

THE POPULAR SCIENCE MONTHLY.

MARCH, 1903.

HITHERTO UNPUBLISHED LETTERS OF CHARLES
DARWIN.*

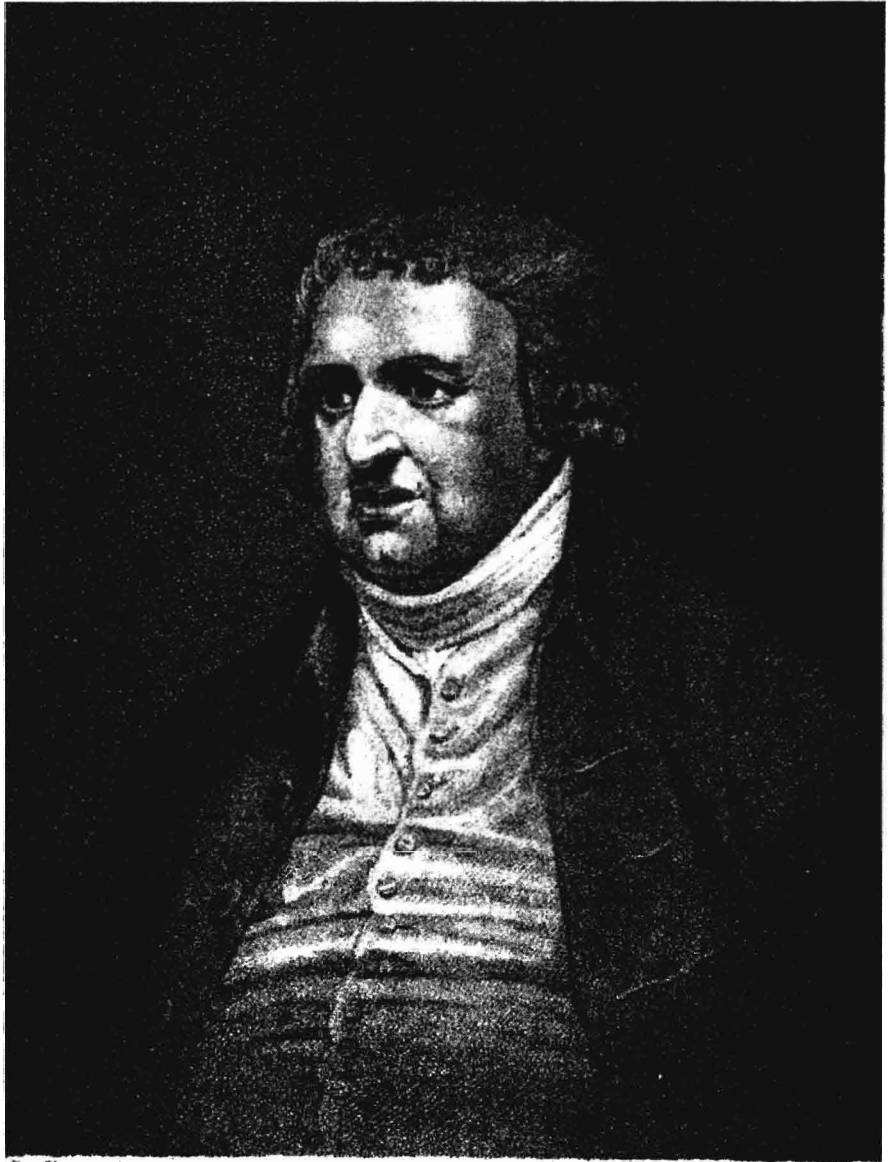
TO A. R. WALLACE.

DOWN, April 6th, 1859.

I this morning received your pleasant and friendly note of November 30th. The first part of my MS.† is in Murray's hands to see if he likes to publish it. There is no preface, but a short introduction, which must be read by every one who reads my book. The second paragraph in the introduction I have had copied verbatim from my foul copy, and you will, I hope, think that I have fairly noticed your paper in the *Linn. Journal*. You must remember that I am now publishing only an abstract, and I give no references. I shall, of course, allude to your paper on distribution; and I have added that I know from correspondence that your explanation of your law is the same as that which I offer. You are right, that I came to the conclusion that selection was the principle of change from the study of domesticated productions; and then, reading Malthus, I saw at once how to apply this principle. Geographical distribution and geological relations of extinct to recent inhabitants of South America

* 'The Life and Letters of Charles Darwin,' edited by his son, Professor Francis Darwin, and published in this country in 1887 by Messrs. D. Appleton and Company, is not surpassed in interest by any similar records, and for the man of science it is of unparalleled importance. From unused material and additional letters, Professor Francis Darwin and Mr. A. C. Seward have compiled a second series, entitled 'More Letters of Charles Darwin: A record of his work in a series of hitherto unpublished letters,' which will be published shortly in two volumes by Messrs. D. Appleton and Company. By their courtesy we are enabled to print here a number of letters which show the surpassing interest of the work.—EDITOR.

† 'Origin of Species.'



Rawlinson pinx.

Houghton sculp.

Erasmus Darwin, M.D.

first led me to the subject: especially the case of the Galapagos Islands. I hope to go to press in the early part of next month. It will be a small volume of about five hundred pages or so. I will of course send you a copy. I forget whether I told you that Hooker, who is our best British botanist and perhaps the best in the world, is a full convert, and is now going immediately to publish his confession of faith; and I expect daily to see proof-sheets. Huxley is changed, and believes in mutation of species: whether a convert to us, I do not quite know. We shall live to see all the younger men converts. My neighbour and an excellent naturalist, J. Lubbock, is an enthusiastic convert. I see that you are doing great work in the Archipelago; and most heartily do I sympathise with you. For God's sake take care of your health. There have been few such noble labourers in the cause of Natural Science as you are.

P. S. You cannot tell how I admire your spirit, in the manner in which you have taken all that was done about publishing all our papers. I had actually written a letter to you, stating that I would not publish anything before you had published. I had not sent that letter to the post when I received one from Lyell and Hooker, urging me to send some MS. to them, and allow them to act as they thought fair and honestly to both of us; and I did so.

TO T. H. HUXLEY.

July 20th [1860].

Many thanks for your pleasant letter. I agree to every word you say about *Fraser* and the *Quarterly*. I have had some really admirable letters from Hopkins. I do not suppose he has ever troubled his head about geographical distribution, classification, morphologies, etc., and it is only those who have that will feel any relief in having some sort of rational explanation of such facts. Is it not grand the way in which the Bishop asserts that all such facts are explained by ideas in God's mind? The *Quarterly* is uncommonly clever; and I chuckled much at the way my grandfather and self are quizzed. I could here and there see Owen's hand. By the way, how comes it that you were not attacked? Does Owen begin to find it more prudent to leave you alone? I would give five shillings to know what tremendous blunder the Bishop made; for I see that a page has been cancelled and a new page gummed in.

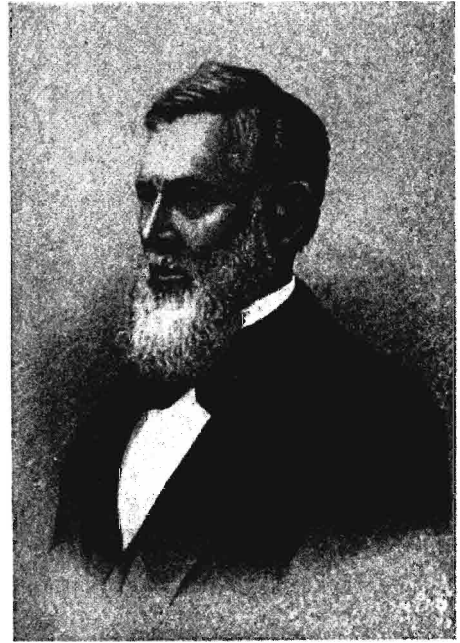
I am indeed most thoroughly contented with the progress of opinion. From all that I hear from several quarters, it seems that Oxford did the subject great good. It is of enormous importance the showing the world that a few first-rate men are not afraid of expressing their opinion. I see daily more and more plainly that my unaided

book would have done absolutely nothing. Asa Gray is fighting admirably in the United States. He is thorough master of the subject, which cannot be said by any means of such men as even Hopkins.

I have been thinking over what you allude to about a natural history review. I suppose you mean really a *review* and not a journal for original communications in Natural History. Of the latter there is now superabundance. With



T. H. HUXLEY.



ASA GRAY.



J. D. HOOKER.

respect to a good review, there can be no doubt of its value and utility; nevertheless, if not too late, I hope you will consider deliberately before you decide. Remember what a deal of work you have on your shoulders, and though you can do much, yet there is a limit to even the hardest worker's power of working. I

should deeply regret to see you sacrificing much time which could be given to original research. I fear, to one who can review as well as

you do, there would be the same temptation to waste time, as there notoriously is for those who can speak well.

A review is only temporary; your work should be perennial. I know well that you may say that unless good men will review there will be no good reviews. And this is true. Would you not do more good by an occasional review in some well-established review, than by giving up much time to the editing, or largely aiding, if not editing, a review which from being confined to one subject would not have a very large circulation? But I must return to the chief idea which strikes me—viz., that it would lessen the amount of original and perennial work which you could do. Reflect how few men there are in England who can do original work in the several lines in which you are excellently fitted. Lyell, I remember, on analogous grounds many years ago resolved he would write no more reviews. I am an old slow-coach, and your scheme makes me tremble. God knows in one sense I am about the last man in England who ought to throw cold water on any review in which you would be concerned, as I have so immensely profited by your labours in this line.

With respect to reviewing myself, I never tried: any work of that kind stops me doing anything else, as I cannot possibly work at odds and ends of time. I have, moreover, an insane hatred of stopping my regular current of work. I have now materials for a little paper or two, but I know I shall never work them up. So I will not promise to help; though not to help, if I could, would make me feel very ungrateful to you. You have no idea during how short a time daily I am able to work. If I had any regular duties, like you and Hooker, I should do absolutely nothing in science.

I am heartily glad to hear that you are better; but how such labour as volunteer-soldiering (all honour to you) does not kill you, I cannot understand.

For God's sake remember that your field of labour is original research in the highest and most difficult branches of Natural History. Not that I wish to underrate the importance of clever and solid reviews.

TO J. D. HOOKER.

DOWN, Feb. 14th [1860].

I succeeded in persuading myself for twenty-four hours that Huxley's lecture* was a success. Parts were eloquent and good, and all very bold; and I heard strangers say, 'What a good lecture!' I told Huxley so; but I demurred much to the time wasted in introductory remarks, especially to his making it appear that sterility was a clear and manifest distinction of species, and to his not having even alluded

* At the Royal Institution.

to the more important parts of the subject. He said that he had much more written out, but time failed. After conversation with others and more reflection, I must confess that as an exposition of the doctrine the lecture seems to me an entire failure. I thank God I did not think so when I saw Huxley; for he spoke so kindly and magnificently of me, that I could hardly have endured to say what I now think. He gave no just idea of Natural Selection. I have always looked at the doctrine of Natural Selection as an hypothesis, which, if it explained several large classes of facts, would deserve to be ranked as a theory deserving acceptance; and this, of course, is my own opinion. But, as Huxley has never alluded to my explanation of classification, morphology, embryology, etc., I thought he was thoroughly dissatisfied with all this part of my book. But to my joy I find it is not so, and that he agrees with my manner of looking at the subject; only that he rates higher than I do the necessity of Natural Selection being shown to be a *vera causa* always in action. He tells me he is writing a long review in the *Westminster*. It was really provoking how he wasted time over the idea of a species as exemplified in the horse, and over Sir J. Hall's old experiment on marble. Murchison was very civil to me over my book after the lecture, in which he was disappointed. I have quite made up my mind to a savage onslaught; but with Lyell, you, and Huxley, I feel confident we are right, and in the long run shall prevail. I do not think Asa Gray has quite done you justice in the beginning of the review of me. The review seemed to me very good, but I read it very hastily.

TO J. D. HOOKER.

DOWN, Nov. 20th [1862].

Your last letter has interested me to an extraordinary degree, and your truly parsonic advice, 'some other wise and discreet person,' etc., etc., amused us not a little. I will put a concrete case to show what I think A. Gray believes about crossing and what I believe. If 1,000 pigeons were bred together in a cage for 10,000 years their number not being allowed to increase by chance killing, then from mutual intercrossing no varieties would arise; but, if each pigeon were a self-fertilising hermaphrodite, a multitude of varieties would arise. This, I believe, is the common effect of crossing, viz., the obliteration of incipient varieties. I do not deny that when two marked varieties have been produced, their crossing will produce a third or more intermediate varieties. Possibly, or probably, with domestic varieties, with a strong tendency to vary, the act of crossing tends to give rise to new characters; and thus a third or more races, not strictly intermediate, may be produced. But there is heavy evi-

dence against new characters arising from crossing wild forms; only intermediate races are then produced. Now, do you agree thus far? if not, it is no use arguing; we must come to swearing, and I am convinced I can swear harder than you, . . . I am right. Q.E.D.

If the number of 1,000 pigeons were prevented increasing not by chance killing, but by, say, all the shorter-beaked birds being killed, then the *whole* body would come to have longer beaks. Do you agree?

Thirdly, if 1,000 pigeons were kept in a hot country, and another 1,000 in a cold country, and fed on different food, and confined in different-size aviary, and kept constant in number by chance killing, then I should expect as rather probable that after 10,000 years the two bodies would differ slightly in size, colour, and perhaps other trifling characters; this I should call the direct action of physical conditions. By this action I wish to imply that the innate vital forces are somehow led to act rather differently in the two cases, just as heat will allow or cause two elements to combine, which otherwise would not have combined. I should be especially obliged if you would tell me what you think on this head.

But the part of your letter which fairly pitched me head over heels with astonishment, is that where you state that every single difference which we see might have occurred without any selection. I do and have always fully agreed; but you have got right round the subject, and viewed it from an entirely opposite and new side, and when you took me there I was astounded. When I say I agree, I must make the proviso, that under your view, as now, each form long remains adapted to certain fixed conditions, and that the conditions of life are in the long run changeable; and second, which is more important, that each individual form is a self-fertilising hermaphrodite, so that each hair-breadth variation is not lost by intercrossing. Your manner of putting the case would be even more striking than it is if the mind could grapple with such numbers—it is grappling with eternity—think of each of a thousand seeds bringing forth its plant, and then each a thousand. A globe stretching to the furthest fixed star would very soon be covered. I cannot even grapple with the idea, even with races of dogs, cattle, pigeons, or fowls; and here all admit and see the accurate strictness of your illustration.

Such men as you and Lyell thinking that I make too much of a Deus of Natural Selection is a conclusive argument against me. Yet I hardly know how I could have put in, in all parts of my book, stronger sentences. The title, as you once pointed out, might have been better. No one ever objects to agriculturists using the strongest language about their selection, yet every breeder knows that he does not produce the modification which he selects. My enormous diffi-

culty for years was to understand adaptation, and this made me, I cannot but think, rightly, insist so much on Natural Selection. God forgive me for writing at such length; but you cannot tell how much your letter has interested me, and how important it is for me with my present book in hand to try and get clear ideas. Do think a bit about what is meant by direct action of physical conditions. I do not mean whether they act; my facts will throw some light on this. I am collecting all cases of bud-variations, in contradistinction to seed-variations (do you like this term, for what some gardeners call 'sports?'); these eliminate all effects of crossing. Pray remember how much I value your opinion as the clearest and most original I ever get.

I see plainly that *Welwitschia* will be a case of Barnacles.

I have another plant to beg, but I write on separate paper as more convenient for you to keep. I meant to have said before, as an excuse for asking for so much from Kew, that I have now lost *two* seasons, by accursed nurserymen not having right plants, and sending me the wrong instead of saying that they did not possess.

TO J. D. HOOKER.

FRESHWATER, Isle of Wight, July 28th [1868].

I am glad to hear that you are going to touch on the statement that the belief in Natural Selection is passing away. I do not suppose that even the *Athenæum* would pretend that the belief in the common descent of species is passing away, and this is the more important point. This now almost universal belief in the evolution (somehow) of species, I think may be fairly attributed in large part to the *Origin*. It would be well for you to look at the short Introduction of Owen's *Anat. of Invertebrates*, and see how fully he admits the descent of species.

Of the *Origin*, four English editions, one or two American, two French, two German, one Dutch, one Italian, and several (as I was told) Russian editions. The translations of my book on *Variation under Domestication* are the results of the *Origin*; and of these two English, one American, one German, one French, one Italian, and one Russian have appeared, or will soon appear. Ernst Hæckel wrote to me a week or two ago, that new discussions and reviews of the *Origin* are continually still coming out in Germany, where the interest on the subject certainly does not diminish. I have seen some of these discussions, and they are good ones. I apprehend that the interest on the subject has not died out in North America, from observing in Professor and Mrs. Agassiz's Book on Brazil how excessively anxious he is to destroy me. In regard to this country, every one can judge

for himself, but you would not say interest was dying out if you were to look at the last number of the *Anthropological Review*, in which I am incessantly sneered at. I think Lyell's *Principles* will produce a considerable effect. I hope I have given you the sort of information which you want. My head is rather unsteady, which makes my handwriting worse than usual.

If you argue about the non-acceptance of Natural Selection, it seems to me a very striking fact that the Newtonian theory of gravitation, which seems to every one now so certain and plain, was rejected by a man so extraordinarily able as Leibnitz. The truth will not penetrate a preoccupied mind.

Wallace, in the *Westminster Review*, in an article on Protection has a good passage, contrasting the success of Natural Selection and its growth with the comprehension of new classes of facts, with false theories, such as the Quinarian Theory, and that of Polarity, by poor Forbes, both of which were promulgated with high advantages and the first temporarily accepted.

TO C. LYELL.

15, MARINE PARADE, EASTBOURNE, Oct. 3rd [1860].

Your last letter has interested me much in many ways.

I enclose a letter of Wyman's which touches on brains. Wyman is mistaken in supposing that I did not know that the Cave-rat was an American form; I made special enquiries. He does not know that the eye of the Tucutuco was carefully dissected.

With respect to reviews by A. Gray. I thought of sending the Dialogue to the *Saturday Review* in a week's time or so, as they have lately discussed Design. I have sent the second, or August, *Atlantic* article to the *Annals and Mag. of Nat. History*. The copy which you have I want to send to Pictet, as I told A. Gray I would, thinking from what he said he would like this to be done. I doubt whether it would be possible to get the October number reprinted in this country; so that I am in no hurry at all for this.

I had a letter a few weeks ago from Symonds on the imperfection of the Geological Record, less clear and forcible than I expected. I answered him at length and very civilly, though I could hardly make out what he was driving at. He spoke about you in a way which it did me good to read.

I am extremely glad that you like A. Gray's reviews. How generous and unselfish he has been in all his labour! Are you not struck by his metaphors and similes? I have told him he is a poet and not a lawyer.

I should altogether doubt on turtles being converted into land tortoises on any one island. Remember how closely similar tortoises are

on all continents, as well as islands; they must have all descended from one ancient progenitor, including the gigantic tortoise of the Himalaya.

I think you must be cautious in not running the convenient doctrine that only one species out of very many ever varies. Reflect on such cases as the fauna and flora of Europe, North America, and Japan, which are so similar, and yet which have a great majority of their species either specifically distinct, or forming well-marked races. We must in such cases incline to the belief that a multitude of species were once identically the same in all the three countries when under a warmer climate and more in connection; and have varied in all the three countries. I am inclined to believe that almost every species (as we see with nearly all our domestic productions) varies sufficiently for Natural Selection to pick out and accumulate new specific differences, under new organic and inorganic conditions of life, whenever a place is open in the polity of nature. But looking to a long lapse of time and to the whole world, or to large parts of the world, I believe only one or a few species of each large genus ultimately becomes victorious, and leaves modified descendants. To give an imaginary instance: the jay has become modified in the three countries into (I believe) three or four species; but the jay genus is not, apparently, so dominant a group as the crows; and in the long run probably all the jays will be exterminated and be replaced perhaps by some modified crows.

I merely give this illustration to show what seems to me probable.

But oh! what work there is before we shall understand the genealogy of organic beings!

With respect to the *Apteryx*, I know not enough of anatomy; but ask Dr. F. whether the clavicle, etc., do not give attachment to some of the muscles of respiration. If my views are at all correct, the wing of the *Apteryx* cannot be (p. 452 of the *Origin*) a nascent organ, as these wings are useless. I dare not trust to memory, but I know I found the whole sternum always reduced in size in all the fancy and confined pigeons relatively to the same bones in the wild Rock-pigeon: the keel was generally still further reduced relatively to the reduced length of the sternum; but in some breeds it was in a most anomalous manner more prominent. I have got a lot of facts on the reduction of the organs of flight in the pigeon, which took me weeks to work out, and which Huxley thought curious.

I am utterly ashamed, and groan over my handwriting. It was 'Natural Preservation.' Natural persecution is what the author ought to suffer. It rejoices me that you do not object to the term. Hooker made the same remark that it ought to have been 'Variation and Natural Selection.' Yet with domestic productions, when selec-

tion is spoken of, variation is always implied. But I entirely agree with your and Hooker's remark.

Have you begun regularly to write your book on the antiquity of man?

I do *not* agree with your remark that I make Natural Selection do too much work. You will perhaps reply that every man rides his hobby-horse to death; and that I am in the galloping state.

TO C. LYELL.

TORQUAY, Aug. 21st [1861].

I am pleased that you approve of Hutton's review. It seemed to me to take a more philosophical view of the manner of judging the question than any other review. The sentence you quote from it seems very true, but I do not agree with the theological conclusion. I think he quotes from Asa Gray, certainly not from me; but I have neither A. Gray nor *Origin* with me. Indeed, I have over and over again said in the *Origin* that Natural Selection does nothing without variability; I have given a whole chapter on laws, and used the strongest language how ignorant we are on these laws. But I agree that I have somehow (Hooker says it is owing to my title) not made the great and manifest importance of previous variability plain enough. Breeders constantly speak of Selection as the one great means of improvement; but of course they imply individual differences, and this I should have thought would have been obvious to all in Natural Selection; but it has not been so.

I have just said that I cannot agree with 'which variations are the effects of an unknown law, ordained and guided without doubt by an intelligent cause on a preconceived and definite plan.' Will you honestly tell me (and I should be really much obliged) whether you believe that the shape of my nose (ehé!) was ordained and 'guided by an intelligent cause?' By the selection of analogous and less differences fanciers make almost generic differences in their pigeons; and can you see any good reason why the Natural Selection of analogous individual differences should not make new species? If you say that God ordained that at some time and place a dozen slight variations should arise, and that one of them alone should be preserved in the struggle for life and the other eleven should perish in the first or few first generations, then the saying seems to me mere verbiage. It comes to merely saying that everything that is, is ordained.

Let me add another sentence. Why should you or I speak of variation as having been ordained and guided, more than does an astronomer, in discussing the fall of a meteoric stone? He would simply say that it was drawn to our earth by the attraction of gravity,

having been displaced in its course by the action of some quite unknown laws. Would you have him say that its fall at some particular place and time was 'ordained and guided without doubt by an intelligent cause on a preconceived and definite plan?' Would you not call this theological pedantry or display? I believe it is not pedantry in the case of species, simply because their formation has hitherto been viewed as beyond law; in fact, this branch of science is still with most people under its theological phase of development. The conclusion which I always come to after thinking of such questions is that they are beyond the human intellect; and the less one thinks on them the better. You may say, Then why trouble me? But I should very much like to know clearly what you think.

TO ASA GRAY.

Down, Nov. 29th [1859].

This shall be such an extraordinary note as you have never received from me, for it shall not contain one single question or request. I thank you for your impression on my views. Every criticism from a good man is of value to me. What you hint at generally is very, very true: that my work will be grievously hypothetical, and large parts by no means worthy of being called induction, my commonest error being probably induction from too few facts. I had not thought of your objection of my using the term 'natural selection' as an agent. I use it much as a geologist does the word denudation—for an agent, expressing the result of several combined actions. I will take care to explain, not merely by inference, what I mean by the term; for I must use it, otherwise I should incessantly have to expand it into some such (here miserably expressed) formula as the following: "The tendency to the preservation (owing to the severe struggle for life to which all organic beings at some time or generation are exposed) of any, the slightest, variation in any part, which is of the slightest use or favourable to the life of the individual which has thus varied; together with the tendency to its inheritance." Any variation, which was of no use whatever to the individual, would not be preserved by this process of 'natural selection.' But I will not weary you by going on, as I do not suppose I could make my meaning clearer without large expansion. I will only add one other sentence: several varieties of sheep have been turned out together on the Cumberland mountains, and one particular breed is found to succeed so much better than all the others that it fairly starves the others to death. I should here say that natural selection picks out this breed, and would tend to improve it, or aboriginally to have formed it. . . .

You speak of species not having any material base to rest on, but is this any greater hardship than deciding what deserves to be called a variety, and be designated by a Greek letter? When I was at systematic work I know I longed to have no other difficulty (great enough) than deciding whether the form was distinct enough to deserve a name, and not to be haunted with undefined and unanswerable questions whether it was a true species. What a jump it is from a well-marked variety, produced by natural cause, to a species produced by the separate act of the hand of God! But I am running on foolishly. By the way, I met the other day Phillips, the palæontologist, and he asked me, 'How do you define a species?' I answered, 'I can not.' Whereupon he said, 'At last I have found out the only true definition—any form which has ever had a specific name! . . .'

TO ASA GRAY.

DOWN, June 8th [1860].

I have to thank you for two notes, one through Hooker, and one with some letters to be posted, which was done. I anticipated your request by making a few remarks on Owen's review. Hooker is so weary of reviews that I do not think you will get any hints from him. I have lately had many more 'kicks than halfpence.' A review in the last *Dublin Nat. Hist. Review* is the most unfair thing which has appeared,—one mass of misrepresentation. It is evidently by Haughton, the geologist, chemist and mathematician. It shows immeasurable conceit and contempt of all who are not mathematicians. He discusses bees' cells, and puts a series which I have never alluded to, and wholly ignores the intermediate comb of *Melipona*, which alone led me to my notions. The article is a curiosity of unfairness and arrogance; but, as he sneers at Malthus, I am content, for it is clear he can not reason. He is a friend of Harvey, with whom I have had some correspondence. Your article has clearly, as he admits, influenced him. He admits to a certain extent Natural Selection, yet I am sure does not understand me. It is strange that very few do, and I am become quite convinced that I must be an extremely bad explainer. To recur for a moment to Owen: he grossly misrepresents and is very unfair to Huxley. You say that you think the article must be by a pupil of Owen; but no one fact tells so strongly against Owen, considering his former position at the College of Surgeons, as that he has never reared one pupil or follower. In the number just out of *Fraser's Magazine* there is an article or review on Lamarck and me by W. Hopkins, the mathematician, who, like Haughton, despises the reasoning power of all naturalists. Personally he is extremely kind towards me; but he evidently in the following number means to blow

me into atoms. He does not in the least appreciate the difference in my views and Lamarck's, as explaining adaptation, the principle of divergence, the increase of dominant groups, and the almost necessary extinction of the less dominant and smaller groups, etc.

TO ASA GRAY.

DOWN, July 23rd [1862].

I received several days ago two large packets, but have as yet read only your letter; for we have been in fearful distress, and I could attend to nothing. Our poor boy had the rare case of second rash and sore throat . . .; and, as if this was not enough, a most serious attack of erysipelas, with typhoid symptoms. I despaired of his life; but this evening he has eaten one mouthful, and I think has passed the crisis. He has lived on port wine every three-quarters of an hour, day and night. This evening, to our astonishment, he asked whether his stamps were safe, and I told him of one sent by you, and that he should see it to-morrow. He answered, 'I should awfully like to see it now'; so with difficulty he opened his eyelids and glanced at it, and, with a sigh of satisfaction, said, 'All right.' Children are one's greatest happiness, but often and often a still greater misery. A man of science ought to have none—perhaps not a wife; for then there would be nothing in this wide world worth caring for, and a man might (whether he could is another question) work away like a Trojan. I hope in a few days to get my brains in order, and then I will pick out all your orchid letters, and return them in hopes of your making use of them. . . .

Of all the carpenters for knocking the right nail on the head, you are the very best; no one else has perceived that my chief interest in my orchid book has been that it was a 'flank movement' on the enemy. I live in such solitude that I hear nothing, and have no idea to what you allude about Bentham and the orchids and species. But I must enquire.

By the way, one of my chief enemies (the sole one who has annoyed me), namely Owen, I hear has been lecturing on birds; and admits that all have descended from one, and advances as his own idea that the oceanic wingless birds have lost their wings by gradual disuse. He never alludes to me, or only with bitter sneers, and coupled with Buffon and the *Vestiges*.

Well, it has been an amusement to me this first evening, scribbling as egotistically as usual about myself and my doings; so you must forgive me, as I know well your kind heart will do. I have managed to skim the newspaper, but had not heart to read all the bloody details. Good God! what will the end be? Perhaps we are too de-

spondent here; but I must think you are too hopeful on your side of the water. I never believed the 'canards' of the army of the Potomac having capitulated. My good dear wife and self are come to wish for peace at any price. Good night, my good friend. I will scribble on no more.

One more word. I should like to hear what you think about what I say in the last chapter of the orchid book on the meaning and cause of the endless diversity of means for the same general purpose. It bears on design, that endless question. Good night, good night!

TO J. D. DANA.

DOWN, Dec. 5th, 1849.

I have not for some years been so much pleased as I have just been by reading your most able discussion on coral reefs. I thank you most sincerely for the very honourable mention you make of me. This day I heard that the atlas has arrived, and this completes your munificent present to me. I have not yet come to the chapter on subsidence, and in that I fancy we shall disagree, but in the descriptive part our agreement has been eminently satisfactory to me, and far more than I ever ventured to anticipate. I consider that now the subsidence theory is established. I have read about half through the descriptive part of the *Volcanic Geology* (last night I ascended the peaks of Tahiti with you, and what I saw in my short excursion was most vividly brought before me by your descriptions), and have been most deeply interested by it. Your observations on the Sandwich craters strike me as the most important and original of any that I have read for a long time. Now that I have read yours, I believe I saw at the Galapagos, at a distance, instances of those most curious fissures of eruption. There are many points of resemblance between the Galapagos and Sandwich Islands (even to the shape of the mound-like hills)—viz., in the liquidity of the lavas, absence of scorïæ, and tuff-craters. Many of your scattered remarks on denudation have particularly interested me; but I see that you attribute less to sea and more to running water than I have been accustomed to do. After your remarks in your last very kind letter I could not help skipping on to the Australian valleys, on which your remarks strike me as exceedingly ingenious and novel, but they have not converted me. I can not conceive how the great lateral bays could have been scooped out, and their sides rendered precipitous by running water. I shall go on and read every word of your excellent volume.

If you look over my *Geological Instructions* you will be amused to see that I urge attention to several points which you have elaborately discussed. I lately read a paper of yours on Chambers' book,

and was interested by it. I really believe the facts of the order described by Chambers, in S. America, which I have described in my Geolog. volume. This leads me to ask you (as I can not doubt that you will have much geological weight in N. America) to look to a discussion at p. 135 in that volume on the importance of subsidence to the formation of deposits, which are to last to a distant age. This view strikes me as of some importance.

When I meet a very good-natured man I have that degree of badness of disposition in me that I always endeavour to take advantage of him; therefore I am going to mention some desiderata, which if you can supply I shall be very grateful, but if not no answer will be required.

Thank you for your *Conspectus Crust.*, but I am sorry to say I am not worthy of it, though I have always thought the Crustacea a beautiful subject.

TO J. D. DANA.

DOWN, July 30th (1860).

I received several weeks ago your note telling me that you could not visit England, which I sincerely regretted, as I should most heartily have liked to have made your personal acquaintance. You gave me an improved, but not very good, account of your health. I should at some time be grateful for a line to tell me how you are. We have had a miserable summer, owing to a terribly long and severe illness of my eldest girl, who improves slightly but is still in a precarious condition. I have been able to do nothing in science of late. My kind friend Asa Gray often writes to me and tells me of the warm discussions on the *Origin of Species* in the United States. Whenever you are strong enough to read it, I know you will be dead against me, but I know equally well that your opposition will be liberal and philosophical. And this is a good deal more than I can say of all my opponents in this country. I have not yet seen Agassiz's attack, but I hope to find it at home when I return in a few days, for I have been for several weeks away from home on my daughter's account. Prof. Silliman sent me an extremely kind message by Asa Gray that your Journal would be open to a reply by me. I cannot decide till I see it, but on principle I have resolved to avoid answering anything, as it consumes much time, often temper, and I have said my say in the *Origin*. No one person understands my views and has defended them so well as A. Gray, though he does not by any means go all the way with me. There was much discussion on the subject at the British Association at Oxford, and I had many defenders, and my side seems (for I was not there) almost to have got the best of the battle. Your correspondent and my neighbour, J. Lubbock, goes on working at such spare time as he has.

This is an egotistical note, but I have not seen a naturalist for months. Most sincerely and deeply do I hope that this note may find you almost recovered.

To A. HYATT.

Down, Dec. 4th, 1872.

I thank you sincerely for your most interesting letter. You refer much too modestly to your own knowledge and judgment, as you are much better fitted to throw light on your own difficult problems than I am.

It has quite annoyed me that I do not clearly understand yours and Prof. Cope's views; and the fault lies in some slight degree, I think, with Prof. Cope, who does not write very clearly. I think I now understand the terms 'acceleration' and 'retardation'; but will you grudge the trouble of telling me, by the aid of the following illustration, whether I do understand rightly? When a fresh-water decapod crustacean is born with an almost mature structure, and therefore does not pass, like other decapods, through the Zoea stage, is this not a case of acceleration? Again, if an imaginary decapod retained, when adult, many Zoea characters, would this not be case of retardation? If these illustrations are correct, I can perceive why I have been so dull in understanding your views. I looked for something else, being familiar with such cases, and classing them in my own mind as simply due to the obliteration of certain larval or embryonic stages. This obliteration I imagined resulted sometimes entirely from that law of inheritance to which you allude; but that it in many cases was aided by Natural Selection, as I inferred from such cases occurring so frequently in terrestrial and fresh-water members of groups, which retain their several embryonic stages in the sea, as long as fitting conditions are present.

Another cause of my misunderstanding was the assumption that in your series

a—ab—abd—ae,
 \ ad

the differences between the successive species, expressed by the terminal letter, was due to acceleration: now, if I understand rightly, this is not the case; and such characters must have been independently acquired by some means.

The two newest and most interesting points in your letter (and in, as far as I think, your former paper) seem to me to be about senile characteristics in one species appearing in succeeding species during maturity; and secondly about certain degraded characters appearing in the last species of a series. You ask for my opinion: I can only send the conjectured impressions which have occurred to me and which are not worth writing. (It ought to be known whether the senile

character appears before or after the period of active reproduction.) I should be inclined to attribute the character in both your cases to the laws of growth and descent, secondarily to Natural Selection. It has been an error on my part, and a misfortune to me, that I did not largely discuss what I mean by laws of growth at an early period in some of my books. I have said something on this head in two new chapters in the last edition of the *Origin*. I should be happy to send you a copy of this edition, if you do not possess it and care to have it. A man in extreme old age differs much from a young man, and I presume every one would account for this by failing powers of growth. On the other hand the skulls of some mammals go on altering during maturity into advancing years; as do the horns of the stag, the tail-feathers of some birds, the size of fishes, etc.; and all such differences I should attribute simply to the laws of growth, as long as full vigour was retained. Endless other changes of structure in successive species may, I believe, be accounted for by various complex laws of growth. Now, any change of character thus induced with advancing years in the individual might easily be inherited at an earlier age than that at which it first supervened, and thus become characteristic of the mature species; or again, such changes would be apt to follow from variation, independently of inheritance, under proper conditions. Therefore I should expect that characters of this kind would often appear in later-formed species without the aid of Natural Selection, or with its aid if the characters were of any advantage. The longer I live, the more I become convinced how ignorant we are of the extent to which all sorts of structures are serviceable to each species. But that characters supervening during maturity in one species should appear so regularly, as you state to be the case, in succeeding species, seems to me very surprising and inexplicable.

With respect to degradation in species towards the close of a series, I have nothing to say, except that before I arrived at the end of your letter, it occurred to me that the earlier and simpler ammonites must have been well adapted to their conditions, and that when the species were verging towards extinction (owing probably to the presence of some more successful competitors) they would naturally become re-adapted to simpler conditions. Before I had read your final remarks I thought also that unfavourable conditions might cause, through the law of growth, aided perhaps by reversion, degradation of character. No doubt many new laws remain to be discovered. Permit me to add that I have never been so foolish as to imagine that I have succeeded in doing more than to lay down some of the broad outlines of the origin of species.

After long reflection I cannot avoid the conviction that no innate tendency to progressive development exists, as is now held by so many

able naturalists, and perhaps by yourself. It is curious how seldom writers define what they mean by progressive development; but this is a point which I have briefly discussed in the *Origin*. I earnestly hope that you may visit Hilgendorf's famous deposit. Have you seen Weismann's pamphlet *Einfluss der Isolirung*, Leipzig, 1872? He makes splendid use of Hilgendorf's admirable observations. I have no strength to spare, being much out of health; otherwise I would have endeavoured to have made this letter better worth sending. I most sincerely wish you success in your valuable and difficult researches.

I have received, and thank you, for your three pamphlets. As far as I can judge, your views seem very probable; but what a fearfully intricate subject is this of the succession of ammonites.

TO B. D. WALSH.

DOWN, Dec. 4th [1864].

I have been greatly interested by your account of your American life. What an extraordinary and self-contained life you have led! and what vigour of mind you must possess to follow science with so much ardour after all that you have undergone! I am very much obliged to you for your pamphlet on Geographical Distribution, on Agassiz, etc. I am delighted at the manner in which you have bearded this lion in his den. I agree most entirely with all that you have written. What I meant when I wrote to Agassiz to thank him for a bundle of his publications, was exactly what you suppose. I confess, however, I did not fully perceive how he had misstated my views; but I only skimmed through his *Methods of Study*, and thought it a very poor book. I am so much accustomed to be utterly misrepresented that it hardly excites my attention. But you really have hit the nail on the head capitally. All the younger good naturalists whom I know think of Agassiz as you do; but he did grand service about glaciers and fish. About the succession of forms, Pictet has given up his whole views, and no geologist now agrees with Agassiz. I am glad that you have attacked Dana's wild notions; [though] I have a great respect for Dana. . . . If you have an opportunity, read in *Trans. Linn. Soc.* Bates on 'Mimetic Lepidoptera of Amazons.' I was delighted with his paper.

I have got a notice of your views about the female *Cynips* inserted in the *Natural History Review*: whether the notice will be favourable, I do not know; but anyhow it will call attention to your views. . . .

As you allude in your paper to the believers in change of species, you will be glad to hear that very many of the very best men are coming round in Germany. I have lately heard of Hackel, Gegenbauer, F.

Müller, Leuckart, Claparède, Alex. Braun, Schleiden, etc. So it is, I hear, with the younger Frenchmen.

TO C. V. RILEY.

DOWN, June 1st [1871].

I received some little time ago your report on noxious insects, and have now read the whole with the greatest interest. There are a vast number of facts and generalisations of value to me, and I am struck with admiration at your powers of observation.

The discussion on mimetic insects seems to me particularly good and original. Pray accept my cordial thanks for the instruction and interest which I have received.

What a loss to Natural Science our poor mutual friend Walsh has been; it is a loss ever to be deplored. . . .

Your country is far ahead of ours in some respects; our Parliament would think any man mad who should propose to appoint a State Entomologist.

TO E. S. MORSE.

DOWN, Oct. 21st, 1879.

Although you are so kind as to tell me not to write, I must just thank you for the proofs of your paper,* which has interested me greatly. The increase in the number of ridges in the three species of *Arca* seems to be a very noteworthy fact, as does the increase of size in so many, yet not all, the species. What a constant state of fluctuation the whole organic world seems to be in! It is interesting to hear that everywhere the first change apparently is in the proportional numbers of the species. I was much struck with the fact in the upraised shells of Coquimbo, in Chili, as mentioned in my *Geological Observations on South America*.

Of all the wonders in the world, the progress of Japan, in which you have been aiding, seems to me about the most wonderful.

TO A. AGASSIZ.

DOWN, May 5th, 1881.

It was very good of you to write to me from Tortugas, as I always feel much interested in hearing what you are about, and in reading your many discoveries. It is a surprising fact that the peninsula of Florida should have remained at the same level for the immense period requisite for the accumulation of so vast a pile of débris.

You will have seen Mr. Murray's views on the formation of atolls and barrier reefs. Before publishing my book, I thought long over the same view, but only as far as ordinary marine organisms are concerned, for at that time little was known of the multitude of minute

* 'The Shell Mounds of Omori.'

oceanic organisms. I rejected this view, as from the few dredgings made in the *Beagle* in the S. Temperate regions, I concluded that shells, the smaller corals, etc., etc., decayed and were dissolved when not protected by the deposition of sediment; and sediment could not accumulate in the open ocean. Certainly shells, etc., were in several cases completely rotten, and crumbled into mud between my fingers; but you will know well whether this is in any degree common. I have expressly said that a bank at the proper depth would give rise to an atoll, which could not be distinguished from one formed during subsidence. I can, however, hardly believe, in the former presence of as many banks (there having been no subsidence) as there are atolls in the great oceans, within a reasonable depth, on which minute oceanic organisms could have accumulated to the thickness of many hundred feet. I think that it has been shown that the oscillations from great waves extend down to a considerable depth, and if so the oscillating water would tend to lift up (according to an old doctrine propounded by Playfair) minute particles lying at the bottom, and allow them to be slowly drifted away from the submarine bank by the slightest current. Lastly, I can not understand Mr. Murray, who admits that small calcareous organisms are dissolved by the carbonic acid in the water at great depths, and that coral reefs, etc., etc., are likewise dissolved near the surface, but that this does not occur at intermediate depths, where he believes that the minute oceanic calcareous organisms accumulate until the bank reaches within the reef-building depth. But I suppose that I must have misunderstood him.

Pray forgive me for troubling you at such length, but it has occurred to me that you might be disposed to give, after your wide experience, your judgment. If I am wrong, the sooner I am knocked on the head and annihilated so much the better. It still seems to me a marvelous thing that there should not have been much and long-continued subsidence in the beds of the great oceans. I wish that some doubly rich millionaire would take it into his head to have borings made in some of the Pacific and Indian atolls, and bring home cores for slicing from a depth of 500 or 600 feet.

TO MRS. EMILY TALBOT, BOSTON.

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.

TO A. R. WALLACE.

DOWN, April 29th [1867].

I have been greatly interested by your letter, but your view is not new to me. If you will look at p. 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 p. 101, or at p. 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. 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. 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 can not say that I am fully satisfied. I remember mentioning to you the case of *Rhynchæa*, 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 cor-

recting 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, 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.

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 Hæckel 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*. 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.

TO AUG. WEISMANN.

DOWN, Feb. 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; 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. I was extremely pleased at hearing from Fritz Müller that he liked my chapter on lepidoptera in the *Descent of Man* more than any other part, excepting the chapter on morals.

TO T. H. HUXLEY.

WORTHING, Sept. 9th, 1881.

We have been paying Mr. Rich* a little visit, and he has often spoken of you, and I think he enjoyed much your and Mrs. Huxley's visit here. But my object in writing now is to tell you something, which I am very doubtful whether it is worth while for you to hear, because it is uncertain. My brother Erasmus has left me half his fortune, which is very considerable. Therefore, I thought myself bound to tell Mr. Rich of this, stating the large amount, as far as the executors as yet know it roughly. I then added that my wife and self thought that, under these new circumstances, he was most fully justified in altering his will and leaving his property in some other way. I begged him to take a week to consider what I had told him, and then by letter to inform me of the result. But he would not, however, hardly allow me to finish what I had to say, and expressed a firm determination not to alter his will, adding that I had five sons to provide for. After a short pause he implied (but unfortunately he here became very confused and forgot a word, which on subsequent reflection I think was probably 'reversionary')—he implied that there was a chance, whether good or bad I know not, of his becoming possessed of some other property, and he finished by saying distinctly, 'I will bequeath this to Huxley.' What the amount may be (I fear not large), and what the chance may be, God only knows; and one can not cross-examine a man about his will. He did not bind me to secrecy, so I think I am justified in telling you what passed, but whether it is wise on my part to send so vague a story, I am not at all sure; but as a general rule it is best to tell everything. As I know that you hate writing letters, do not trouble yourself to answer this.

P. S.—On further reflection I should like to hear that you receive this note safely. I have used up all my black-edged paper.

TO ANTHONY RICH.

DOWN, Feb. 4th, 1882.

It is always a pleasure to me to receive a letter from you. I am very sorry to hear that you have been more troubled than usual with your old complaint. Any one who looked at you would think that you

* Anthony Rich (1804?–1891). Educated at Caius College, Cambridge, of which he was afterwards an Honorary Fellow. Author of *Illustrated Companion to the Latin Dictionary and Greek Lexicon*, 1849, said to be a useful book on classical antiquities. Mr. Darwin made his acquaintance in a curious way—namely, by Mr. Rich writing to inform him that he intended to leave him his fortune, in token of his admiration for his work. Mr. Rich was the survivor, but left his property to Mr. Darwin's children, with the exception of his house at Worthing, bequeathed to Mr. Huxley.

had passed through life with few evils, and yet you have had an unusual amount of suffering. As a turnkey remarked in one of Dickens' novels, 'Life is a rum thing.' As for myself, I have been better than usual until about a fortnight ago, when I had a cough, and this pulled me down and made me miserable to a strange degree; but my dear old wife insisted on my taking quinine, and, though I have very little faith in medicine, this, I think, has done me much good. Well, we are both so old that we must expect some troubles: I shall be seventy-three on Feb. 12th. I have been glad to hear about the pine-leaves, and you are the first man who has confirmed my account that they are drawn in by the base, with a very few exceptions. With respect to your Wandsworth case, I think that if I had heard of it before publishing, I would have said nothing about the ledges; for the Grisedale case, mentioned in my book and observed whilst I was correcting the proof-sheets, made me feel rather doubtful. Yet the Corniche case shows that worms at least aid in making the ledges. Nevertheless, I wish I had said nothing about the confounded ledges. The success of this worm book has been almost laughable. I have, however, been plagued with an endless stream of letters on the subject; most of them very foolish and enthusiastic, but some containing good facts, which I have used in correcting yesterday the 'sixth Thousand.'

Your friend George's work about the viscous state of the earth and tides and the moon has lately been attracting much attention, and all the great judges think highly of the work. He intends to try for the Plumian Professorship of Mathematics and Natural Philosophy at Cambridge, which is a good and honourable post of about £800 a year. I think that he will get it when Challis is dead, and he is very near his end. He has all the great men—Sir W. Thomson, Adams, Stokes, etc.—on his side. He has lately been chief examiner for the Mathematical Tripos, which was tremendous work; and the day before yesterday he started for Southampton for a five-weeks' tour to Jamaica for complete rest, to see the Blue Mountains, and escape the rigour of the early spring. I believe that George will some day be a great scientific swell. The War Office has just offered Leonard a post in the Government Survey at Southampton, and very civilly told him to go down and inspect the place, and accept or not as he liked. So he went down, but has decided that it would not be worth his while to accept, as it would entail his giving up his expedition (on which he had been ordered) to Queensland, in Australia, to observe the Transit of Venus. Dear old William at Southampton has not been very well, but is now better. He has had too much work—a willing horse is always overworked—and all the arrangements for receiving the British Association there this summer have been thrown on his shoulders.

But good Heavens! what a deal I have written about my sons. I have had some hard work this autumn with the microscope; but this is over, and I have only to write out the papers for the Linnean Society. We have had a good many visitors; but none who would have interested you, except perhaps Mrs. Ritchie, the daughter of Thackeray, who is a most amusing and pleasant person. I have not seen Huxley for some time, but my wife heard this morning from Mrs. Huxley, who wrote from her bed, with a bad account of herself and several of her children; but none, I hope, are at all dangerously ill. Farewell, my kind, good friend.

Many thanks about the picture, which if I survive you, and this I do not expect, shall be hung in my study as a perpetual memento of you.*

* Charles Darwin died on April 19, 1882, in his seventy-fourth year.