about the Eschscholtzia (682/1. See Letter 677.); for I am now rearing crossed and self-fertilised plants, in antagonism to each other, from your semi-sterile plants so that I may compare this comparative growth with that of the offspring of English fertile plants. I have forwarded your postscript about Passiflora, with the seeds, to Mr. Farrer, who I am sure will be greatly obliged to you; the turning up of the pendant flower plainly indicates some adaptation. When I next go to London I will take up the specimens of butterflies, and show them to Mr. Butler, of the British Museum, who is a learned lepidopterist and interested on the subject. This reminds me to ask you whether you received my letter {asking} about the ticking butterfly, described at page 33 of my "Journal of Researches"; viz., whether the sound is in anyway sexual? Perhaps the species does not inhabit your island. (682/2. Papilio feronia, a Brazilian species capable of making "a clicking noise, similar to that produced by a toothed wheel passing under a spring catch."—"Journal," 1879, page 34.)

The case described in your last letter of the trimorphic monocotyledon Pontederia is grand. (682/3. This case interested Darwin as the only instance of heterostylism in Monocotyledons. See "Forms of Flowers," Edition II., page 183. F. Muller's paper is in the "Jenaische Zeitschrift," 1871.) I wonder whether I shall ever have time to recur to this subject; I hope I may, for I have a good deal of unpublished material.

Thank you for telling me about the first-formed flower having additional petals, stamens, carpels, etc., for it is a possible means of transition of form; it seems also connected with the fact on which I have insisted of peloric flowers being so often terminal. As pelorism is strongly inherited (and {I} have just got a curious case of this in a leguminous plant from India), would it not be worth while to fertilise some of your early flowers having additional organs with pollen from a similar flower, and see whether you could not make a race thus characterised? (682/4. See Letters 588, 589. Also "Variation under Domestication," Edition II., Volume I., pages 388-9.) Some of your Abutilons have germinated, but I have been very unfortunate with most of your seed.

You will remember having given me in a former letter an account of a very curious popular belief in regard to the subsequent progeny of asses, which have borne mules; and now I have another case almost exactly like that of Lord Morton's mare, in which it is said the shape of the hoofs in the subsequent progeny are affected. (Pangenesis will turn out true some day!) (682/5. See "Animals and Plants," Edition II., Volume I., page 435. For recent work on telegony see Ewart's "Experimental Investigations on Telegony," "Phil. Trans. R. Soc." 1899. A good account of the subject is given in the "Quarterly Review," 1899, page 404. See also Letter 275, Volume I.)

A few months ago I received an interesting letter and paper from your brother, who has taken up a new and good line of investigation, viz., the adaptation in insects for the fertilisation of flowers.

The only scientific man I have seen for several months is Kolliker, who came here with Gunther, and whom I liked extremely.

I am working away very hard at my book on man and on sexual selection, but I do not suppose I shall go to press till late in the autumn.

LETTER 683. TO F. MULLER. Down, January 1st, 1874.

No doubt I owe to your kindness two pamphlets received a few days ago, which have interested me in an extraordinary degree. (683/1. This refers to F. Muller's "Bestaubungsversuche an Abutilon-Arten" in the "Jenaische Zeitschr." Volume VII., which are thus referred to by Darwin ("Cross and Self Fert." pages 305-6): "Fritz Muller has shown by his valuable experiments on hybrid Abutilons, that the union of brothers and sisters, parents and children, and of other near relations is highly injurious to the fertility of the offspring." The Termite paper is in the same volume (viz., VII.) of the "Jenaische Zeitschr.") It is quite new to me what you show about the effects of relationship in hybrids—that is to say, as far as direct proof is concerned. I felt hardly any doubt on the subject, from the fact of hybrids becoming more fertile when grown in number in nursery gardens, exactly the reverse of what occurred with Gartner. (683/2. When many hybrids are grown together the pollination by near relatives is minimised.) The paper on Termites is even still more interesting, and the analogy with cleistogene flowers is wonderful. (683/3. On the back of his copy of Muller's paper Darwin wrote: "There exist imperfectly developed male and female Termites, with wings much shorter than those of queen and king, which serve to continue the species if a fully developed king and queen do not after swarming (which no doubt is for an occasional cross) enter {the} nest. Curiously like cleistogamic flowers.") The manner in which you refer to to my chapter on crossing is one of the most elegant compliments which I have ever received.

I have directed to be sent to you Belt's "Nicaragua," which seems to me the best Natural History book of travels ever published. Pray look to what he says about the leaf-carrying ant storing the leaves up in a minced state to generate mycelium, on which he supposes that the larvae feed. Now, could you open the stomachs of these ants and examine the contents, so as to prove or disprove this remarkable hypothesis? (683/4. The hypothesis has been completely confirmed by the researches of Moller, a nephew of F. Muller's: see his "Brasilische Pilzblumen" ("Botan. Mittheilgn. aus den Tropen," hrsg. von A.F.W. Schimper, Heft 7).)

LETTER 684. TO F. MULLER. Down, May 9th, 1877.

I have been particularly glad to receive your letter of March 25th on Pontederia, for I am now printing a small book on heterostyled plants, and on some allied subjects. I feel sure you will not object to my giving a short account of the flowers of the new species which you have sent me. I am the more anxious to do so as a writer in the United States has described a species, and seems to doubt whether it is heterostyled, for he thinks the difference in the length of the pistil depends merely on its growth! In my new book I shall use all the information and specimens which you have sent me with respect to the heterostyled plants, and your published notices.

One chapter will be devoted to cleistogamic species, and I will just notice your new grass case. My son Francis desires me to thank you much for your kindness with respect to the plants which bury their seeds.

I never fail to feel astonished, when I receive one of your letters, at the number of new facts you are continually observing. With respect to the great supposed subterranean animal, may not the belief have arisen from the natives having seen large skeletons embedded in cliffs? I remember finding on the banks of the Parana a skeleton of a Mastodon, and the Gauchos concluded that it was a borrowing animal like the Bizcacha. (684/1. On the supposed existence in Patagonia of a gigantic land-sloth, see "Natural Science," XIII., 1898, page 288, where Ameghino's discovery of the skin of Neomylodon listai was practically first made known, since his privately published pamphlet was not generally seen. The animal was afterwards identified with a Glossotherium, closely allied to Owen's G. Darwini, which has been named Glossotherium listai or Grypotherium domesticum. For a good account of the discoveries see Smith Woodward in "Natural Science," XV., 1899, page 351, where the literature is given.)

LETTER 685. TO F. MULLER. Down, May 14th {1877}.

I wrote to you a few days ago to thank you about Pontederia, and now I am going to ask you to add one more to the many kindnesses which you have done for me. I have made many observations on the waxy secretion on leaves which throw off water (e.g., cabbage, Tropoeolum), and I am now going to continue my observations. Does any sensitive species of Mimosa grow in your neighbourhood? If so, will you observe whether the leaflets keep shut during long-continued warm rain. I find that the leaflets open if they are continuously syringed with water at a temperature of about 19 deg C., but if the water is at a temperature of 33-35 deg C., they keep shut for more than two hours, and probably longer. If the plant is continuously shaken so as to imitate wind the leaflets soon open. How is this with the native plants during a windy day? I find that some other plants—for instance, Desmodium and Cassia—when syringed with water, place their leaves so that the drops fall quickly off; the position assumed differing somewhat from that in the so-called sleep. Would you be so kind as to observe whether any {other} plants place their leaves during rain so as to shoot off the water; and if there are any such I should be very glad of a leaf or two to ascertain whether they are coated with a waxy secretion. (685/1. See Letters 737-41.)

There is another and very different subject, about which I intend to write, and should be very glad of a little information. Are earthworms (Lumbricus) common in S. Brazil (685/2. F. Muller's reply is given in "Vegetable Mould," page 122.), and do they throw up on the surface of the ground numerous castings or vermicular masses such as we so commonly see in Europe? Are such castings found in the forests beneath the dead withered leaves? I am sure I can trust to your kindness to forgive me for asking you so many questions.

LETTER 686. TO F. MULLER. Down, July 24th, 1878.

Many thanks for the five kinds of seeds; all have germinated, and the Cassia seedlings have interested me much, and I daresay that I shall find something curious in the other plants. Nor have I alone profited, for Sir J. Hooker, who was here on Sunday, was very glad of some of the seeds for Kew. I am particularly obliged for the information about the earthworms. I suppose the soil in your forests is very loose, for in ground which has lately been dug in England the worms do not come to the surface, but deposit their castings in the midst of the loose soil.

I have some grand plants (and I formerly sent seeds to Kew) of the cleistogamic grass, but they show no signs of producing flowers of any kind as yet. Your case of the panicle with open flowers being sterile is parallel to that of Leersia oryzoides. I have always fancied that cross-fertilisation would perhaps make such panicles fertile. (686/1. The meaning of this sentence is somewhat obscure. Darwin apparently implies that the perfect flowers, borne on the panicles which occasionally emerge from the sheath, might be fertile if pollinated from another individual. See "Forms of Flowers," page 334.)

I am working away as hard as I can at all the multifarious kinds of movements of plants, and am trying to reduce them to some simple rules, but whether I shall succeed I do not know.

I have sent the curious lepidopteron case to Mr. Meldola.

LETTER 687. F. MULLER TO CHARLES DARWIN.

(687/1. In November, 1880, on receipt of an account of a flood in Brazil from which Fritz Muller had barely escaped with his life ("Life and Letters," III., 242); Darwin immediately wrote to Hermann Muller begging to be allowed to help in making good any loss in books or scientific instruments that his brother had sustained. It is this offer of help that is referred to in the first paragraph of the following letter: Darwin repeats the offer in Letter 690.)

Blumenau, Sa Catharina, Brazil, January 9th, 1881.

I do not know how to express {to} you my deep heartfelt gratitude for the generous offer which you made to my brother on hearing of the late dreadful flood of the Itajahy. From you, dear sir, I should have accepted assistance without hesitation if I had been in need of it; but fortunately, though we had to leave our house for more than a week, and on returning found it badly damaged, my losses have not been very great.

I must thank you also for your wonderful book on the movements of plants, which arrived here on New Year's Day. I think nobody else will have been delighted more than I was with the results which you have arrived at by so many admirably conducted experiments and observations; since I observed the spontaneous revolving movement of Alisma I had seen similar movements in so many and so different plants that I felt much inclined to consider spontaneous revolving movement or circumnutation as common to all plants and the movements of climbing plants as a special modification of that general phenomenon. And this you have now convincingly, nay, superabundantly, proved to be the case.

I was much struck with the fact that with you Maranta did not sleep for two nights after having its leaves violently shaken by wind, for here we have very cold nights only after storms from the west or south-west, and it would be very strange if the leaves of our numerous species of Marantaceae should be prevented by these storms to assume their usual nocturnal position, just when nocturnal radiation was most to be feared. It is rather strange, also, that Phaseolus vulgaris should not sleep during the early part of the summer, when the leaves are most likely to be injured during cold nights. On the contrary, it would not do any harm to many sub-tropical plants, that their leaves must be well illuminated during the day in order that they may assume at night a vertical position; for, in our climate at least, cold nights are always preceded by sunny days.

Of nearly allied plants sleeping very differently I can give you some more instances. In the genus Olyra (at least, in the one species observed by me) the leaves bend down vertically at night; now, in Endlicher's "Genera plantarum" this genus immediately precedes Strephium, the leaves of which you saw rising vertically.

In one of two species of Phyllanthus, growing as weeds near my house, the leaves of the erect branches bend upwards at night, while in the second species, with horizontal branches, they sleep like those of Phyllanthus Niruri or of Cassia. In this second species the tips of the branches also are curled downwards at night, by which movement the youngest leaves are yet better protected. From their vertical nyctitropic position the leaves of this Phyllanthus might return to horizontality, traversing 90 deg, in two ways, either to their own or to the opposite side of the branch; on the latter way no rotation would be required, while on the former each leaf must rotate on its own axis in order that its upper surface may be turned upwards. Thus the way to the wrong side appears to be even less troublesome. And indeed, in some rare cases I have seen three, four or even almost all the leaves of one side of a branch horizontally expanded on the opposite side, with their upper surfaces closely appressed to the lower surfaces of the leaves of that side.

This Phyllanthus agrees with Cassia not only in its manner of sleeping, but also by its leaves being paraheliotropic. (687/2. Paraheliotropism is the movement by which some leaves temporarily direct their edges to the source of light. See "Movements of Plants," page 445.) Like those of some Cassiae its leaves take an almost perfectly vertical position, when at noon, on a summer day, the sun is nearly in the zenith; but I doubt whether this paraheliotropism will be observable in England. To-day, though continuing to be fully exposed to the sun, at 3 p.m. the leaves had already returned to a nearly horizontal position. As soon as there are ripe seeds I will send you some; of our other species of Phyllanthus I enclose a few seeds in this letter.

In several species of Hedychium the lateral halves of the leaves when exposed to bright sunshine, bend downwards so that the lateral margins meet. It is curious that a hybrid Hedychium in my garden shows scarcely any trace of this paraheliotropism, while both the parent species are very paraheliotropic.

Might not the inequality of the cotyledons of Citrus and of Pachira be attributed to the pressure, which the several embryos enclosed in the same seed exert upon each other? I do not know Pachira aquatica, but {in} a species, of which I have a tree in my garden, all the seeds are polyembryonic, and so were almost all the seeds of Citrus which I examined. With Coffea arabica also seeds including two embryos are not very rare; but I have not yet observed whether in this case the cotyledons be inequal.

I repeated to-day Duval-Jouve's measurements on Bryophyllum calycinum (687/3. "Power of Movement in Plants," page 237. F. Muller's measurements show, however, that there is a tendency in the leaves to be more highly inclined at night than in the middle of the day, and so far they agree with Duval-Jouve's results.); but mine did not agree with his; they are as follows:—

Distances in mm. between the tips of the upper pair of leaves.

January 9th, 1881	*3 A.M.*	*1 P.M.*	*6 P.M.*
1st plant	*54*	*43*	*36*
2nd plant	*28*	*25*	*23*
3rd plant	*28*	*27*	*27*
4th plant	*51*	*46*	*39*
5th plant	*61*	*52*	*45*
	222	*193*	*170*

LETTER 688. TO F. MULLER. Down, February 23rd, 1881.

Your letter has interested me greatly, as have so many during many past years. I thought that you would not object to my publishing in "Nature" (688/1. "Nature," March 3rd, 1881, page 409.) some of the more striking facts about the movements of plants, with a few remarks added to show the bearing of the facts. The case of the Phyllanthus (688/2. See Letter 687.), which turns up its leaves on the wrong side, is most extraordinary and ought to be further investigated. Do the leaflets sleep on the following night in the usual manner? Do the same leaflets on successive nights move in the same strange manner? I was particularly glad to hear of the strongly marked cases of paraheliotropism. I shall look out with much interest for the publication about the figs. (688/3. F. Muller published on Caprification in "Kosmos," 1882.) The creatures which you sketch are marvellous, and I should not have guessed that they were hymenoptera. Thirty or forty years ago I read all that I could find about caprification, and was utterly puzzled. I suggested to Dr. Cruger in Trinidad to investigate the wild figs, in relation to their cross-fertilisation, and just before he died he wrote that he had arrived at some very curious results, but he never published, as I believe, on the subject.

I am extremely glad that the inundation did not so greatly injure your scientific property, though it would have been a real pleasure to me to have been allowed to have replaced your scientific apparatus. (688/4. See Letter 687.) I do not believe that there is any one in the world who admires your zeal in science and wonderful powers of observation more than I do. I venture to say this, as I feel myself a very old man, who probably will not last much longer.

P.S.—With respect to Phyllanthus, I think that it would be a good experiment to cut off most of the leaflets on one side of the petiole, as soon as they are asleep and vertically dependent; when the pressure is thus removed, the opposite leaflets will perhaps bend beyond their vertically dependent position; if not, the main petiole might be a little twisted so that the upper surfaces of the dependent and now unprotected leaflets should face obliquely the sky when the morning comes. In this case diaheliotropism would perhaps conquer the ordinary movements of the leaves when they awake, and {assume} their diurnal horizontal position. As the leaflets are alternate, and as the upper surface will be somewhat exposed to the dawning light, it is perhaps diaheliotropism which explains your extraordinary case.

LETTER 689. TO F. MULLER. Down, April 12th, 1881.

I have delayed answering your last letter of February 25th, as I was just sending to the printers the MS. of a very little book on the habits of earthworms, of which I will of course send you a copy when published. I have been very much interested by your new facts on paraheliotropism, as I think that they justify my giving a name to this kind of movement, about which I long doubted. I have this morning drawn up an account of your observations, which I will send in a few days to "Nature." (689/1. "Nature," 1881, page 603. Curious facts are given on the movements of Cassia, Phyllanthus, sp., Desmodium sp. Cassia takes up a sunlight position unlike its own characteristic night-position, but resembling rather that of Haematoxylon (see "Power of Movement," figure 153, page 369). One species of Phyllanthus takes up in sunshine the nyctitropic attitude of another species. And the same sort of relation occurs in the genus Bauhinia.) I have thought that you would not object to my giving precedence to paraheliotropism, which has been so little noticed. I will send you a copy of "Nature" when published. I am glad that I was not in too great a hurry in publishing about Lagerstroemia. (689/2. Lagerstraemia was doubtfully placed among the heterostyled plants ("Forms of Flowers," page 167). F. Muller's observations showed that a totally different interpretation of the two sizes of stamen is possible. Namely, that one set serves merely to attract pollen-collecting bees, who in the act of visiting the flowers transfer the pollen of the longer stamens to other flowers. A case of this sort in Heeria, a Melastomad, was described by Muller ("Nature," August 4th, 1881, page 308), and the view was applied to the cases of Lagerstroemia and Heteranthera at a later date ("Nature," 1883, page 364). See Letters 620-30.) I have procured some plants of Melastomaceae, but I fear that they will not flower for two years, and I may be in my grave before I can repeat my trials. As far as I can imperfectly judge from my observations, the difference in colour of the anthers in this family depends on one set of anthers being partially aborted. I wrote to Kew to get plants with differently coloured anthers, but I learnt very little, as describers of dried plants do not attend to such points. I have, however, sowed seeds of two kinds, suggested to me as probable. I have, therefore, been extremely glad to receive the seeds of Heteranthera reniformis. As far as I can make out it is an aquatic plant; and whether I shall succeed in getting it to flower is doubtful. Will you be so kind as to send me a postcard telling me in what kind of station it grows. In the course of next autumn or winter, I think that I shall put together my notes (if they seem worth publishing) on the use or meaning of "bloom" (689/3. See Letters 736-40.), or the waxy secretion which makes some leaves glaucous. I think that I told you that my experiments had led me to suspect that the movement of the leaves of Mimosa, Desmodium and Cassia, when shaken and syringed, was to shoot off the drops of water. If you are caught in heavy rain, I should be very much obliged if you would keep this notion in your mind, and look to the position of such leaves. You have such wonderful powers of observation that your opinion would be more valued by me than that of any other man. I have among my notes one letter from you on the subject, but I forget its purport. I hope, also, that you may be led to follow up your very ingenious and novel view on the two-coloured anthers or pollen, and observe which kind is most gathered by bees.

LETTER 690. TO F. MULLER. {Patterdale}, June 21st, 1881.

I should be much obliged if you could without much trouble send me seeds of any heterostyled herbaceous plants (i.e. a species which would flower soon), as it would be easy work for me to raise some illegitimate seedlings to test their degree of infertility. The plant ought not to have very small flowers. I hope that you received the copies of "Nature," with extracts from your interesting letters (690/1. "Nature," March 3rd, 1881, Volume XXIII., page 409, contains a letter from C. Darwin on "Movements of Plants," with extracts from Fritz Muller's letter. Another letter, "On the Movements of Leaves," was published in "Nature," April 28th, 1881, page 603, with notes on leaf-movements sent to Darwin by Muller.), and I was glad to see a notice in "Kosmos" on Phyllanthus. (690/2. "Verirrte Blatter," by Fritz Muller ("Kosmos," Volume V., page 141, 1881). In this article an account is given of a species of Phyllanthus, a weed in Muller's garden. See Letter 687.) I am writing this note away from my home, but before I left I had the satisfaction of seeing Phyllanthus sleeping. Some of the seeds which you so kindly sent me would not germinate, or had not then germinated. I received a letter yesterday from Dr. Breitenbach, and he tells me that you lost many of your books in the desolating flood from which you suffered. Forgive me, but why should you not order, through your brother Hermann, books, etc., to the amount of 100 pounds, and I would send a cheque to him as soon as I heard the exact amount? This would be no inconvenience to me; on the contrary, it would be an honour and lasting pleasure to me to have aided you in your invaluable scientific work to this small and trifling extent. (690/3. See Letter 687, also "Life and Letters," III., page 242.)

LETTER 691. TO F. MULLER.

(691/1. The following extract from a letter to F. Muller shows what was the nature of Darwin's interest in the effect of carbonate of ammonia on roots, etc. He was, we think, wrong in adhering to the belief that the movements of aggregated masses are of an amoeboid nature. The masses change shape, just as clouds do under the moulding action of the wind. In the plant cell the moulding agent is the flowing protoplasm, but the masses themselves are passive.)

September 10th, 1881.

Perhaps you may remember that I described in "Insectivorous Plants" a really curious phenomenon, which I called the aggregation of the protoplasm in the cells of the tentacles. None of the great German botanists will admit that the moving masses are composed of protoplasm, though it is astonishing to me that any one could watch the movement and doubt its nature. But these doubts have led me to observe analogous facts, and I hope to succeed in proving my case.

LETTER 692. TO F. MULLER. Down, November 13th, 1881.

I received a few days ago a small box (registered) containing dried flower-heads with brown seeds somewhat sculptured on the sides. There was no name, and I should be much obliged if some time you would tell me what these seeds are. I have planted them.

I sent you some time ago my little book on earthworms, which, though of no importance, has been largely read in England. I have little or nothing to tell you about myself. I have for a couple of months been observing the effects of carbonate of ammonia on chlorophyll and on the roots of certain plants (692/1. Published under the title "The Action of Carbonate of Ammonia on the Roots of Certain Plants and on Chlorophyll Bodies," "Linn. Soc. Journ." XIX., 1882, pages 239-61, 262-84.), but the subject is too difficult for me, and I cannot understand the meaning of some strange facts which I have observed. The mere recording new facts is but dull work.

Professor Wiesner has published a book (692/2. See Letter 763.), giving a different explanation to almost every fact which I have given in my "Power of Movement in Plants." I am glad to say that he admits that almost all my statements are true. I am convinced that many of his interpretations of the facts are wrong, and I am glad to hear that Professor Pfeffer is of the same opinion; but I believe that he is right and I wrong on some points. I have not the courage to retry all my experiments, but I hope to get my son Francis to try some fresh ones to test Wiesner's explanations. But I do not know why I have troubled you with all this.

LETTER 693. TO F. MULLER. {4, Bryanston Street}, December 19th, 1881.

I hope that you may find time to go on with your experiments on such plants as Lagerstroemia, mentioned in your letter of October 29th, for I believe you will arrive at new and curious results, more especially if you can raise two sets of seedlings from the two kinds of pollen.

Many thanks for the facts about the effect of rain and mud in relation to the waxy secretion. I have observed many instances of the lower side being protected better than the upper side, in the case, as I believe, of bushes and trees, so that the advantage in low-growing plants is probably only an incidental one. (693/1. The meaning is here obscure: it appears to us that the significance of bloom on the lower surface of the leaves of both trees and herbs depends on the frequency with which all or a majority of the stomata are on the lower surface—where they are better protected from wet (even without the help of bloom) than on the exposed upper surface. On the correlation between bloom and stomata, see Francis Darwin "Linn. Soc. Journ." XXII., page 99.) As I am writing away from my home, I have been unwilling to try more than one leaf of the Passiflora, and this came out of the water quite dry on the lower surface and quite wet on the upper. I have not yet begun to put my notes together on this subject, and do not at all know whether I shall be able to make much of it. The oddest little fact which I have observed is that with Trifolium resupinatum, one half of the leaf (I think the right-hand side, when the leaf is viewed from the apex) is protected by waxy secretion, and not the other half (693/2. In the above passage "leaf" should be "leaflet": for a figure of Trifolium resupinatum see Letter 740.); so that when the leaf is dipped into water, exactly half the leaf comes out dry and half wet. What the meaning of this can be I cannot even conjecture. I read last night your very interesting article in "Kosmos" on Crotalaria, and so was very glad to see the dried leaves sent by you: it seems to me a very curious case. I rather doubt whether it will apply to Lupinus, for, unless my memory deceives me, all the leaves of the same plant sometimes behaved in the same manner; but I will try and get some of the same seeds of the Lupinus, and sow them in the spring. Old age, however, is telling on me, and it troubles me to have more than one subject at a time on hand.

(693/3. In a letter to F. Muller (September 10, 1881) occurs a sentence which may appropriately close this series: "I often feel rather ashamed of myself for asking for so many things from you, and for taking up so much of your valuable time, but I can assure you that I feel grateful.")

2.XI.III. MISCELLANEOUS, 1868-1881.

LETTER 694. TO G. BENTHAM. Down, April 22nd, 1868.

I have been extremely much pleased by your letter, and I take it as a very great compliment that you should have written to me at such length...I am not at all surprised that you cannot digest pangenesis: it is enough to give any one an indigestion; but to my mind the idea has been an immense relief, as I could not endure to keep so many large classes of facts all floating loose in my mind without some thread of connection to tie them together in a tangible method.

With respect to the men who have recently written on the crossing of plants, I can at present remember only Hildebrand, Fritz Muller, Delpino, and G. Henslow; but I think there are others. I feel sure that Hildebrand is a very good observer, for I have read all his papers, and during the last twenty years I have made unpublished observations on many of the plants which he describes. {Most of the criticisms which I sometimes meet with in French works against the frequency of crossing I am certain are the result of mere ignorance. I have never hitherto found the rule to fail that when an author describes the structure of a flower as specially adapted for self-fertilisation, it is really adapted for crossing. The Fumariaceae offer a good instance of this, and Treviranus threw this order in my teeth; but in Corydalis Hildebrand shows how utterly false the idea of self-fertilisation is. This author's paper on Salvia (694/1. Hildebrand, "Pringsheim's Jahrbucher," IV.) is really worth reading, and I have observed some species, and know that he is accurate}. (694/2. The passage within {} was published in the "Life and Letters," III., page 279.) Judging from a long review in the "Bot. Zeitung", and from what I know of some the plants, I believe Delpino's article especially on the Apocynaea, is excellent; but I cannot read Italian. (694/3.

Hildebrand's paper in the "Bot. Zeitung," 1867, refers to Delpino's work on the Asclepiads, Apocyneae and other Orders.) Perhaps you would like just to glance at such pamphlets as I can lay my hands on, and therefore I will send them, as if you do not care to see them you can return them at once; and this will cause you less trouble than writing to say you do not care to see them. With respect to Primula, and one point about which I feel positive is that the Bardfield and common oxlips are fundamentally distinct plants, and that the common oxlip is a sterile hybrid. (694/4. For a general account of the Bardfield oxlip (Primula elatior) see Miller Christy, "Linn. Soc. Journ." Volume XXXIII., page 172, 1897.) I have never heard of the common oxlip being found in great abundance anywhere, and some amount of difference in number might depend on so small a circumstance as the presence of some moth which habitually sucked the primrose and cowslip. To return to the subject of crossing: I am experimenting on a very large scale on the difference in power and growth between plants raised from self-fertilised and crossed seeds, and it is no exaggeration to say that the difference in growth and vigour is sometimes truly wonderful. Lyell, Huxley, and Hooker have seen some of my plants, and been astonished; and I should much like to show them to you. I always supposed until lately that no evil effects would be visible until after several generations of self-fertilisation, but now I see that one generation sometimes suffices, and the existence of dimorphic plants and all the wonderful contrivances of orchids are quite intelligible to me.

LETTER 695. TO T.H. FARRER (Lord Farrer). Down, June 5th, 1868.

I must write a line to cry peccavi. I have seen the action in Ophrys exactly as you describe, and am thoroughly ashamed of my inaccuracy. (695/1. See "Fertilisation of Orchids," Edition II., page 46, where Lord Farrer's observations on the movement of the pollinia in Ophrys muscifera are given.) I find that the pollinia do not move if kept in a very damp atmosphere under a glass; so that it is just possible, though very improbable, that I may have observed them during a very damp day.

I am not much surprised that I overlooked the movement in Habenaria, as it takes so long. (695/2. This refers to Peristylus viridis, sometimes known as Habenaria viridis. Lord Farrer's observations are given in "Fertilisation of Orchids," Edition II., page 63.)

I am glad you have seen Listera; it requires to be seen to believe in the co-ordination in the position of the parts, the irritability, and the chemical nature of the viscid fluid. This reminds me that I carefully described to Huxley the shooting out of the pollinia in Catasetum, and received for an answer, "Do you really think that I can believe all that!" (695/3. See Letter 665.)

LETTER 696. TO J.D. HOOKER. Down, December 2nd, 1868.

It is a splendid scheme, and if you make only a beginning on a "Flora," which shall serve as an index to all papers on curious points in the life-history of plants, you will do an inestimable good service. Quite recently I was asked by a man how he could find out what was known on various biological points in our plants, and I answered that I knew of no such book, and that he might ask half a dozen botanists before one would chance to remember what had been published on this or that point. Not long ago another man, who had been experimenting on the quasi-bulbs on the leaves of Cardamine, wrote to me to complain that he could not find out what was known on the subject. It is almost certain that some early or even advanced students, if they found in their "Flora" a line or two on various curious points, with references for further investigation, would be led to make further observations. For instance, a reference to the viscid threads emitted by the seeds of Compositae, to the apparatus (if it has been described) by which Oxalis spurts out its seeds, to the sensitiveness of the young leaves of Oxalis acetosella with reference to O. sensitiva. Under Lathyrus nissolia it would {be} better to refer to my hypothetical explanation of the grass-like leaves than to nothing. (696/1. No doubt the view given in "Climbing Plants," page 201, that L. nissolia has been evolved from a form like L. aphaca.) Under a twining plant you might say that the upper part of the shoot steadily revolves with or against the sun, and so, when it strikes against any object it turns to the right or left, as the case may be. If, again, references were given to the parasitism of

Euphrasia, etc., how likely it would be that some young man would go on with the investigation; and so with endless other facts. I am quite enthusiastic about your idea; it is a grand idea to make a "Flora" a guide for knowledge already acquired and to be acquired. I have amused myself by speculating what an enormous number of subjects ought to be introduced into a Eutopian (696/2. A mis-spelling of Utopian.) Flora, on the quickness of the germination of the seeds, on their means of dispersal; on the fertilisation of the flower, and on a score of other points, about almost all of which we are profoundly ignorant. I am glad to read what you say about Bentham, for my inner consciousness tells me that he has run too many forms together. Should you care to see an elaborate German pamphlet by Hermann Muller on the gradation and distinction of the forms of Epipactis and of Platanthera? (696/3. "Verhand. d. Nat. Ver. f. Pr. Rh. u. Wesfal." Jahrg. XXV.: see "Fertilisation of Orchids," Edition II., pages 74, 102.) It may be absurd in me to suggest, but I think you would find curious facts and references in Lecoq's enormous book (696/4. "Geographie Botanique," 9 volumes, 1854-58.), in Vaucher's four volumes (696/5. "Plantes d'Europe," 4 volumes, 1841.), in Hildebrand's "Geschlechter Vertheilung" (696/6 "Geschlechter Vertheilung bei den Pflanzen," 1 volume, Leipzig, 1867.), and perhaps in Fournier's "De la Fecondation." (696/7. "De la Fecondation dans les Phanerogames," par Eugene Fournier: thesis published in Paris in 1863. The facts noted in Darwin's copy are the explosive stamens of Parietaria, the submerged flowers of Alisma containing air, the manner of fertilisation of Lopezia, etc.) I wish you all success in your gigantic undertaking; but what a pity you did not think of it ten years ago, so as to have accumulated references on all sorts of subjects. Depend upon it, you will have started a new era in the floras of various countries. I can well believe that Mrs. Hooker will be of the greatest possible use to you in lightening your labours and arranging your materials.

LETTER 697. TO J.D. HOOKER. Down, December 5th, 1868.

...Now I want to beg for assistance for the new edition of "Origin." Nageli himself urges that plants offer many morphological differences, which from being of no service cannot have been selected, and which he accounts for by an innate principle of progressive development. (697/1. Nageli's "Enstehung und Begriff der Naturhistorischen Art." An address delivered at the public session of the Royal Academy of Sciences of Munich, March 28th, 1865; published by the Academy. Darwin's copy is the 2nd edition; it bears signs, in the pencilled notes on the margins, of having been read with interest. Much of it was translated for him by a German lady, whose version lies with the original among his pamphlets. At page 27 Nageli writes: "It is remarkable that the useful adaptations which Darwin brings forward in the case of animals, and which may be discovered in numbers among plants, are exclusively of a physiological kind, that they always show the formation or transformation of an organ to a special function. I do not know among plants a morphological modification which can be explained on utilitarian principles." Opposite this passage Darwin has written "a very good objection": but Nageli's sentence seems to us to be of the nature of a truism, for it is clear that any structure whose evolution can be believed to have come about by Natural Selection must have a function, and the case falls into the physiological category. The various meanings given to the term morphological makes another difficulty. Nageli cannot use it in the sense of "structural"—in which sense it is often applied, since that would mean that no plant structures have a utilitarian origin. The essence of morphology (in the better and more precise sense) is descent; thus we say that a pollen-grain is morphologically a microspore. And this very example serves to show the falseness of Nageli's view, since a pollen-grain is an adaptation to aerial as opposed to aquatic fertilisation. In the 5th edition of the "Origin," 1869, page 151, Darwin discusses Nageli's essay, confining himself to the simpler statement that there are many structural characters in plants to which we cannot assign uses. See Volume I., Letter 207.) I find old notes about this difficulty; but I have hitherto slurred it over. Nageli gives as instances the alternate and spiral arrangement of leaves, and the arrangement of the cells in the tissues. Would you not consider as a morphological difference the trimerous, tetramerous, etc., divisions of flowers, the ovules being erect or suspended, their attachment being parietal or placental, and even the shape of the seed when of no service to the plant.

Now, I have thought, and want to show, that such differences follow in some unexplained manner from the growth or development of plants which have passed through a long series of adaptive changes. Anyhow, I want to show that these differences do not support the idea of progressive development. Cassini states that the ovaria on the circumference and centre of Compos. flowers differ in essential characters, and so do the seeds in sculpture. The seeds of Umbelliferae in the same relative positions are coelospermous and orthospermous. There is a case given by Augt. St. Hilaire of an erect and suspended ovule in the same ovarium, but perhaps this hardly bears on the point. The summit flower, in Adoxa and rue differ from the lower flowers. What is the difference in flowers of the rue? how is the ovarium, especially in the rue? As Augt. St. Hilaire insists on the locularity of the ovarium varying on the same plant in some of the Rutaceae, such differences do not speak, as it seems to me, in favour of progressive development. Will you turn the subject in your mind, and tell me any more facts. Difference in structure in flowers in different parts of the same plant seems best to show that they are the result of growth or position or amount of nutriment.

I have got your photograph (697/2. A photograph by Mrs. Cameron.) over my chimneypiece, and like it much; but you look down so sharp on me that I shall never be bold enough to wriggle myself out of any contradiction.

Owen pitches into me and Lyell in grand style in the last chapter of volume 3 of "Anat. of Vertebrates." He is a cool hand. He puts words from me in inverted commas and alters them. (697/3. The passage referred to seems to be in Owen's "Anatomy of Vertebrata," III., pages 798, 799, note. "I deeply regretted, therefore, to see in a 'Historical Sketch' of the Progress of Enquiry into the origin of species, prefixed to the fourth edition of that work (1866), that Mr. Darwin, after affirming inaccurately and without evidence, that I admitted Natural Selection to have done something toward that end, to wit, the 'origin of species,' proceeds to remark: 'It is surprising that this admission should not have been made earlier, as Prof. Owen now believes that he promulgated the theory of Natural Selection in a passage read before the Zoological Society in February, 1850, ("Trans." Volume IV., page 15).'" The first of the two passages quoted by Owen from the fourth edition of the "Origin" runs: "Yet he {Prof. Owen} at the same time admits that Natural Selection MAY {our italics} have done something towards this end."

In the sixth edition of the "Origin," page xviii., Darwin, after referring to a correspondence in the "London Review" between the Editor of that Journal and Owen, goes on: "It appeared manifest to the editor, as well as to myself, that Prof. Owen claimed to have promulgated the theory of Natural Selection before I had done so;...but as far as it is possible to understand certain recently published passages (Ibid. {"Anat. of Vert."}, Volume III., page 798), I have either partly or wholly again fallen into error. It is consolatory to me that others find Prof. Owen's controversial writings as difficult to understand and to reconcile with each other, as I do. As far as the mere enunciation of the principle of Natural Selection is concerned, it is quite immaterial whether or no Prof. Owen preceded me, for both of us, as shown in this historical sketch, were long ago preceded by Dr. Wells and Mr. Matthews.")

LETTER 698. TO J.D. HOOKER. Down, December 29th, 1868.

Your letter is quite invaluable, for Nageli's essay (698/1. See preceding Letter.) is so clever that it will, and indeed I know it has produced a great effect; so that I shall devote three or four pages to an answer. I have been particularly struck by your statements about erect and suspended ovules. You have given me heart, and I will fight my battle better than I should otherwise have done. I think I cannot resist throwing the contrivances in orchids into his teeth. You say nothing about the flowers of the rue. (698/2. For Ruta see "Origin," Edition V., page 154.) Ask your colleagues whether they know anything about the structure of the flower and ovarium in the uppermost flower. But don't answer on purpose.

I have gone through my long Index of "Gardeners' Chronicle," which was made solely for my own use, and am greatly disappointed to find, as I fear, hardly anything which will be of use to you. (698/3. For Hooker's projected biological book, see Letter 696.) I send such as I have for the chance of their being of use.

LETTER 699. TO J.D. HOOKER. Down, January 16th {1869}.

Your two notes and remarks are of the utmost value, and I am greatly obliged to you for your criticism on the term. "Morphological" seems quite just, but I do not see how I can avoid using it. I found, after writing to you, in Vaucher about the Rue

(699/1. "Plantes d'Europe," Volume I., page 559, 1841.), but from what you say I will speak more cautiously. It is the Spanish Chesnut that varies in divergence. Seeds named Viola nana were sent me from Calcutta by Scott. I must refer to the plants as an "Indian species," for though they have produced hundreds of closed flowers, they have not borne one perfect flower. (699/2. The cleistogamic flowers of Viola are used in the discussion on Nageli's views. See "Origin," Edition V., page 153.) You ask whether I want illustrations "of ovules differing in position in different flowers on the same plant." If you know of such cases, I should certainly much like to hear them. Again you speak of the angle of leaf-divergence varying and the variations being transmitted. Was the latter point put in in a hurry to round the sentence, or do you really know of cases?

Whilst looking for notes on the variability of the divisions of the ovarium, position of the ovules, aestivation, etc., I found remarks written fifteen or twenty years ago, showing that I then supposed that characters which were nearly uniform throughout whole groups must be of high vital importance to the plants themselves; consequently I was greatly puzzled how, with organisms having very different habits of life, this uniformity could have been acquired through Natural Selection. Now, I am much inclined to believe, in accordance with the view given towards the close of my MS., that the near approach to uniformity in such structures depends on their not being of vital importance, and therefore not being acted on by Natural Selection. (699/3. This view is given in the "Origin," Edition VI., page 372.) If you have reflected on this point, what do you think of it? I hope that you approved of the argument deduced from the modifications in the small closed flowers.

It is only about two years since last edition of "Origin," and I am fairly disgusted to find how much I have to modify, and how much I ought to add; but I have determined not to add much. Fleeming Jenkin has given me much trouble, but has been of more real use to me than any other essay or review. (699/4. On Fleeming Jenkin's review, "N. British Review," June, 1867, see "Life and Letters," III., page 107.)

LETTER 700. TO J.D. HOOKER. Down {January 22nd, 1869}.

Your letter is quite splenditious. I am greatly tempted, but shall, I hope, refrain from using some of your remarks in my chapter on Classification. It is very true what you say about unimportant characters being so important systematically; yet it is hardly paradoxical bearing in mind that the natural system is genetic, and that we have to discover the genealogies anyhow. Hence such parts as organs of generation are so useful for classification though not concerned with the manner of life. Hence use for same purpose of rudimentary organs, etc. You cannot think what a relief it is that you do not object to this view, for it removes PARTLY a heavy burden from my shoulders. If I lived twenty more years and was able to work, how I should have to modify the "Origin," and how much the views on all points will have to be modified! Well, it is a beginning, and that is something...

LETTER 701. TO T.H. FARRER (Lord Farrer). Down, August 10th, 1869.

Your view seems most ingenious and probable; but ascertain in a good many cases that the nectar is actually within the staminal tube. (701/1. It seems that Darwin did not know that the staminal tube in the diadelphous Leguminosae serves as a nectar-holder, and this is surprising, as Sprengel was aware of the fact.) One can see that if there is to be a split in the tube, the law of symmetry would lead it to be double, and so free one stamen. Your view, if confirmed, would be extremely well worth publication before the Linnean Society. It is to me delightful to see what appears a mere morphological character found to be of use. It pleases me the more as Carl Nageli has lately been pitching into me on this head. Hooker, with whom I discussed the subject, maintained that uses would be found for lots more structures, and cheered me by throwing my own orchids into my teeth. (701/2. See Letters 697-700.)

All that you say about changed position of the peduncle in bud, in flower, and in seed, is quite new to me, and reminds me of analogous cases with tendrils. (701/3. See Vochting, "Bewegung der Bluthen und Fruchte," 1882; also Kerner, "Pflanzenleben," Volume I., page 494, Volume II., page 121.) This is well worth working out, and I dare say the brush of the stigma.

With respect to the hairs or filaments (about which I once spoke) within different parts of flowers, I have a splendid Tacsonia with perfectly pendent flowers, and there is only a microscopical vestige of the corona of coloured filaments; whilst in most common passion-flowers the flowers stand upright, and there is the splendid corona which apparently would catch pollen. (701/4. Sprengel ("Entdeckte Geheimniss," page 164) imagined that the crown of the Passion-flower served as a nectar-guide and as a platform for insects, while other rings of filaments served to keep rain from the nectar. F. Muller, quoted in H. Muller ("Fertilisation," page 268), looks at the crowns of hairs, ridges in some species, etc., as gratings serving to imprison flies which attract the fertilising humming-birds. There is, we believe, no evidence that the corona catches pollen. See Letter 704, note.)

On the lower side of corolla of foxglove there are some fine hairs, but these seem of not the least use (701/5. It has been suggested that the hairs serve as a ladder for humble bees; also that they serve to keep out "unbidden guests.")—a mere purposeless exaggeration of down on outside—as I conclude after watching the bees at work, and afterwards covering up some plants; for the protected flowers rarely set any seed, so that the hairy lower part of corolla does not come into contact with stigma, as some Frenchman says occurs with some other plants, as Viola odorata and I think Iris.

I heartily wish I could accept your kind invitation, for I am not by nature a savage, but it is impossible. Forgive my dreadful handwriting, none of my womenkind are about to act as amanuensis.

LETTER 702. TO WILLIAM C. TAIT.

(702/1. Mr. Tait, to whom the following letter is addressed, was resident in Portugal. His kindness in sending plants of Drosophyllum lusitanicum is acknowledged in "Insectivorous Plants.")

Down, March 12th, 1869.

I have received your two letters of March 2nd and 5th, and I really do not know how to thank you enough for your extraordinary kindness and energy. I am glad to hear that the inhabitants notice the power of the Drosophyllum to catch flies, for this is the subject of my studies. (702/2. The natives are said to hang up plants of Drosophyllum in their cottages to act as fly-papers ("Insectivorous Plants," page 332).) I have observed during several years the manner in which this is effected, and the results produced in several species of Drosera, and in the wonderful American Dionoea, the leaves of which catch insects just like a steel rat-trap. Hence I was most anxious to learn how the Drosophyllum would act, so that the Director of the Royal Gardens at Kew wrote some years ago to Portugal to obtain specimens for me, but quite failed. So you see what a favour you have conferred on me. With Drosera it is nothing less than marvellous how minute a fraction of a grain of any nitrogenised matter the plant can detect; and how differently it behaves when matter, not containing nitrogen, of the same consistence, whether fluid or solid, is applied to the glands. It is also exquisitely sensitive to a weight of even the 1/70000 of a grain. From what I can see of the glands on Drosophyllum I suspect that I shall find only the commencement, or nascent state of the wonderful capacities of the Drosera, and this will be eminently interesting to me. My MS. on this subject has been nearly ready for publication during some years, but when I shall have strength and time to publish I know not.

And now to turn to other points in your letter. I am quite ignorant of ferns, and cannot name your specimen. The variability of ferns passes all bounds. With respect to your Laugher Pigeons, if the same with the two sub-breeds which I kept, I feel sure from the structure of the skeleton, etc., that it is a descendant of C. livia. In regard to beauty, I do not feel the difficulty which you and some others experience. In the last edition of my "Origin" I have discussed the question, but necessarily very briefly. (702/3. Fourth Edition, page 238.) A new and I hope amended edition of the "Origin" is now passing through the press, and will be published in a month or two, and it will give me great pleasure to send you a copy. Is there any place in London where parcels are received for you, or shall I send it by post? With reference to dogs' tails, no doubt you are aware that a rudimentary stump is regularly inherited by certain breeds of sheep-dogs, and by Manx cats. You speak of a change in the position of the axis of the earth: this is a subject quite beyond me, but I believe the astronomers reject the idea. Nevertheless, I have long suspected that some periodical astronomical or cosmical cause must be the agent of the incessant oscillations of level in the earth's crust. About a month ago I suggested this to a man well capable of judging, but he could not conceive any such agency; he promised, however, to keep it in mind. I wish I had time and strength to write to you more fully. I had intended to send this letter off at once, but on reflection will keep it till I receive the plants.

LETTER 703. TO H. MULLER. Down, March 14th, 1870.

I think you have set yourself a new, very interesting, and difficult line of research. As far as I know, no one has carefully observed the structure of insects in relation to flowers, although so many have now attended to the converse relation. (703/1. See Letter 462, also H. Muller, "Fertilisation of Flowers," English Translation, page 30, on "The insects which visit flowers." In Muller's book references are given to several of his papers on this subject.) As I imagine few or no insects are adapted to suck the nectar or gather the pollen of any single family of plants, such striking adaptations can hardly, I presume, be expected in insects as in flowers.

LETTER 704. TO T.H. FARRER (Lord Farrer).

Down, May 28th, 1870.

I suppose I must have known that the stamens recovered their former position in Berberis (704/1. See Farrer, "Nature," II., 1870, page 164. Lord Farrer was before H. Muller in making out the mechanism of the barberry.), for I formerly tried experiments with anaesthetics, but I had forgotten the facts, and I quite agree with you that it is a sound argument that the movement is not for self-fertilisation. The N. American barberries (Mahonia) offer a good proof to what an extent natural crossing goes on in this genus; for it is now almost impossible in this country to procure a true specimen of the two or three forms originally introduced.

I hope the seeds of Passiflora will germinate, for the turning up of the pendent flower must be full of meaning. (704/2. Darwin had (May 12th, 1870) sent to Farrer an extract from a letter from F. Muller, containing a description of a Passiflora visited by humming-birds, in which the long flower-stalk curls up so that "the flower itself is upright." Another species visited by bees is described as having "dependent flowers." In a letter, June 29th, 1870, Mr. Farrer had suggested that P. princeps, which he described as having sub-erect flowers, is fitted for humming-birds' visits. In another letter, October 13th, 1869, he says that Tacsonia, which has pendent flowers and no corona, is not fertilised by insects in English glass-houses, and may be adapted for humming-birds. See "Life and Letters," III., page 279, for Farrer's remarks on Tacsonia and Passiflora; also H. Muller's "Fertilisation of Flowers," page 268, for what little is known on the subject; also Letter 701 in the present volume.) I am so glad that you are able to occupy yourself a little with flowers: I am sure it is most wise in you, for your own sake and children's sakes.

Some little time ago Delpino wrote to me praising the Swedish book on the fertilisation of plants; as my son George can read a little Swedish, I should like to have it back for a time, just to hear a little what it is about, if you would be so kind as to return it by book-post. (704/3. Severin Axell, "Om anordningarna for de Fanerogama Vaxternas Befruktning," Stockholm, 1869.)

I am going steadily on with my experiments on the comparative growth of crossed and self-fertilised plants, and am now coming to some very curious anomalies and some interesting results. I forget whether I showed you any of them when you were here for a few hours. You ought to see them, as they explain at a glance why Nature has taken such extraordinary pains to ensure frequent crosses between distinct individuals.

If in the course of the summer you should feel any inclination to come here for a day or two, I hope that you will propose to do so, for we should be delighted to see you...

LETTER 705. TO ASA GRAY. Down, December 7th, 1870.

I have been very glad to receive your letter this morning. I have for some time been wishing to write to you, but have been half worked to death in correcting my uncouth English for my new book. (705/1. "Descent of Man.") I have been glad to hear of your cases appearing like incipient dimorphism. I believe that they are due to mere variability, and have no significance. I found a good instance in Nolana prostrata, and experimented on it, but the forms did not differ in fertility. So it was with Amsinckia, of which you told me. I have long thought that such variations afforded the basis for the development of dimorphism. I was not aware of such cases in Phlox, but have often admired the arrangement of the anthers, causing them to be all raked by an inserted proboscis. I am glad also to hear of your curious case of variability in ovules, etc.

I said that I had been wishing to write to you, and this was about your Drosera, which after many fluctuations between life and death, at last made a shoot which I could observe. The case is rather interesting; but I must first remind you that the filament of Dionoea is not sensitive to very light prolonged pressure, or to nitrogenous matter, but is exquisitely sensitive to the slightest touch. (705/2. In another connection the following reference to Dionoea is of some interest: "I am sure I never heard of Curtis's observations on Dionoea, nor have I met with anything more than general statements about this plant or about Nepenthes catching insects." (From a letter to Sir J.D. Hooker, July 12th, 1860.)) In our Drosera the filaments are not sensitive to a slight touch, but are sensitive to prolonged pressure from the smallest object of any nature; they are also sensitive to solid or fluid nitrogenous matter. Now in your Drosera the filaments are not sensitive to a rough touch or to any pressure from non-nitrogenous matter, but are sensitive to solid or fluid nitrogenous matter. (705/3. Drosera filiformis: see "Insectivorous Plants," page 281. The above account does not entirely agree with Darwin's published statement. The filaments moved when bits of cork or cinder were placed on them; they did not, however, respond to repeated touches with a needle, thus behaving differently from D. rotundifolia. It should be remembered that the last-named species is somewhat variable in reacting to repeated touches.) Is it not curious that there should be such diversified sensitiveness in allied plants?

I received a very obliging letter from Mr. Morgan, but did not see him, as I think he said he was going to start at once for the Continent. I am sorry to hear rather a poor account of Mrs. Gray, to whom my wife and I both beg to be very kindly remembered.

LETTER 706. TO C.V. RILEY.

(706/1. In Riley's opinion his most important work was the series entitled "Annual Report on the Noxious, Beneficial, and other Insects of the State of Missouri" (Jefferson City), beginning in 1869. These reports were greatly admired by Mr. Darwin, and his copies of them, especially of Nos. 3 and 4, show signs of careful reading.)

Down, June 1st {1871}.

I received some little time ago your report on noxious insects, and have now read the whole with the greatest interest. (706/2. "Third Annual Report on the Noxious, Beneficial, and other Insects of the State of Missouri" (Jefferson City, Mo.). The mimetic case occurs at page 67; the 1875 pupae of Pterophorus periscelidactylus, the "Grapevine Plume," have pupae either green or reddish brown, the former variety being found on the leaves, the latter on the brown stems of the vine.) There are a vast number of facts and generalisations of value to me, and I am struck with admiration at your powers of observation.

The discussion on mimetic insects seems to me particularly good and original. Pray accept my cordial thanks for the instruction and interest which I have received.

What a loss to Natural Science our poor mutual friend Walsh has been; it is a loss ever to be deplored...

Your country is far ahead of ours in some respects; our Parliament would think any man mad who should propose to appoint a State Entomologist.

LETTER 707A. TO C.V. RILEY.

(706A/1. We have found it convenient to place the two letters to Riley together, rather than separate them chronologically.)

Down, September 28th, 1881.

I must write half a dozen lines to say how much interested I have been by your "Further Notes" on Pronuba which you were so kind as to send me. (706A/2. "Proc. Amer. Assoc. Adv. Sci." 1880.) I had read the various criticisms, and though I did not know what answer could be made, yet I felt full confidence in your result, and now I see that I was right...If you make any further observation on Pronuba it would, I think, be well worth while for you to observe whether the moth can or does occasionally bring pollen from one plant to the stigma of a distinct one (706A/3. Riley discovered the remarkable fact that the Yucca moth (Pronuba yuccasella) lays its eggs in the ovary of Yucca flowers, which it has previously pollinated, thus making sure of a supply of ovules for the larvae.), for I have shown that the cross-fertilisation of the flowers on the same plant does very little good; and, if I am not mistaken, you believe that Pronuba gathers pollen from the same flower which she fertilises.

What interesting and beautiful observations you have made on the metamorphoses of the grasshopper-destroying insects.

LETTER 707. TO F. HILDEBRAND. Down, February 9th {1872}.

Owing to other occupations I was able to read only yesterday your paper on the dispersal of the seeds of Compositae. (707/1. "Ueber die Verbreitungsmittel der Compositenfruchte." "Bot. Zeitung," 1872, page 1.) Some of the facts which you mention are extremely interesting.

I write now to suggest as worthy of your examination the curious adhesive filaments of mucus emitted by the achenia of many Compositae, of which no doubt you are aware. My attention was first called to the subject by the achenia of an Australian Pumilio (P. argyrolepis), which I briefly described in the "Gardeners' Chronicle," 1861, page 5. As the threads of mucus dry and contract they draw the seeds up into a vertical position on the ground. It subsequently occurred to me that if these seeds were to fall on the wet hairs of any quadruped they would adhere firmly, and might be carried to any distance. I was informed that Decaisne has written a paper on these adhesive threads. What is the meaning of the mucus so copiously emitted from the moistened seeds of Iberis, and of at least some species of Linum? Does the mucus serve as a protection against their being devoured, or as a means of attachment. (707/2. Various theories have been suggested, e.g., that the slime by anchoring the seed to the soil facilitates the entrance of the radicle into the soil: the slime has also been supposed to act as a temporary water-store. See

Klebs in Pfeffer's "Untersuchungen aus dem Bot. Inst. zu Tubingen," I., page 581.) I have been prevented reading your paper sooner by attempting to read Dr. Askenasy's pamphlet, but the German is too difficult for me to make it all out. (707/3. E. Askenasy, "Beitrage zur Kritik der Darwin'schen Lehre." Leipzig, 1872.) He seems to follow Nageli completely. I cannot but think that both much underrate the utility of various parts of plants; and that they greatly underrate the unknown laws of correlated growth, which leads to all sorts of modifications, when some one structure or the whole plant is modified for some particular object.

LETTER 708. TO T.H. FARRER. (Lord Farrer).

(708/1. The following letter refers to a series of excellent observations on the fertilisation of Leguminosae, made by Lord Farrer in the autumn of 1869, in ignorance of Delpino's work on the subject. The result was published in "Nature," October 10th and 17th, 1872, and is full of interesting suggestions. The discovery of the mechanism in Coronilla mentioned in a note was one of the cases in which Lord Farrer was forestalled.)

Down {1872}.

I declare I am almost as sorry as if I had been myself forestalled—indeed, more so, for I am used to it. It is, however, a paramount, though bothersome duty in every naturalist to try and make out all that has been done by others on the subject. By all means publish next summer your confirmation and a summary of Delpino's observations, with any new ones of your own. Especially attend about the nectary exterior to the staminal tube. (708/2. This refers to a species of Coronilla in which Lord Farrer made the remarkable discovery that the nectar is secreted on the outside of the calyx. See "Nature," July 2nd, 1874, page 169; also Letter 715.) This will in every way be far better than writing to Delpino. It would not be at all presumptuous in you to criticise Delpino. I am glad you think him so clever; for so it struck me.

Look at hind legs yourself of some humble and hive-bees; in former take a very big individual (if any can be found) for these are the females, the males being smaller, and they have no pollen-collecting apparatus. I do not remember where it is figured—probably in Kirby & Spence—but actual inspection better...

Please do not return any of my books until all are finished, and do not hurry.

I feel certain you will make fine discoveries.

LETTER 709. TO T.H. FARRER. (Lord Farrer). Sevenoaks, October 13th, 1872.

I must send you a line to say how extremely good your article appears to me to be. It is even better than I thought, and I remember thinking it very good. I am particularly glad of the excellent summary of evidence about the common pea, as it will do for me hereafter to quote; nocturnal insects will not do. I suspect that the aboriginal parent had bluish flowers. I have seen several times bees visiting common and sweet peas, and yet varieties, purposely grown close together, hardly ever intercross. This is a point which for years has half driven me mad, and I have discussed it in my "Var. of Animals and Plants under Dom." (709/1. In the second edition (1875) of the "Variation of Animals and Plants," Volume I., page 348, Darwin added, with respect to the rarity of spontaneous crosses in Pisum: "I have reason to believe that this is due to their stignas being prematurely fertilised in this country by pollen from the same flower." This explanation is, we think, almost certainly applicable to Lathyrus odoratus, though in Darwin's latest publication on the subject he gives reasons to the contrary. See "Cross and Self-Fertilisation," page 156, where the problem is left unsolved. Compare Letter 714 to Delpino. In "Life and Letters," III., page 261, the absence of cross-fertilisation is explained as due to want of perfect adaptation between the pea and our native insects. This is Hermann Muller's view: see his "Fertilisation of Flowers," page 214. See Letter 583, note.) I now suspect (and I wish I had strength to experimentise next spring) that from changed climate both species are prematurely fertilised, and therefore hardly ever cross. When artificially crossed by removal of own pollen in bud, the offspring are very vigorous.

Farewell.—I wish I could compel you to go on working at fertilisation instead of so insignificant a subject as the commerce of the country!

You pay me a very pretty compliment at the beginning of your paper.

LETTER 710. TO J.D. HOOKER.

(710/1. The following letters to Sir J.D. Hooker and the late Mr. Moggridge refer to Moggridge's observation that seeds stored in the nest of the ant Atta at Mentone do not germinate, though they are certainly not dead. Moggridge's observations are given in his book, "Harvesting Ants and Trap-Door Spiders," 1873, which is full of interesting details. The book is moreover remarkable in having resuscitated our knowledge of the existence of the seed-storing habit. Mr. Moggridge points out that the ancients were familiar with the facts, and quotes the well-known fable of the ant and the grasshopper, which La Fontaine borrowed from Aesop. Mr. Moggridge (page 5) goes on: "So long as Europe was taught Natural History by southern writers the belief prevailed; but no sooner did the tide begin to turn, and the current of information to flood from north to south, than the story became discredited."

In Moggridge's "supplement" on the same subject, published in 1874, the author gives an account of his experiments made at Darwin's suggestion, and concludes (page 174) that "the vapour of formic acid is incapable of rendering the seeds dormant after the manner of the ants," and that indeed "its influence is always injurious to the seeds, even when present only in excessively minute quantities." Though unable to explain the method employed, he was convinced "that the non-germination of the seeds is due to some direct influence voluntarily exercised by the ants, and not merely to the conditions found in the nest" (page 172). See Volume I., Letter 251.)

Down, February 21st {1873}.

You have given me exactly the information which I wanted.

Geniuses jump. I have just procured formic acid to try whether its vapour or minute drops will delay germination of fresh seeds; trying others at same time for comparison. But I shall not be able to try them till middle of April, as my despotic wife insists on taking a house in London for a month from the middle of March.

I am glad to hear of the Primer (710/2. "Botany" (Macmillan's Science Primers).); it is not at all, I think, a folly. Do you know Asa Gray's child book on the functions of plants, or some such title? It is very good in giving an interest to the subject.

By the way, can you lend me the January number of the "London Journal of Botany" for an article on insect-agency in fertilisation?

LETTER 711. TO J. TRAHERNE MOGGRIDGE. Down, August 27th, 1873.

I thank you for your very interesting letter, and I honour you for your laborious and careful experiments. No one knows till he tries how many unexpected obstacles arise in subjecting plants to experiments.

I can think of no suggestions to make; but I may just mention that I had intended to try the effects of touching the dampened seeds with the minutest drop of formic acid at the end of a sharp glass rod, so as to imitate the possible action of the sting of the ant. I heartily hope that you may be rewarded by coming to some definite result; but I fail five times out of six in my own experiments. I have lately been trying some with poor success, and suppose that I have done too much, for I have been completely knocked up for some days.

LETTER 712. TO J. TRAHERNE MOGGRIDGE. Down, March 10th, 1874.

I am very sorry to hear that the vapour experiments have failed; but nothing could be better, as it seems to me, than your plan of enclosing a number of the ants with the seeds. The incidental results on the power of different vapours in killing seeds and stopping germination appear very curious, and as far as I know are quite new.

P.S.—I never before heard of seeds not germinating except during a certain season; it will be a very strange fact if you can prove this. (712/1. Certain seeds pass through a resting period before germination. See Pfeffer's "Pflanzenphysiologie," Edition I., Volume II., page III.)

LETTER 713. TO H. MULLER. Down, May 30th, 1873.

I am much obliged for your letter received this morning. I write now chiefly to give myself the pleasure of telling you how cordially I admire the last part of your book, which I have finished. (713/1. "Die Befruchtung der Blumen durch Insekten": Leipzig, 1873. An English translation was published in 1883 by Prof. D'Arcy Thompson. The "Prefatory Notice" to this work (February 6th, 1882) is almost the last of Mr. Darwin's writings. See "Life and Letters," page 281.) The whole discussion seems to me quite excellent, and it has pleased me not a little to find that in the rough MS. of my last chapter I have arrived on many points at nearly the same conclusions that you have done, though we have reached them by different routes. (713/2. "The Effects of Cross and Self-Fertilisation in the Vegetable Kingdom": London, 1876.)

LETTER 714. TO F. DELPINO. Down, June 25th {1873}.

I thank you sincerely for your letter. I am very glad to hear about Lathyrus odoratus, for here in England the vars. never cross, and yet are sometimes visited by bees. (714/1. In "Cross and Self-Fertilisation," page 156, Darwin quotes the information received from Delpino and referred to in the present letter—namely, that it is the fixed opinion of the Italian gardeners that the varieties do intercross. See Letter 709.) Pisum sativum I have also many times seen visited by Bombus. I believe the cause of the many vars. not crossing is that under our climate the flowers are self-fertilised at an early period, before the corolla is fully expanded. I shall examine this point with L. odoratus. I have read H. Muller's book, and it seems to me very good. Your criticism had not occurred to me, but is, I think just—viz. that it is much more important to know what insects habitually visit any flower than the various kinds which occasionally visit it. Have you seen A. Kerner's book "Schutzmittel des Pollens," 1873, Innsbruck. (714/2. Afterwards translated by Dr. Ogle as "Flowers and their Unbidden Guests," with a prefatory letter by Charles Darwin, 1878.) It is very interesting, but he does not seem to know anything about the work of other authors.

I have Bentham's paper in my house, but have not yet had time to read a word of it. He is a man with very sound judgment, and fully admits the principle of evolution.

I have lately had occasion to look over again your discussion on anemophilous plants, and I have again felt much admiration at your work. (714/3. "Atti della Soc. Italiana di Scienze Nat." Volume XIII.)

(714/4. In the beginning of August, 1873, Darwin paid the first of several visits to Lord Farrer's house at Abinger. When sending copies of Darwin's letters for the "Life and Letters," Lord Farrer was good enough to add explanatory notes and recollections, from which we quote the following sketch.)

"Above my house are some low hills, standing up in the valley, below the chalk range on the one hand and the more distant range of Leith Hill on the other, with pretty views of the valley towards Dorking in one direction and Guildford in the other. They are composed of the less fertile Greensand strata, and are covered with fern, broom, gorse, and heath. Here it was a particular pleasure of his to wander, and his tall figure, with his broad-brimmed Panama hat and long stick like an alpenstock, sauntering solitary and slow over our favourite walks, is one of the pleasantest of the many pleasant associations I have with the place."

LETTER 715. TO T.H. FARRER (Lord Farrer).

(715/1. The following note by Lord Farrer explains the main point of the letter, which, however, refers to the "bloom" problem as well as to Coronilla:—

"I thought I had found out what puzzled us in Coronilla varia: in most of the Papilionaceae, when the tenth stamen is free, there is nectar in the staminal tube, and the opening caused by the free stamen enables the bee to reach the nectar, and in so doing the bee fertilises the plant. In Coronilla varia, and in several other species of Coronilla, there is no nectar in the staminal tube or in the tube of the corolla. But there are peculiar glands with nectar on the outside of the calyx, and peculiar openings in the tube of the corolla through which the proboscis of the bee, whilst entering the flower in the usual way and dusting itself with pollen, can reach these glands, thus fertilising the plant in getting the nectar. On writing this to Mr. Darwin, I received the following characteristic note.

The first postscript relates to the rough ground behind my house, over which he was fond of strolling. It had been ploughed up and then allowed to go back, and the interest was to watch how the numerous species of weeds of cultivation which followed the plough gradually gave way in the struggle for existence to the well-known and much less varied flora of an English common.")

Bassett, Southampton, August 14th, 1873.

You are the man to conquer a Coronilla. (715/2. In a former letter to Lord Farrer, Darwin wrote: "Here is a maxim for you, 'It is disgraceful to be beaten by a Coronilla.'") I have been looking at the half-dried flowers, and am prepared to swear that you have solved the mystery. The difference in the size of the cells on the calyx under the vexillum right down to the common peduncle is conspicuous. The flour still adhered to this side; I see little bracteae or stipules apparently with glandular ends at the base of the calyces. Do these secrete? It seems to me a beautiful case. When I saw the odd shape of the base of the vexillum, I concluded that it must have some meaning, but little dreamt what that was. Now there remains only the one serious point—viz.the separation of the one stamen. I daresay that you are right in that nectar was originally secreted within the staminal tube; but why has not the one stamen long since cohered? The great difference in structure for fertilisation within the same genus makes one believe that all such points are vary variable. (715/3. Coronilla emerus is of the ordinary papilionaceous type.) With respect to the non-coherence of the one stamen, do examine some flower-buds at a very early age; for parts which are largely developed are often developed to an unusual degree at a very early age, and it seems to me quite possible that the base of the vexillum (to which the single stamen adhered) might thus be developed, and thus keep it separate for a time from the other stamens. The cohering stamens to the right and left of the single one seem to me to be pushed out a little laterally. When you have finished your observations, you really ought to send an account with a diagram to "Nature," recalling your generalisation about the diadelphous structure, and now explaining the exception of Coronilla. (715/4. The observations were published in "Nature," Volume X., 1874, page 169.)

Do add a remark how almost every detail of structure has a meaning where a flower is well examined.

Your observations pleased me so much that I could not sit still for half an hour.

Please to thank Mr. Payne (715/5. Lord Farrer's gardener.) for his remarks, which are of value to me, with reference to Mimosa. I am very much in doubt whether opening the sashes can act by favouring the evaporation of the drops; may not the movement of the leaves shake off the drops, or change their places? If Mr. Payne remembers any plant which is easily injured by drops, I wish he would put a drop or two on a leaf on a bright day, and cover the plant with a clean bell-glass, and do the same for another plant, but without a bell-glass over it, and observe the effects.

Thank you much for wishing to see us again at Abinger, and it is very doubtful whether it will be Coronilla, Mr. Payne, the new garden, the children, E. {Lady Farrer}, or yourself which will give me the most pleasure to see again.

P.S. 1.—It will be curious to note in how many years the rough ground becomes quite uniform in its flora.

P.S. 2.—One may feel sure that periodically nectar was secreted within the flower and then secreted by the calyx, as in some species of Iris and orchids. This latter being taken advantage of in Coronilla would allow of the secretion within the flower ceasing, and as this change was going on in the two secretions, all the parts of the flower would become modified and correlated.

LETTER 716. TO J. BURDON SANDERSON. Down, Tuesday, September 9th {1873}.

(716/1. Sir J. Burdon Sanderson showed that in Dionoea movement is accompanied by electric disturbances closely analogous to those occurring in muscle (see "Nature," 1874, pages 105, 127; "Proc. R. Soc." XXI., and "Phil. Trans." Volume CLXXIII., 1883, where the results are finally discussed).)

I will send up early to-morrow two plants {of Dionoea} with five goodish leaves, which you will know by their being tied to sticks. Please remember that the slightest touch, even by a hair, of the three filaments on each lobe makes the leaf close, and it will not open for twenty-four hours. You had better put 1/4 in. of water into the saucers of the pots. The plants have been kept too cool in order to retard them. You had better keep them rather warm (i.e. temperature of warm greenhouse) for a day, and in a good light.

I am extremely glad you have undertaken this subject. If you get a positive result, I should think you ought to publish it separately, and I could quote it; or I should be most glad to introduce any note by you into my account.

I have no idea whether it is troublesome to try with the thermo-electric pile any change of temperature when the leaf closes. I could detect none with a common thermometer. But if there is any change of temperature I should expect it would occur some eight to twelve or twenty-four hours after the leaf has been given a big smashed fly, and when it is copiously secreting its acid digestive fluid.

I forgot to say that, as far as I can make out, the inferior surface of the leaf is always in a state of tension, and that the contraction is confined to the upper surface; so that when this contraction ceases or suddenly fails (as by immersion in boiling water) the leaf opens again, or more widely than is natural to it.

Whenever you have quite finished, I will send for the plants in their basket. My son Frank is staying at 6, Queen Anne Street, and comes home on Saturday afternoon, but you will not have finished by that time.

P.S. I have repeated my experiment on digestion in Drosera with complete success. By giving leaves a very little weak hydrochloric acid, I can make them digest albumen—i.e. white of egg—quicker than they can do naturally. I most heartily thank you for all your kindness. I have been pretty bad lately, and must work very little.

LETTER 717. TO J. BURDON SANDERSON. September 13th {1873}.

How very kind it was of you to telegraph to me. I am quite delighted that you have got a decided result. Is it not a very remarkable fact? It seems so to me, in my ignorance. I wish I could remember more distinctly what I formerly read of Du Bois Raymond's results. My poor memory never serves me for more than a vague guide. I really think you ought to try Drosera. In a weak solution of phosphate of ammonia (viz. 1 gr. to 20 oz. of water) it will contract in about five minutes, and even more quickly in pure warm water; but then water, I suppose, would prevent your trial. I forget, but I think it contracts pretty quickly (i.e. in an hour or two) with a large drop of a rather stronger solution of the phosphate, or with an atom of raw meat on the disc of the leaf.

LETTER 718. TO J.D. HOOKER. October 31st, 1873.

Now I want to tell you, for my own pleasure, about the movements of Desmodium.

1. When the plant goes to sleep, the terminal leaflets hang vertically down, but the petioles move up towards the axis, so that the dependent leaves are all crowded round it. The little leaflets never go to sleep, and this seems to me very odd; they are at their games of play as late as 11 o'clock at night and probably later. (718/1. Stahl ("Botanische Zeitung," 1897, page 97) has suggested that the movements of the dwarf leaflets in Desmodium serve to shake the large terminal leaflets, and thus increase transpiration. According to Stahl's view their movement would be more useful at night than by day, because stagnation of the transpiration-current is more likely to occur at night.)

2. If the plant is shaken or syringed with tepid water, the terminal leaflets move down through about an angle of 45 deg, and the

petioles likewise move about 11 deg downwards; so that they move in an opposite direction to what they do when they go to sleep. Cold water or air produces the same effect as does shaking. The little leaflets are not in the least affected by the plant being shaken or syringed. I have no doubt, from various facts, that the downward movement of the terminal leaflets and petioles from shaking and syringing is to save them from injury from warm rain.

3. The axis, the main petiole, and the terminal leaflets are all, when the temperature is high, in constant movement, just like that of climbing plants. This movement seems to be of no service, any more than the incessant movement of amoeboid bodies. The movement of the terminal leaflets, though insensible to the eye, is exactly the same as that of the little lateral leaflets—viz. from side to side, up and down, and half round their own axes. The only difference is that the little leaflets move to a much greater extent, and perhaps more rapidly; and they are excited into movement by warm water, which is not the case with the terminal leaflet. Why the little leaflets, which are rudimentary in size and have lost their sleep-movements and their movements from being shaken, should not only have retained, but have their spontaneous movements exaggerated, I cannot conceive. It is hardly credible that it is a case of compensation. All this makes me very anxious to examine some plant (if possible one of the Leguminosae) with either the terminal or lateral leaflets greatly reduced in size, in comparison with the other leaflets on the same leaf. Can you or any of your colleagues think of any such plant? It is indirectly on this account that I so much want the seeds of Lathyrus nissolia.

I hear from Frank that you think that the absence of both lateral leaflets, or of one alone, is due to their having dropped off; I thought so at first, and examined extremely young leaves from the tips of the shoots, and some of them presented the same characters. Some appearances make me think that they abort by becoming confluent with the main petiole.

I hear also that you doubt about the little leaflets ever standing not opposite to each other: pray look at the enclosed old leaf which has been for a time in spirits, and can you call the little leaflets opposite?

I have seen many such cases on both my plants, though few so well marked.

LETTER 719. TO J.D. HOOKER. Down, October 23rd {1873}.

How good you have been about the plants; but indeed I did not intend you to write about Drosophyllum, though I shall be very glad to have a specimen. Experiments on other plants lead to fresh experiments. Neptunia is evidently a hopeless case. I shall be very glad of the other plants whenever they are ready. I constantly fear that I shall become to you a giant of bores.

I am delighted to hear that you are at work on Nepenthes, and I hope that you will have good luck. It is good news that the fluid is acid; you ought to collect a good lot and have the acid analysed. I hope that the work will give you as much pleasure as analogous work has me. (719/1. Hooker's work on Nepenthes is referred to in "Insectivorous Plants," page 97: see also his address at the Belfast meeting of the British Association, 1874.) I do not think any discovery gave me more pleasure than proving a true act of digestion in Drosera.

LETTER 720. TO J.D. HOOKER. Down, November 24th, 1873.

I have been greatly interested by Mimosa albida, on which I have been working hard. Whilst your memory is pretty fresh, I want to ask a question. When this plant was most sensitive, and you irritated it, did the opposite leaflets shut up quite close, as occurs during sleep, when even a lancet could not be inserted between the leaflets? I can never cause the leaflets to come into contact, and some reasons make me doubt whether they ever do so except during sleep; and this makes me wish much to hear from you. I grieve to say that the plant looks more unhealthy, even, than it was at Kew. I have nursed it like the tenderest infant; but I was forced to cut off one leaf to try the bloom, and one was broken by the manner of packing. I have never syringed (with tepid water) more than one leaf per day; but if it dies, I shall feel like a murderer. I am pretty well convinced that I shall make out my case of movements as a protection against rain lodging on the leaves. As far as I have as yet made out, M. albida is a splendid case.

I have had no time to examine more than one species of Eucalyptus. The seedlings of Lathyrus nissolia are very interesting to me; and there is something wonderful about them, unless seeds of two distinct leguminous species have got somehow mingled together.

LETTER 721. TO W. THISELTON-DYER. Down, December 4th, 1873.

As Hooker is so busy, I should be very much obliged if you could give me the name of the enclosed poor specimen of Cassia. I want much to know its name, as its power of movement, when it goes to sleep, is very remarkable. Linnaeus, I find, was aware of this. It twists each separate leaflet almost completely round (721/1. See "Power of Movement in Plants," Figure 154, page 370.), so that the lower surface faces the sky, at the same time depressing them all. The terminal leaflets are pointed towards the base of the leaf. The whole leaf is also raised up about 12 deg. When I saw that it possessed such complex powers of movement, I thought it would utilise its power to protect the leaflets from rain. Accordingly I syringed the plant for two minutes, and it was really beautiful to see how each leaflet on the younger leaves twisted its short sub-petiole, so that the blade was immediately directed at an angle between 45 and 90 deg to the horizon. I could not resist the pleasure of just telling you why I want to know the name of the Cassia. I should add that it is a greenhouse plant. I suppose that there will not be any better flowers till next summer or autumn.

LETTER 722. TO T. BELT.

(722/1. Belt's account, discussed in this letter, is probably that published in his "Naturalist in Nicaragua" (1874), where he describes "the relation between the presence of honey-secreting glands on plants, and the protection to the latter secured by the attendance of ants attracted by the honey." (Op. cit., pages 222 et seq.))

Thursday {1874?}.

Your account of the ants and their relations seems to me to possess extraordinary interest. I do not doubt that the excretion of sweet fluid by the glands is in your cases of great advantage to the plants by means of the ants, but I cannot avoid believing that primordially it is a simple excretion, as occasionally occurs from the surface of the leaves of lime trees. It is quite possible that the primordial excretion may have been beneficially increased to serve the plant. In the common laurel {Prunus laurocerasus} of our gardens the hive-bees visit incessantly the glands of the young leaves, on their under sides; and I should altogether doubt whether their visits or the occasional visits of ants was of any service to the laurel. The stipules of the common vetch secrete largely during sunshine, and hive-bees collect the sweet fluid. So I think it is with the common bean.

I am writing this away from home, and I have come away to get some rest, having been a good deal overworked. I shall read your book with great interest when published, but will not trouble you to send the MS., as I really have no spare strength or time. I believe that your book, judging by the chapter sent, will be extremely valuable.

LETTER 723. TO J.D. HOOKER.

(723/1. The following letter refers to Darwin's prediction as to the manner in which Hedychium (Zinziberaceae) is fertilised. Sir J.D. Hooker seems to have made inquiries in India in consequence of which Darwin received specimens of the moth which there visits the flower, unfortunately so much broken as to be useless (see "Life and Letters," III., page 284).)

Down, March 25th {1874}.

I am glad to hear about the Hedychium, and how soon you have got an answer! I hope that the wings of the Sphinx will hereafter prove to be bedaubed with pollen, for the case will then prove a fine bit of prophecy from the structure of a flower to special and new means of fertilisation.

By the way, I suppose you have noticed what a grand appearance the plant makes when the green capsules open, and display the orange and crimson seeds and interior, so as to attract birds, like the pale buff flowers to attract dusk-flying lepidoptera. I presume you do not want seeds of this plant, as I have plenty from artificial fertilisation.

(723/2. In "Nature," June 22nd, 1876, page 173, Hermann Muller communicated F. Muller's observation on the fertilisation of a bright-red-flowered species of Hedychium, which is visited by Callidryas, chiefly the males of C. Philea. The pollen is carried by the tips of the butterfly's wing, to which it is temporarily fixed by the slimy layer produced by the degeneration of the anther-wall.

LETTER 724. TO W. THISELTON-DYER. Down, June 4th {1874}.

I am greatly obliged to you about the Opuntia, and shall be glad if you can remember Catalpa. I wish some facts on the action of water, because I have been so surprised at a stream not acting on Dionoea and Drosera. (724/1. See Pfeffer, "Untersuchungen Bot. Inst. zu Tubingen," Bd. I., 1885, page 518. Pfeffer shows that in some cases—Drosera, for instance—water produces movement only when it contains fine particles in suspension. According to Pfeffer the stamens of Berberis, and the stigma of Mimulus, are both stimulated by gelatine, the action of which is, generally speaking, equivalent to that of water.) Water does not act on the stamens of Berberis, but it does on the stigma of Mimulus. It causes the flowers of the bedding-out Mesembryanthemum and Drosera to close, but it has not this effect on Gazania and the daisy, so I can make out no rule.

I hope you are going on with Nepenthes; and if so, you will perhaps like to hear that I have just found out that Pinguicula can digest albumen, gelatine, etc. If a bit of glass or wood is placed on a leaf, the secretion is not increased; but if an insect or animal-matter is thus placed, the secretion is greatly increased and becomes feebly acid, which was not the case before. I have been astonished and much disturbed by finding that cabbage seeds excite a copious secretion, and am now endeavouring to discover what this means. (724/2. Clearly it had not occurred to Darwin that seeds may supply nitrogenous food as well as insects: see "Insectivorous Plants," page 390.) Probably in a few days' time I shall have to beg a little information from you, so I will write no more now.

P.S. I heard from Asa Gray a week ago, and he tells me a beautiful fact: not only does the lid of Sarracenia secrete a sweet fluid, but there is a line or trail of sweet exudation down to the ground so as to tempt insects up. (724/3. A dried specimen of Sarracenia, stuffed with cotton wool, was sometimes brought from his study by Mr. Darwin, and made the subject of a little lecture to visitors of natural history tastes.)

LETTER 725. TO W. THISELTON-DYER. Down, June 23rd, 1874.

I wrote to you about a week ago, thanking you for information on cabbage seeds, asking you the name of Luzula or Carex, and on some other points; and I hope before very long to receive an answer. You must now, if you can, forgive me for being very troublesome, for I am in that state in which I would sacrifice friend or foe. I have ascertained that bits of certain leaves, for instance spinach, excite much secretion in Pinguicula, and that the glands absorb matter from the leaves. Now this morning I have received a lot of leaves from my future daughter-in-law in North Wales, having a surprising number of captured insects on them, a good many leaves, and two seed-capsules. She informs me that the little leaves had excited secretion; and my son and I have ascertained this morning that the protoplasm in the glands beneath the little leaves has undoubtedly undergone aggregation. Therefore, absurd as it may sound, I am prepared to affirm that Pinguicula is not only insectivorous, but graminivorous, and granivorous! Now I want to beg you to look under the simple microscope at the enclosed leaves and seeds, and, if you possibly can, tell me their genera. The little narrow leaves are remarkable (725/1. Those of Erica tetralix.); they are fleshy, with the edges much curled from the axis of the plant, and bear a few long glandular hairs; these grow in little tufts. These are the commonest in Pinguicula, and seem to afford most nutritious matter. A second leaf is like a miniature sycamore. With respect to the seeds, I suppose that one is a Carex; the other looks like that of Rumex, but is enclosed in a globular capsule. The Pinguicula grew on marshy, low, mountainous land.

I hope you will think this subject sufficiently interesting to make you willing to aid me as far as you can. Anyhow, forgive me for being so very troublesome.

LETTER 726. TO J.D. HOOKER. Down, August 30th {1874}.

I am particularly obliged for your address. (726/1. Presidential address (Biological Section) at the Belfast meeting of the British Association, 1874.) It strikes me as quite excellent, and has interested me in the highest degree. Nor is this due to my having worked at the subject, for I feel sure that I should have been just as much struck, perhaps more so, if I had known nothing about it. You could not, in my opinion, have put the case better. There are several lights (besides the facts) in your essay new to me, and you have greatly honoured me. I heartily congratulate you on so splendid a piece of work. There is a misprint at page 7, Mitschke for Nitschke. There is a partial error at page 8, where you say that Drosera is nearly indifferent to organic substances. This is much too strong, though they do act less efficiently than organic with soluble nitrogenous matter; but the chief difference is in the widely different period of subsequent re-expansion. Thirdly, I did not suggest to Sanderson his electrical experiments, though, no doubt, my remarks led to his thinking of them.

Now for your letter: you are very generous about Dionoea, but some of my experiments will require cutting off leaves, and therefore injuring plants. I could not write to Lady Dorothy {Nevill}. Rollisson says that they expect soon a lot from America. If Dionoea is not despatched, have marked on address, "to be forwarded by foot-messenger."

Mrs. Barber's paper is very curious, and ought to be published (726/2. Mrs. Barber's paper on the pupa of Papilio Nireus assuming different tints corresponding to the objects to which it was attached, was communicated by Mr. Darwin to the "Trans. Entomolog. Soc." 1874.); but when you come here (and REMEMBER YOU OFFERED TO COME) we will consult where to send it. Let me hear when you recommence on Cephalotus or Sarracenia, as I think I am now on right track about Utricularia, after wasting several weeks in fruitless trials and observations. The negative work takes five times more time than the positive.

LETTER 727. TO J.D. HOOKER. Down, September 18th {1874}.

I have had a splendid day's work, and must tell you about it.

Lady Dorothy sent me a young plant of U{tricularia} montana (727/1. See "Life and Letters," III., page 327, and "Insectivorous Plants," page 431.), which I fancy is the species you told me of. The roots or rhizomes (for I know not which they are; I can see no scales or internodes or absorbent hairs) bear scores of bladders from 1/20 to 1/100 of an inch in diameter; and I traced these roots to the depth of 1 1/2 in. in the peat and sand. The bladders are like glass, and have the same essential structure as those of our species, with the exception that many exterior parts are aborted. Internally the structure is perfect, as is the minute valvular opening into the bladder, which is filled with water. I then felt sure that they captured subterranean insects, and after a time I found two with decayed remnants, with clear proof that something had been absorbed, which had generated protoplasm. When you are here I shall be very curious to know whether they are roots or rhizomes.

Besides the bladders there are great tuber-like swellings on the rhizomes; one was an inch in length and half in breadth. I suppose these must have been described. I strongly suspect that they serve as reservoirs for water. (727/2. The existence of water-stores is quite in accordance with the epiphytic habit of the plant.) But I shall experimentise on this head. A thin slice is a beautiful object, and looks like coarsely reticulated glass.

If you have an old plant which could be turned out of its pot (and can spare the time), it would be a great gain to me if you would tear off a bit of the roots near the bottom, and shake them well in water, and see whether they bear these minute glass-like bladders. I should also much like to know whether old plants bear the solid bladder-like bodies near the upper surface of the pot. These bodies are evidently enlargements of the roots or rhizomes. You must forgive this long letter, and make allowance for my delight at finding this new sub-group of insect-catchers. Sir E. Tennent speaks of an aquatic species of Utricularia in Ceylon, which has bladders on its roots, and rises annually to the surface, as he says, by this means. (727/3. Utricularia stellaris. Emerson Tennent's "Ceylon," Volume I., page 124, 1859.)

We shall be delighted to see you here on the 26th; if you will let us know your train we will send to meet you. You will have to work like a slave while you are here.

LETTER 728. TO J. JENNER WEIR.

(728/1. In 1870 Mr. Jenner Weir wrote to Darwin: "My brother has but two kinds of laburnum, viz., Cytisus purpureus, very erect, and Cytisus alpinus, very pendulous. He has several stocks of the latter grafted with the purple one; and this year, the grafts being two years old, I saw in one, fairly above the stock, about four inches, a raceme of purely yellow flowers with the usual dark markings, and above them a bunch of purely purple flowers; the branches of the graft in no way showed an intermediate character, but had the usual rigid growth of purpureus."

Early in July 1875, when Darwin was correcting a new edition of "Variation under Domestication," he again corresponded with Mr. Weir on the subject.)

Down, July 8th {1875}.

I thank you cordially. The case interests me in a higher degree than anything which I have heard for a very long time. Is it your brother Harrison W., whom I know? I should like to hear where the garden is. There is one other very important point which I am most anxious to hear—viz., the nature of the leaves at the base of the yellow racemes, for leaves are always there produced with the yellow laburnums, and I suppose so in the case of C. purpureus. As the tree has produced yellow racemes several times, do you think you could ask your brother to cut off and send me by post in a box a small branch of the purple stock with the pods or leaves of the yellow sport? (728/2. "The purple stock" here means the supposed C. purpureus, on which a yellow-flowered branch was borne.) This would be an immense favour, for then I would cut the point of junction longitudinally and examine slice under the microscope, to be able to state no trace of bud of yellow kind having been inserted. I do not suspect anything of the kind, but it is sure to be said that your brother's gardener, either by accident or fraud, inserted a bud. Under this point of view it would be very good to gather from your brother how many times the yellow sport has appeared. The case appears to me so very important as to be worth any trouble. Very many thanks for all assistance so kindly given.

I will of course send a copy of new edition of "Variation under Domestication" when published in the autumn.

LETTER 729. TO J. JENNER WEIR.

(729/1. On July 9th Mr. Weir wrote to say that a branch of the Cytisus had been despatched to Down. The present letter was doubtless written after Darwin had examined the specimen. In "Variation under Domestication," Edition II., Volume I., page 417, note, he gives for a case recorded in the "Gardeners' Chronicle" in 1857 the explanation here offered (viz. that the graft was not C. purpureus but C. Adami), and adds, "I have ascertained that this occurred in another instance." This second instance is doubtless Mr. Weir's.)

Down, July 10th, 1875.

I do not know how to thank you enough; pray give also my thanks and kind remembrances to your brother. I am sure you will forgive my expressing my doubts freely, as I well know that you desire the truth more than anything else. I cannot avoid the belief that some nurseryman has sold C{ytisus} Adami to your brother in place of the true C. purpureus. The latter is a little bush only 3 feet high (Loudon), and when I read your account, it seemed to me a physical impossibility that a sporting branch of C. alpinus could grow to any size and be supported on the extremely delicate branches of C. purpureus. If I understand rightly your letter, you consider the tuft of small shoots on one side of the sporting C. alpinus from Weirleigh as C. purpureus; but these shoots are certainly those of C. Adami. I earnestly beg you to look at the specimens enclosed. The branch of the true C. purpureus is the largest which I could find. If C. Adami was sold to your brother as C. purpureus, everything is explained; for then the gardener has grafted C. Adami on C. alpinus, and the former has sported in the usual manner; but has not sported into C. purpureus, only into C. alpinus. C. Adami does not sport less frequently into C. purpureus than into C. alpinus. Are the purple flowers borne on moderately long racemes? If so, the plant is certainly C. Adami, for the true C. purpureus bears flowers close to the branches. I am very sorry to be so troublesome, but I am very anxious to hear again from you.

C. purpureus bears "flowers axillary, solitary, stalked."

P.S.—I think you said that the purple {tree} at Weirleigh does not seed, whereas the C. purpureus seeds freely, as you may see in enclosed. C. Adami never produces seeds or pods.

LETTER 730. TO E. HACKEL.

(730/1. The following extract refers to Darwin's book on "Cross and Self-Fertilisation.")

November 13th, 1875.

I am now busy in drawing up an account of ten years' experiments in the growth and fertility of plants raised from crossed and self-fertilised flowers. It is really wonderful what an effect pollen from a distinct seedling plant, which has been exposed to different conditions of life, has on the offspring in comparison with pollen from the same flower or from a distinct individual, but which has been long subjected to the same conditions. The subject bears on the very principle of life, which seems almost to require changes in the conditions.

LETTER 731. TO G.J. ROMANES.

(731/1. The following extract from a letter to Romanes refers to Francis Darwin's paper, "Experiments on the Nutrition of Drosera rotundifolia." "Linn. Soc. Journ." {1878}, published 1880, page 17.)

August 9th {1876}.

The second point which delights me, seeing that half a score of botanists throughout Europe have published that the digestion of meat by plants is of no use to them (a mere pathological phenomenon, as one man says!), is that Frank has been feeding under exactly similar conditions a large number of plants of Drosera, and the effect is wonderful. On the fed side the leaves are much larger, differently coloured, and more numerous; flower-stalks taller and more numerous, and I believe far more seed capsules,—but these not yet counted. It is particularly interesting that the leaves fed on meat contain very many more starch granules (no doubt owing to more protoplasm being first formed); so that sections stained with iodine, of fed and unfed leaves, are to the naked eye of very different colours.

There, I have boasted to my heart's content, and do you do the same, and tell me what you have been doing.

LETTER 732. TO J.D. HOOKER. Down, October 25th {1876}.

If you can put the following request into any one's hands pray do so; but if not, ignore my request, as I know how busy you are.

I want any and all plants of Hoya examined to see if any imperfect flowers like the one enclosed can be found, and if so to send them to me, per post, damp. But I especially want them as young as possible.

They are very curious. I have examined some sent me from Abinger (732/1. Lord Farrer's house.), but they were a month or two too old, and every trace of pollen and anthers had disappeared or had never been developed. Yet a very fine pod with apparently good seed had been formed by one such flower. (732/2. The seeds did not germinate; see the account of Hoya carnosa in "Forms of Flowers," page 331.)

LETTER 733. TO G.J. ROMANES.

(733/1. Published in the "Life of Romanes," page 62.)

Down, August 10th {1877}.

When I went yesterday I had not received to-day's "Nature," and I thought that your lecture was finished. (733/2. Abstract of a lecture on "Evolution of Nerves and Nervo-Systems," delivered at the Royal Institution, May 25th, 1877. "Nature," July 19th, August 2nd, August 9th, 1877.) This final part is one of the grandest essays which I ever read.

It was very foolish of me to demur to your lines of conveyance like the threads in muslin (733/3. "Nature," August 2nd, page 271.), knowing how you have considered the subject: but still I must confess I cannot feel quite easy. Everyone, I suppose, thinks on what he has himself seen, and with Drosera, a bit of meat put on any one gland on its disc causes all the surrounding tentacles to bend to this point, and here there can hardly be differentiated lines of conveyance. It seems to me that the tentacles probably bend to that point wherever a molecular wave strikes them, which passes through the cellular tissue with equal ease in all directions in this particular case. (733/4. Speaking generally, the transmission takes place more readily in the longitudinal direction than across the leaf: see "Insectivorous Plants," page 239.) But what a fine case that of the Aurelia is! (733/5. Aurelia aurita, one of the medusae. "Nature," pages 269-71.)

LETTER 734. TO W. THISELTON-DYER. 6, Queen Anne Street {December 1876}.

Tell Hooker I feel greatly aggrieved by him: I went to the Royal Society to see him for once in the chair of the Royal, to admire his dignity and enjoy it, and lo and behold, he was not there. My outing gave me much satisfaction, and I was particularly glad to see Mr. Bentham, and to see him looking so wonderfully well and young. I saw lots of people, and it has not done me a penny's worth of harm, though I could not get to sleep till nearly four o'clock.

LETTER 735. TO D. OLIVER. Down, October, 13th {1876?}.

You must be a clair-voyant or something of that kind to have sent me such useful plants. Twenty-five years ago I described in my father's garden two forms of Linum flavum (thinking it a case of mere variation); from that day to this I have several times looked, but never saw the second form till it arrived from Kew. Virtue is never its own reward: I took paper this summer to write to you to ask you to send me flowers, {so} that I might beg plants of this Linum, if you had the other form, and refrained, from not wishing to trouble you. But I am now sorry I did, for I have hardly any doubt that L. flavum never seeds in any garden that I have seen, because one form alone is cultivated by slips. (735/1. Id est, because, the plant being grown from slips, one form alone usually occurs in any one garden. It is also arguable that it is grown by slips because only one form is common, and therefore seedlings cannot be raised.)

(736/1. The following five letters refer to Darwin's work on "bloom"—a subject on which he did not live to complete his researches:—

One of his earliest letters on this subject was addressed in August, 1873, to Sir Joseph Hooker (736/2. Published in "Life and Letters," III., page 339.):

"I want a little information from you, and if you do not yourself know, please to enquire of some of the wise men of Kew.

"Why are the leaves and fruit of so many plants protected by a thin layer of waxy matter (like the common cabbage), or with fine hair, so that when such leaves or fruit are immersed in water they appear as if encased in thin glass? It is really a pretty sight to put a pod of the common pea, or a raspberry, into water. I find several leaves are thus protected on the under surface and not on the upper.

"How can water injure the leaves, if indeed this is at all the case?"

On this latter point Darwin wrote to the late Lord Farrer:

"I am now become mad about drops of water injuring leaves. Please ask Mr. Payne (736/3. Lord Farrer's gardener.) whether he believes, FROM HIS OWN EXPERIENCE, that drops of water injure leaves or fruit in his conservatories. It is said that the drops act as burning-glasses; if this is true, they would not be at all injurious on cloudy days. As he is so acute a man, I should very much like to hear his opinion. I remember when I grew hothouse orchids I was cautioned not to wet their leaves; but I never then thought on the subject."

The next letter, though of later date than some which follow it, is printed here because it briefly sums his results and serves as guide to the letters dealing with the subject.)

LETTER 736. TO W. THISELTON-DYER.

(736/4. Published in "Life and Letters," III., page 341.)

Down, September 5th {1877}.

One word to thank you. I declare, had it not been for your kindness, we should have broken down. As it is we have made out clearly that with some plants (chiefly succulent) the bloom checks evaporation—with some certainly prevents attacks of insects; with SOME sea-shore plants prevents injury from salt water, and, I believe, with a few prevents injury from pure water resting on the leaves. This latter is as yet the most doubtful and the most interesting point in relation to the movements of plants.

(736/5. Modern research, especially that of Stahl on transpiration ("Bot. Zeitung," 1897, page 71) has shown that the question is more complex than it appeared in 1877. Stahl's point of view is that moisture remaining on a leaf checks the transpiration-current; and by thus diminishing the flow of mineral nutriment interferes with the process of assimilation. Stahl's idea is doubtless applicable to the whole problem of bloom on leaves. For other references to bloom see letters 685, 689 and 693.)

LETTER 737. TO J.D. HOOKER. Down, August 19th, 1873.

The next time you walk round the garden ask Mr. Smith (737/1. Probably John Smith (1798-1888), for some years Curator, Royal Gardens, Kew.), or any of your best men, what they think about injury from watering during sunshine. One of your men—viz., Mr. Payne, at Abinger, who seems very acute—declares that you may water safely any plant out of doors in sunshine, and that you may do the same for plants under glass if the sashes are opened. This seems to me very odd, but he seems positive on the point, and acts on it in raising splendid grapes. Another good gardener maintains that it is only COLD water dripping often on the same point of a leaf that ever injures it. I am utterly perplexed, but interested on the point. Give me what you learn when you come to Down.

I should like to hear what plants are believed to be most injured by being watered in sunshine, so that I might get such.

I expect that I shall be utterly beaten, as on so many other points; but I intend to make a few experiments and observations. I have already convinced myself that drops of water do NOT act as burning lenses.

LETTER 738. TO J.D. HOOKER. December 20th {1873}.

I find that it is no use going on with my experiments on the evil effects of water on bloom-divested leaves. Either I erred in the early autumn or summer in some incomprehensible manner, or, as I suspect to be the case, water is only injurious to leaves when there is a good supply of actinic rays. I cannot believe that I am all in the wrong about the movements of the leaves to shoot off water.

The upshot of all this is that I want to keep all the plants from Kew until the spring or early summer, as it is mere waste of time going on at present.

LETTER 739. TO W. THISELTON-DYER. Down, July 22nd {1877}.

Many thanks for seeds of the Malva and information about Averrhoa, which I perceived was sensitive, as A. carambola is said to be; and about Mimosa sensitiva. The log-wood {Haematoxylon} has interested me much. The wax is very easily removed, especially from the older leaves, and I found after squirting on the leaves with water at 95 deg, all the older leaves became coated, after forty-eight hours, in an astonishing manner with a black Uredo, so that they looked as if sprinkled with soot and water. But not one of the younger leaves was affected. This has set me to work to see whether the "bloom" is not a protection against parasites. As soon as I have ascertained a little more about the case (and generally I am quite wrong at first) I will ask whether I could have a very small plant, which should never be syringed with water above 60 deg, and then I suspect the leaves would not be spotted, as were the older ones on the plant, when it arrived from Kew, but nothing like what they were after my squirting.

In an old note of yours (which I have just found) you say that you have a sensitive Schrankia: could this be lent me?

I have had lent me a young Coral-tree (Erythrina), which is very sickly, yet shows odd sleep movements. I suppose I could buy one, but Hooker told me first to ask you for anything.

Lastly, have you any seaside plants with bloom? I find that drops of sea-water corrode sea-kale if bloom is removed; also the var.

littorum of Triticum repens. (By the way, my plants of the latter, grown in pots here, are now throwing up long flexible green blades, and it is very odd to see, ON THE SAME CULM, the rigid grey bloom-covered blades and the green flexible ones.) Cabbages, ill-luck to them, do not seem to be hurt by salt water. Hooker formerly told me that Salsola kali, a var. of Salicornia, one species of Suaeda, Euphorbia peplis, Lathyrus maritimus, Eryngium maritimum, were all glaucous and seaside plants. It is very improbable that you have any of these or of foreigners with the same attributes.

God forgive me: I hope that I have not bored you greatly.

By all the rules of right the leaves of the logwood ought to move (as if partially going to sleep) when syringed with tepid water. The leaves of my little plant do not move at all, and it occurs to me as possible, though very improbable, that it would be different with a larger plant with perhaps larger leaves. Would you some day get a gardener to syringe violently, with water kept in a hothouse, a branch on one of your largest logwood plants and observe {whether?} leaves move together towards the apex of leaf?

By the way, what astonishing nonsense Mr. Andrew Murray has been writing about leaves and carbonic acid! I like to see a man behaving consistently...

What a lot I have scribbled to you!

LETTER 740. TO W. THISELTON-DYER. {August, 1877.}

There is no end to my requests. Can you spare me a good plant (or even two) of Oxalis sensitiva? The one which I have (formerly from Kew) has been so maltreated that I dare not trust my results any longer.

Please give the enclosed to Mr. Lynch. (740/1. Mr. Lynch, now Curator of the Cambridge Botanic Garden, was at this time in the R. Bot. Garden, Kew. Mr. Lynch described the movements of Averrhoa bilimbi in the "Linn. Soc. Journ," Volume XVI., page 231. See also "The Power of Movement in Plants," page 330.) The spontaneous movements of the Averrhoa are very curious.

You sent me seeds of Trifolium resupinatum, and I have raised plants, and some former observations which I did not dare to trust have proved accurate. It is a very little fact, but curious. The half of the lateral leaflets (marked by a cross) on the lower side have no bloom and are wetted, whereas the other half has bloom and is not wetted, so that the two sides look different to the naked eye. The cells of the eipdermis appear of a different shape and size on the two sides of the leaf.

When we have drawings and measurements of cells made, and are sure of our facts, I shall ask you whether you know of any case of the same leaf differing histologically on the two sides, for Hooker always says you are a wonderful man for knowing what has been made out.

(740/2. The biological meaning of the curious structure of the leaves of Trifolium resupinatum remains a riddle. The stomata and (speaking from memory) the trichomes differ on the two halves of the lateral leaflets.)

LETTER 741. TO L. ERRERA.

(741/1. Professor L. Errera, of Brussels wrote, as a student, to Darwin, asking permission to send the MS. of an essay by his friend S. Gevaert and himself on cross and self-fertilisation, and which was afterwards published in the "Bull. Soc. Bot. Belg." XVII., 1878. The terms xenogamy, geitonogamy, and autogamy were first suggested by Kerner in 1876; their definition will be found at page 9 of Ogle's translation of Kerner's "Flowers and their Unbidden Guests," 1878. In xenogamy the pollen comes from another PLANT; in geitonogamy from another FLOWER on the same PLANT; in autogamy from the androecium of the fertilised FLOWER. Allogamy embraces xenogamy and geitonogamy.)

Down, October 4th, 1877.

I have now read your MS. The whole has interested me greatly, and is very clearly written. I wish that I had used some such terms as autogamy, xenogamy, etc...I entirely agree with you on the a priori probability of geitonogamy being more advantageous than autogamy; and I cannot remember having ever expressed a belief that autogamy, as a general rule, was better than geitonogamy; but the cases recorded by me seem too strong not to make me suspect that there was some unknown advantage in autogamy. In one place I insert the caution "if this be really the case," which you quote. (741/2. See "Cross and Self-Fertilisation," pages 352, 386. The phrase referred to occurs in both passages; that on page 386 is as follows: "We have also seen reason to suspect that self-fertilisation is in some peculiar manner beneficial to certain plants; but if this be really the case, the benefit thus derived is far more than counterbalanced by a cross with a fresh stock or with a slightly different variety." Errera and Gevaert conclude (pages 79-80) that the balance of the available evidence is in favour of the belief that geitonogamy is intermediate, in effectiveness, between autogamy and xenogamy.) I shall be very glad to be proved to be altogether in error on this point.

Accept my thanks for pointing out the bad erratum at page 301. I hope that you will experimentise on inconspicuous flowers (741/3. See Miss Bateson, "Annals of Botany," 1888, page 255, "On the Cross-Fertilisation of Inconspicuous Flowers:" Miss Bateson showed that Senecio vulgaris clearly profits by cross-fertilisation; Stellaria media and Capsella bursa-pastoris less certainly.); if I were not too old and too much occupied I would do so myself.

Finally let me thank you for the kind manner in which you refer to my work, and with cordial good wishes for your success...

LETTER 742. TO W. THISELTON-DYER. Down, October 9th, 1877.

One line to thank you much about Mertensia. The former plant has begun to make new leaves, to my great surprise, so that I shall be now well supplied. We have worked so well with the Averrhoa that unless the second species arrives in a very good state it would be superfluous to send it. I am heartily glad that you and Mrs. Dyer are going to have a holiday. I will look at you as a dead man for the next month, and nothing shall tempt me to trouble you. But before you enter your grave aid me if you can. I want seeds of three or four plants (not Leguminosae or Cruciferae) which produce large cotyledons. I know not in the least what plants have large cotyledons. Why I want to know is as follows: The cotyledons of Cassia go to sleep, and are sensitive to a touch; but what has surprised me much is that they are in constant movement up and down. So it is with the cotyledons of the cabbage, and therefore I am very curious to ascertain how far this is general.

LETTER 743. TO W. THISELTON-DYER. Down, October 11th {1877}.

The fine lot of seeds arrived yesterday, and are all sown, and will be most useful. If you remember, pray thank Mr. Lynch for his aid. I had not thought of beech or sycamore, but they are now sown.

Perhaps you may like to see a rough copy of the tracing of movements of one of the cotyledons of red cabbage, and you can throw it into the fire. A line joining the two cotyledons stood facing a north-east window, and the day was uniformly cloudy. A bristle was gummed to one cotyledon, and beyond it a triangular bit of card was fixed, and in front a vertical glass. A dot was made in the glass every quarter or half hour at the point where the end of the bristle and the apex of card coincided, and the dots were joined by straight lines. The observations were from 10 a.m. to 8.45 p.m. During this time the enclosed figure was described; but between 4 p.m. and 5.38 p.m. the cotyledon moved so that the prolonged line was beyond the limits of the glass, and the course is here shown by an imaginary dotted line. The cotyledon of Primula sinensis moved in closely analogous manner, as do those of a Cassia. Hence I expect to find such movements very general with cotyledons, and I am inclined to look at them as the foundation for all the other adaptive movements of leaves. They certainly are of the so-called sleep of plants.

I hope I have not bothered you. Do not answer. I am all on fire at the work.

I have had a short and very prosperous note from Asa Gray, who says Hooker is very prosperous, and both are tremendously hard at work. (743/1. "Hooker is coming over, and we are going in summer to the Rocky Mountains together, according to an old promise of mine." Asa Gray to G.F. Wright, May 24th, 1877 ("Letters of Asa Gray," II., page 666).)

LETTER 744. TO H. MULLER. Down, January 1st {1878?}.

I must write two or three lines to thank you cordially for your very handsome and very interesting review of my last book in "Kosmos," which I have this minute finished. (744/1. "Forms of Flowers," 1877. H. Muller's article is in "Kosmos," II., page 286.) It is wonderful how you have picked out everything important in it. I am especially glad that you have called attention to the parallelism between illegitimate offspring of heterostyled plants and hybrids. Your previous article in "Kosmos" seemed to me very important, but for some unknown reason the german was very difficult, and I was sadly overworked at the time, so that I could not understand a good deal of it. (744/2. "Kosmos," II., pages 11, 128. See "Forms of Flowers," Edition II., page 308.) But I have put it on one side, and when I have to prepare a new edition of my book I must make it out. It seems that you attribute such cases as that of the dioecious Rhamnus and your own of Valeriana to the existence of two forms with larger and smaller flowers. I cannot follow the steps by which such plants have been rendered dioecious, but when I read your article with more care I hope I shall understand. (744/3. See "Forms of Flowers," Edition II., pages 9 and 304. H. Muller's view is briefly that conspicuous and less conspicuous varieties occurred, and that the former were habitually visited first by insects; thus the less conspicuous form would play the part of females and their pollen would tend to become superfluous. See H. Muller in "Kosmos," II.) If you have succeeded in explaining this class of cases I shall heartily rejoice, for they utterly perplexed me, and I could not conjecture what their meaning was. It is a grievous evil to have no faculty for new languages.

With the most sincere respect and hearty good wishes to you and all your family for the new year...

P.S.—What interesting papers your wonderful brother has lately been writing!

LETTER 745. TO W. THISELTON-DYER.

(745/1. This letter refers to the purchase of instruments for the Jodrell Laboratory in the Royal Gardens, Kew. "The Royal Commission on Scientific Instruction and the Advancement of

Science, commonly spoken of as the Devonshire Commission, in its fourth Report (1874), page 10, expressed the opinion that 'it is highly desirable that opportunities for the pursuit of investigations in Physiological Botany should be afforded at Kew to those persons who may be inclined to follow that branch of science.' Effect was given to this recommendation by the liberality of the late T.J. Phillips-Jodrell, M.A., who built and equipped the small laboratory, which has since borne his name, at his own expense. It was completed and immediately brought into use in 1876." The above is taken from the "Bulletin of Miscellaneous Information," R. Botanic Gardens, Kew, 1901, page 102, which also gives a list of work carried out in the laboratory between 1876 and 1900.)

Down, March 14th, 1878.

I have a very strong opinion that it would be the greatest possible pity if the Phys{iological} Lab., now that it has been built, were not supplied with as many good instruments as your funds can possibly afford. It is quite possible that some of them may become antiquated before they are much or even at all used. But this does not seem to me any argument at all against getting them, for the Laboratory cannot be used until well provided; and the mere fact of the instruments being ready may suggest to some one to use them. You at Kew, as guardians and promoters of botanical science, will then have done all in your power, and if your Lab. is not used the disgrace will lie at the feet of the public. But until bitter experience proves the contrary I will never believe that we are so backward. I should think the German laboratories would be very good guides as to what to get; but Timiriazeff of Moscow, who travelled over Europe to see all Bot. Labs., and who seemed so good a fellow, would, I should think, give the best list of the most indispensable instruments. Lately I thought of getting Frank or Horace to go to Cambridge for the use of the heliostat there; but our observations turned out of less importance than I thought, yet if there had been one at Kew we should probably have used it, and might have found out something curious. It is impossible for me to predict whether or not we should ever want this or that instrument, for we are guided in our work by what turns up. Thus I am now observing something about geotropism, and I had no idea a few weeks ago that this would have been necessary. In a short time we might earnestly wish for a centrifugal apparatus or a heliostat. In all such cases it would make a great difference if a man knew that he could use a particular instrument without great loss of time. I have now given my opinion, which is very decided, whether right or wrong, and Frank quite agrees with me. You can, of course, show this letter to Hooker.

LETTER 746. TO F. LUDWIG. Down, May 29th, 1878.

I thank you sincerely for the trouble which you have taken in sending me so long and interesting a letter, together with the specimens. Gradations are always very valuable, and you have been remarkably successful in discovering the stages by which the Plantago has become gyno-dioecious. (746/1. See F. Ludwig, "Zeitsch. f. d. Geo. Naturwiss." Bd. LII., 1879. Professor Ludwig's observations are quoted in the preface to "Forms of Flowers," Edition II., page ix.) Your view of its origin, from being proterogynous, seems to me very probable, especially as the females are generally the later-flowering plants. If you can prove the reverse case with Thymus your view will manifestly be rendered still more probable. I have never felt satisfied with H. Muller's view, though he is so careful and admirable an observer. (746/2. See "Forms of Flowers," Edition II., page 308. Also letter 744.) It is more than seventeen years since I attended to Plantago, and when nothing had been published on the subject, and in consequence I omitted to attend to several points; and now, after so long an interval, I cannot pretend to say to which of your forms the English one belongs; I well remember that the anther of the females contained a good deal {of} pollen, though not one sound grain.

P.S.—Delpino is Professor of Botany in Genoa, Italy (746/3. Now at Naples.); I have always found him a most obliging correspondent.

LETTER 747. TO W. THISELTON-DYER. Down, August 24th {1878}.

Many thanks for seeds of Trifolium resupinatum, which are invaluable to us. I enclose seeds of a Cassia, from Fritz Muller, and they are well worth your cultivation; for he says they come from a unique, large and beautiful tree in the interior, and though looking out for years, he has never seen another specimen. One of the most splendid, largest and rarest butterflies in S. Brazil, he has never seen except near this one tree, and he has just discovered that its caterpillars feed on its leaves.

I have just been looking at fine young pods beneath the ground of Arachis. (747/1. Arachis hypogoea, cultivated for its "ground nuts.") I suppose that the pods are not withdrawn when ripe from the ground; but should this be the case kindly inform me; if I do not hear I shall understand that {the} pods ripen and are left permanently beneath the ground.

If you ever come across heliotropic or apheliotropic aerial roots on a plant not valuable (but which should be returned), I should like to observe them. Bignonia capreolata, with its strongly apheliotropic tendrils (which I had from Kew), is now interesting me greatly. Veitch tells me it is not on sale in any London nursery, as I applied to him for some additional plants. So much for business.

I have received from the Geographical Soc. your lecture, and read it with great interest. (747/2. "On Plant-Distribution as a field for Geographical Research." "Geog. Soc. Proc." XXII., 1878, page 412.) But it ought not merely to be read; it requires study. The sole criticism which I have to make is that parts are too much condensed: but, good Lord, how rare a fault is this! You do not quote Saporta, I think; and some of his work on the Tertiary plants would have been useful to you. In a former note you spoke contemptuously of your lecture: all I can say is that I never heard any one speak more unjustly and shamefully of another than you have done of yourself!

LETTER 748. TO H. MULLER. Down, September 20th, 1878.

I am working away on some points in vegetable physiology, but though they interest me and my son, yet they have none of the fascination which the fertilisation of flowers possesses. Nothing in my life has ever interested me more than the fertilisation of such plants as Primula and Lythrum, or again Anacamptis (748/1. Orchis pyramidalis.) or Listera.

LETTER 749. TO H. MULLER. Down, February 12th {1879}.

I have just heard that some misfortune has befallen you, and that you have been treated shamefully. (749/1. Hermann Muller was accused by the Ultramontane party of introducing into his school-teaching crude hypotheses ("unreife Hypothesen"), which were assumed to have a harmful influence upon the religious sentiments of his pupils. Attempts were made to bring about Muller's dismissal, but the active hostility of his opponents, which he met in a dignified spirit, proved futile. ("Prof. Dr. Hermann Muller von Lippstadt. Ein Gedenkblatt," von Ernst Krause. "Kosmos," VII., page 393, 1883.)) I grieve deeply to hear this, and as soon as you can find a few minutes to spare, I earnestly beg you to let me hear what has happened.

LETTER 750. TO A. STEPHEN WILSON.

(750/1. The following letters refer to two forms of wheat cultivated in Russia under the names Kubanka and Saxonka, which had been sent to Mr. Darwin by Dr. Asher from Samara, and were placed in the hands of Mr. Wilson that he might test the belief prevalent in Russia that Kubanka "grown repeatedly on inferior soil," assumes "the form of Saxonka." Mr. Wilson's paper of 1880 gives the results of his inquiry. He concludes (basing his views partly on analogous cases and partly on his study of the Russian wheats) that the supposed transformation is explicable in chief part by the greater fertility of the Saxonka wheat leading to extermination of the other form. According to Mr. Wilson, therefore, the Saxonka survivors are incorrectly assumed to be the result of the conversion of one form into the other.)

Down, April 24th, 1878.

I send you herewith some specimens which may perhaps interest you, as you have so carefully studied the varieties of wheat. Anyhow, they are of no use to me, as I have neither knowledge nor time sufficient. They were sent me by the Governor of the Province of Samara, in Russia, at the request of Dr. Asher (son of the great Berlin publisher) who farmed for some years in the province. The specimen marked Kubanka is a very valuable kind, but which keeps true only when cultivated in fresh steppe-land in Samara, and in Saratoff. After two years it degenerates into the variety Saxonica, or its synonym Ghirca. The latter alone is imported into this country. Dr. Asher says that it is universally known, and he has himself witnessed the fact, that if grain of the Kubanka is sown in the same steppe-land for more than two years it changes into Saxonica. He has seen a field with parts still Kubanka and the remainder Saxonica. On this account the Government, in letting steppe-land, contracts that after two years wheat must not be sown until an interval of eight years. The ears of the two kinds appear different, as you will see, but the chief difference is in the quality of the grains. Dr. Asher has witnessed sales of equal weights of Kubanka and Saxonica grain, and the price of the former was to that of the latter as 7 to 4. The peasants say that the change commences in the terminal grain of the ear. The most remarkable point, as Dr. Asher positively asserts, is that there are no intermediate varieties; but that a grain produces a plant yielding either true Kubanka or true Saxonica. He thinks that it would be interesting to sow here both kinds in good and bad wheat soil and observe the result. Should you think it worth while to make any such trial, and should you require further information, Dr. Asher, whose address I enclose, will be happy to give any in his power.

LETTER 751. TO A. STEPHEN WILSON. Basset, Southampton, April 29th {1878}.

Your kind note and specimens have been forwarded to me here, where I am staying at my son's house for a fortnight's complete rest, which I required from rather too hard work. For this reason I will not now examine the seeds, but will wait till returning home, when, with my son Francis' aid, I will look to them.

I always felt, though without any good reason, rather sceptical about Prof. Buckman's experiment, and I afterwards heard that a most wicked and cruel trick had been played on him by some of the agricultural students at Cirencester, who had sown seeds unknown to him in his experimental beds. Whether he ever knew this I did not hear.

I am exceedingly glad that you are willing to look into the Russian wheat case. It may turn out a mare's nest, but I have often incidentally observed curious facts when making what I call "a fool's experiment."

LETTER 752. TO A. STEPHEN WILSON. Down, March 5th, 1879.

I have just returned home after an absence of a week, and your letter was not forwarded to me; I mention this to account for my apparent discourtesy in not having sooner thanked you. You have worked out the subject with admirable care and clearness, and your drawings are beautiful. I suspected that there was some error in the Russian belief, but I did not think of the explanation which you have almost proved to be the true one. It is an extremely interesting instance of a more fertile variety beating out a less fertile one, and, in this case, one much more valuable to man. With respect to publication, I am at a loss to advise you, for I live a secluded life and do not see many periodicals, or hear what is done at the various societies. It seems to me that your paper should be published in some agricultural journal; for it is not simply scientific, and would therefore not be published by the Linnean or Royal Societies.

Would the Royal Agricultural Society be a fitting place? Unfortunately I am not a member, and could not myself present it. Unless you think of some better journal, there is the "Agricultural Gazette": I have occasionally suggested articles for publication to the editor (though personally unknown to me) which he has always accepted.

Permit me again to thank you for the thorough manner in which you have worked out this case; to kill an error is as good a service as, and sometimes even better than, the establishing a new truth or fact.

LETTER 753. TO A. STEPHEN WILSON. Down, February 13th, 1880.

It was very kind of you to send me two numbers of the "Gardeners' Chronicle" with your two articles, which I have read with much interest. (753/1. "Gardeners' Chronicle," 1879, page 652; 1880, pages 108, 173.) You have quite convinced me, whatever Mr. Asher may say to the contrary. I want to ask you a question, on the bare chance of your being able to answer it, but if you cannot, please do not take the trouble to write. The lateral branches of the silver fir often grow out into knobs through the action of a fungus, Aecidium; and from these knobs shoots grow vertically (753/2. The well-known "Witches-Brooms," or "Hexen-Besen," produced by the fungus Aecidium elatinum.) instead of horizontally, like all the other twigs on the same branch. Now the roots of Cruciferae and probably other plants are said to become knobbed through the action of a fungus: now, do these knobs give rise to rootlets? and, if so, do they grow in a new or abnormal direction? (753/3. The parasite is probably Plasmodiophora: in this case no abnormal rootlets have been observed, as far as we know.)

LETTER 754. TO W. THISELTON-DYER. Down, June 18th, 1879.

The plants arrived last night in first-rate order, and it was very very good of you to take so much trouble as to hunt them up yourself. They seem exactly what I wanted, and if I fail it will not be for want of perfect materials. But a confounded painter (I beg his pardon) comes here to-night, and for the next two days I shall be half dead with sitting to him; but after then I will begin to work at the plants and see what I can do, and very curious I am about the results.

I have to thank you for two very interesting letters. I am delighted to hear, and with surprise, that you care about old Erasmus D. God only knows what I shall make of his life—it is such new kind of work to me. (754/1. "Erasmus Darwin." By Ernst Krause. Translated from the German by W.S. Dallas: with a preliminary notice by Charles Darwin. London, 1879. See "Life and Letters," III., pages 218-20.)

Thanks for case of sleeping Crotalaria—new to me. I quite agree to every word you say about Ball's lecture (754/2. "On the Origin of the Flora of the European Alps," "Geogr. Soc. Proc." Volume I., 1879, page 564. See Letter 395, Volume II.)—it is, as you say, like Sir W. Thomson's meteorite. (754/3. In 1871 Lord Kelvin (Presidential Address Brit. Assoc.) suggested that meteorites, "the moss-grown fragments from the ruins of another world," might have introduced life to our planet.) It is really a pity; it is enough to make Geographical Distribution ridiculous in the eyes of the world. Frank will be interested about the Auriculas; I never attended to this plant, for the powder did {not} seem to me like true "bloom." (754/4. See Francis Darwin, on the relation between "bloom" on leaves and the distribution of the stomata. "Linn. Soc. Journ." Volume XXII., page

114.) This subject, however, for the present only, has gone to the dogs with me.

I am sorry to hear of such a struggle for existence at Kew; but I have often wondered how it is that you are all not killed outright.

I can most fully sympathise with you in your admiration of your little girl. There is nothing so charming in this world, and we all in this house humbly adore our grandchild, and think his little pimple of a nose quite beautiful.

LETTER 755. TO G. BENTHAM. Down, February 16th, 1880.

I have had real pleasure in signing Dyer's certificate. (755/1. As a candidate for the Royal Society.) It was very kind in you to write to me about the Orchideae, for it has pleased me to an extreme degree that I could have been of the least use to you about the nature of the parts. They are wonderful creatures, these orchids, and I sometimes think with a glow of pleasure, when I remember making out some little point in their method of fertilisation. (755/2. Published in "Life and Letters," III., page 288.) With respect to terms, no doubt you will be able to improve them greatly, for I knew nothing about the terms as used in other groups of plants. Could you not invent some quite new term for gland, implying viscidity? or append some word to gland. I used for cirripedes "cement gland."

Your present work must be frightfully difficult. I looked at a few dried flowers, and could make neither heads nor tails of them; and I well remember wondering what you would do with them when you came to the group in the "Genera Plantarum." I heartily wish you safe through your work,...

LETTER 756. TO F.M. BALFOUR. Down, September 4th, 1880.

I hope that you will not think me a great bore, but I have this minute finished reading your address at the British Association; and it has interested me so much that I cannot resist thanking you heartily for the pleasure derived from it, not to mention the honour which you have done me. (756/1. Presidential address delivered by Prof. F.M. Balfour before the Biological Section at the British Association meeting at Swansea (1880).) The recent progress of embryology is indeed splendid. I have been very stupid not to have hitherto read your book, but I have had of late no spare time; I have now ordered it, and your address will make it the more interesting to read, though I fear that my want of knowledge will make parts unintelligible to me. (756/2. "A Treatise on Comparative Embryology," 2 volumes. London, 1880.) In my recent work on plants I have been astonished to find to how many very different stimuli the same small part—viz., the tip of the radicle—is sensitive, and has the power of transmitting some influence to the adjoining part of the radicle, exciting it to bend to or from the source of irritation according to the needs of the plant (756/3. See Letter 757.); and all this takes place without any nervous system! I think that such facts should be kept in mind when speculating on the genesis of the nervous system. I always feel a malicious pleasure when a priori conclusions are knocked on the head: and therefore I felt somewhat like a devil when I read your remarks on Herbert Spencer (756/4. Prof. Balfour discussed Mr. Herbert Spencer's views on the genesis of the nervous system, and expressed the opinion that his hypothesis was not borne out by recent discoveries. "The discovery that nerves have been developed from processes of epithelial cells gives a very different conception of their genesis to that of Herbert Spencer, which makes them originate from the passage of nervous impulses through a track of mingled colloids..." (loc. cit., page 644.))...Our recent visit to Cambridge was a brilliant success to us all, and will ever be remembered by me with much pleasure.

LETTER 757. TO JAMES PAGET.

(757/1. During the closing years of his life, Darwin began to experimentise on the possibility of producing galls artificially. A letter to Sir J.D. Hooker (November 3rd, 1880) shows the interest which he felt in the question:—

"I was delighted with Paget's essay (757/2. An address on "Elemental Pathology," delivered before the British Medical Association, August 1880, and published in the Journal of the Association.); I hear that he has occasionally attended to this subject from his youth...I am very glad he has called attention to galls: this has always seemed to me a profoundly interesting subject; and if I had been younger would take it up."

His interest in this subject was connected with his ever-present wish to learn something of the causes of variation. He imagined to himself wonderful galls caused to appear on the ovaries of plants, and by these means he thought it possible that the seed might be influenced, and thus new varieties arise. (757/3. There would have been great difficulties about this line of research, for when the sexual organs of plants are deformed by parasites (in the way he hoped to effect by poisons) sterility almost always results. See Molliard's "Les Cecidies Florales," "Ann. Sci. Nat." 1895, Volume I., page 228.) He made a considerable number of experiments by injecting various reagents into the tissues of leaves, and with some slight indications of success. (757/4. The above passage is reprinted, with alterations, from "Life and Letters," III., page 346.)

The following letter to the late Sir James Paget refers to the same subject.)

Down, November 14th, 1880.

I am very much obliged for your essay, which has interested me greatly. What indomitable activity you have! It is a surprising thought that the diseases of plants should illustrate human pathology. I have the German "Encyclopaedia," and a few weeks ago told my son Francis that the article on the diseases of plants would be well worth his study; but I did not know it was written by Dr. Frank, for whom I entertain a high respect as a first-rate observer and experimentiser, though for some unknown reason he has been a good deal snubbed in Germany. I can give you one good case of regrowth in plants, recently often observed by me, though only externally, as I do not know enough of histology to follow out details. It is the tip of the radicle of a germinating common bean. The case is remarkable in some respects, for the tip is sensitive to various stimuli, and transmits an order, causing the upper part of the radicle to bend. When the tip (for a length of about 1 mm.) is cut transversely off, the radicle is not acted on by gravitation or other irritants, such as contact, etc., etc., but a new tip is regenerated in from two to four days, and then the radicle is again acted on by gravitation, and will bend to the centre of the earth. The tip of the radicle is a kind of brain to the whole growing part of the radicle! (757/5. We are indebted to Mr. Archer-Hind for the translation of the following passage from Plato ("Timaeus," 90A): "The reason is every man's guardian genius (daimon), and has its habitation in our brain; it is this that raises man (who is a plant, not of earth but of heaven) to an erect posture, suspending the head and root of us from the heavens, which are the birthplace of our soul, and keeping all the body upright." On the perceptions of plants, see "Nature," November 14th, 1901—a lecture delivered at the Glasgow meeting of the British Association by Francis Darwin. See also Bonitz, "Index Aristotelicus," S.V. phuton.)

My observation will be published in about a week's time, and I would have sent you the book, but I do not suppose that there is anything else in the book which would interest you. I am delighted that you have drawn attention to galls. They have always seemed to me profoundly interesting. Many years ago I began (but failed for want of time, strength, and health, as on infinitely many other occasions) to experimentise on plants, by injecting into their tissues some alkaloids and the poison of wasps, to see if I could make anything like galls. If I remember rightly, in a few cases the tissues were thickened and hardened. I began these experiments because if by different poisons I could have affected slightly and differently the tissues of the same plant, I thought there would be no insuperable difficulty in the fittest poisons being developed by insects so as to produce galls adapted for them. Every character, as far as I can see, is apt to vary. Judging from one of your sentences you will smile at this.

To any one believing in my pangenesis (if such a man exists) there does not seem to me any extreme difficulty in understanding why plants have such little power of regeneration; for there is reason to think that my imaginary gemmules have small power of passing from cell to cell. (757/6. On regeneration after injury, see Massart, "La Cicatrisation chez les Vegetaux," in Volume 57 (1898) of the "Memoires Couronnes," published by the Royal Academy of Belgium. An account of the literature is given by the author.)

Forgive me for scribbling at such unreasonable length; but you are to blame for having interested me so much.

P.S.—Perhaps you may remember that some two years ago you asked me to lunch with you, and proposed that I should offer myself again. Whenever I next come to London, I will do so, and thus have the pleasure of seeing you.

LETTER 758. TO W. THISELTON-DYER.

(758/1. "The Power of Movement in Plants" was published early in November, 1880. Sir W. Thiselton-Dyer, in writing to thank Darwin for a copy of the book, had (November 20th) compared a structure in the seedling Welwitschia with the "peg" of Cucurbita (see "Power of Movement," page 102). Dyer wrote: "One peculiar feature in the germinating embryo is a lateral hypocotyledonary process, which eventually serves as an absorbent organ, by which the nutriment of the endosperm is conveyed to the seedling. Such a structure was quite new to me, and Bower and I were disposed to see in it a representative of the foot in Selaginella, when I saw the account of Flahault's 'peg.'" Flahault, it should be explained, was the discoverer of the curious peg in Cucurbita. Prof. Bower wrote a paper ("On the Germination and Histology of the seedling of Welwitschia mirabilis" in the "Quart. Journ. Microscop. Sci." XXI., 1881, page 15.)

Down, November 28th {1880}.

Very many thanks for your most kind note, but you think too highly of our work—not but what this is very pleasant.

I am deeply interested about Welwitschia. When at work on the pegs or projections I could not imagine how they were first developed, before they could have been of mere mechanical use. Now it seems possible that a circle between radicle and hypocotyl may be permeable to fluids, and thus have given rise to projections so as to expose larger surface. Could you test Welwitschia with permanganate of potassium: if, like my pegs, the lower surface would be coloured brown like radicle, and upper surface left white like hypocotyl. If such an idea as yours, of an absorbing organ, had ever crossed my mind, I would have tried many hypocotyls in weak citrate of ammonia, to see if it penetrated on line of junction more easily than elsewhere. I daresay the projection in Abronia and Mirabilis may be an absorbent organ. It was very good fun bothering the seeds of Cucurbita by planting them edgeways, as would never naturally occur, and then the peg could not act properly. Many of the Germans are very contemptuous about making out use of organs; but they may sneer the souls out of their bodies, and I for one shall think it the most interesting part of natural history. Indeed, you are greatly mistaken if you doubt for one moment on the very great value of your constant and most kind assistance to us. I have not seen the pamphlet, and shall be very glad to keep it. Frank, when he comes home, will be much interested and pleased with your letter. Pray give my kindest remembrance to Mrs. Dyer.

This is a very untidy note, but I am very tired with dissecting worms all day. Read the last chapter of our book, and then you will know the whole contents.

LETTER 759. TO H. VOCHTING. Down, December 16th, 1880.

Absence from home has prevented me from sooner thanking you for your kind present of your several publications. I procured some time ago your "Organbilding" (759/1. "Organbildung im Pflanzenreich," 1878.) etc., but it was too late for me to profit by it for my book, as I was correcting the press. I read only parts, but my son Francis read the whole with care and told me much about it, which greatly interested me. I also read your article in the "Bot. Zeitung." My son began at once experimenting, to test your views, and this very night will read a paper before the Linnean Society on the roots of Rubus (759/2. Francis Darwin, "The Theory of the Growth of Cuttings" ("Linn. Soc. Journ." XVIII.). {I take this opportunity of expressing my regret that at page 417, owing to neglect of part of Vochting's facts, I made a criticism of his argument which cannot be upheld.—F.D.}.), and I think that you will be pleased to find how well his conclusions agree with yours. He will of course send you a copy of his paper when it is printed. I have sent him your letter, which will please him if he agrees with me; for your letter has given me real pleasure, and I did not at all know what the many great physiologists of Germany, Switzerland, and Holland would think of it {"The Power of Movement," etc.}. I was quite sorry to read Sachs' views about root-forming matter, etc., for I have an unbounded admiration for Sachs. In this country we are dreadfully behind in Physiological Botany.

LETTER 760. TO A. DE CANDOLLE. Down, January 24th, 1881.

It was extremely kind of you to write me so long and valuable a letter, the whole of which deserves careful consideration. I have been particularly pleased at what you say about the new terms used, because I have often been annoyed at the multitude of new terms lately invented in all branches of Biology in Germany; and I doubted much whether I was not quite as great a sinner as those whom I have blamed. When I read your remarks on the word "purpose" in your "Phytographie," I vowed that I would not use it again; but it is not easy to cure oneself of a vicious habit. It is also difficult for any one who tries to make out the use of a structure to avoid the word purpose. I see that I have probably gone beyond my depth in discussing plurifoliate and unifoliate leaves; but in such a case as that of Mimosa albida, where rudiments of additional leaflets are present, we must believe that they were well developed in the progenitor of the plant. So again, when the first true leaf differs widely in shape from the older leaves, and resembles the older leaves in allied species, is it not the most simple explanation that such leaves have retained their ancient character, as in the case of the embryos of so many animals?

Your suggestion of examining the movements of vertical leaves with an equal number of stomata on both sides, with reference to the light, seems to me an excellent one, and I hope that my son Francis may follow it up. But I will not trouble you with any more remarks about our book. My son will write to you about the diagram.

Let me add that I shall ever remember with pleasure your visit here last autumn.

LETTER 761. TO J. LUBBOCK (Lord Avebury). Down, April 16th {1881}.

Will you be so kind as to send and lend me the Desmodium gyrans by the bearer who brings this note.

Shortly after you left I found my notice of the seeds in the "Gardeners' Chronicle," which please return hereafter, as I have no other copy. (761/1. "Note on the Achenia of Pumilio argyrolepis." "Gardeners' Chronicle," 1861, page 4.) I do not think that I made enough about the great power of absorption of water by the corolla-like calyx or pappus. It seems to me not unlikely that the pappus of other Compositae may be serviceable to the seeds, whilst lying on the ground, by absorbing the dew which would be especially apt to condense on the fine points and filaments of the pappus. Anyhow, this is a point which might be easily investigated. Seeds of Tussilago, or groundsel (761/2. It is not clear whether Tussilago or groundsel (Senecio vulgaris) is meant; or whether he was not sure which of the two plants becomes slimy when wetted.), emit worm-like masses of mucus, and it would be curious to ascertain whether wetting the pappus alone would suffice to cause such secretion. (761/3. See Letter 707.)

LETTER 762. TO G.J. ROMANES. Down, April 18th, 1881.

I am extremely glad of your success with the flashing light. (762/1. Romanes' paper on the effect of intermittent light on heliotropism was the "Proc. Royal Soc." Volume LIV., page 333.) If plants are acted on by light, like some of the lower animals, there is an additional point of interest, as it seems to me, in your results. Most botanists believe that light causes a plant to bend to it in as direct a manner as light affects nitrate of silver. I believe that it merely tells the plant to which side to bend, and I see indications of this belief prevailing even with Sachs. Now it might be expected that light would act on a plant in something the same manner as on the lower animals. As you are at work on this subject, I will call your attention to another point. Wiesner, of Vienna (who has lately published a great book on heliotropism) finds that an intermittent light, say of 20 minutes, produces the same effect as a continuous light of, say 60 m. (762/2. Wiesner's papers on heliotropism are in the "Denkschriften" of the Vienna Academy, Volumes 39 and 43.) So that Van Tieghem, in the first part of his book which has just appeared, remarks, the light during 40 m. out of the 60 m. produced no effect. I observed an analogous case described in my book. (762/3. "Power of Movement," page 459.)

Wiesner and Van Tieghem seem to think that this is explained by calling the whole process "induction," borrowing a term used by some physico-chemists (of whom I believe Roscoe is one) and implying an agency which does not produce any effect for some time, and continues its effect for some time after the cause has ceased. I believe that photographic paper is an instance. I must ask Leonard (762/4. Mr. Darwin's son.) whether an interrupted light acts on it in the same manner as on a plant. At present I must still believe in my explanation that it is the contrast between light and darkness which excites a plant.

I have forgotten my main object in writing—viz., to say that I believe (and have so stated) that seedlings vary much in their sensitiveness to light; but I did not prove this, for there are many difficulties, whether the time of incipient curvature or the amount of curvature is taken as the criterion. Moreover they vary according to age, and perhaps from vigour of growth, and there seems inherent variability, as Strasburger (whom I quote) found with spores. If the curious anomaly observed by you is due to varying sensitiveness, ought not all the seedlings to bend if the flashes were at longer intervals of time? According to my notion of contrast between light and darkness being the stimulus, I should expect that if flashes were made sufficiently slow it would be a powerful stimulus, and that you would suddenly arrive at a period when the result would SUDDENLY become great. On the other hand, as far as my experience goes, what one expects rarely happens.

LETTER 763. TO JULIUS WIESNER. Down, October 4th, 1881.

I thank you sincerely for your very kind letter, and for the present of your new work. (763/1. "Das Bewegungsvermogen der Pflanze," 1881. One of us has given some account of Wiesner's book in the presidential address to Section D of the British Association, 1891. Wiesner's divergence from Darwin's views is far-reaching, and includes the main thesis of the "Power of Movement." See "Life and Letters," III., page 336, for an interesting letter to Wiesner.) My son Francis, if he had been at home, would have likewise sent his thanks. I will immediately begin to read your book, and when I have finished it will write again. But I read german so very slowly that your book will take me a considerable time, for I cannot read for more than half an hour each day. I have, also, been working too hard lately, and with very little success, so that I am going to leave home for a time and try to forget science.

I quite expect that you will find some gross errors in my work, for you are a very much more skilful and profound experimentalist than I am. Although I always am endeavouring to be cautious and to mistrust myself, yet I know well how apt I am to make blunders. Physiology, both animal and vegetable, is so difficult a subject, that it seems to me to progress chiefly by the elimination or correction of ever-recurring mistakes. I hope that you will not have upset my fundamental notion that various classes of movement result from the modification of a universally present movement of circumnutation.

I am very glad that you will again discuss the view of the turgescence of the cells being the cause of the movement of parts. I adopted De Vries' views as seeming to me the most probable, but of late I have felt more doubts on this head. (763/2. See "Power of Movement," page 2. De Vries' work is published in the "Bot. Zeitung," 1879, page 830.)

LETTER 764. TO J.D. HOOKER. Glenrhydding House, Patterdale, Penrith, June 15th, 1881.

It was real pleasure to me to see once again your well-known handwriting on the outside of your note. I do not know how long you have returned from Italy, but I am very sorry that you are so bothered already with work and visits. I cannot but think that you are too kind and civil to visitors, and too conscientious about your official work. But a man cannot cure his virtues, any more than his vices, after early youth; so you must bear your burthen. It is, however, a great misfortune for science that you have so very little spare time for the "Genera." I can well believe what an awful job the palms must be. Even their size must be very inconvenient. You and Bentham must hate the monocotyledons, for what work the Orchideae must have been, and Gramineae and Cyperaceae will be. I am rather despondent about myself, and my troubles are of an exactly opposite nature to yours, for idleness is downright misery to me, as I find here, as I cannot forget my discomfort for an hour. I have not the heart or strength at my age to begin any investigation lasting years, which is the only thing which I enjoy; and I have no little jobs which I can do. So I must look forward to Down graveyard as the sweetest place on earth. This place is magnificently beautiful, and I enjoy the scenery, though weary of it; and the weather has been very cold and almost always hazy.

I am so glad that your tour has answered for Lady Hooker. We return home on the first week of July, and should be truly glad to aid Lady Hooker in any possible manner which she will suggest.

I have written to my gardener to send you plants of Oxalis corniculata (and seeds if possible). I should think so common a weed was never asked for before,—and what a poor return for the hundreds of plants which I have received from Kew! I hope that I have not bothered you by writing so long a note, and I did not intend to do so.

If Asa Gray has returned with you, please give him my kindest remembrances.

LETTER 765. TO J.D. HOOKER. October 22nd, 1881.

I am investigating the action of carbonate of ammonia on chlorophyll, which makes me want the plants in my list. (765/1. "The Action of Carbonate of Ammonia on Chlorophyll Bodies." "Linn. Soc. Journ." XIX., page 262, 1882.) I have incidentally observed one point in Euphorbia, which has astonished me—viz. that in the fine fibrous roots of Euphorbia, the alternate rows of cells in their roots must differ physiologically, though not in external appearance, as their contents after the action of carbonate of ammonia differ most conspicuously...

Wiesner of Vienna has just published a book vivisecting me in the most courteous, but awful manner, about the "Power of Movement in Plants." (765/2. See Letter 763, note.) Thank heaven, he admits almost all my facts, after re-trying all my experiments; but gives widely different interpretation of the facts. I think he proves me wrong in several cases, but I am convinced that he is utterly erroneous and fanciful in other explanations. No man was ever vivisected in so sweet a manner before, as I am in this book.

CHAPTER 2.XII.

VIVISECTION AND MISCELLANEOUS SUBJECTS, 1867-1882.

2.XII.I. VIVISECTION, 1875-1882. LETTER 766. TO LORD PLAYFAIR.

(766/1. A Bill was introduced to the House of Commons by Messrs. Lyon Playfair, Walpole and Ashley, in the spring of 1875, but was withdrawn on the appointment of a Royal Commission to inquire into the whole question. Some account of the Anti-Vivisection agitation, the introduction of bills, and the appointment of a Royal Commission is given in the "Life and Letters," III., page 201, where the more interesting of Darwin's letters on the question are published.)

Down, May 26th, 1875.

I hope that you will excuse my troubling you once again. I received some days ago a letter from Prof. Huxley, in Edinburgh, who says with respect to your Bill: "the professors here are all in arms about it, and as the papers have associated my name with the Bill, I shall have to repudiate it publicly, unless something can be done. But what in the world is to be done?" (766/2. The letter is published in full in Mr. L. Huxley's interesting chapter on the vivisection question in his father's "Life," I., page 438.) Dr. Burdon Sanderson is in nearly the same frame of mind about it. The newspapers take different views of the purport of the Bill, but it seems generally supposed that it would prevent demonstrations on animals rendered insensible, and this seems to me a monstrous provision. It would, moreover, probably defeat the end desired; for Dr. B. Sanderson, who demonstrates to his class on animals rendered insensible, told me that some of his students had declared to him that unless he had shown them what he had, they would have experimented on live animals for themselves. Certainly I do not believe that any one could thoroughly understand the action of the heart without having seen it in action. I do not doubt that you wish to aid the progress of Physiology, and at the same time save animals from all useless suffering; and in this case I believe that you could not do a greater service than to warn the Home Secretary with respect to the appointment of Royal Commissioners, that ordinary doctors know little or nothing about Physiology as a science, and are incompetent to judge of its high importance and of the probability of its hereafter conferring great benefits on mankind.

LETTER 767. TO LORD PLAYFAIR. Down, May 28th.

I must write one line to thank you for your very kind letter, and to say that, after despatching my last note, it suddenly occurred to me that I had been rude in calling one of the provisions of your Bill "monstrous" or "absurd"—I forget which. But when I wrote the expression it was addressed to the bigots who, I believed, had forced you to a compromise. I cannot understand what Dr. B. Sanderson could have been about not to have objected with respect to the clause of not demonstrating on animals rendered insensible. I am extremely sorry that you have had trouble and vexation on the subject. It is a most disagreeable and difficult one. I am not personally concerned, as I never tried an experiment on a living animal, nor am I a physiologist; but I know enough to see how ruinous it would be to stop all progress in so grand a science as Physiology. I commenced the agitation amongst the physiologists for this reason, and because I have long felt very keenly on the question of useless vivisection, and believed, though without any good evidence, that there was not always, even in this country, care enough taken. Pray forgive me this note, so much about myself...

LETTER 768. TO G.J. ROMANES.

(768/1. Published in "Life of Romanes," page 61, under 1876-77.)

Down, June 4th {1876}.

Your letter has made me as proud and conceited as ten peacocks. (768/2. This may perhaps refer to Darwin being elected the only honorary member of the Physiological Society, a fact that was announced in a letter from Romanes June 1st, 1876, published in the "Life" of Romanes, page 50. Dr. Sharpey was subsequently elected a second honorary member.) I am inclined to think that writing against the bigots about vivisection is as hopeless as stemming a torrent with a reed. Frank, who has just come here, and who sputters with indignation on the subject, takes an opposite line, and perhaps he is right; anyhow, he had the best of an argument with me on the subject...It seems to me the physiologists are now in the position of a persecuted religious sect, and they must grin and bear the persecution, however cruel and unjust, as well as they can.

LETTER 769. TO T. LAUDER BRUNTON.

(769/1. In November, 1881, an absolutely groundless charge was brought by the Victoria Street Society for the Protection of Animals from Vivisection against Dr. Ferrier for an infringement of the Vivisection Act. The experiment complained of was the removal of the brain of a monkey and the subsequent testing of the animal's powers of reacting to certain treatment. The fact that the operation had been performed six months before the case came into court would alone have been fatal to the prosecution. Moreover, it was not performed by Dr. Ferrier, but by another observer, who was licensed under the Act to keep the monkey alive after the operation, which was performed under anaesthetics. Thus the prosecution completely broke down, and the case was dismissed. (769/2. From the "British Medical Journal," November 19th, 1881. See also "Times," November 18th, 1881.) The sympathy with Dr. Ferrier in the purely scientific and medical world was very strong, and the British Medical Association undertook the defence. The prosecution did good in one respect, inasmuch as it led to the formation of the Science Defence Association, to which reference is made in some of Mr. Darwin's letters to Sir Lauder Brunton. The Association still exists, and continues to do good work.

Part of the following letter was published in the "British Medical Journal," December 3rd, 1881.)

Down, November 19th, 1881.

I saw in some paper that there would probably be a subscription to pay Dr. Ferrier's legal expenses in the late absurd and wicked prosecution. As I live so retired I might not hear of the subscription, and I should regret beyond measure not to have the pleasure and honour of showing my sympathy {with} and admiration of Dr. Ferrier's researches. I know that you are his friend, as I once met him at your house; so I earnestly beg you to let me hear if there is any means of subscribing, as I should much like to be an early subscriber. I am sure that you will forgive me for troubling you under these circumstances.

P.S.—I finished reading a few days ago the several physiological and medical papers which you were so kind as to send me. (769/3. Some of Lauder Brunton's publications.) I was much interested by several of them, especially by that on night-sweating, and almost more by others on digestion. I have seldom been made to realise more vividly the wondrous complexity of our whole system. How any one of us keeps alive for a day is a marvel!

LETTER 770. T. LAUDER BRUNTON TO CHARLES DARWIN. 50, Welbeck Street, London, November 21st, 1881.

I thank you most sincerely for your kind letter and your offer of assistance to Dr. Ferrier. There is at present no subscription list, as the British Medical Association have taken up the case, and ought to pay the expenses. Should these make such a call upon the funds of the Association as to interfere with its other objects, the whole or part of the expenses will be paid by those who have subscribed to a guarantee fund. To this fund there are already a number of subscribers, whose names are taken by Professor Gerald Yeo, one of the secretaries of the Physiological Society. They have not subscribed a definite sum, but have simply fixed a maximum which they will subscribe, if necessary, on the understanding that only so much as is required shall be asked from each subscriber in proportion to his subscription. It is proposed to send by-and-by a list of the most prominent members of this guarantee fund to the "Times" and other papers, and not only every scientific man, but every member of the medical profession, will rejoice to see your name in the list. Dr. Ferrier has been quite worn out by the worry of this prosecution, or, as it might well be called, persecution, and has gone down to Shanklin for a couple of days. He returns this afternoon, and I have sent on your letter to await his arrival, knowing as I do that it will be to him like cold water to a thirsty soul.

LETTER 771. TO T. LAUDER BRUNTON. Down, November 22nd, 1881.

Many thanks for your very kind and interesting letter...

I write now to beg a favour. I do not in the least know what others have guaranteed in relation to Dr. Ferrier. (771/1. In a letter dated November 27th, 1881, Sir Lauder Brunton wrote in reply to Mr. Darwin's inquiry as to the amount of the subscriptions: "When I ascertain what they intend to give under the new conditions—viz., that the subscriptions are not to be applied to Ferrier's defence, but to the defence of others who may be attacked and to a diffusion of knowledge regarding the nature and purposes of vivisection, I will let you know...") Would twenty guineas be sufficient? If not, will you kindly take the trouble to have my name put down for thirty or forty guineas, as you may think best. If, on the other hand, no one else has guaranteed for as much as twenty guineas, will you put me down for ten or fifteen guineas, though I should like to give twenty best.

You can understand that I do not wish to be conspicuous either by too little or too much; so I beg you to be so very kind as to act for me. I have a multitude of letters which I must answer, so excuse haste.

LETTER 772. TO T. LAUDER BRUNTON.

(772/1. The following letter was written in reply to Sir T. Lauder Brunton's suggestion that Mr. Darwin should be proposed as President of the Science Defence Association.)

4, Bryanston Street, Portman Square, December 17th, 1881.

I have been thinking a good deal about the suggestion which you made to me the other day, on the supposition that you could not get some man like the President of the College of Physicians to accept the office. My wife is strongly opposed to my accepting the office, as she feels sure that the anxiety thus caused would tell heavily on my health. But there is a much stronger objection suggested to me by one of my relations—namely, no man ought to allow himself to be placed at the head (though only nominally so) of an associated movement, unless he has the means of judging of the acts performed by the association, after hearing each point discussed. This occurred to me when you spoke to me, and I think that I said something to this effect. Anyhow, I have in several analogous cases acted on this principle.

Take, for instance, any preliminary statement which the Association may publish. I might feel grave doubts about the wisdom or justice of some points, and this solely from my not having heard them discussed. I am therefore inclined to think that it would not be right in me to accept the nominal Presidency of your Association, and thus have to act blindly.

As far as I can at present see, I fear that I must confine my assistance to subscribing as large a sum to the Association as any member gives.

I am sorry to trouble you, but I have thought it best to tell you at once of the doubts which have arisen in my mind.

LETTER 773. TO LAUDER BRUNTON.

(773/1. Sir T. Lauder Brunton had written (February 12th) to Mr. Darwin explaining that two opinions were held as to the constitution of the proposed Science Defence Association: one that it should consist of a small number of representative men; the other that it should, if possible, embrace every medical practitioner in the country. Sir Lauder Brunton adds: "I should be very greatly obliged if you would kindly say what you think of the two schemes.")

Down, February 14th, 1882.

I am very much obliged for your information in regard to the Association, about which I feel a great interest. It seems to me highly desirable that the Association should include as many medical and scientific men as possible throughout the whole country, who could illumine those capable of illumination on the necessity of physiological research; but that the Association should be governed by a council of powerful men, not too many in number. Such a council, as representing a large body of medical men, would have more power in the eyes of vote-hunting politicians than a small body representing only themselves.

From what I see of country practitioners, I think that their annual subscription ought to be very small. But would it not be possible to add to the rules some such statement as the following one: "That by a donation of... pounds, or of any larger sum, from those who feel a deep interest in the progress of medical science, the donor shall become a life member." I, for one, would gladly subscribe 50 or 100 pounds. If such a plan were approved by the leading medical men of London, two or three thousand pounds might at once be collected; and if any such sum could be announced as already subscribed, when the program of the Association is put forth, it would have, as I believe, a considerable influence on the country, and would attract the attention of country practitioners. The Anti-Corn Law League owed much of its enormous power to several wealthy men laying down 1,000 pounds; for the subscription of a good sum of money is the best proof of earnest conviction. You asked for my opinion on the above points, and I have given it freely, though well aware that from living so retired a life my judgment cannot be worth much.

Have you read Mr. Gurney's articles in the "Fortnightly" and "Cornhill?" (773/2. "Fortnightly Review," XXX., page 778; "Cornhill Magazine," XLV., page 191. The articles are by the late Edmund Gurney, author of "The power of Sound," 1880.) They seem to me very clever, though obscurely written; and I agree with almost everything he says, except with some passages which appear to imply that no experiments should be tried unless some immediate good can be predicted, and this is a gigantic mistake contradicted by the whole history of science.

P.S.—That is a curious fact about babies. I remember hearing on good authority that very young babies when moved are apt to clutch hold of anything, and I thought of your explanation; but your case during sleep is a much more interesting one. Very many thanks for the book, which I much wanted to see; it shall be sent back to-day, as from you, to the Society.

2.XII.II. MISCELLANEOUS SUBJECTS, 1867-1882. LETTER 774. TO CANON FARRAR.

(774/1. The lecture which forms the subject of this letter was one delivered by Canon Farrar at the Royal Institution, "On Some Defects in Public School Education.")

Down, March 5th, 1867.

I am very much obliged for your kind present of your lecture. We have read it aloud with the greatest interest, and I agree to every word. I admire your candour and wonderful freedom from prejudice; for I feel an inward conviction that if I had been a great classical scholar I should never have been able to have judged fairly on the subject. As it is, I am one of the root and branch men, and would leave classics to be learnt by those alone who have sufficient zeal and the high taste requisite for their appreciation. You have indeed done a great public service in speaking out so boldly. Scientific men might rail forever, and it would only be said that they railed at what they did not understand. I was at school at Shrewsbury under a great scholar, Dr. Butler; I learnt absolutely nothing, except by amusing myself by reading and experimenting in chemistry. Dr. Butler somehow found this out, and publicly sneered at me before the whole school for such gross waste of time; I remember he called me a Pococurante (774/2. Told in "Life and Letters," I., page 35.), which, not understanding, I thought was a dreadful name. I wish you had shown in your lecture how science could practically be taught in a great school; I have often heard it objected that this could not be done, and I never knew what to say in answer.

I heartily hope that you may live to see your zeal and labour produce good fruit.

LETTER 775. TO HERBERT SPENCER. Down, December 9th {1867}.

I thank you very sincerely for your kind present of your "First Principles." (775/1. "This must have been the second edition." (Note by Mr. Spencer.)) I earnestly hope that before long I may have strength to study the work as it ought to be studied, for I am certain to find or re-find much that is deeply interesting. In many parts of your "Principles of Biology" I was fairly astonished at the prodigality of your original views. (775/2. See "Life and Letters," III., pages 55, 56.) Most of the chapters furnished suggestions for whole volumes of future researches. As I have heard that you have changed your residence, I am forced to address this to Messrs. Williams & Norgate; and for the same reason I gave some time ago the same address to Mr. Murray for a copy of my book on variation, etc., which is now finished, but delayed by the index-maker.

LETTER 776. TO T.H. HUXLEY.

(776/1. This letter refers to a movement set on foot at a meeting held at the Freemasons' Tavern, on November 16th, 1872, of which an account is given in the "Times" of November 23rd, 1872, at which Mark Pattison, Mr. Henry Sidgwick, Sir Benjamin Brodie, Professors Rolleston, Seeley, Huxley, etc., were present. The "Times" says that the meeting was held "by members of the Universities and others interested in the promotion of mature study and scientific research in England." One of the headings of the "Program of Discussion" was "The Abolition of Prize Fellowships.")

Sevenoaks, October 22nd {1872}.

I have been glad to sign and forward the paper, for I have very long thought it a sin that the immense funds of the Universities should be wasted in Fellowships, except a few for paying for education. But when I was at Cambridge it would have been an unjustifiable sneer to have spoken of the place as one for education, always excepting the men who went in for honours. You speak of another resolution "in the interest of the anti-letter-writing association"—but alas, this never arrived! I should like a society formed so that every one might receive pleasant letters and never answer them.

We return home on Saturday, after three weeks of the most astounding dullness, doing nothing and thinking of nothing. I hope my Brain likes it—as for myself, it is dreadful doing nothing. (776/2. Darwin returned to Down from Sevenoaks on Saturday, October 26th, 1872, which fixes the date of the letter.)

LETTER 777. TO LADY DERBY. Down, Saturday {1874?}.

If you had called here after I had read the article you would have found a much perplexed man. (777/1. Probably Sir W. Crookes' "Researches in the Phenomena of Spiritualism" (reprinted from the "Quarterly Journal of Science"), London, 1874. Other papers by Crookes are in the "Proceedings of the Society for Psychical

Research.") I cannot disbelieve Mr. Crooke's statement, nor can I believe in his result. It has removed some of my difficulty that the supposed power is not an anomaly, but is common in a lesser degree to various persons. It is also a consolation to reflect that gravity acts at any distance, in some wholly unknown manner, and so may nerve-force. Nothing is so difficult to decide as where to draw a just line between scepticism and credulity. It was a very long time before scientific men would believe in the fall of aerolites; and this was chiefly owing to so much bad evidence, as in the present case, being mixed up with the good. All sorts of objects were said to have been seen falling from the sky. I very much hope that a number of men, such as Professor Stokes, will be induced to witness Mr. Crooke's experiments.

(778/1. The two following extracts may be given in further illustration of Darwin's guiding principle in weighing evidence. He wrote to Robert Chambers, April 30th, 1861: "Thanks also for extract out of newspaper about rooks and crows; I wish I dared trust it. I see in cutting the pages {of Chambers' book, "Ice and Water"}...that you fulminate against the scepticism of scientific men. You would not fulminate quite so much if you had had so many wild-goose chases after facts stated by men not trained to scientific accuracy. I often vow to myself that I will utterly disregard every statement made by any one who has not shown the world he can observe accurately." In a letter to Dr. Dohrn, of Naples, January 4th, 1870, Darwin wrote: "Forgive me for suggesting one caution; as Demosthenes said, 'Action, action, action,' was the soul of eloquence, so is caution almost the soul of science.")

LETTER 778. TO J. BURDON SANDERSON. Down, July 16th, 1875.

Some little time ago Mr. Simon (778/1. Now Sir John Simon) sent me the last Report, and your statements about contagion deeply interested me. By the way, if you see Mr. Simon, and can remember it, will you thank him for me; I was so busy at the time that I did not write. Having been in correspondence with Paget lately on another subject, I mentioned to him an analogy which has struck me much, now that we know that sheep-pox is fungoid; and this analogy pleased him. It is that of fairy rings, which are believed to spread from a centre, and when they intersect the intersecting portion dies out, as the mycelium cannot grow where it has grown during previous years. So, again, I have never seen a ring within a ring; this seems to me a parallel case to a man commonly having the smallpox only once. I imagine that in both cases the mycelium must consume all the matter on which it can subsist.

LETTER 779. TO A. GAPITCHE.

(779/1. The following letter was written to the author (under the pseudonym of Gapitche) of a pamphlet entitled "Quelques mots sur l'Eternite du Corps Humaine" (Nice, 1880). Mr. Gapitche's idea was that man might, by perfect adaptation to his surroundings, indefinitely prolong the duration of life. We owe Mr. Darwin's letter to the kindness of Herr Vetter, editor of the well-known journal "Kosmos.")

Down, February 24th, 1880.

I suppose that no one can prove that death is inevitable, but the evidence in favour of this belief is overwhelmingly strong from the evidence of all other living creatures. I do not believe that it is by any means invariably true that the higher organisms always live longer than the lower ones. Elephants, parrots, ravens, tortoises, and some fish live longer than man. As evolution depends on a long succession of generations, which implies death, it seems to me in the highest degree improbable that man should cease to follow the general law of evolution, and this would follow if he were to be immortal.

This is all that I can say.

LETTER 780. TO J. POPPER.

(780/1. Mr. Popper had written about a proposed flying machine in which birds were to take a part.)

Down, February 15th, 1881.

I am sorry to say that I cannot give you the least aid, as I have never attended to any mechanical subjects. I should doubt whether it would be possible to train birds to fly in a certain direction in a body, though I am aware that they have been taught some tricks. Their mental powers are probably much below those of mammals. It is said, and I suppose truly, that an eagle will carry a lamb. This shows that a bird may have great power for a short distance. I cannot remember your essay with sufficient distinctness to make any remarks on it. When a man is old and works hard, one subject drives another out of his head.

LETTER 781. TO T.H. HUXLEY. Worthing, September 9th, 1881.

(781/1. Mr. Anthony Rich left his house at Worthing as a legacy to Mr. Huxley. See Huxley's "Life and Letters," II., pages 286, 287.)

We have been paying Mr. Rich a little visit, and he has often spoken of you, and I think he enjoyed much your and Mrs. Huxley's visit here. But my object in writing now is to tell you something, which I am very doubtful whether it is worth while for you to hear, because it is uncertain. My brother Erasmus has left me half his fortune, which is very considerable. Therefore, I thought myself bound to tell Mr. Rich of this, stating the large amount, as far as the executors as yet know it roughly. I then added that my wife and self thought that, under these new circumstances, he was most fully justified in altering his will and leaving his property in some other way. I begged him to take a week to consider what I had told him, and then by letter to inform me of the result. But he would not, however, hardly allow me to finish what I had to say, and expressed a firm determination not to alter his will, adding that I had five sons to provide for. After a short pause he implied (but unfortunately he here became very confused and forgot a word, which on subsequent reflection I think was probably "reversionary")—he implied that there was a chance, whether good or bad I know not, of his becoming possessed of some other property, and he finished by saying distinctly, "I will bequeath this to Huxley." What the amount may be (I fear not large), and what the chance may be, God only knows; and one cannot cross-examine a man about his will. He did not bind me to secrecy, so I think I am justified in telling you what passed, but whether it is wise on my part to send so vague a story, I am not at all sure; but as a general rule it is best to tell everything. As I know that you hate writing letters, do not trouble yourself to answer this.

P.S.—On further reflection I should like to hear that you receive this note safely. I have used up all my black-edged paper.

LETTER 782. TO ANTHONY RICH. Down, February 4th, 1882.

It is always a pleasure to me to receive a letter from you. I am very sorry to hear that you have been more troubled than usual with your old complaint. Any one who looked at you would think that you had passed through life with few evils, and yet you have had an unusual amount of suffering. As a turnkey remarked in one of Dickens' novels, "Life is a rum thing." (782/1. This we take to be an incorrect version of Mr. Roker's remark (in reference to Tom Martin, the Butcher), "What a rum thing Time is, ain't it, Neddy?" ("Pickwick," Chapter XLII.). A careful student finds that women are also apostrophised as "rum": see the remarks of the dirty-faced man ("Pickwick," Chapter XIV.).) As for myself, I have been better than usual until about a fortnight ago, when I had a cough, and this pulled me down and made me miserable to a strange degree; but my dear old wife insisted on my taking quinine, and, though I have very little faith in medicine, this, I think, has done me much good. Well, we are both so old that we must expect some troubles: I shall be seventy-three on Feb. 12th. I have been glad to hear about the pine-leaves, and you are the first man who has confirmed my account that they are drawn in by the base, with a very few exceptions. (782/2. "The Formation of Vegetable Mould through the Action of Worms," 1881, page 71.) With respect to your Wandsworth case, I think that if I had heard of it before publishing, I would have said nothing about the ledges (782/3. "Ledges of Earth on Steep Hill-sides" (ibid., page 278).); for the Grisedale case (782/4. "The steep, grass-covered sides of a mountainous valley in Westmorland, called Grisedale, were marked in many places with innumerable, almost horizontal, little ledges...Their formation was in no way connected with the action of worms (and their absence is an inexplicable fact)...(ibid., page 282.), mentioned in my book and observed whilst I was correcting the proof-sheets, made me feel rather doubtful. Yet the Corniche case (782/5. Ibid., page 281.) shows that worms at least aid in making the ledges. Nevertheless, I wish I had said nothing about the confounded ledges. The success of this worm book has been almost laughable. I have, however, been plagued with an endless stream of letters on the subject; most of them very foolish and enthusiastic, but some containing good facts, which I have used in correcting yesterday the "sixth Thousand."

Your friend George's work about the viscous state of the earth and tides and the moon has lately been attracting much attention (782/6. Published in the "Philosophical Transactions of the Royal Society," 1879, 1880, 1881.), and all the great judges think highly of the work. He intends to try for the Plumian Professorship of Mathematics and Natural Philosophy at Cambridge, which is a good and honourable post of about 800 pounds a year. I think that he will get it (782/7. He was elected Plumian Professor of Astronomy and Experimental Philosophy in 1883.) when Challis is dead, and he is very near his end. He has all the great men—Sir W. Thomson, Adams, Stokes, etc.—on his side. He has lately been chief examiner for the Mathematical Tripos, which was tremendous work; and the day before yesterday he started for Southampton for a five-weeks' tour to Jamaica for complete rest, to see the Blue Mountains, and escape the rigour of the early spring. I believe that George will some day be a great scientific swell. The War Office has just offered Leonard a post in the Government Survey at Southampton, and very civilly told him to go down and inspect the place, and accept or not as he liked. So he went down, but has decided that it would not be worth his while to accept, as it would entail his giving up his expedition (on which he had been ordered) to Queensland, in Australia, to observe the Transit of Venus. (782/8. Major Leonard Darwin, late R.E., served in several scientific expeditions, including the Transits of Venus of 1874 and 1882.) Dear old William at Southampton has not been very well, but is now better. He has had too much work—a willing horse is always overworked—and all the arrangements for receiving the British Association there this summer have been thrown on his shoulders.

But, good Heavens! what a deal I have written about my sons. I have had some hard work this autumn with the microscope; but this is over, and I have only to write out the papers for the Linnean Society. (782/9. i. "The Action of Carbonate of Ammonia on the Roots of Certain plants." {Read March 16th, 1882.} "Journ. Linn. Soc." Volume XIX., 1882, page 239. ii. "The Action of Carbonate of Ammonia on Chlorophyll-bodies." {Read March 6th, 1882.} Ibid., page 262.) We have had a good many visitors; but none who would have interested you, except perhaps Mrs. Ritchie, the daughter of Thackeray, who is a most amusing and pleasant person. I have not seen Huxley for some time, but my wife heard this morning from Mrs. Huxley, who wrote from her bed, with a bad account of herself and several of her children; but none, I hope, are at all dangerously ill. Farewell, my kind, good friend.

Many thanks about the picture, which if I survive you, and this I do not expect, shall be hung in my study as a perpetual memento of you.

(782/10. The concluding chapter of the "Life and Letters" gives some account of the gradual failure in health which was perceptible in the last year of Mr. Darwin's life. He died on April 19th, 1882, in his 74th year.)

THE END