MORE LETTERS OF CHARLES DARWIN VOLUME II

CHARLES DARWIN*

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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 sub ject. 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 ob ject in view. The sub ject 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

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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 sub ject, 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
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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 sub ject. In fact, I do not suppose any man could master so comprehensive a sub ject 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
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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 sub jects, 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.

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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 ob ject 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

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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
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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 sub ject.

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 ob jects of Mr. Darwin, and sometimes formed the sub ject 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
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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 sub ject 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
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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 sub ject, 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 sub ject.
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].
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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 CO_2 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 sub ject 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
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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 sub ject 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 sub ject. 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
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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 sub ject 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 sub ject 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