DARWIN CHARLES

MORE LETTERS OF CHARLES DARWIN — VOLUME 2

Charles Darwin

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More Letters of Charles Darwin — Volume 2 / A Record of His Work in a Series of Hitherto Unpublished Letters:

Содержание

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More Letters of Charles Darwin — Volume 2 / A Record of His Work in a Series of Hitherto Unpublished Letters

VOLUME II. DEDICATED WITH AFFECTION AND RESPECT, TO SIR JOSEPH HOOKER IN REMEMBRANCE OF HIS LIFELONG FRIENDSHIP WITH CHARLES DARWIN "You will never know how much I owe to you for your constant kindness and encouragement" CHARLES DARWIN TO SIR JOSEPH HOOKER, SEPTEMBER 14, 1862

MORE LETTERS OF CHARLES DARWIN

VOLUME II

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 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 CO2 was small. It is not clear to us that Ball relies so largely on the condition of the atmosphere as regards CO2. 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 CO2, 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 CO2, 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 CO2 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 CO2. But there seems to be a disturbance in metabolism, and the plants fail to take advantage of the increased supply of CO2. 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 CO2 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 pea-hen 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 (figures 5-8). Mr. T.W. Wood supplied similar drawings of a cat (figures 9, 10), also a sketch of the head of a snarling dog (figure 14).) 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.

Конец ознакомительного фрагмента.