# DARWIN CHARLES

# LIFE AND LETTERS OF CHARLES DARWIN — VOLUME 2

Чарльз Дарвин

# Life and Letters of Charles Darwin — Volume 2

**Дарвин Ч. Р.**

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# Содержание

# Charles Darwin
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## TRANSCRIPT OF A FACSIMILE OF A PAGE FROM A NOTE-BOOK OF 1837

— led to comprehend true affinities. My theory would give zest to recent & Fossil Comparative Anatomy: it would lead to study of instincts, heredity, & mind heredity, whole metaphysics, it would lead to closest examination of hybridity & generation, causes of change in order to know what we have come from & to what we tend, to what circumstances favour crossing & what prevents it, this & direct examination of direct passages of structure in species, might lead to laws of change, which would then be main object of study, to guide our speculations.

# VOLUME II

## CHAPTER 2.I. — THE PUBLICATION OF THE 'ORIGIN OF SPECIES.'

### OCTOBER 3, 1859, TO DECEMBER 31, 1859

1859.

[Under the date of October 1st, 1859, in my father's Diary occurs the entry: "Finished proofs (thirteen months and ten days) of Abstract on 'Origin of Species'; 1250 copies printed. The first edition was published on November 24th, and all copies sold first day."

On October 2d he started for a water-cure establishment at Ilkley, near Leeds, where he remained with his family until December, and on the 9th of that month he was again at Down. The only other entry in the Diary for this year is as follows: "During end of November and beginning of December, employed in correcting for second edition of 3000 copies; multitude of letters."

The first and a few of the subsequent letters refer to proof sheets, and to early copies of the 'Origin' which were sent to friends before the book was published.]

C. LYELL TO CHARLES DARWIN. (Part of this letter is given in the 'Life of Sir Charles Lyell,' volume ii. page 325.) October 3d, 1859.

My dear Darwin,

I have just finished your volume and right glad I am that I did my best with Hooker to persuade you to publish it without waiting for a time which probably could never have arrived, though you lived till the age of a hundred, when you had prepared all your facts on which you ground so many grand generalizations.

It is a splendid case of close reasoning, and long substantial argument throughout so many pages; the condensation immense, too great perhaps for the uninitiated, but an effective and important preliminary statement, which will admit, even before your detailed proofs appear, of some occasional useful exemplification, such as your pigeons and cirripedes, of which you make such excellent use.

I mean that, when, as I fully expect, a new edition is soon called for, you may here and there insert an actual case to relieve the vast number of abstract propositions. So far as I am concerned, I am so well prepared to take your statements of facts for granted, that I do not think the "pieces justificatives" when published will make much difference, and I have long seen most clearly that if any concession is made, all that you claim in your concluding pages will follow. It is this which has made me so long hesitate, always feeling that the case of Man and his races, and of other animals, and that of plants is one and the same, and that if a "vera causa" be admitted for one, instead of a purely unknown and imaginary one, such as the word "Creation," all the consequences must follow.

I fear I have not time to-day, as I am just leaving this place, to indulge in a variety of comments, and to say how much I was delighted with Oceanic Islands — Rudimentary Organs — Embryology — the genealogical key to the Natural System, Geographical Distribution, and if I went on I should be copying the heads of all your chapters. But I will say a word of the Recapitulation, in case some slight alteration, or at least, omission of a word or two be still possible in that.

In the first place, at page 480, it cannot surely be said that the most eminent naturalists have rejected the view of the mutability of species? You do not mean to ignore G. St. Hilaire and Lamarck. As to the latter, you may say, that in regard to animals you substitute natural selection for volition to a certain considerable extent, but in his theory of the changes of plants he could not introduce volition; he may, no doubt, have laid an undue comparative stress on changes in physical conditions, and too little on those of contending organisms. He at least was for the universal mutability of species and for a genealogical link between the first and the present. The men of his school also appealed to domesticated varieties. (Do you mean LIVING naturalists?) (In the published copies of the first edition, page 480, the words are "eminent living naturalists.")

The first page of this most important summary gives the adversary an advantage, by putting forth so abruptly and crudely such a startling objection as the formation of "the eye," not by means analogous to man's reason, or rather by some power immeasurably superior to human reason, but by superinduced variation like those of which a cattle-breeder avails himself. Pages would be required thus to state an objection and remove it. It would be better, as you wish to persuade, to say nothing. Leave out several sentences, and in a future edition bring it out more fully. Between the throwing down of such a stumbling-block in the way of the reader, and the passage to the working ants, in page 460, there are pages required; and these ants are a bathos to him before he has recovered from the shock of being called upon to believe the eye to have been brought to perfection, from a state of blindness or purblindness, by such variations as we witness. I think a little omission would greatly lessen the objectionableness of these sentences if you have not time to recast and amplify.

... But these are small matters, mere spots on the sun. Your comparison of the letters retained in words, when no longer wanted for the sound, to rudimentary organs is excellent, as both are truly genealogical.

The want of peculiar birds in Madeira is a greater difficulty than seemed to me allowed for. I could cite passages where you show that variations are superinduced from the new circumstances of new colonists, which would require some Madeira birds, like those of the Galapagos, to be peculiar. There has been ample time in the case of Madeira and Porto Santo...

You enclose your sheets in old MS., so the Post Office very properly charge them as letters, 2 pence extra. I wish all their fines on MS. were worth as much. I paid 4 shillings 6 pence for such wash the other day from Paris, from a man who can prove 300 deluges in the valley of the Seine.

With my hearty congratulations to you on your grand work, believe me,

Ever very affectionately yours, CHAS. LYELL.

CHARLES DARWIN TO C. LYELL. Ilkley, Yorkshire, October 11th [1859].

My dear Lyell,

I thank you cordially for giving me so much of your valuable time in writing me the long letter of 3d, and still longer of 4th. I wrote a line with the missing proof-sheet to Scarborough. I have adopted most thankfully all your minor corrections in the last chapter, and the greater ones as far as I could with little trouble. I damped the opening passage about the eye (in my bigger work I show the gradations in structure of the eye) by putting merely "complex organs." But you are a pretty Lord Chancellor to tell the barrister on one side how best to win the cause! The omission of "living" before eminent naturalists was a dreadful blunder.

MADEIRA AND BERMUDA BIRDS NOT PECULIAR.

You are right, there is a screw out here; I thought no one would have detected it; I blundered in omitting a discussion, which I have written out in full. But once for all, let me say as an excuse, that it was most difficult to decide what to omit. Birds, which have struggled in their own homes, when settled in a body, nearly simultaneously in a new country, would not be subject to much modification, for their mutual relations would not be much disturbed. But I quite agree with you, that in time they ought to undergo some. In Bermuda and Madeira they have, as I believe, been kept constant by the frequent arrival, and the crossing with unaltered immigrants of the same species from the mainland. In Bermuda this can be proved, in Madeira highly probable, as shown me by letters from E.V. Harcourt. Moreover, there are ample grounds for believing that the crossed offspring of the new immigrants (fresh blood as breeders would say), and old colonists of the same species would be extra vigorous, and would be the most likely to survive; thus the effects of such crossing in keeping the old colonists unaltered would be much aided.

ON GALAPAGOS PRODUCTIONS HAVING AMERICAN TYPE ON VIEW OF CREATION.

I cannot agree with you, that species if created to struggle with American forms, would have to be created on the American type. Facts point diametrically the other way. Look at the unbroken and untilled ground in La Plata, COVERED with European products, which have no near affinity to the indigenous products. They are not American types which conquer the aborigines. So in every island throughout the world. Alph. De Candolle's results (though he does not see its full importance), that thoroughly well naturalised [plants] are in general very different from the aborigines (belonging in large proportion of cases to non-indigenous genera) is most important always to bear in mind. Once for all, I am sure, you will understand that I thus write dogmatically for brevity sake.

ON THE CONTINUED CREATION Of MONADS.

This doctrine is superfluous (and groundless) on the theory of Natural Selection, which implies no NECESSARY tendency to progression. A monad, if no deviation in its structure profitable to it under its EXCESSIVELY SIMPLE conditions of life occurred, might remain unaltered from long before the Silurian Age to the present day. I grant there will generally be a tendency to advance in complexity of organisation, though in beings fitted for very simple conditions it would be slight and slow. How could a complex organisation profit a monad? if it did not profit it there would be no advance. The Secondary Infusoria differ but little from the living. The parent monad form might perfectly well survive unaltered and fitted for its simple conditions, whilst the offspring of this very monad might become fitted for more complex conditions. The one primordial prototype of all living and extinct creatures may, it is possible, be now alive! Moreover, as you say, higher forms might be occasionally degraded, the snake Typhlops SEEMS (?!) to have the habits of earth-worms. So that fresh creatures of simple forms seem to me wholly superfluous.

"MUST YOU NOT ASSUME A PRIMEVAL CREATIVE POWER WHICH DOES NOT ACT WITH UNIFORMITY, OR HOW COULD MAN SUPERVENE?"

I am not sure that I understand your remarks which follow the above. We must under present knowledge assume the creation of one or of a few forms in the same manner as philosophers assume the existence of a power of attraction without any explanation. But I entirely reject, as in my judgment quite unnecessary, any subsequent addition "of new powers and attributes and forces;" or of any "principle of improvement," except in so far as every character which is naturally selected or preserved is in some way an advantage or improvement, otherwise it would not have been selected. If I were convinced that I required such additions to the theory of natural selection, I would reject it as rubbish, but I have firm faith in it, as I cannot believe, that if false, it would explain so many whole classes of facts, which, if I am in my senses, it seems to explain. As far as I understand your remarks and illustrations, you doubt the possibility of gradations of intellectual powers. Now, it seems to me, looking to existing animals alone, that we have a very fine gradation in the intellectual powers of the Vertebrata, with one rather wide gap (not half so wide as in many cases of corporeal structure), between say a Hottentot and a Ourang, even if civilised as much mentally as the dog has been from the wolf. I suppose that you do not doubt that the intellectual powers are as important for the welfare of each being as corporeal structure; if so, I can see no difficulty in the most intellectual individuals of a species being continually selected; and the intellect of the new species thus improved, aided probably by effects of inherited mental exercise. I look at this process as now going on with the races of man; the less intellectual races being exterminated. But there is not space to discuss this point. If I understand you, the turning-point in our difference must be, that you think it impossible that the intellectual powers of a species should be much improved by the continued natural selection of the most intellectual individuals. To show how minds graduate, just reflect how impossible every one has yet found it, to define the difference in mind of man and the lower animals; the latter seem to have the very same attributes in a much lower stage of perfection than the lowest savage. I would give absolutely nothing for the theory of Natural Selection, if it requires miraculous additions at any one stage of descent. I think Embryology, Homology, Classification, etc., etc., show us that all vertebrata have descended from one parent; how that parent appeared we know not. If you admit in ever so little a degree, the explanation which I have given of Embryology, Homology and Classification, you will find it difficult to say: thus far the explanation holds good, but no further; here we must call in "the addition of new creative forces." I think you will be driven to reject all or admit all: I fear by your letter it will be the former alternative; and in that case I shall feel sure it is my fault, and not the theory's fault, and this will certainly comfort me. With regard to the descent of the great Kingdoms (as Vertebrata, Articulata, etc.) from one parent, I have said in the conclusion, that mere analogy makes me think it probable; my arguments and facts are sound in my judgment only for each separate kingdom.

THE FORMS WHICH ARE BEATEN INHERITING SOME INFERIORITY IN COMMON.

I dare say I have not been guarded enough, but might not the term inferiority include less perfect adaptation to physical conditions?

My remarks apply not to single species, but to groups or genera; the species of most genera are adapted at least to rather hotter, and rather less hot, to rather damper and dryer climates; and when the several species of a group are beaten and exterminated by the several species of another group, it will not, I think, generally be from EACH new species being adapted to the climate, but from all the new species having some common advantage in obtaining sustenance, or escaping enemies. As groups are concerned, a fairer illustration than negro and white in Liberia would be the almost certain future extinction of the genus ourang by the genus man, not owing to man being better fitted for the climate, but owing to the inherited intellectual inferiority of the Ourang-genus to Man-genus, by his intellect, inventing fire-arms and cutting down forests. I believe from reasons given in my discussion, that acclimatisation is readily effected under nature. It has taken me so many years to disabuse my mind of the TOO great importance of climate — its important influence being so conspicuous, whilst that of a struggle between creature and creature is so hidden — that I am inclined to swear at the North Pole, and, as Sydney Smith said, even to speak disrespectfully of the Equator. I beg you often to reflect (I have found NOTHING so instructive) on the case of thousands of plants in the middle point of their respective ranges, and which, as we positively know, can perfectly well withstand a little more heat and cold, a little more damp and dry, but which in the metropolis of their range do not exist in vast numbers, although if many of the other inhabitants were destroyed [they] would cover the ground. We thus clearly see that their numbers are kept down, in almost every case, not by climate, but by the struggle with other organisms. All this you will perhaps think very obvious; but, until I repeated it to myself thousands of times, I took, as I believe, a wholly wrong view of the whole economy of nature...

HYBRIDISM.

I am so much pleased that you approve of this chapter; you would be astonished at the labour this cost me; so often was I, on what I believe was, the wrong scent.

RUDIMENTARY ORGANS.

On the theory of Natural Selection there is a wide distinction between Rudimentary Organs and what you call germs of organs, and what I call in my bigger book "nascent" organs. An organ should not be called rudimentary unless it be useless — as teeth which never cut through the gums — the papillae, representing the pistil in male flowers, wing of Apteryx, or better, the little wings under soldered elytra. These organs are now plainly useless, and a fortiori, they would be useless in a less developed state. Natural Selection acts exclusively by preserving successive slight, USEFUL modifications. Hence Natural Selection cannot possibly make a useless or rudimentary organ. Such organs are solely due to inheritance (as explained in my discussion), and plainly bespeak an ancestor having the organ in a useful condition. They may be, and often have been, worked in for other purposes, and then they are only rudimentary for the original function, which is sometimes plainly apparent. A nascent organ, though little developed, as it has to be developed must be useful in every stage of development. As we cannot prophesy, we cannot tell what organs are now nascent; and nascent organs will rarely have been handed down by certain members of a class from a remote period to the present day, for beings with any important organ but little developed, will generally have been supplanted by their descendants with the organ well developed. The mammary glands in Ornithorhynchus may, perhaps, be considered as nascent compared with the udders of a cow — Ovigerous frena, in certain cirripedes, are nascent branchiae — in [illegible] the swim bladder is almost rudimentary for this purpose, and is nascent as a lung. The small wing of penguin, used only as a fin, might be nascent as a wing; not that I think so; for the whole structure of the bird is adapted for flight, and a penguin so closely resembles other birds, that we may infer that its wings have probably been modified, and reduced by natural selection, in accordance with its sub-aquatic habits. Analogy thus often serves as a guide in distinguishing whether an organ is rudimentary or nascent. I believe the Os coccyx gives attachment to certain muscles, but I can not doubt that it is a rudimentary tail. The bastard wing of birds is a rudimentary digit; and I believe that if fossil birds are found very low down in the series, they will be seen to have a double or bifurcated wing. Here is a bold prophecy!

To admit prophetic germs, is tantamount to rejecting the theory of Natural Selection.

I am very glad you think it worth while to run through my book again, as much, or more, for the subject's sake as for my own sake. But I look at your keeping the subject for some little time before your mind — raising your own difficulties and solving them — as far more important than reading my book. If you think enough, I expect you will be perverted, and if you ever are, I shall know that the theory of Natural Selection, is, in the main, safe; that it includes, as now put forth, many errors, is almost certain, though I cannot see them. Do not, of course, think of answering this; but if you have other OCCASION to write again, just say whether I have, in ever so slight a degree, shaken any of your objections. Farewell. With my cordial thanks for your long letters and valuable remarks,

Believe me, yours most truly, C. DARWIN.

P.S. — You often allude to Lamarck's work; I do not know what you think about it, but it appeared to me extremely poor; I got not a fact or idea from it.

CHARLES DARWIN TO L. AGASSIZ. (Jean Louis Rodolphe Agassiz, born at Mortier, on the lake of Morat in Switzerland, on May 28, 1807. He emigrated to America in 1846, where he spent the rest of his life, and died December 14, 1873. His 'Life,' written by his widow, was published in 1885. The following extract from a letter to Agassiz (1850) is worth giving, as showing how my father regarded him, and it may be added that his cordial feelings towards the great American naturalist remained strong to the end of his life: —

"I have seldom been more deeply gratified than by receiving your most kind present of 'Lake Superior.' I had heard of it, and had much wished to read it, but I confess that it was the very great honour of having in my possession a work with your autograph as a presentation copy that has given me such lively and sincere pleasure. I cordially thank you for it. I have begun to read it with uncommon interest, which I see will increase as I go on.") Down, November 11th [1859].

My dear Sir,

I have ventured to send you a copy of my book (as yet only an abstract) on the 'Origin of Species.' As the conclusions at which I have arrived on several points differ so widely from yours, I have thought (should you at any time read my volume) that you might think that I had sent it to you out of a spirit of defiance or bravado; but I assure you that I act under a wholly different frame of mind. I hope that you will at least give me credit, however erroneous you may think my conclusions, for having earnestly endeavoured to arrive at the truth. With sincere respect, I beg leave to remain,

Yours, very faithfully, CHARLES DARWIN.

CHARLES DARWIN TO A. DE CANDOLLE. Down, November 11th [1859].

Dear Sir,

I have thought that you would permit me to send you (by Messrs. Williams and Norgate, booksellers) a copy of my work (as yet only an abstract) on the 'Origin of Species.' I wish to do this, as the only, though quite inadequate manner, by which I can testify to you the extreme interest which I have felt, and the great advantage which I have derived, from studying your grand and noble work on Geographical Distribution. Should you be induced to read my volume, I venture to remark that it will be intelligible only by reading the whole straight through, as it is very much condensed. It would be a high gratification to me if any portion interested you. But I am perfectly well aware that you will entirely disagree with the conclusion at which I have arrived.

You will probably have quite forgotten me; but many years ago you did me the honour of dining at my house in London to meet M. and Madame Sismondi (Jessie Allen, sister of Mrs. Josiah Wedgwood of Maer.), the uncle and aunt of my wife. With sincere respect, I beg to remain,

Yours, very faithfully, CHARLES DARWIN.

CHARLES DARWIN TO HUGH FALCONER. Down, November 11th [1859].

My dear Falconer,

I have told Murray to send you a copy of my book on the 'Origin of Species,' which as yet is only an abstract.

If you read it, you must read it straight through, otherwise from its extremely condensed state it will be unintelligible.

Lord, how savage you will be, if you read it, and how you will long to crucify me alive! I fear it will produce no other effect on you; but if it should stagger you in ever so slight a degree, in this case, I am fully convinced that you will become, year after year, less fixed in your belief in the immutability of species. With this audacious and presumptuous conviction,

I remain, my dear Falconer, Yours most truly, CHARLES DARWIN.

CHARLES DARWIN TO ASA GRAY. Down, November 11th [1859].

My dear Gray,

I have directed a copy of my book (as yet only an abstract) on the 'Origin of Species' to be sent you. I know how you are pressed for time; but if you can read it, I shall be infinitely gratified...If ever you do read it, and can screw out time to send me (as I value your opinion so highly), however short a note, telling me what you think its weakest and best parts, I should be extremely grateful. As you are not a geologist, you will excuse my conceit in telling you that Lyell highly approves of the two Geological chapters, and thinks that on the Imperfection of the Geological Record not exaggerated. He is nearly a convert to my views...

Let me add I fully admit that there are very many difficulties not satisfactorily explained by my theory of descent with modification, but I cannot possibly believe that a false theory would explain so many classes of facts as I think it certainly does explain. On these grounds I drop my anchor, and believe that the difficulties will slowly disappear...

CHARLES DARWIN TO J.S. HENSLOW. Down, November 11th, 1859.

My dear Henslow,

I have told Murray to send a copy of my book on Species to you, my dear old master in Natural History; I fear, however, that you will not approve of your pupil in this case. The book in its present state does not show the amount of labour which I have bestowed on the subject.

If you have time to read it carefully, and would take the trouble to point out what parts seem weakest to you and what best, it would be a most material aid to me in writing my bigger book, which I hope to commence in a few months. You know also how highly I value your judgment. But I am not so unreasonable as to wish or expect you to write detailed and lengthy criticisms, but merely a few general remarks, pointing out the weakest parts.

If you are IN EVEN SO SLIGHT A DEGREE staggered (which I hardly expect) on the immutability of species, then I am convinced with further reflection you will become more and more staggered, for this has been the process through which my mind has gone. My dear Henslow, Yours affectionately and gratefully, C. DARWIN.

CHARLES DARWIN TO JOHN LUBBOCK. (The present Sir John Lubbock.) Ilkley, Yorkshire, Saturday [November 12th, 1859].

... Thank you much for asking me to Brighton. I hope much that you will enjoy your holiday. I have told Murray to send a copy for you to Mansion House Street, and I am surprised that you have not received it. There are so many valid and weighty arguments against my notions, that you, or any one, if you wish on the other side, will easily persuade yourself that I am wholly in error, and no doubt I am in part in error, perhaps wholly so, though I cannot see the blindness of my ways. I dare say when thunder and lightning were first proved to be due to secondary causes, some regretted to give up the idea that each flash was caused by the direct hand of God.

Farewell, I am feeling very unwell to-day, so no more.

Yours very truly, C. DARWIN.

CHARLES DARWIN TO JOHN LUBBOCK. Ilkley, Yorkshire, Tuesday [November 15th, 1859].

My dear Lubbock,

I beg pardon for troubling you again. I do not know how I blundered in expressing myself in making you believe that we accepted your kind invitation to Brighton. I meant merely to thank you sincerely for wishing to see such a worn-out old dog as myself. I hardly know when we leave this place, — not under a fortnight, and then we shall wish to rest under our own roof-tree.

I do not think I hardly ever admired a book more than Paley's 'Natural Theology.' I could almost formerly have said it by heart.

I am glad you have got my book, but I fear that you value it far too highly. I should be grateful for any criticisms. I care not for Reviews; but for the opinion of men like you and Hooker and Huxley and Lyell, etc.

Farewell, with our joint thanks to Mrs. Lubbock and yourself. Adios.

C. DARWIN.

CHARLES DARWIN TO L. JENYNS. (Now Rev. L. Blomefield.) Ilkley, Yorkshire, November 13th, 1859.

My dear Jenyns,

I must thank you for your very kind note forwarded to me from Down. I have been much out of health this summer, and have been hydropathising here for the last six weeks with very little good as yet. I shall stay here for another fortnight at least. Please remember that my book is only an abstract, and very much condensed, and, to be at all intelligible, must be carefully read. I shall be very grateful for any criticisms. But I know perfectly well that you will not at all agree with the lengths which I go. It took long years to convert me. I may, of course, be egregiously wrong; but I cannot persuade myself that a theory which explains (as I think it certainly does) several large classes of facts, can be wholly wrong; notwithstanding the several difficulties which have to be surmounted somehow, and which stagger me even to this day.

I wish that my health had allowed me to publish in extenso; if ever I get strong enough I will do so, as the greater part is written out, and of which MS. the present volume is an abstract.

I fear this note will be almost illegible; but I am poorly, and can hardly sit up. Farewell; with thanks for your kind note and pleasant remembrance of good old days.

Yours very sincerely, C. DARWIN.

CHARLES DARWIN TO A.R. WALLACE. Ilkley, November 13th, 1859.

My dear Sir,

I have told Murray to send you by post (if possible) a copy of my book, and I hope that you will receive it at nearly the same time with this note. (N.B. I have got a bad finger, which makes me write extra badly.) If you are so inclined, I should very much like to hear your general impression of the book, as you have thought so profoundly on the subject, and in so nearly the same channel with myself. I hope there will be some little new to you, but I fear not much. Remember it is only an abstract, and very much condensed. God knows what the public will think. No one has read it, except Lyell, with whom I have had much correspondence. Hooker thinks him a complete convert, but he does not seem so in his letters to me; but is evidently deeply interested in the subject. I do not think your share in the theory will be overlooked by the real judges, as Hooker, Lyell, Asa Gray, etc. I have heard from Mr. Slater that your paper on the Malay Archipelago has been read at the Linnean Society, and that he was EXTREMELY much interested by it.

I have not seen one naturalist for six or nine months, owing to the state of my health, and therefore I really have no news to tell you. I am writing this at Ilkley Wells, where I have been with my family for the last six weeks, and shall stay for some few weeks longer. As yet I have profited very little. God knows when I shall have strength for my bigger book.

I sincerely hope that you keep your health; I suppose that you will be thinking of returning (Mr. Wallace was in the Malay Archipelago.) soon with your magnificent collections, and still grander mental materials. You will be puzzled how to publish. The Royal Society fund will be worth your consideration. With every good wish, pray believe me,

Yours very sincerely, CHARLES DARWIN.

P.S. I think that I told you before that Hooker is a complete convert. If I can convert Huxley I shall be content.

CHARLES DARWIN TO W.D. FOX. Ilkley, Yorkshire, Wednesday [November 16th, 1859].

... I like the place very much, and the children have enjoyed it much, and it has done my wife good. It did H. good at first, but she has gone back again. I have had a series of calamities; first a sprained ankle, and then a badly swollen whole leg and face, much rash, and a frightful succession of boils — four or five at once. I have felt quite ill, and have little faith in this "unique crisis," as the doctor calls it, doing me much good...You will probably have received, or will very soon receive, my weariful book on species, I naturally believe it mainly includes the truth, but you will not at all agree with me. Dr. Hooker, whom I consider one of the best judges in Europe, is a complete convert, and he thinks Lyell is likewise; certainly, judging from Lyell's letters to me on the subject, he is deeply staggered. Farewell. If the spirit moves you, let me have a line...

CHARLES DARWIN TO W.B. CARPENTER. Ilkley, Yorkshire, November 18th [1859].

My dear Carpenter,

I must thank you for your letter on my own account, and if I know myself, still more warmly for the subject's sake. As you seem to have understood my last chapter without reading the previous chapters, you must have maturely and most profoundly self-thought out the subject; for I have found the most extraordinary difficulty in making even able men understand at what I was driving. There will be strong opposition to my views. If I am in the main right (of course including partial errors unseen by me), the admission in my views will depend far more on men, like yourself, with well-established reputations, than on my own writings. Therefore, on the supposition that when you have read my volume you think the view in the main true, I thank and honour you for being willing to run the chance of unpopularity by advocating the view. I know not in the least whether any one will review me in any of the Reviews. I do not see how an author could enquire or interfere; but if you are willing to review me anywhere, I am sure from the admiration which I have long felt and expressed for your 'Comparative Physiology,' that your review will be excellently done, and will do good service in the cause for which I think I am not selfishly deeply interested. I am feeling very unwell to-day, and this note is badly, perhaps hardly intelligibly, expressed; but you must excuse me, for I could not let a post pass, without thanking you for your note. You will have a tough job even to shake in the slightest degree Sir H. Holland. I do not think (privately I say it) that the great man has knowledge enough to enter on the subject. Pray believe me with sincerity, Yours truly obliged,

C. DARWIN.

P.S. — As you are not a practical geologist, let me add that Lyell thinks the chapter on the Imperfection of the Geological Record NOT exaggerated.

CHARLES DARWIN TO W.B. CARPENTER. Ilkley, Yorkshire, November 19th [1859].

My dear Carpenter,

I beg pardon for troubling you again. If, after reading my book, you are able to come to a conclusion in any degree definite, will you think me very unreasonable in asking you to let me hear from you. I do not ask for a long discussion, but merely for a brief idea of your general impression. From your widely extended knowledge, habit of investigating the truth, and abilities, I should value your opinion in the very highest rank. Though I, of course, believe in the truth of my own doctrine, I suspect that no belief is vivid until shared by others. As yet I know only one believer, but I look at him as of the greatest authority, viz., Hooker. When I think of the many cases of men who have studied one subject for years, and have persuaded themselves of the truth of the foolishest doctrines, I feel sometimes a little frightened, whether I may not be one of these mon-maniacs.

Again pray excuse this, I fear, unreasonable request. A short note would suffice, and I could bear a hostile verdict, and shall have to bear many a one.

Yours very sincerely, C. DARWIN.

CHARLES DARWIN TO J.D. HOOKER. Ilkley, Yorkshire, Sunday [November 1859].

My dear Hooker,

I have just read a review on my book in the "Athenaeum" (November 19, 1859.), and it excites my curiosity much who is the author. If you should hear who writes in the "Athenaeum" I wish you would tell me. It seems to me well done, but the reviewer gives no new objections, and, being hostile, passes over every single argument in favour of the doctrine... I fear from the tone of the review, that I have written in a conceited and cocksure style (The Reviewer speaks of the author's "evident self-satisfaction," and of his disposing of all difficulties "more or less confidently."), which shames me a little. There is another review of which I should like to know the author, viz., of H.C. Watson in the "Gardener's Chronicle". Some of the remarks are like yours, and he does deserve punishment; but surely the review is too severe. Don't you think so?

I hope you got the three copies for Foreign Botanists in time for your parcel, and your own copy. I have heard from Carpenter, who, I think, is likely to be a convert. Also from Quatrefages, who is inclined to go a long way with us. He says that he exhibited in his lecture a diagram closely like mine!

I shall stay here one fortnight more, and then go to Down, staying on the road at Shrewsbury a week. I have been very unfortunate: out of seven weeks I have been confined for five to the house. This has been bad for me, as I have not been able to help thinking to a foolish extent about my book. If some four or five GOOD men came round nearly to our view, I shall not fear ultimate success. I long to learn what Huxley thinks. Is your introduction (Introduction to the 'Flora of Australia.') published? I suppose that you will sell it separately. Please answer this, for I want an extra copy to send away to Wallace. I am very bothersome, farewell.

Yours affectionately, C. DARWIN.

I was very glad to see the Royal Medal for Mr. Bentham.

CHARLES DARWIN TO J.D. HOOKER. Down, December 21st, 1859.

My dear Hooker,

Pray give my thanks to Mrs. Hooker for her extremely kind note, which has pleased me much. We are very sorry she cannot come here, but shall be delighted to see you and W. (our boys will be at home) here in the 2nd week of January, or any other time. I shall much enjoy discussing any points in my book with you...

I hate to hear you abuse your own work. I, on the contrary, so sincerely value all that you have written. It is an old and firm conviction of mine, that the Naturalists who accumulate facts and make many partial generalisations are the REAL benefactors of science. Those who merely accumulate facts I cannot very much respect.

I had hoped to have come up for the Club to-morrow, but very much doubt whether I shall be able. Ilkley seems to have done me no essential good. I attended the Bench on Monday, and was detained in adjudicating some troublesome cases 1 1/2 hours longer than usual, and came home utterly knocked up, and cannot rally. I am not worth an old button... Many thanks for your pleasant note.

Ever yours, C. DARWIN.

P.S. — I feel confident that for the future progress of the subject of the origin and manner of formation of species, the assent and arguments and facts of working naturalists, like yourself, are far more important than my own book; so for God's sake do not abuse your Introduction.

H.C. WATSON TO CHARLES DARWIN. Thames Ditton, November 21st [1859].

My dear Sir,

Once commenced to read the 'Origin,' I could not rest till I had galloped through the whole. I shall now begin to re-read it more deliberately. Meantime I am tempted to write you the first impressions, not doubting that they will, in the main, be the permanent impressions: —

1st. Your leading idea will assuredly become recognised as an established truth in science, i.e. "Natural Selection." It has the characteristics of all great natural truths, clarifying what was obscure, simplifying what was intricate, adding greatly to previous knowledge. You are the greatest revolutionist in natural history of this century, if not of all centuries.

2nd. You will perhaps need, in some degree, to limit or modify, possibly in some degree also to extend, your present applications of the principle of natural selection. Without going to matters of more detail, it strikes me that there is one considerable primary inconsistency, by one failure in the analogy between varieties and species; another by a sort of barrier assumed for nature on insufficient grounds and arising from "divergence." These may, however, be faults in my own mind, attributable to yet incomplete perception of your views. And I had better not trouble you about them before again reading the volume.

3rd. Now these novel views are brought fairly before the scientific public, it seems truly remarkable how so many of them could have failed to see their right road sooner. How could Sir C. Lyell, for instance, for thirty years read, write, and think, on the subject of species AND THEIR SUCCESSION, and yet constantly look down the wrong road!

A quarter of a century ago, you and I must have been in something like the same state of mind on the main question, but you were able to see and work out the quo modo of the succession, the all-important thing, while I failed to grasp it. I send by this post a little controversial pamphlet of old date — Combe and Scott. If you will take the trouble to glance at the passages scored on the margin, you will see that, a quarter of a century ago, I was also one of the few who then doubted the absolute distinctness of species, and special creations of them. Yet I, like the rest, failed to detect the quo modo which was reserved for your penetration to DISCOVER, and your discernment to APPLY.

You answered my query about the hiatus between Satyrus and Homo as was expected. The obvious explanation really never occurred to me till some months after I had read the papers in the 'Linnean Proceedings.' The first species of Fere-homo ("Almost-man.") would soon make direct and exterminating war upon his Infra-homo cousins. The gap would thus be made, and then go on increasing, into the present enormous and still widening hiatus. But how greatly this, with your chronology of animal life, will shock the ideas of many men!

Very sincerely, HEWETT C. WATSON.

J.D. HOOKER TO CHARLES DARWIN. Athenaeum, Monday [November 21st, 1859].

My dear Darwin,

I am a sinner not to have written you ere this, if only to thank you for your glorious book — what a mass of close reasoning on curious facts and fresh phenomena — it is capitally written, and will be very successful. I say this on the strength of two or three plunges into as many chapters, for I have not yet attempted to read it. Lyell, with whom we are staying, is perfectly enchanted, and is absolutely gloating over it. I must accept your compliment to me, and acknowledgment of supposed assistance from me, as the warm tribute of affection from an honest (though deluded) man, and furthermore accept it as very pleasing to my vanity; but, my dear fellow, neither my name nor my judgment nor my assistance deserved any such compliments, and if I am dishonest enough to be pleased with what I don't deserve, it must just pass. How different the BOOK reads from the MS. I see I shall have much to talk over with you. Those lazy printers have not finished my luckless Essay; which, beside your book, will look like a ragged handkerchief beside a Royal Standard...

All well, ever yours affectionately, JOS. D. HOOKER.

CHARLES DARWIN TO J.D. HOOKER. Ilkley, Yorkshire [November 1859].

My dear Hooker,

I cannot help it, I must thank you for your affectionate and most kind note. My head will be turned. By Jove, I must try and get a bit modest. I was a little chagrined by the review. (This refers to the review in the "Athenaeum", November 19, 1859, where the reviewer, after touching on the theological aspects of the book, leaves the author to "the mercies of the Divinity Hall, the College, the Lecture Room, and the Museum.") I hope it was NOT — . As advocate, he might think himself justified in giving the argument only on one side. But the manner in which he drags in immortality, and sets the priests at me, and leaves me to their mercies, is base. He would, on no account, burn me, but he will get the wood ready, and tell the black beasts how to catch me... It would be unspeakably grand if Huxley were to lecture on the subject, but I can see this is a mere chance; Faraday might think it too unorthodox.

... I had a letter from [Huxley] with such tremendous praise of my book, that modesty (as I am trying to cultivate that difficult herb) prevents me sending it to you, which I should have liked to have done, as he is very modest about himself.

You have cockered me up to that extent, that I now feel I can face a score of savage reviewers. I suppose you are still with the Lyells. Give my kindest remembrance to them. I triumph to hear that he continues to approve.

Believe me, your would-be modest friend, C.D.

CHARLES DARWIN TO C. LYELL. Ilkley Wells, Yorkshire, November 23 [1859].

My dear Lyell,

You seemed to have worked admirably on the species question; there could not have been a better plan than reading up on the opposite side. I rejoice profoundly that you intend admitting the doctrine of modification in your new edition (It appears from Sir Charles Lyell's published letters that he intended to admit the doctrine of evolution in a new edition of the 'Manual,' but this was not published till 1865. He was, however, at work on the 'Antiquity of Man' in 1860, and had already determined to discuss the 'Origin' at the end of the book.); nothing, I am convinced, could be more important for its success. I honour you most sincerely. To have maintained in the position of a master, one side of a question for thirty years, and then deliberately give it up, is a fact to which I much doubt whether the records of science offer a parallel. For myself, also, I rejoice profoundly; for, thinking of so many cases of men pursuing an illusion for years, often and often a cold shudder has run through me, and I have asked myself whether I may not have devoted my life to a phantasy. Now I look at it as morally impossible that investigators of truth, like you and Hooker, can be wholly wrong, and therefore I rest in peace. Thank you for criticisms, which, if there be a second edition, I will attend to. I have been thinking that if I am much execrated as an atheist, etc., whether the admission of the doctrine of natural selection could injure your works; but I hope and think not, for as far as I can remember, the virulence of bigotry is expended on the first offender, and those who adopt his views are only pitied as deluded, by the wise and cheerful bigots.

I cannot help thinking that you overrate the importance of the multiple origin of dogs. The only difference is, that in the case of single origins, all difference of the races has originated since man domesticated the species. In the case of multiple origins part of the difference was produced under natural conditions. I should INFINITELY prefer the theory of single origin in all cases, if facts would permit its reception. But there seems to me some a priori improbability (seeing how fond savages are of taming animals), that throughout all times, and throughout all the world, that man should have domesticated one single species alone, of the widely distributed genus Canis. Besides this, the close resemblance of at least three kinds of American domestic dogs to wild species still inhabiting the countries where they are now domesticated, seem to almost compel admission that more than one wild Canis has been domesticated by man.

I thank you cordially for all the generous zeal and interest you have shown about my book, and I remain, my dear Lyell,

Your affectionate friend and disciple, CHARLES DARWIN.

Sir J. Herschel, to whom I sent a copy, is going to read my book. He says he leans to the side opposed to me. If you should meet him after he has read me, pray find out what he thinks, for, of course, he will not write; and I should excessively like to hear whether I produce any effect on such a mind.

T.H. HUXLEY TO CHARLES DARWIN. Jermyn Street W., November 23rd, 1859.

My dear Darwin,

I finished your book yesterday, a lucky examination having furnished me with a few hours of continuous leisure.

Since I read Von Baer's (Karl Ernst von Baer, born 1792, died at Dorpat 1876 — one of the most distinguished biologists of the century. He practically founded the modern science of embryology.) essays, nine years ago, no work on Natural History Science I have met with has made so great an impression upon me, and I do most heartily thank you for the great store of new views you have given me. Nothing, I think, can be better than the tone of the book, it impresses those who know nothing about the subject. As for your doctrine, I am prepared to go to the stake, if requisite, in support of Chapter IX., and most parts of Chapters X., XI., XII., and Chapter XIII. contains much that is most admirable, but on one or two points I enter a caveat until I can see further into all sides of the question.

As to the first four chapters, I agree thoroughly and fully with all the principles laid down in them. I think you have demonstrated a true cause for the production of species, and have thrown the onus probandi that species did not arise in the way you suppose, on your adversaries.

But I feel that I have not yet by any means fully realized the bearings of those most remarkable and original Chapters III., IV. and V., and I will write no more about them just now.

The only objections that have occurred to me are, 1st that you have loaded yourself with an unnecessary difficulty in adopting Natura non facit saltum so unreservedly... And 2nd, it is not clear to me why, if continual physical conditions are of so little moment as you suppose, variation should occur at all.

However, I must read the book two or three times more before I presume to begin picking holes.

I trust you will not allow yourself to be in any way disgusted or annoyed by the considerable abuse and misrepresentation which, unless I greatly mistake, is in store for you. Depend upon it you have earned the lasting gratitude of all thoughtful men. And as to the curs which will bark and yelp, you must recollect that some of your friends, at any rate, are endowed with an amount of combativeness which (though you have often and justly rebuked it) may stand you in good stead.

I am sharpening up my claws and beak in readiness.

Looking back over my letter, it really expresses so feebly all I think about you and your noble book that I am half ashamed of it; but you will understand that, like the parrot in the story, "I think the more."

Ever yours faithfully, T.H. HUXLEY.

CHARLES DARWIN TO T.H. HUXLEY. Ilkley, November 25th [1859].

My dear Huxley,

Your letter has been forwarded to me from Down. Like a good Catholic who has received extreme unction, I can now sing "nunc dimittis." I should have been more than contented with one quarter of what you have said. Exactly fifteen months ago, when I put pen to paper for this volume, I had awful misgivings; and thought perhaps I had deluded myself, like so many have done, and I then fixed in my mind three judges, on whose decision I determined mentally to abide. The judges were Lyell, Hooker, and yourself. It was this which made me so excessively anxious for your verdict. I am now contented, and can sing my nunc dimittis. What a joke it would be if I pat you on the back when you attack some immovable creationist! You have most cleverly hit on one point, which has greatly troubled me; if, as I must think, external conditions produce little DIRECT effect, what the devil determines each particular variation? What makes a tuft of feathers come on a cock's head, or moss on a moss-rose? I shall much like to talk over this with you...

My dear Huxley, I thank you cordially for your letter.

Yours very sincerely, C. DARWIN.

P.S. — Hereafter I shall be particularly curious to hear what you think of my explanation of Embryological similarity. On classification I fear we shall split. Did you perceive the argumentum ad hominem Huxley about kangaroo and bear?

ERASMUS DARWIN (His brother.) TO CHARLES DARWIN. November 23rd [1859].

Dear Charles,

I am so much weaker in the head, that I hardly know if I can write, but at all events I will jot down a few things that the Dr. (Dr., afterwards Sir Henry Holland.) has said. He has not read much above half, so as he says he can give no definite conclusion, and it is my private belief he wishes to remain in that state... He is evidently in a dreadful state of indecision, and keeps stating that he is not tied down to either view, and that he has always left an escape by the way he has spoken of varieties. I happened to speak of the eye before he had read that part, and it took away his breath — utterly impossible — structure, function, etc., etc., etc., but when he had read it he hummed and hawed, and perhaps it was partly conceivable, and then he fell back on the bones of the ear, which were beyond all probability or conceivability. He mentioned a slight blot, which I also observed, that in speaking of the slave-ants carrying one another, you change the species without giving notice first, and it makes one turn back...

... For myself I really think it is the most interesting book I ever read, and can only compare it to the first knowledge of chemistry, getting into a new world or rather behind the scenes. To me the geographical distribution, I mean the relation of islands to continents, is the most convincing of the proofs, and the relation of the oldest forms to the existing species. I dare say I don't feel enough the absence of varieties, but then I don't in the least know if everything now living were fossilized whether the paleontologists could distinguish them. In fact the a priori reasoning is so entirely satisfactory to me that if the facts won't fit in, why so much the worse for the facts is my feeling. My ague has left me in such a state of torpidity that I wish I had gone through the process of natural selection.

Yours affectionately, E.A.D.

CHARLES DARWIN TO C. LYELL. Ilkley, November [24th, 1859].

My dear Lyell,

Again I have to thank you for a most valuable lot of criticisms in a letter dated 22nd.

This morning I heard also from Murray that he sold the whole edition (First edition, 1250 copies.) the first day to the trade. He wants a new edition instantly, and this utterly confounds me. Now, under water-cure, with all nervous power directed to the skin, I cannot possibly do head-work, and I must make only actually necessary corrections. But I will, as far as I can without my manuscript, take advantage of your suggestions: I must not attempt much. Will you send me one line to say whether I must strike out about the secondary whale (The passage was omitted in the second edition.), it goes to my heart. About the rattle-snake, look to my Journal, under Trigonocephalus, and you will see the probable origin of the rattle, and generally in transitions it is the premier pas qui coute.

Madame Belloc wants to translate my book into French; I have offered to look over proofs for SCIENTIFIC errors. Did you ever hear of her? I believe Murray has agreed at my urgent advice, but I fear I have been rash and premature. Quatrefages has written to me, saying he agrees largely with my views. He is an excellent naturalist. I am pressed for time. Will you give us one line about the whales? Again I thank you for neve-tiring advice and assistance; I do in truth reverence your unselfish and pure love of truth.

My dear Lyell, ever yours, C. DARWIN.

[With regard to a French translation, he wrote to Mr. Murray in November 1859: "I am EXTREMELY anxious, for the subject's sake (and God knows not for mere fame), to have my book translated; and indirectly its being known abroad will do good to the English sale. If it depended on me, I should agree without payment, and instantly send a copy, and only beg that she [Mme. Belloc] would get some scientific man to look over the translation... You might say that, though I am a very poor French scholar, I could detect any scientific mistake, and would read over the French proofs."

The proposed translation was not made, and a second plan fell through in the following year. He wrote to M. de Quatrefages: "The gentleman who wished to translate my 'Origin of Species' has failed in getting a publisher. Balliere, Masson, and Hachette all rejected it with contempt. It was foolish and presumptuous in me, hoping to appear in a French dress; but the idea would not have entered my head had it not been suggested to me. It is a great loss. I must console myself with the German edition which Prof. Bronn is bringing out." (See letters to Bronn, page 70.)

A sentence in another letter to M. de Quatrefages shows how anxious he was to convert one of the greatest of contemporary Zoologists: "How I should like to know whether Milne Edwards had read the copy which I sent him, and whether he thinks I have made a pretty good case on our side of the question. There is no naturalist in the world for whose opinion I have so profound a respect. Of course I am not so silly as to expect to change his opinion."]

CHARLES DARWIN TO C. LYELL. Ilkley, [November 26th, 1859].

My dear Lyell,

I have received your letter of the 24th. It is no use trying to thank you; your kindness is beyond thanks. I will certainly leave out the whale and bear...

The edition was 1250 copies. When I was in spirits, I sometimes fancied that my book would be successful, but I never even built a castle in the air of such success as it has met with; I do not mean the sale, but the impression it has made on you (whom I have always looked at as chief judge) and Hooker and Huxley. The whole has infinitely exceeded my wildest hopes.

Farewell, I am tired, for I have been going over the sheets.

My kind friend, farewell, yours, C. DARWIN.

CHARLES DARWIN TO C. LYELL. Ilkley, Yorkshire, December 2nd [1859].

My dear Lyell,

Every note which you have sent me has interested me much. Pray thank Lady Lyell for her remark. In the chapters she refers to, I was unable to modify the passage in accordance with your suggestion; but in the final chapter I have modified three or four. Kingsley, in a note (The letter is given below) to me, had a capital paragraph on such notions as mine being NOT opposed to a high conception of the Deity. I have inserted it as an extract from a letter to me from a celebrated author and divine. I have put in about nascent organs. I had the greatest difficulty in partially making out Sedgwick's letter, and I dare say I did greatly underrate its clearness. Do what I could, I fear I shall be greatly abused. In answer to Sedgwick's remark that my book would be "mischievous," I asked him whether truth can be known except by being victorious over all attacks. But it is no use. H.C. Watson tells me that one zoologist says he will read my book, "but I will never believe it." What a spirit to read any book in! Crawford writes to me that his notice (John Crawford, orientalist, ethnologist, etc., 1783-1868. The review appeared in the "Examiner", and, though hostile, is free from bigotry, as the following citation will show: "We cannot help saying that piety must be fastidious indeed that objects to a theory the tendency of which is to show that all organic beings, man included, are in a perpetual progress of amelioration, and that is expounded in the reverential language which we have quoted.") will be hostile, but that "he will not calumniate the author." He says he has read my book, "at least such parts as he could understand." He sent me some notes and suggestions (quite unimportant), and they show me that I have unavoidably done harm to the subject, by publishing an abstract. He is a real Pallasian; nearly all our domestic races descended from a multitude of wild species now commingled. I expected Murchison to be outrageous. How little he could ever have grappled with the subject of denudation! How singular so great a geologist should have so unphilosophical a mind! I have had several notes from — , very civil and less decided. Says he shall not pronounce against me without much reflection, PERHAPS WILL SAY NOTHING on the subject. X. says — will go to that part of hell, which Dante tells us is appointed for those who are neither on God's side nor on that of the devil.

I fully believe that I owe the comfort of the next few years of my life to your generous support, and that of a very few others. I do not think I am brave enough to have stood being odious without support; now I feel as bold as a lion. But there is one thing I can see I must learn, viz., to think less of myself and my book. Farewell, with cordial thanks.

Yours most truly, C. DARWIN.

I return home on the 7th, and shall sleep at Erasmus's. I will call on you about ten o'clock, on Thursday, the 8th, and sit with you, as I have so often sat, during your breakfast.

I wish there was any chance of Prestwich being shaken; but I fear he is too much of a catastrophist.

[In December there appeared in 'Macmillan's Magazine' an article, "Time and Life," by Professor Huxley. It is mainly occupied by an analysis of the argument of the 'Origin,' but it also gives the substance of a lecture delivered at the Royal Institution before that book was published. Professor Huxley spoke strongly in favour of evolution in his Lecture, and explains that in so doing he was to a great extent resting on a knowledge of "the general tenor of the researches in which Mr. Darwin had been so long engaged," and was supported in so doing by his perfect confidence in his knowledge, perseverance, and "high-minded love of truth." My father was evidently deeply pleased by Mr. Huxley's words, and wrote:

"I must thank you for your extremely kind notice of my book in 'Macmillan.' No one could receive a more delightful and honourable compliment. I had not heard of your Lecture, owing to my retired life. You attribute much too much to me from our mutual friendship. You have explained my leading idea with admirable clearness. What a gift you have of writing (or more properly) thinking clearly."]

CHARLES DARWIN TO W.B. CARPENTER. Ilkley, Yorkshire, December 3rd [1859].

My dear Carpenter,

I am perfectly delighted at your letter. It is a great thing to have got a great physiologist on our side. I say "our" for we are now a good and compact body of really good men, and mostly not old men. In the long run we shall conquer. I do not like being abused, but I feel that I can now bear it; and, as I told Lyell, I am well convinced that it is the first offender who reaps the rich harvest of abuse. You have done an essential kindness in checking the odium theologicum in the E.R. (This must refer to Carpenter's critique which would now have been ready to appear in the January number of the "Edinburgh Review", 1860, and in which the odium theologicum is referred to.) It much pains all one's female relations and injures the cause.

I look at it as immaterial whether we go quite the same lengths; and I suspect, judging from myself, that you will go further, by thinking of a population of forms like Ornithorhyncus, and by thinking of the common homological and embryological structure of the several vertebrate orders. But this is immaterial. I quite agree that the principle is everything. In my fuller MS. I have discussed a good many instincts; but there will surely be more unfilled gaps here than with corporeal structure, for we have no fossil instincts, and know scarcely any except of European animals. When I reflect how very slowly I came round myself, I am in truth astonished at the candour shown by Lyell, Hooker, Huxley, and yourself. In my opinion it is grand. I thank you cordially for taking the trouble of writing a review for the 'National.' God knows I shall have few enough in any degree favourable. (See a letter to Dr. Carpenter below.)

CHARLES DARWIN TO C. LYELL. Saturday [December 5th, 1859].

... I have had a letter from Carpenter this morning. He reviews me in the 'National.' He is a convert, but does not go quite so far as I, but quite far enough, for he admits that all birds are from one progenitor, and probably all fishes and reptiles from another parent. But the last mouthful chokes him. He can hardly admit all vertebrates from one parent. He will surely come to this from Homology and Embryology. I look at it as grand having brought round a great physiologist, for great I think he certainly is in that line. How curious I shall be to know what line Owen will take; dead against us, I fear; but he wrote me a most liberal note on the reception of my book, and said he was quite prepared to consider fairly and without prejudice my line of argument.

J.D. HOOKER TO CHARLES DARWIN. Kew, Monday.

Dear Darwin,

You have, I know, been drenched with letters since the publication of your book, and I have hence forborne to add my mite. I hope now that you are well through Edition II., and I have heard that you were flourishing in London. I have not yet got half-through the book, not from want of will, but of time — for it is the very hardest book to read, to full profits, that I ever tried — it is so cram-full of matter and reasoning. I am all the more glad that you have published in this form, for the three volumes, unprefaced by this, would have choked any Naturalist of the nineteenth century, and certainly have softened my brain in the operation of assimilating their contents. I am perfectly tired of marvelling at the wonderful amount of facts you have brought to bear, and your skill in marshalling them and throwing them on the enemy; it is also extremely clear as far as I have gone, but very hard to fully appreciate. Somehow it reads very different from the MS., and I often fancy I must have been very stupid not to have more fully followed it in MS. Lyell told me of his criticisms. I did not appreciate them all, and there are many little matters I hope one day to talk over with you. I saw a highly flattering notice in the 'English Churchman,' short and not at all entering into discussion, but praising you and your book, and talking patronizingly of the doctrine!.. Bentham and Henslow will still shake their heads I fancy...

Ever yours affectionately, JOS. D. HOOKER.

CHARLES DARWIN TO C. LYELL. Down, Saturday [December 12th, 1859].

... I had very long interviews with — , which perhaps you would like to hear about... I infer from several expressions that, at bottom, he goes an immense way with us...

He said to the effect that my explanation was the best ever published of the manner of formation of species. I said I was very glad to hear it. He took me up short: "You must not at all suppose that I agree with you in all respects." I said I thought it no more likely that I should be right in nearly all points, than that I should toss up a penny and get heads twenty times running. I asked him what he thought the weakest part. He said he had no particular objection to any part. He added: —

"If I must criticise, I should say, 'we do not want to know what Darwin believes and is convinced of, but what he can prove.'" I agreed most fully and truly that I have probably greatly sinned in this line, and defended my general line of argument of inventing a theory and seeing how many classes of facts the theory would explain. I added that I would endeavour to modify the "believes" and "convinceds." He took me up short: "You will then spoil your book, the charm of (!) it is that it is Darwin himself." He added another objection, that the book was too teres atque rotundus — that it explained everything, and that it was improbable in the highest degree that I should succeed in this. I quite agree with this rather queer objection, and it comes to this that my book must be very bad or very good...

I have heard, by roundabout channel, that Herschel says my book "is the law of higgledy-piggledy." What this exactly means I do not know, but it is evidently very contemptuous. If true this is a great blow and discouragement.

CHARLES DARWIN TO JOHN LUBBOCK. December 14th [1859].

... The latter part of my stay at Ilkley did me much good, but I suppose I never shall be strong, for the work I have had since I came back has knocked me up a little more than once. I have been busy in getting a reprint (with a very few corrections) through the press.

My book has been as yet VERY MUCH more successful than I ever dreamed of: Murray is now printing 3000 copies. Have you finished it? If so, pray tell me whether you are with me on the GENERAL issue, or against me. If you are against me, I know well how honourable, fair, and candid an opponent I shall have, and which is a good deal more than I can say of all my opponents...

Pray tell me what you have been doing. Have you had time for any Natural History?..

P.S. — I have got — I wish and hope I might say that WE have got — a fair number of excellent men on our side of the question on the mutability of species.

CHARLES DARWIN TO J.D. HOOKER. Down, December 14th [1859].

My dear Hooker,

Your approval of my book, for many reasons, gives me intense satisfaction; but I must make some allowance for your kindness and sympathy. Any one with ordinary faculties, if he had PATIENCE enough and plenty of time, could have written my book. You do not know how I admire your and Lyell's generous and unselfish sympathy, I do not believe either of you would have cared so much about your own work. My book, as yet, has been far more successful than I ever even formerly ventured in the wildest day-dreams to anticipate. We shall soon be a good body of working men, and shall have, I am convinced, all young and rising naturalists on our side. I shall be intensely interested to hear whether my book produces any effect on A. Gray; from what I heard at Lyell's, I fancy your correspondence has brought him some way already. I fear that there is no chance of Bentham being staggered. Will he read my book? Has he a copy? I would send him one of the reprints if he has not. Old J.E. Gray (John Edward Gray (1800-1875), was the son of S.F. Gray, author of the 'Supplement to the Pharmacopoeia.' In 1821 he published in his father's name 'The Natural Arrangement of British Plants,' one of the earliest works in English on the natural method. In 1824 he became connected with the Natural History Department of the British Museum, and was appointed Keeper of the Zoological collections in 1840. He was the author of 'Illustrations of Indian Zoology,' 'The Knowsley Menagerie,' etc., and of innumerable descriptive Zoological papers.), at the British Museum, attacked me in fine style: "You have just reproduced Lamarck's doctrine and nothing else, and here Lyell and others have been attacking him for twenty years, and because YOU (with a sneer and laugh) say the very same thing, they are all coming round; it is the most ridiculous inconsistency, etc., etc."

You must be very glad to be settled in your house, and I hope all the improvements satisfy you. As far as my experience goes, improvements are never perfection. I am very sorry to hear that you are still so very busy, and have so much work. And now for the main purport of my note, which is to ask and beg you and Mrs. Hooker (whom it is really an age since I have seen), and all your children, if you like, to come and spend a week here. It would be a great pleasure to me and to my wife... As far as we can see, we shall be at home all the winter; and all times probably would be equally convenient; but if you can, do not put it off very late, as it may slip through. Think of this and persuade Mrs. Hooker, and be a good man and come.

Farewell, my kind and dear friend, Yours affectionately, C. DARWIN.

P.S. — I shall be very curious to hear what you think of my discussion on Classification in Chapter XIII.; I believe Huxley demurs to the whole, and says he has nailed his colours to the mast, and I would sooner die than give up; so that we are in as fine a frame of mind to discuss the point as any two religionists.

Embryology is my pet bit in my book, and, confound my friends, not one has noticed this to me.

CHARLES DARWIN TO ASA GRAY. Down, December 21st [1859].

My dear Gray,

I have just received your most kind, long, and valuable letter. I will write again in a few days, for I am at present unwell and much pressed with business: to-day's note is merely personal. I should, for several reasons, be very glad of an American Edition. I have made up my mind to be well abused; but I think it of importance that my notions should be read by intelligent men, accustomed to scientific argument, though NOT naturalists. It may seem absurd, but I think such men will drag after them those naturalists who have too firmly fixed in their heads that a species is an entity. The first edition of 1250 copies was sold on the first day, and now my publisher is printing off, as RAPIDLY AS POSSIBLE, 3000 more copies. I mention this solely because it renders probable a remunerative sale in America. I should be infinitely obliged if you could aid an American reprint; and could make, for my sake and the publisher's, any arrangement for any profit. The new edition is only a reprint, yet I have made a FEW important corrections. I will have the clean sheets sent over in a few days of as many sheets as are printed off, and the remainder afterwards, and you can do anything you like, — if nothing, there is no harm done. I should be glad for the new edition to be reprinted and not the old. — In great haste, and with hearty thanks,

Yours very sincerely, C. DARWIN.

I will write soon again.

CHARLES DARWIN TO C. LYELL. Down, 22nd [December, 1859].

My dear Lyell, Thanks about "Bears" (See 'Origin,' edition i., page 184.), a word of il-omen to me.

I am too unwell to leave home, so shall not see you.

I am very glad of your remarks on Hooker. (Sir C. Lyell wrote to Sir J.D. Hooker, December 19, 1859 ('Life,' ii. page 327): "I have just finished the reading of your splendid Essay [the 'Flora of Australia'] on the origin of species, as illustrated by your wide botanical experience, and think it goes very far to raise the variety-making hypothesis to the rank of a theory, as accounting for the manner in which new species enter the world.") I have not yet got the essay. The parts which I read in sheets seemed to me grand, especially the generalization about the Australian flora itself. How superior to Robert Brown's celebrated essay! I have not seen Naudin's paper ('Revue Horticole,' 1852. See historical Sketch in the later editions of the 'Origin of Species.'), and shall not be able till I hunt the libraries. I am very anxious to see it. Decaisne seems to think he gives my whole theory. I do not know when I shall have time and strength to grapple with Hooker...

P.S. — I have heard from Sir W. Jardine (Jardine, Sir William, Bart., 1800-1874), was the son of Sir A. Jardine of Applegarth, Dumfriesshire. He was educated at Edinburgh, and succeeded to the title on his father's decease in 1821. He published, jointly with Mr. Prideaux, J. Selby, Sir Stamford Raffles, Dr. Horsfield, and other ornithologists, 'Illustrations of Ornithology,' and edited the 'Naturalist's Library,' in 40 volumes, which included the four branches: Mammalia, Ornithology, Ichnology, and Entomology. Of these 40 volumes 14 were written by himself. In 1836 he became editor of the 'Magazine of Zoology and Botany,' which, two years later, was transformed into 'Annals of Natural History,' but remained under his direction. For Bohn's Standard Library he edited White's 'Natural History of Selborne.' Sir W. Jardine was also joint editor of the 'Edinburgh Philosophical Journal,' and was author of 'British Salmonidae,' 'Ichthyology of Annandale,' 'Memoirs of the late Hugh Strickland,' 'Contributions to Ornithology,' 'Ornithological Synonyms,' etc. — (Taken from Ward, 'Men of the Reign,' and Cates, 'Dictionary of General Biography.'): his criticisms are quite unimportant; some of the Galapagos so-called species ought to be called varieties, which I fully expected; some of the sub-genera, thought to be wholly endemic, have been found on the Continent (not that he gives his authority), but I do not make out that the species are the same. His letter is brief and vague, but he says he will write again.

CHARLES DARWIN TO J.D. HOOKER. Down [23rd December, 1859].

My dear Hooker,

I received last night your 'Introduction,' for which very many thanks; I am surprised to see how big it is: I shall not be able to read it very soon. It was very good of you to send Naudin, for I was very curious to see it. I am surprised that Decaisne should say it was the same as mine. Naudin gives artificial selection, as well as a score of English writers, and when he says species were formed in the same manner, I thought the paper would certainly prove exactly the same as mine. But I cannot find one word like the struggle for existence and natural selection. On the contrary, he brings in his principle (page 103) of finality (which I do not understand), which, he says, with some authors is fatality, with others providence, and which adapts the forms of every being, and harmonises them all throughout nature.

He assumes like old geologists (who assumed that the forces of nature were formerly greater), that species were at first more plastic. His simile of tree and classification is like mine (and others), but he cannot, I think, have reflected much on the subject, otherwise he would see that genealogy by itself does not give classification; I declare I cannot see a MUCH closer approach to Wallace and me in Naudin than in Lamarck — we all agree in modification and descent. If I do not hear from you I will return the 'Revue' in a few days (with the cover). I dare say Lyell would be glad to see it. By the way, I will retain the volume till I hear whether I shall or not send it to Lyell. I should rather like Lyell to see this note, though it is foolish work sticking up for independence or priority.

Ever yours, C. DARWIN.

A. SEDGWICK (Rev. Adam Sedgwick, 1785-1873, Woodwardian Professor of Geology in the University of Cambridge.) TO CHARLES DARWIN. Cambridge, December 24th, [1859].

My dear Darwin,

I write to thank you for your work on the 'Origin of Species.' It came, I think, in the latter part of last week; but it MAY have come a few days sooner, and been overlooked among my book-parcels, which often remain unopened when I am lazy or busy with any work before me. So soon as I opened it I began to read it, and I finished it, after many interruptions, on Tuesday. Yesterday I was employed — 1st, in preparing for my lecture; 2ndly, in attending a meeting of my brother Fellows to discuss the final propositions of the Parliamentary Commissioners; 3rdly, in lecturing; 4thly, in hearing the conclusion of the discussion and the College reply, whereby, in conformity with my own wishes, we accepted the scheme of the Commissioners; 5thly, in dining with an old friend at Clare College; 6thly, in adjourning to the weekly meeting of the Ray Club, from which I returned at 10 P.M., dog-tired, and hardly able to climb my staircase. Lastly, in looking through the "Times" to see what was going on in the busy world.

I do not state this to fill space (though I believe that Nature does abhor a vacuum), but to prove that my reply and my thanks are sent to you by the earliest leisure I have, though that is but a very contracted opportunity. If I did not think you a good-tempered and truth-loving man, I should not tell you that (spite of the great knowledge, store of facts, capital views of the correlation of the various parts of organic nature, admirable hints about the diffusion, through wide regions of many related organic beings, etc., etc.) I have read your book with more pain than pleasure. Parts of it I admired greatly, parts I laughed at till my sides were almost sore; other parts I read with absolute sorrow, because I think them utterly false and grievously mischievous. You have DESERTED — after a start in that tra-road of all solid physical truth — the true method of induction, and started us in machinery as wild, I think, as Bishop Wilkins's locomotive that was to sail with us to the moon. Many of your wide conclusions are based upon assumptions which can neither be proved nor disproved, why then express them in the language and arrangement of philosophical induction? As to your grand principle — NATURAL SELECTION — what is it but a secondary consequence of supposed, or known, primary facts! Development is a better word, because more close to the cause of the fact? For you do not deny causation. I call (in the abstract) causation the will of God; and I can prove that He acts for the good of His creatures. He also acts by laws which we can study and comprehend. Acting by law, and under what is called final causes, comprehends, I think, your whole principle. You write of "natural selection" as if it were done curiously by the selecting agent. 'Tis but a consequence of the presupposed development, and the subsequent battle for life. This view of nature you have stated admirably, though admitted by all naturalists and denied by no one of common sense. We all admit development as a fact of history: but how came it about? Here, in language, and still more in logic, we are point-blank at issue. There is a moral or metaphysical part of nature as well a physical. A man who denies this is deep in the mire of folly. 'Tis the crown and glory of organic science that it DOES through FINAL CAUSE, link material and moral; and yet DOES NOT allow us to mingle them in our first conception of laws, and our classification of such laws, whether we consider one side of nature or the other. You have ignored this link; and, if I do not mistake your meaning, you have done your best in one or two pregnant cases to break it. Were it possible (which, thank God, it is not) to break it, humanity, in my mind, would suffer a damage that might brutalize it, and sink the human race into a lower grade of degradation than any into which it has fallen since its written records tell us of its history. Take the case of the bee-cells. If your development produced the successive modification of the bee and its cells (which no mortal can prove), final cause would stand good as the directing cause under which the successive generations acted and gradually improved. Passages in your book, like that to which I have alluded (and there are others almost as bad), greatly shocked my moral taste. I think, in speculating on organic descent, you OVER-state the evidence of geology; and that you UNDER-state it while you are talking of the broken links of your natural pedigree: but my paper is nearly done, and I must go to my lecture-room. Lastly, then, I greatly dislike the concluding chapter — not as a summary, for in that light it appears good — but I dislike it from the tone of triumphant confidence in which you appeal to the rising generation (in a tone I condemned in the author of the 'Vestiges') and prophesy of things not yet in the womb of time, nor (if we are to trust the accumulated experience of human sense and the inferences of its logic) ever likely to be found anywhere but in the fertile womb of man's imagination. And now to say a word about a son of a monkey and an old friend of yours: I am better, far better, than I was last year. I have been lecturing three days a week (formerly I gave six a week) without much fatigue, but I find by the loss of activity and memory, and of all productive powers, that my bodily frame is sinking slowly towards the earth. But I have visions of the future. They are as much a part of myself as my stomach and my heart, and these visions are to have their antitype in solid fruition of what is best and greatest. But on one condition only — that I humbly accept God's revelation of Himself both in his works and in His word, and do my best to act in conformity with that knowledge which He only can give me, and He only can sustain me in doing. If you and I do all this we shall meet in heaven.

I have written in a hurry, and in a spirit of brotherly love, therefore forgive any sentence you happen to dislike; and believe me, spite of any disagreement in some points of the deepest moral interest, your tru-hearted old friend,

A. SEDGWICK.

CHARLES DARWIN TO T.H. HUXLEY. Down, December 25th [1859].

My dear Huxley,

One part of your note has pleased me so much that I must thank you for it. Not only Sir H.H. [Holland], but several others, have attacked me about analogy leading to belief in one primordial CREATED form. ('Origin,' edition i. page 484. — "Therefore I should infer from analogy that probably all the organic beings which have ever lived on this earth have descended from some one primordial form, into which life was first breathed.") (By which I mean only that we know nothing as yet [of] how life originates.) I thought I was universally condemned on this head. But I answered that though perhaps it would have been more prudent not to have put it in, I would not strike it out, as it seemed to me probable, and I give it on no other grounds. You will see in your mind the kind of arguments which made me think it probable, and no one fact had so great an effect on me as your most curious remarks on the apparent homologies of the head of Vertebrata and Articulata.

You have done a real good turn in the Agency business ("My General Agent" was a sobriquet applied at this time by my father to Mr. Huxley.) (I never before heard of a hard-working, unpaid agent besides yourself), in talking with Sir H.H., for he will have great influence over many. He floored me from my ignorance about the bones of the ear, and I made a mental note to ask you what the facts were.

With hearty thanks and real admiration for your generous zeal for the subject.

Yours most truly, C. DARWIN.

You may smile about the care and precautions I have taken about my ugly MS. (Manuscript left with Mr. Huxley for his perusal.); it is not so much the value I set on them, but the remembrance of the intolerable labour — for instance, in tracing the history of the breeds of pigeons.

CHARLES DARWIN TO J.D. HOOKER. Down, 25th [December, 1859].

... I shall not write to Decaisne (With regard to Naudin's paper in the 'Revue Horticole,' 1852.); I have always had a strong feeling that no one had better defend his own priority. I cannot say that I am as indifferent to the subject as I ought to be, but one can avoid doing anything in consequence.

I do not believe one iota about your having assimilated any of my notions unconsciously. You have always done me more than justice. But I do think I did you a bad turn by getting you to read the old MS., as it must have checked your own original thoughts. There is one thing I am fully convinced of, that the future progress (which is the really important point) of the subject will have depended on really good and well-known workers, like yourself, Lyell, and Huxley, having taken up the subject, than on my own work. I see plainly it is this that strikes my no-scientific friends.

Last night I said to myself, I would just cut your Introduction, but would not begin to read, but I broke down, and had a good hour's read.

Farewell, yours affectionately, C. DARWIN.

CHARLES DARWIN TO J.D. HOOKER. December 28th, 1859.

... Have you seen the splendid essay and notice of my book in the "Times"? (December 26th.) I cannot avoid a strong suspicion that it is by Huxley; but I never heard that he wrote in the "Times". It will do grand service...

C. DARWIN TO T.H. HUXLEY. Down, December 28th [1859].

My dear Huxley,

Yesterday evening, when I read the "Times" of a previous day, I was amazed to find a splendid essay and review of me. Who can the author be? I am intensely curious. It included an eulogium of me which quite touched me, though I am not vain enough to think it all deserved. The author is a literary man, and German scholar. He has read my book very attentively; but, what is very remarkable, it seems that he is a profound naturalist. He knows my Barnacle-book, and appreciates it too highly. Lastly, he writes and thinks with quite uncommon force and clearness; and what is even still rarer, his writing is seasoned with most pleasant wit. We all laughed heartily over some of the sentences. I was charmed with those unreasonable mortals, who know anything, all thinking fit to range themselves on one side. (The reviewer proposes to pass by the orthodox view, according to which the phenomena of the organic world are "the immediate product of a creative fiat, and consequently are out of the domain of science altogether." And he does so "with less hesitation, as it so happens that those persons who are practically conversant with the facts of the case (plainly a considerable advantage) have always thought fit to range themselves" in the category of those holding "views which profess to rest on a scientific basis only, and therefore admit of being argued to their consequences.") Who can it be? Certainly I should have said that there was only one man in England who could have written this essay, and that YOU were the man. But I suppose I am wrong, and that there is some hidden genius of great calibre. For how could you influence Jupiter Olympius and make him give three and a half columns to pure science? The old fogies will think the world will come to an end. Well, whoever the man is, he has done great service to the cause, far more than by a dozen reviews in common periodicals. The grand way he soars above common religious prejudices, and the admission of such views into the

"Times", I look at as of the highest importance, quite independently of the mere question of species. If you should happen to be ACQUAINTED with the author, for Heaven-sake tell me who he is?

My dear Huxley, yours most sincerely, C. DARWIN.

[It is impossible to give in a short space an adequate idea of Mr. Huxley's article in the "Times" of December 26. It is admirably planned, so as to claim for the 'Origin' a respectful hearing, and it abstains from anything like dogmatism in asserting the truth of the doctrines therein upheld. A few passages may be quoted: — "That this most ingenious hypothesis enables us to give a reason for many apparent anomalies in the distribution of living beings in time and space, and that it is not contradicted by the main phenomena of life and organisation, appear to us to be unquestionable." Mr. Huxley goes on to recommend to the readers of the 'Origin' a condition of "thatige Skepsis" — a state of "doubt which so loves truth that it neither dares rest in doubting, nor extinguish itself by unjustified belief." The final paragraph is in a strong contrast to Professor Sedgwick and his "ropes of bubbles" (see below). Mr. Huxley writes: "Mr. Darwin abhors mere speculation as nature abhors a vacuum. He is as greedy of cases and precedents as any constitutional lawyer, and all the principles he lays down are capable of being brought to the test of observation and experiment. The path he bids us follow professes to be not a mere airy track, fabricated of ideal cobwebs, but a solid and broad bridge of facts. If it be so, it will carry us safely over many a chasm in our knowledge, and lead us to a region free from the snares of those fascinating but barren virgins, the Final Causes, against whom a high authority has so justly warned us."

There can be no doubt that this powerful essay, appearing as it did in the leading daily Journal, must have had a strong influence on the reading public. Mr. Huxley allows me to quote from a letter an account of the happy chance that threw into his hands the opportunity of writing it.

"The 'Origin' was sent to Mr. Lucas, one of the staff of the "Times" writers at that day, in what I suppose was the ordinary course of business. Mr. Lucas, though an excellent journalist, and, at a later period, editor of 'Once a Week,' was as innocent of any knowledge of science as a babe, and bewailed himself to an acquaintance on having to deal with such a book. Whereupon he was recommended to ask me to get him out of his difficulty, and he applied to me accordingly, explaining, however, that it would be necessary for him formally to adopt anything I might be disposed to write, by prefacing it with two or three paragraphs of his own.

"I was too anxious to seize upon the opportunity thus offered of giving the book a fair chance with the multitudinous readers of the "Times" to make any difficulty about conditions; and being then very full of the subject, I wrote the article faster, I think, than I ever wrote anything in my life, and sent it to Mr. Lucas, who duly prefixed his opening sentences.

"When the article appeared, there was much speculation as to its authorship. The secret leaked out in time, as all secrets will, but not by my aid; and then I used to derive a good deal of innocent amusement from the vehement assertions of some of my more acute friends, that they knew it was mine from the first paragraph!

"As the "Times" some years since, referred to my connection with the review, I suppose there will be no breach of confidence in the publication of this little history, if you think it worth the space it will occupy."]

# CHAPTER 2.II. — THE 'ORIGIN OF SPECIES' (continued)

## 1860

[I extract a few entries from my father's Diary: —

"January 7th. The second edition, 3000 copies, of 'Origin' was published."

"May 22nd. The first edition of 'Origin' in the United States was 2500 copies."

My father has here noted down the sums received for the 'Origin.'

First Edition...180 pounds Second Edition...636 pounds 13 shillings 4 pence

Total......816 pounds 13 shillings 4 pence.

After the publication of the second edition he began at once, on January 9th, looking over his materials for the 'Variation of Animals and Plants;' the only other work of the year was on Drosera.

He was at Down during the whole of this year, except for a visit to Dr. Lane's Water-cure Establishment at Sudbrooke, and in June, and for visits to Miss Elizabeth Wedgwood's house at Hartfield, in Sussex (July), and to Eastbourne, September 22 to November 16.]

CHARLES DARWIN TO J.D. HOOKER. Down, January 3rd [1860].

My dear Hooker,

I have finished your Essay. ('Australian Flora.') As probably you would like to hear my opinion, though a non-botanist, I will give it without any exaggeration. To my judgment it is by far the grandest and most interesting essay, on subjects of the nature discussed, I have ever read. You know how I admired your former essays, but this seems to me far grander. I like all the part after page xxvi better than the first part, probably because newer to me. I dare say you will demur to this, for I think every author likes the most speculative parts of his own productions. How superior your essay is to the famous one of Brown (here will be sneer 1st from you). You have made all your conclusions so admirably clear, that it would be no use at all to be a botanist (sneer No. 2). By Jove, it would do harm to affix any idea to the long names of outlandish orders. One can look at your conclusions with the philosophic abstraction with which a mathematician looks at his a times x + the square root of z squared, etc. etc. I hardly know which parts have interested me most; for over and over again I exclaimed, "this beats all." The general comparison of the Flora of Australia with the rest of the world, strikes me (as before) as extremely original, good, and suggestive of many reflections.

... The invading Indian Flora is very interesting, but I think the fact you mention towards the close of the essay — that the Indian vegetation, in contradistinction to the Malayan vegetation, is found in low and level parts of the Malay Islands, GREATLY lessens the difficulty which at first (page 1) seemed so great. There is nothing like one's own hobby-horse. I suspect it is the same case as of glacial migration, and of naturalised production — of production of greater area conquering those of lesser; of course the Indian forms would have a greater difficulty in seizing on the cool parts of Australia. I demur to your remarks (page 1), as not "conceiving anything in soil, climate, or vegetation of India," which could stop the introduction of Australian plants. Towards the close of the essay (page civ), you have admirable remarks on our profound ignorance of the cause of possible naturalisation or introduction; I would answer page 1, by a later page, viz. page civ.

Your contrast of the south-west and south-east corners is one of the most wonderful cases I ever heard of... You show the case with wonderful force. Your discussion on mixed invaders of the south-east corner (and of New Zealand) is as curious and intricate a problem as of the races of men in Britain. Your remark on mixed invading Flora keeping down or destroying an original Flora, which was richer in number of species, strikes me as EMINENTLY NEW AND IMPORTANT. I am not sure whether to me the discussion on the New Zealand Flora is not even more instructive. I cannot too much admire both. But it will require a long time to suck in all the facts. Your case of the largest

Australian orders having none, or very few, species in New Zealand, is truly marvellous. Anyhow, you have now DEMONSTRATED (together with no mammals in New Zealand) (bitter sneer No. 3), that New Zealand has never been continuously, or even nearly continuously, united by land to Australia!! At page lxxxix, is the only sentence (on this subject) in the whole essay at which I am much inclined to quarrel, viz. that no theory of trans-oceanic migration can explain, etc. etc. Now I maintain against all the world, that no man knows anything about the power of trans-oceanic migration. You do not know whether or not the absent orders have seeds which are killed by sea-water, like almost all Leguminosae, and like another order which I forget. Birds do not migrate from Australia to New Zealand, and therefore floatation SEEMS the only possible means; but yet I maintain that we do not know enough to argue on the question, especially as we do not know the main fact whether the seeds of Australian orders are killed by sea-water.

The discussion on European Genera is profoundly interesting; but here alone I earnestly beg for more information, viz. to know which of these genera are absent in the Tropics of the world, i.e. confined to temperate regions. I excessively wish to know, ON THE NOTION OF GLACIAL MIGRATION, how much modification has taken place in Australia. I had better explain when we meet, and get you to go over and mark the list.

... The list of naturalised plants is extremely interesting, but why at the end, in the name of all that is good and bad, do you not sum up and comment on your facts? Come, I will have a sneer at you in return for the many which you will have launched at this letter. Should you have remarked on the number of plants naturalised in Australia and the United States UNDER EXTREMELY DIFFERENT CLIMATES, as showing that climate is so important, and [on] the considerable sprinkling of plants from India, North America, and South Africa, as showing that the frequent introduction of seeds is so important? With respect to "abundance of unoccupied ground in Australia," do you believe that European plants introduced by man now grow on spots in Australia which were absolutely bare? But I am an impudent dog, one must defend one's own fancy theories against such cruel men as you. I dare say this letter will appear very conceited, but one must form an opinion on what one reads with attention, and in simple truth, I cannot find words strong enough to express my admiration of your essay.

My dear old friend, yours affectionately, C. DARWIN.

P.S. — I differ about the "Saturday Review". ("Saturday Review", December 24, 1859. The hostile arguments of the reviewer are geological, and he deals especially with the denudation of the Weald. The reviewer remarks that, "if a million of centuries, more or less, is needed for any part of his argument, he feels no scruple in taking them to suit his purpose.") One cannot expect fairness in a reviewer, so I do not complain of all the other arguments besides the 'Geological Record' being omitted. Some of the remarks about the lapse of years are very good, and the reviewer gives me some good and well-deserved raps — confound it. I am sorry to confess the truth: but it does not at all concern the main argument. That was a nice notice in the "Gardeners' Chronicle". I hope and imagine that Lindley is almost a convert. Do not forget to tell me if Bentham gets all the more staggered.

With respect to tropical plants during the Glacial period, I throw in your teeth your own facts, at the base of the Himalaya, on the possibility of the co-existence of at least forms of the tropical and temperate regions. I can give a parallel case for animals in Mexico. Oh! my dearly beloved puny child, how cruel men are to you! I am very glad you approve of the Geographical chapters...

CHARLES DARWIN TO C. LYELL. Down, [January 4th, 1860].

My dear L.

"Gardeners' Chronicle" returned safe. Thanks for note. I am beyond measure glad that you get more and more roused on the subject of species, for, as I have always said, I am well convinced that your opinions and writings will do far more to convince the world than mine. You will make a grand discussion on man. You are very bold in this, and I honour you. I have been, like you, quite surprised at the want of originality in opposed arguments and in favour too. Gwyn Jeffreys attacks me justly in his letter about strictly littoral shells not being often embedded at least in Tertiary deposits. I was in a muddle, for I was thinking of Secondary, yet Chthamalus applied to Tertiary...

Possibly you might like to see the enclosed note (Dr. Whewell wrote (January 2, 1860): "... I cannot, yet at least, become a convert. But there is so much of thought and of fact in what you have written that it is not to be contradicted without careful selection of the ground and manner of the dissent." Dr. Whewell dissented in a practical manner for some years, by refusing to allow a copy of the 'Origin of Species' to be placed in the Library of Trinity College.) from Whewell, merely as showing that he is not horrified with us. You can return it whenever you have occasion to write, so as not to waste your time.

C.D.

CHARLES DARWIN TO C. LYELL. Down, [January 4th? 1860].

... I have had a brief note from Keyserling (Joint author with Murchison of the 'Geology of Russia,' 1845.), but not worth sending you. He believes in change of species, grants that natural selection explains well adaptation of form, but thinks species change too regularly, as if by some chemical law, for natural selection to be the sole cause of change. I can hardly understand his brief note, but this is I think the upshot.

... I will send A. Murray's paper whenever published. (The late Andrew Murray wrote two papers on the 'Origin' in the Proc. R. Soc. Edin. 1860. The one referred to here is dated January 16, 1860. The following is quoted from page 6 of the separate copy: "But the second, and, as it appears to me, by much the most important phase of reversion to type (and which is practically, if not altogether ignored by Mr. Darwin), is the instinctive inclination which induces individuals of the same species by preference to intercross with those possessing the qualities which they themselves want, so as to preserve the purity or equilibrium of the breed... It is trite to a proverb, that tall men marry little women... a man of genius marries a fool... and we are told that this is the result of the charm of contrast, or of qualities admired in others because we do not possess them. I do not so explain it. I imagine it is the effort of nature to preserve the typical medium of the race.") It includes speculations (which he perhaps will modify) so rash, and without a single fact in support, that had I advanced them he or other reviewers would have hit me very hard. I am sorry to say that I have no "consolatory view" on the dignity of man. I am content that man will probably advance, and care not much whether we are looked at as mere savages in a remotely distant future. Many thanks for your last note.

Yours affectionately, C. DARWIN.

I have received, in a Manchester newspaper, rather a good squib, showing that I have proved "might is right," and therefore that Napoleon is right, and every cheating tradesman is also right.

CHARLES DARWIN TO W.B. CARPENTER. Down, January 6th [1860]?

My dear Carpenter,

I have just read your excellent article in the 'National.' It will do great good; especially if it becomes known as your production. It seems to me to give an excellently clear account of Mr. Wallace's and my views. How capitally you turn the flanks of the theological opposers by opposing to them such men as Bentham and the more philosophical of the systematists! I thank you sincerely for the EXTREMELY honourable manner in which you mention me. I should have liked to have seen some criticisms or remarks on embryology, on which subject you are so well instructed. I do not think any candid person can read your article without being much impressed with it. The old doctrine of immutability of specific forms will surely but slowly die away. It is a shame to give you trouble, but I should be very much obliged if you could tell me where differently coloured eggs in individuals of the cuckoo have been described, and their laying in twent-seven kinds of nests. Also do you know from your own observation that the limbs of sheep imported into the West Indies change colour? I have had detailed information about the loss of wool; but my accounts made the change slower than you describe.

With most cordial thanks and respect, believe me, my dear Carpenter, yours very sincerely, CH. DARWIN.

CHARLES DARWIN TO L. JENYNS. (Rev. L. Blomefield.) Down, January 7th, 1860.

My dear Jenyns,

I am very much obliged for your letter. It is of great use and interest to me to know what impression my book produces on philosophical and instructed minds. I thank you for the kind things which you say; and you go with me much further than I expected. You will think it presumptuous, but I am convinced, IF CIRCUMSTANCES LEAD YOU TO KEEP THE SUBJECT IN MIND, that you will go further. No one has yet cast doubts on my explanation of the subordination of group to group, on homologies, embryology, and rudimentary organs; and if my explanation of these classes of facts be at all right, whole classes of organic beings must be included in one line of descent.

The imperfection of the Geological Record is one of the greatest difficulties... During the earliest period the record would be most imperfect, and this seems to me sufficient to account for our not finding intermediate forms between the classes in the same great kingdoms. It was certainly rash in me putting in my belief of the probability of all beings having descended from ONE primordial form; but as this seems yet to me probable, I am not willing to strike it out. Huxley alone supports me in this, and something could be said in its favour. With respect to man, I am very far from wishing to obtrude my belief; but I thought it dishonest to quite conceal my opinion. Of course it is open to every one to believe that man appeared by a separate miracle, though I do not myself see the necessity or probability.

Pray accept my sincere thanks for your kind note. Your going some way with me gives me great confidence that I am not very wrong. For a very long time I halted half way; but I do not believe that any enquiring mind will rest half-way. People will have to reject all or admit all; by ALL I mean only the members of each great kingdom.

My dear Jenyns, yours most sincerely, C. DARWIN.

CHARLES DARWIN TO C. LYELL. Down, January 10th [1860].

... It is perfectly true that I owe nearly all the corrections (The second edition of 3000 copies of the 'Origin' was published on January 7th.) to you, and several verbal ones to you and others; I am heartily glad you approve of them, as yet only two things have annoyed me; those confounded millions (This refers to the passage in the 'Origin of Species' (2nd edition, page 285), in which the lapse of time implied by the denudation of the Weald is discussed. The discussion closes with the sentence: "So that it is not improbable that a longer period than 300 million years has elapsed since the latter part of the Secondary period." This passage is omitted in the later editions of the 'Origin,' against the advice of some of his friends, as appears from the pencil notes in my father's copy of the second edition.) of years (not that I think it is probably wrong), and my not having (by inadvertance) mentioned Wallace towards the close of the book in the summary, not that any one has noticed this to me. I have now put in Wallace's name at page 484 in a conspicuous place. I cannot refer you to tables of mortality of children, etc. etc. I have notes somewhere, but I have not the LEAST idea where to hunt, and my notes would now be old. I shall be truly glad to read carefully any MS. on man, and give my opinion. You used to caution me to be cautious about man. I suspect I shall have to return the caution a hundred fold! Yours will, no doubt, be a grand discussion; but it will horrify the world at first more than my whole volume; although by the sentence (page 489, new edition (First edition, page 488.)) I show that I believe man is in the same predicament with other animals. It is, in fact, impossible to doubt it. I have thought (only vaguely) on man. With respect to the races, one of my best chances of truth has broken down from the impossibility of getting facts. I have one good speculative line, but a man must have entire credence in Natural Selection before he will even listen to it. Psychologically, I have done scarcely anything. Unless, indeed, expression of countenance can be included, and on that subject I have collected a good many facts, and speculated, but I do not suppose I shall ever publish, but it is an uncommonly curious subject. By the way, I sent off a lot of questions the day before yesterday to Tierra del Fuego on expression! I suspect (for I have never read it) that Spencer's 'Psychology' has a bearing on Psychology as we should look at it. By all means read the Preface, in about 20 pages, of Hensleigh Wedgwood's new Dictionary on the first origin of Language; Erasmus would lend it. I agree about Carpenter, a very good article, but with not much original... Andrew Murray has criticised, in an address to the Botanical Society of Edinburgh, the notice in the 'Linnean Journal,' and "has disposed of" the whole theory by an ingenious difficulty, which I was very stupid not to have thought of; for I express surprise at more and analogous cases not being known. The difficulty is, that amongst the blind insects of the caves in distant parts of the world there are some of the same genus, and yet the genus is not found out of the caves or living in the free world. I have little doubt that, like the fish Amblyopsis, and like Proteus in Europe, these insects are "wrecks of ancient life," or "living fossils," saved from competition and extermination. But that formerly SEEING insects of the same genus roamed over the whole area in which the cases are included.

Farewell, yours affectionately, C. DARWIN.

P.S. — OUR ancestor was an animal which breathed water, had a swim bladder, a great swimming tail, an imperfect skull, and undoubtedly was an hermaphrodite!

Here is a pleasant genealogy for mankind.

CHARLES DARWIN TO C. LYELL. Down, January 14th [1860].

... I shall be much interested in reading your man discussion, and will give my opinion carefully, whatever that may be worth; but I have so long looked at you as the type of cautious scientific judgment (to my mind one of the highest and most useful qualities), that I suspect my opinion will be superfluous. It makes me laugh to think what a joke it will be if I have to caution you, after your cautions on the same subject to me!

I will order Owen's book ('Classification of the Mammalia,' 1859.); I am very glad to hear Huxley's opinion on his classification of man; without having due knowledge, it seemed to me from the very first absurd; all classifications founded on single characters I believe have failed.

... What a grand, immense benefit you conferred on me by getting Murray to publish my book. I never till to-day realised that it was getting widely distributed; for in a letter from a lady to-day to E., she says she heard a man enquiring for it at the RAILWAY STATION!!! at Waterloo Bridge; and the bookseller said that he had none till the new edition was out. The bookseller said he had not read it, but had heard it was a very remarkable book!!!..

CHARLES DARWIN TO J.D. HOOKER. Down, 14th [January, 1860].

... I heard from Lyell this morning, and he tells me a piece of news. You are a good-for-nothing man; here you are slaving yourself to death with hardly a minute to spare, and you must write a review of my book! I thought it ('Gardeners' Chronicle', 1860. Referred to above. Sir J.D. Hooker took the line of complete impartiality, so as not to commit Lindley.) a very good one, and was so much struck with it that I sent it to Lyell. But I assumed, as a matter of course, that it was Lindley's. Now that I know it is yours, I have re-read it, and, my kind and good friend, it has warmed my heart with all the honourable and noble things you say of me and it. I was a good deal surprised at Lindley hitting on some of the remarks, but I never dreamed of you. I admired it chiefly as so well adapted to tell on the readers of the 'Gardeners' Chronicle'; but now I admired it in another spirit. Farewell, with hearty thanks... Lyell is going at man with an audacity that frightens me. It is a good joke; he used always to caution me to slip over man.

[In the "Gardeners' Chronicle", January 21, 1860, appeared a short letter from my father which was called forth by Mr. Westwood's communication to the previous number of the journal, in which certain phenomena of cros-breeding are discussed in relation to the 'Origin of Species.' Mr. Westwood wrote in reply (February 11) and adduced further evidence against the doctrine of descent, such as the identity of the figures of ostriches on the ancient "Egyptian records," with the bird as we now know it. The correspondence is hardly worth mentioning, except as one of the very few cases in which my father was enticed into anything resembling a controversy.]

ASA GRAY TO J.D. HOOKER. Cambridge, Mass., January 5th, 1860.

My dear Hooker,

Your last letter, which reached me just before Christmas, has got mislaid during the upturnings in my study which take place at that season, and has not yet been discovered. I should be very sorry to lose it, for there were in it some botanical mems. which I had not secured...

The principal part of your letter was high laudation of Darwin's book.

Well, the book has reached me, and I finished its careful perusal four days ago; and I freely say that your laudation is not out of place.

It is done in a MASTERLY MANNER. It might well have taken twenty years to produce it. It is crammed full of most interesting matter — thoroughly digested — well expressed — close, cogent, and taken as a system it makes out a better case than I had supposed possible...

Agassiz, when I saw him last, had read but a part of it. He says it is POOR — VERY POOR!! (entre nous). The fact [is] he is very much annoyed by it... and I do not wonder at it. To bring all IDEAL systems within the domain of science, and give good physical or natural explanations of all his capital points, is as bad as to have Forbes take the glacier materials... and give scientific explanation of all the phenomena.

Tell Darwin all this. I will write to him when I get a chance. As I have promised, he and you shall have fair-play here... I must myself write a review of Darwin's book for 'Silliman's Journal' (the more so that I suspect Agassiz means to come out upon it) for the next (March) No., and I am now setting about it (when I ought to be every moment working the Expl[oring] Expedition Compositae, which I know far more about). And really it is no easy job, as you may well imagine.

I doubt if I shall please you altogether. I know I shall not please Agassiz at all. I hear another reprint is in the Press, and the book will excite much attention here, and some controversy...

CHARLES DARWIN TO ASA GRAY. Down, January 28th [1860].

My dear Gray,

Hooker has forwarded to me your letter to him; and I cannot express how deeply it has gratified me. To receive the approval of a man whom one has long sincerely respected. And whose judgment and knowledge are most universally admitted, is the highest reward an author can possibly wish for; and I thank you heartily for your most kind expressions.

I have been absent from home for a few days, and so could not earlier answer your letter to me of the 10th of January. You have been extremely kind to take so much trouble and interest about the edition. It has been a mistake of my publisher not thinking of sending over the sheets. I had entirely and utterly forgotten your offer of receiving the sheets as printed off. But I must not blame my publisher, for had I remembered your most kind offer I feel pretty sure I should not have taken advantage of it; for I never dreamed of my book being so successful with general readers; I believe I should have laughed at the idea of sending the sheets to America. (In a letter to Mr. Murray, 1860, my father wrote: — "I am amused by Asa Gray's account of the excitement my book has made amongst naturalists in the United States. Agassiz has denounced it in a newspaper, but yet in such terms that it is in fact a fine advertisement!" This seems to refer to a lecture given before the Mercantile Library Association.)

After much consideration, and on the strong advice of Lyell and others, I have resolved to leave the present book as it is (excepting correcting errors, or here and there inserting short sentences) and to use all my strength, WHICH IS BUT LITTLE, to bring out the first part (forming a separate volume with index, etc.) of the three volumes which will make my bigger work; so that I am very unwilling to take up time in making corrections for an American edition. I enclose a list of a few corrections in the second reprint, which you will have received by this time complete, and I could send four or five corrections or additions of equally small importance, or rather of equal brevity.

I also intend to write a SHORT preface with a brief history of the subject. These I will set about, as they must some day be done, and I will send them to you in a short time — the few corrections first, and the preface afterwards, unless I hear that you have given up all idea of a separate edition. You will then be able to judge whether it is worth having the new edition with YOUR REVIEW PREFIXED. Whatever be the nature of your review, I assure you I should feel it a GREAT honour to have my book thus preceded...

ASA GRAY TO CHARLES DARWIN. Cambridge, January 23rd, 1860.

My dear Darwin,

You have my hurried letter telling you of the arrival of the remainder of the sheets of the reprint, and of the stir I had made for a reprint in Boston. Well, all looked pretty well, when, lo, we found that a second New York publishing house had announced a reprint also! I wrote then to both New York publishers, asking them to give way to the AUTHOR and his reprint of a revised edition. I got an answer from the Harpers that they withdraw — from the Appletons that they had got the book OUT (and the next day I saw a copy); but that, "if the work should have any considerable sale, we certainly shall be disposed to pay the author reasonably and liberally."

The Appletons being thus out with their reprint, the Boston house declined to go on. So I wrote to the Appletons taking them at their word, offering to aid their reprint, to give them the use of the alterations in the London reprint, as soon as I find out what they are, etc. etc. And I sent them the first leaf, and asked them to insert in their future issue the additional matter from Butler (A quotation from Butler's 'Analogy,' on the use of the word natural, which in the second edition is placed with the passages from Whewell and Bacon on page ii, opposite the title-page.), which tells just right. So there the matter stands. If you furnish any matter in advance of the London third edition, I will make them pay for it.

I may get something for you. All got is clear gain; but it will not be very much, I suppose.

Such little notices in the papers here as have yet appeared are quite handsome and considerate.

I hope next week to get printed sheets of my review from New Haven, and send [them] to you, and will ask you to pass them on to Dr. Hooker.

To fulfil your request, I ought to tell you what I think the weakest, and what the best, part of your book. But this is not easy, nor to be done in a word or two. The BEST PART, I think, is the WHOLE, i.e., its PLAN and TREATMENT, the vast amount of facts and acute inferences handled as if you had a perfect mastery of them. I do not think twenty years too much time to produce such a book in.

Style clear and good, but now and then wants revision for little matters (page 97, self-fertilises ITSELF, etc.).

Then your candour is worth everything to your cause. It is refreshing to find a person with a new theory who frankly confesses that he finds difficulties, insurmountable, at least for the present. I know some people who never have any difficulties to speak of.

The moment I understood your premisses, I felt sure you had a real foundation to hold on. Well, if one admits your premisses, I do not see how he is to stop short of your conclusions, as a probable hypothesis at least.

It naturally happens that my review of your book does not exhibit anything like the full force of the impression the book has made upon me. Under the circumstances I suppose I do your theory more good here, by bespeaking for it a fair and favourable consideration, and by standing non-committed as to its full conclusions, than I should if I announced myself a convert; nor could I say the latter, with truth.

Well, what seems to me the weakest point in the book is the attempt to account for the formation of organs, the making of eyes, etc., by natural selection. Some of this reads quite Lamarckian.

The chapter on HYBRIDISM is not a WEAK, but a STRONG chapter. You have done wonders there. But still you have not accounted, as you may be held to account, for divergence up to a certain extent producing increased fertility of the crosses, but carried one short almost imperceptible step more, giving rise to sterility, or reversing the tendency. Very likely you are on the right track; but you have something to do yet in that department.

Enough for the present.

... I am not insensible to your compliments, the very high compliment which you pay me in valuing my opinion. You evidently think more of it than I do, though from the way I write [to] you, and especially [to] Hooker, this might not be inferred from the reading of my letters.

I am free to say that I never learnt so much from one book as I have from yours, there remain a thousand things I long to say about it.

Ever yours, ASA GRAY.

CHARLES DARWIN TO ASA GRAY. [February? 1860].

... Now I will just run through some points in your letter. What you say about my book gratifies me most deeply, and I wish I could feel all was deserved by me. I quite think a review from a man, who is not an entire convert, if fair and moderately favourable, is in all respects the best kind of review. About the weak points I agree. The eye to this day gives me a cold shudder, but when I think of the fine known gradations, my reason tells me I ought to conquer the cold shudder.

Pray kindly remember and tell Prof. Wyman how very grateful I should be for any hints, information, or criticisms. I have the highest respect for his opinion. I am so sorry about Dana's health. I have already asked him to pay me a visit.

Farewell, you have laid me under a load of obligation — not that I feel it a load. It is the highest possible gratification to me to think that you have found my book worth reading and reflection; for you and three others I put down in my own mind as the judges whose opinions I should value most of all.

My dear Gray, yours most sincerely, C. DARWIN.

P.S. — I feel pretty sure, from my own experience, that if you are led by your studies to keep the subject of the origin of species before your mind, you will go further and further in your belief. It took me long years, and I assure you I am astonished at the impression my book has made on many minds. I fear twenty years ago, I should not have been half as candid and open to conviction.

CHARLES DARWIN TO J.D. HOOKER. Down, [January 31st, 1860].

My dear Hooker,

I have resolved to publish a little sketch of the progress of opinion on the change of species. Will you or Mrs. Hooker do me the favour to copy ONE sentence out of Naudin's paper in the 'Revue Horticole,' 1852, page 103, namely, that on his principle of Finalite. Can you let me have it soon, with those confounded dashes over the vowels put in carefully? Asa Gray, I believe, is going to get a second edition of my book, and I want to send this little preface over to him soon. I did not think of the necessity of having Naudin's sentence on finality, otherwise I would have copied it.

Yours affectionately, C. DARWIN.

P.S. — I shall end by just alluding to your Australian Flora Introduction. What was the date of publication: December 1859, or January 1860? Please answer this.

My preface will also do for the French edition, which I BELIEVE, is agreed on.

CHARLES DARWIN TO J.D. HOOKER. February [1860].

... As the 'Origin' now stands, Harvey's (William Henry Harvey was descended from a Quaker family of Youghal, and was born in February, 1811, at Summerville, a country house on the banks of the Shannon. He died at Torquay in 1866. In 1835, Harvey went to Africa (Table Bay) to pursue his botanical studies, the results of which were given in his 'Genera of South African Plants.' In 1838, ill-health compelled him to obtain leave of absence, and return to England for a time; in 1840 he returned to Cape Town, to be again compelled by illness to leave. In 1843 he obtained the appointment of Botanical Professor at Trinity College, Dublin. In 1854, 1855, and 1856 he visited Australia, New Zealand, the Friendly and Fiji Islands. In 1857 Dr. Harvey reached home, and was appointed the successor of Professor Allman to the Chair of Botany in Dublin University. He was author of several botanical works, principally on Algae. — (From a Memoir published in 1869.)) is a good hit against my talking so much of the insensibly fine gradations; and certainly it has astonished me that I should be pelted with the fact, that I had not allowed abrupt and great enough variations under nature. It would take a good deal more evidence to make me admit that forms have often changed by saltum.

Have you seen Wollaston's attack in the 'Annals'? ('Annals and Magazine of Natural History,' 1860.) The stones are beginning to fly. But Theology has more to do with these two attacks than Science...

[In the above letter a paper by Harvey in the "Gardeners' Chronicle", February 18, 1860, is alluded to. He describes a case of monstrosity in Begonia frigida, in which the "sport" differed so much from a normal Begonia that it might have served as the type of a distinct natural order. Harvey goes on to argue that such a case is hostile to the theory of natural selection, according to which changes are not supposed to take place per saltum, and adds that "a few such cases would overthrow it [Mr. Darwin's hypothesis] altogether." In the following number of the "Gardeners' Chronicle" Sir J.D. Hooker showed that Dr. Harvey had misconceived the bearing of the Begonia case, which he further showed to be by no means calculated to shake the validity of the doctrine of modification by means of natural selection. My father mentions the Begonia case in a letter to Lyell (February 18, 1860): —

"I send by this post an attack in the "Gardeners' Chronicle", by Harvey (a first-rate Botanist, as you probably know). It seems to me rather strange; he assumes the permanence of monsters, whereas, monsters are generally sterile, and not often inheritable. But grant his case, it comes that I have been too cautious in not admitting great and sudden variations. Here again comes in the mischief of my ABSTRACT. In the fuller MS. I have discussed a parallel case of a normal fish like the monstrous gold-fish."

With reference to Sir J.D. Hooker's reply, my father wrote:]

Down, [February 26th, 1860].

My dear Hooker,

Your answer to Harvey seems to me ADMIRABLY good. You would have made a gigantic fortune as a barrister. What an omission of Harvey's about the graduated state of the flowers! But what strikes me most is that surely I ought to know my own book best, yet, by Jove, you have brought forward ever so many arguments which I did not think of! Your reference to classification (viz. I presume to such cases as Aspicarpa) is EXCELLENT, for the monstrous Begonia no doubt in all details would be Begonia. I did not think of this, nor of the RETROGRADE step from separated sexes to an hermaphrodite state; nor of the lessened fertility of the monster. Proh pudor to me.

The world would say what a lawyer has been lost in a MERE botanist!

Farewell, my dear master in my own subject,

Yours affectionately, C. DARWIN.

I am so heartily pleased to see that you approve of the chapter on Classification.

I wonder what Harvey will say. But no one hardly, I think, is able at first to see when he is beaten in an argument.

[The following letters refer to the first translation (1860) of the 'Origin of Species' into German, which was superintended by H.G. Bronn, a good zoologist and palaeontologist, who was at the time at Freiburg, but afterwards Professor at Heidelberg. I have been told that the translation was not a success, it remained an obvious translation, and was correspondingly unpleasant to read. Bronn added to the translation an appendix of the difficulties that occurred to him. For instance, how can natural selection account for differences between species, when these differences appear to be of no service to their possessors; e.g., the length of the ears and tail, or the folds in the enamel of the teeth of various species of rodents? Krause, in his book, 'Charles Darwin,' page 91, criticises Bronn's conduct in this manner, but it will be seen that my father actually suggested the addition of Bronn's remarks. A more serious charge against Bronn made by Krause (op. cit. page 87) is that he left out passages of which he did not approve, as, for instance, the passage ('Origin,' first edition, page 488) "Light will be thrown on the origin of man and his history." I have no evidence as to whether my father did or did not know of these alterations.]

CHARLES DARWIN TO H.G. BRONN. Down, February 4 [1860].

Dear and much honoured Sir,

I thank you sincerely for your most kind letter; I feared that you would much disapprove of the 'Origin,' and I sent it to you merely as a mark of my sincere respect. I shall read with much interest your work on the productions of Islands whenever I receive it. I thank you cordially for the notice in the 'Neues Jahrbuch fur Mineralogie,' and still more for speaking to Schweitzerbart about a translation; for I am most anxious that the great and intellectual German people should know something about my book.

I have told my publisher to send immediately a copy of the NEW (Second edition.) edition to Schweitzerbart, and I have written to Schweitzerbart that I gave up all right to profit for myself, so that I hope a translation will appear. I fear that the book will be difficult to translate, and if you could advise Schweitzerbart about a GOOD translator, it would be of very great service. Still more, if you would run your eye over the more difficult parts of the translation; but this is too great a favour to expect. I feel sure that it will be difficult to translate, from being so much condensed.

Again I thank you for your noble and generous sympathy, and I remain, with entire respect,

Yours, truly obliged, C. DARWIN.

P.S. — The new edition has some few corrections, and I will send in MS. some additional corrections, and a short historical preface, to Schweitzerbart.

How interesting you could make the work by EDITING (I do not mean translating) the work, and appending notes of REFUTATION or confirmation. The book has sold so very largely in England, that an editor would, I think, make profit by the translation.

CHARLES DARWIN TO H.G. BRONN. Down, February 14 [1860].

My dear and much honoured Sir,

I thank you cordially for your extreme kindness in superintending the translation. I have mentioned this to some eminent scientific men, and they all agree that you have done a noble and generous service. If I am proved quite wrong, yet I comfort myself in thinking that my book may do some good, as truth can only be known by rising victorious from every attack. I thank you also much for the review, and for the kind manner in which you speak of me. I send with this letter some corrections and additions to M. Schweitzerbart, and a short historical preface. I am not much acquainted with German authors, as I read German very slowly; therefore I do not know whether any Germans have advocated similar views with mine; if they have, would you do me the favour to insert a foot-note to the preface? M. Schweitzerbart has now the reprint ready for a translator to begin. Several scientific men have thought the term "Natural Selection" good, because its meaning is NOT obvious, and each man could not put on it his own interpretation, and because it at once connects variation under domestication and nature. Is there any analogous term used by German breeders of animals? "Adelung," ennobling, would, perhaps, be too metaphysical. It is folly in me, but I cannot help doubting whether "Wahl der Lebensweise" expresses my notion. It leaves the impression on my mind of the Lamarckian doctrine (which I reject) of habits of life being al-important. Man has altered, and thus improved the English race-horse by SELECTING successive fleeter individuals; and I believe, owing to the struggle for existence, that similar SLIGHT variations in a wild horse, IF ADVANTAGEOUS TO IT, would be SELECTED or PRESERVED by nature; hence Natural Selection. But I apologise for troubling you with these remarks on the importance of choosing good German terms for "Natural Selection." With my heartfelt thanks, and with sincere respect,

I remain, dear Sir, yours very sincerely, CHARLES DARWIN.

CHARLES DARWIN TO H.G. BRONN. Down, July 14 [1860].

Dear and honoured Sir,

On my return home, after an absence of some time, I found the translation of the third part (The German translation was published in three pamphle-like numbers.) of the 'Origin,' and I have been delighted to see a final chapter of criticisms by yourself. I have read the first few paragraphs and final paragraph, and am perfectly contented, indeed more than contented, with the generous and candid spirit with which you have considered my views. You speak with too much praise of my work. I shall, of course, carefully read the whole chapter; but though I can read descriptive books like Gaertner's pretty easily, when any reasoning comes in, I find German excessively difficult to understand. At some FUTURE time I should very much like to hear how my book has been received in Germany, and I most sincerely hope M. Schweitzerbart will not lose money by the publication. Most of the reviews have been bitterly opposed to me in England, yet I have made some converts, and SEVERAL naturalists who would not believe in a word of it, are now coming slightly round, and admit that natural selection may have done something. This gives me hope that more will ultimately come round to a certain extent to my views.

I shall ever consider myself deeply indebted to you for the immense service and honour which you have conferred on me in making the excellent translation of my book. Pray believe me, with most sincere respect,

Dear Sir, yours gratefully, CHARLES DARWIN.

CHARLES DARWIN TO C. LYELL. Down, [February 12th, 1860].

... I think it was a great pity that Huxley wasted so much time in the lecture on the preliminary remarks;... but his lecture seemed to me very fine and very bold. I have remonstrated (and he agrees) against the impression that he would leave, that sterility was a universal and infallible criterion of species.

You will, I am sure, make a grand discussion on man. I am so glad to hear that you and Lady Lyell will come here. Pray fix your own time; and if it did not suit us we would say so. We could then discuss man well...

How much I owe to you and Hooker! I do not suppose I should hardly ever have published had it not been for you.

[The lecture referred to in the last letter was given at the Royal Institution, February 10, 1860. The following letter was written in reply to Mr. Huxley's request for information about breeding, hybridisation, etc. It is of interest as giving a vivid retrospect of the writer's experience on the subject.]

CHARLES DARWIN TO T.H. HUXLEY. Ilkley, Yorks, November 27 [1859].

My dear Huxley,

Gartner grand, Kolreuter grand, but papers scattered through many volumes and very lengthy. I had to make an abstract of the whole. Herbert's volume on Amaryllidaceae very good, and two excellent papers in the 'Horticultural Journal.' For animals, no resume to be trusted at all; facts are to be collected from all original sources. (This caution is exemplified in the following extract from an earlier letter to Professor Huxley: — "The inaccuracy of the blessed gang (of which I am one) of compilers passes all bounds. MONSTERS have frequently been described as hybrids without a tittle of evidence. I must give one other case to show how we jolly fellows work. A Belgian Baron (I forget his name at this moment) crossed two distinct geese and got SEVEN hybrids, which he proved subsequently to be quite sterile; well, compiler the first, Chevreul, says that the hybrids were propagated for SEVEN generations inter se. Compiler second (Morton) mistakes the French name, and gives Latin names for two more distinct geese, and says CHEVREUL himself propagated them inter se for seven generations; and the latter statement is copied from book to book.") I fear my MS. for the bigger book (twice or thrice as long as in present book), with all references, would be illegible, but it would save you infinite labour; of course I would gladly lend it, but I have no copy, so care would have to be taken of it. But my accursed handwriting would be fatal, I fear.

About breeding, I know of no one book. I did not think well of Lowe, but I can name none better. Youatt I look at as a far better and MORE PRACTICAL authority; but then his views and facts are scattered through three or four thick volumes. I have picked up most by reading really numberless special treatises and ALL agricultural and horticultural journals; but it is a work of long years. THE DIFFICULTY IS TO KNOW WHAT TO TRUST. No one or two statements are worth a farthing; the facts are so complicated. I hope and think I have been really cautious in what I state on this subject, although all that I have given, as yet, is FAR too briefly. I have found it very important associating with fanciers and breeders. For instance, I sat one evening in a gin palace in the Borough amongst a set of pigeon fanciers, when it was hinted that Mr. Bull had crossed his Pouters with Runts to gain size; and if you had seen the solemn, the mysterious, and awful shakes of the head which all the fanciers gave at this scandalous proceeding, you would have recognised how little crossing has had to do with improving breeds, and how dangerous for endless generations the process was. All this was brought home far more vividly than by pages of mere statements, etc. But I am scribbling foolishly. I really do not know how to advise about getting up facts on breeding and improving breeds. Go to Shows is one way. Read ALL treatises on any ONE domestic animal, and believe nothing without largely confirmed. For your lectures I can give you a few amusing anecdotes and sentences, if you want to make the audience laugh.

I thank you particularly for telling me what naturalists think. If we can once make a compact set of believers we shall in time conquer. I am EMINENTLY glad Ramsey is on our side, for he is, in my opinion, a firs-rate geologist. I sent him a copy. I hope he got it. I shall be very curious to hear whether any effect has been produced on Prestwich; I sent him a copy, not as a friend, but owing to a sentence or two in some paper, which made me suspect he was doubting.

Rev. C. Kingsley has a mind to come round. Quatrefages writes that he goes some long way with me; says he exhibited diagrams like mine. With most hearty thanks,

Yours very tired, C. DARWIN.

[I give the conclusion of Professor Huxley's lecture, as being one of the earliest, as well as one of the most eloquent of his utterances in support of the 'Origin of Species']:

"I have said that the man of science is the sworn interpreter of nature in the high court of reason. But of what avail is his honest speech, if ignorance is the assessor of the judge, and prejudice the foreman of the jury? I hardly know of a great physical truth, whose universal reception has not been preceded by an epoch in which most estimable persons have maintained that the phenomena investigated were directly dependent on the Divine Will, and that the attempt to investigate them was not only futile, but blasphemous. And there is a wonderful tenacity of life about this sort of opposition to physical science. Crushed and maimed in every battle, it yet seems never to be slain; and after a hundred defeats it is at this day as rampant, though happily not so mischievous, as in the time of Galileo.

"But to those whose life is spent, to use Newton's noble words, in picking up here a pebble and there a pebble on the shores of the great ocean of truth — who watch, day by day, the slow but sure advance of that mighty tide, bearing on its bosom the thousand treasures wherewith man ennobles and beautifies his life — it would be laughable, if it were not so sad, to see the little Canutes of the hour enthroned in solemn state, bidding that great wave to stay, and threatening to check its beneficent progress. The wave rises and they fly; but, unlike the brave old Dane, they learn no lesson of humility: the throne is pitched at what seems a safe distance, and the folly is repeated.

"Surely it is the duty of the public to discourage anything of this kind, to discredit these foolish meddlers who think they do the Almighty a service by preventing a thorough study of His works.

"The Origin of Species is not the first, and it will not be the last, of the great questions born of science, which will demand settlement from this generation. The general mind is seething strangely, and to those who watch the signs of the times, it seems plain that this nineteenth century will see revolutions of thought and practice as great as those which the sixteenth witnessed. Through what trials and sore contests the civilised world will have to pass in the course of this new reformation, who can tell?

"But I verily believe that come what will, the part which England may play in the battle is a grand and a noble one. She may prove to the world that, for one people, at any rate, despotism and demagogy are not the necessary alternatives of government; that freedom and order are not incompatible; that reverence is the handmaid of knowledge; that free discussion is the life of truth, and of true unity in a nation.

"Will England play this part? That depends upon how you, the public, deal with science. Cherish her, venerate her, follow her methods faithfully and implicitly in their application to all branches of human thought, and the future of this people will be greater than the past.

"Listen to those who would silence and crush her, and I fear our children will see the glory of England vanishing like Arthur in the mist; they will cry too late the woful cry of Guinever: —

'It was my duty to have loved the highest;
It surely was my profit had I known;
It would have been my pleasure had I seen.'"]

CHARLES DARWIN TO C. LYELL. Down [February 15th, 1860].

... I am perfectly convinced (having read this morning) that the review in the 'Annals' (Annals and Mag. of Nat. Hist. third series, vol. 5, page 132. My father has obviously taken the expression "pestilent" from the following passage (page 138): "But who is this Nature, we have a right to ask, who has such tremendous power, and to whose efficiency such marvellous performances are ascribed? What are her image and attributes, when dragged from her wordy lurking-place? Is she aught but a pestilent abstraction, like dust cast in our eyes to obscure the workings of an Intelligent First Cause of all?" The reviewer pays a tribute to my father's candour, "so manly and outspoken as almost to 'cover a multitude of sins.'" The parentheses (to which allusion is made above) are so frequent as to give a characteristic appearance to Mr. Wollaston's pages.) is by Wollaston; no one else in the world would have used so many parentheses. I have written to him, and told him that the "pestilent" fellow thanks him for his kind manner of speaking about him. I have also told him that he would be pleased to hear that the Bishop of Oxford says it is the most unphilosophical (Another version of the words is given by Lyell, to whom they were spoken, viz. "the most illogical book ever written." — 'Life,' volume ii. page 358.) work he ever read. The review seems to me clever, and only misinterprets me in a few places. Like all hostile men, he passes over the explanation given of Classification, Morphology, Embryology, and Rudimentary Organs, etc. I read Wallace's paper in MS. ("On the Zoological Geography of the Malay Archipelago." — Linn. Soc. Journ. 1860.), and thought it admirably good; he does not know that he has been anticipated about the depth of intervening sea determining distribution... The most curious point in the paper seems to me that about the African character of the Celebes productions, but I should require further confirmation...

Henslow is staying here; I have had some talk with him; he is in much the same state as Bunbury (The late Sir Charles Bunbury, well-known as a Palaeo-botanist.), and will go a very little way with us, but brings up no real argument against going further. He also shudders at the eye! It is really curious (and perhaps is an argument in our favour) how differently different opposers view the subject. Henslow used to rest his opposition on the imperfection of the Geological Record, but he now thinks nothing of this, and says I have got well out of it; I wish I could quite agree with him. Baden Powell says he never read anything so conclusive as my statement about the eye!! A stranger writes to me about sexual selection, and regrets that I boggle about such a trifle as the brush of hair on the male turkey, and so on. As L. Jenyns has a really philosophical mind, and as you say you like to see everything, I send an old letter of his. In a later letter to Henslow, which I have seen, he is more candid than any opposer I have heard of, for he says, though he CANNOT go so far as I do, yet he can give no good reason why he should not. It is funny how each man draws his own imaginary line at which to halt. It reminds me so vividly what I was told (By Professor Henslow.) about you when I first commenced geology — to believe a LITTLE, but on no account to believe all.

Ever yours affectionately, C. DARWIN.

CHARLES DARWIN TO ASA GRAY. Down, February 18th [1860].

My dear Gray,

I received about a week ago two sheets of your Review (The 'American Journal of Science and Arts,' March, 1860. Reprinted in 'Darwiniana,' 1876.); read them, and sent them to Hooker; they are now returned and r-read with care, and to-morrow I send them to Lyell. Your Review seems to me ADMIRABLE; by far the best which I have read. I thank you from my heart both for myself, but far more for the subject's sake. Your contrast between the views of Agassiz and such as mine is very curious and instructive. (The contrast is briefly summed up thus: "The theory of Agassiz regards the origin of species and their present general distribution over the world as equally primordial, equally supernatural; that of Darwin as equally derivative, equally natural." — 'Darwiniana,' page 14.) By the way, if Agassiz writes anything on the subject, I hope you will tell me. I am charmed with your metaphor of the streamlet never running against the force of gravitation. Your distinction between an hypothesis and theory seems to me very ingenious; but I do not think it is ever followed. Every one now speaks of the undulatory THEORY of light; yet the ether is itself hypothetical, and the undulations are inferred only from explaining the phenomena of light. Even in the THEORY of gravitation is the attractive power in any way known, except by explaining the fall of the apple, and the movements of the Planets? It seems to me that an hypothesis is DEVELOPED into a theory solely by explaining an ample lot of facts. Again and again I thank you for your generous aid in discussing a view, about which you very properly hold yourself unbiassed.

My dear Gray, yours most sincerely, C. DARWIN.

P.S. — Several clergymen go far with me. Rev. L. Jenyns, a very good naturalist. Henslow will go a very little way with me, and is not shocked with me. He has just been visiting me.

[With regard to the attitude of the more liberal representatives of the Church, the following letter (already referred to) from Charles Kingsley is of interest:]

C. KINGSLEY TO CHARLES DARWIN. Eversley Rectory, Winchfield, November 18th, 1859.

Dear Sir,

I have to thank you for the unexpected honour of your book. That the Naturalist whom, of all naturalists living, I most wish to know and to learn from, should have sent a scientist like me his book, encourages me at least to observe more carefully, and perhaps more slowly.

I am so poorly (in brain), that I fear I cannot read your book just now as I ought. All I have seen of it AWES me; both with the heap of facts and the prestige of your name, and also with the clear intuition, that if you be right, I must give up much that I have believed and written.

In that I care little. Let God be true, and every man a liar! Let us know what IS, and, as old Socrates has it, epesthai to logo — follow up the villainous shifty fox of an argument, into whatsoever unexpected bogs and brakes he may lead us, if we do but run into him at last.

From two common superstitions, at least, I shall be free while judging of your books: —

1. I have long since, from watching the crossing of domesticated animals and plants, learnt to disbelieve the dogma of the permanence of species.

2. I have gradually learnt to see that it is just as noble a conception of Deity, to believe that he created primal forms capable of self development into all forms needful pro tempore and pro loco, as to believe that He required a fresh act of intervention to supply the lacunas which He Himself had made. I question whether the former be not the loftier thought.

Be it as it may, I shall prize your book, both for itself, and as a proof that you are aware of the existence of such a person as

Your faithful servant, C. KINGSLEY.

[My father's old friend, the Rev. J. Brodie Innes, of Milton Brodie, who was for many years Vicar of Down, writes in the same spirit:

"We never attacked each other. Before I knew Mr. Darwin I had adopted, and publicly expressed, the principle that the study of natural history, geology, and science in general, should be pursued without reference to the Bible. That the Book of Nature and Scripture came from the same Divine source, ran in parallel lines, and when properly understood would never cross...

"His views on this subject were very much to the same effect from his side. Of course any conversations we may have had on purely religious subjects are as sacredly private now as in his life; but the quaint conclusion of one may be given. We had been speaking of the apparent contradiction of some supposed discoveries with the Book of Genesis; he said, 'you are (it would have been more correct to say you ought to be) a theologian, I am a naturalist, the lines are separate. I endeavour to discover facts without considering what is said in the Book of Genesis. I do not attack Moses, and I think Moses can take care of himself.' To the same effect he wrote more recently, 'I cannot remember that I ever published a word directly against religion or the clergy; but if you were to read a little pamphlet which I received a couple of days ago by a clergyman, you would laugh, and admit that I had some excuse for bitterness. After abusing me for two or three pages, in language sufficiently plain and emphatic to have satisfied any reasonable man, he sums up by saying that he has vainly searched the English language to find terms to express his contempt for me and all Darwinians.' In another letter, after I had left Down, he writes, 'We often differed, but you are one of those rare mortals from whom one can differ and yet feel no shade of animosity, and that is a thing [of] which I should feel very proud, if any one could say [it] of me.'

"On my last visit to Down, Mr. Darwin said, at his dinner-table, 'Brodie Innes and I have been fast friends for thirty years, and we never thoroughly agreed on any subject but once, and then we stared hard at each other, and thought one of us must be very ill.'"]

CHARLES DARWIN TO C. LYELL. Down, February 23rd [1860].

My dear Lyell,

That is a splendid answer of the father of Judge Crompton. How curious that the Judge should have hit on exactly the same points as yourself. It shows me what a capital lawyer you would have made, how many unjust acts you would have made appear just! But how much grander a field has science been than the law, though the latter might have made you Lord Kinnordy. I will, if there be another edition, enlarge on gradation in the eye, and on all forms coming from one prototype, so as to try and make both less glaringly improbable...

With respect to Bronn's objection that it cannot be shown how life arises, and likewise to a certain extent Asa Gray's remark that natural selection is not a vera causa, I was much interested by finding accidentally in Brewster's 'Life of Newton,' that Leibnitz objected to the law of gravity because Newton could not show what gravity itself is. As it has chanced, I have used in letters this very same argument, little knowing that any one had really thus objected to the law of gravity. Newton answers by saying that it is philosophy to make out the movements of a clock, though you do not know why the weight descends to the ground. Leibnitz further objected that the law of gravity was opposed to Natural Religion! Is this not curious? I really think I shall use the facts for some introductory remarks for my bigger book.

... You ask (I see) why we do not have monstrosities in higher animals; but when they live they are almost always sterile (even giants and dwarfs are GENERALLY sterile), and we do not know that Harvey's monster would have bred. There is I believe only one case on record of a peloric flower being fertile, and I cannot remember whether this reproduced itself.

To recur to the eye. I really think it would have been dishonest, not to have faced the difficulty; and worse (as Talleyrand would have said), it would have been impolitic I think, for it would have been thrown in my teeth, as H. Holland threw the bones of the ear, till Huxley shut him up by showing what a fine gradation occurred amongst living creatures.

I thank you much for your most pleasant letter.

Yours affectionately, C. DARWIN.

P.S. — I send a letter by Herbert Spencer, which you can read or not as you think fit. He puts, to my mind, the philosophy of the argument better than almost any one, at the close of the letter. I could make nothing of Dana's idealistic notions about species; but then, as Wollaston says, I have not a metaphysical head.

By the way, I have thrown at Wollaston's head, a paper by Alexander Jordan, who demonstrates metaphysically that all our cultivated races are Go-created species.

Wollaston misrepresents accidentally, to a wonderful extent, some passages in my book. He reviewed, without relooking at certain passages.

CHARLES DARWIN TO C. LYELL. Down, February 25th [1860].

... I cannot help wondering at your zeal about my book. I declare to heaven you seem to care as much about my book as I do myself. You have no right to be so eminently unselfish! I have taken off my spit [i.e. file] a letter of Ramsay's, as every geologist convert I think very important. By the way, I saw some time ago a letter from H.D. Rogers (Professor of Geology in the University of Glasgow. Born in the United States 1809, died 1866.) to Huxley, in which he goes very far with us...

CHARLES DARWIN TO J.D. HOOKER. Down, Saturday, March 3rd, [1860].

My dear Hooker,

What a day's work you had on that Thursday! I was not able to go to London till Monday, and then I was a fool for going, for, on Tuesday night, I had an attack of fever (with a touch of pleurisy), which came on like a lion, but went off as a lamb, but has shattered me a good bit.

I was much interested by your last note... I think you expect too much in regard to change of opinion on the subject of Species. One large class of men, more especially I suspect of naturalists, never will care about ANY general question, of which old Gray, of the British Museum, may be taken as a type; and secondly, nearly all men past a moderate age, either in actual years or in mind, are, I am fully convinced, incapable of looking at facts under a new point of view. Seriously, I am astonished and rejoiced at the progress which the subject has made; look at the enclosed memorandum. (See table of names below.) — says my book will be forgotten in ten years, perhaps so; but, with such a list, I feel convinced the subject will not. The outsiders, as you say, are strong.

You say that you think that Bentham is touched, "but, like a wise man, holds his tongue." Perhaps you only mean that he cannot decide, otherwise I should think such silence the reverse of magnanimity; for if others behaved the same way, how would opinion ever progress? It is a dereliction of actual duty. (In a subsequent letter to Sir J.D. Hooker (March 12th, 1860), my father wrote, "I now quite understand Bentham's silence.")

I am so glad to hear about Thwaites. (Dr. G.J.K. Thwaites, who was born in 1811, established a reputation in this country as an expert microscopist, and an acute observer, working especially at cryptogamic botany. On his appointment as Director of the Botanic Gardens at Peradenyia, Ceylon, Dr. Thwaites devoted himself to the flora of Ceylon. As a result of this he has left numerous and valuable collections, a description of which he embodied in his 'Enumeratio Plantarum Zeylaniae' (1864). Dr. Thwaites was a fellow of the Linnean Society, but beyond the above facts little seems to have been recorded of his life. His death occurred in Ceylon on September 11th, 1882, in his seventy-second year. "Athenaeum", October 14th, 1882, page 500.)... I have had an astounding letter from Dr. Boott (The letter is enthusiastically laudatory, and obviously full of genuine feeling.); it might be turned into ridicule against him and me, so I will not send it to any one. He writes in a noble spirit of love of truth.

I wonder what Lindley thinks; probably too busy to read or think on the question.

I am vexed about Bentham's reticence, for it would have been of real value to know what parts appeared weakest to a man of his powers of observation.

Farewell, my dear Hooker, yours affectionately, C. DARWIN.

P.S. — Is not Harvey in the class of men who do not at all care for generalities? I remember your saying you could not get him to write on Distribution. I have found his works very unfruitful in every respect.

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  [Here follows the memorandum referred to:]

  Geologists.    Zoologists and       Physiologists.    Botanists.
                 Palaeontologists.

  Lyell.         Huxley.              Carpenter.        Hooker.

   Ramsay.*        J.  Lubbock.           Sir  H.  Holland    H.C.
Watson.
                                      (to large extent).

  Jukes.*        L. Jenyns                              Asa Gray
                  (to large extent).                      (to some
extent).

  H.D. Rogers.   Searles Wood.*                         Dr. Boott
                                                        (to large
extent).

                                                        Thwaites.
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Joseph Beete Jukes, M.A., F.R.S., 1811-1869. He was educated at Cambridge, and from 1842 to 1846 he acted as naturalist to H.M.S. "Fly", on an exploring expedition in Australia and New Guinea. He was afterwards appointed Director of the Geological Survey of Ireland. He was the author of many papers, and of more than one good hand-book of geology.

Searles Valentine Wood, February 14, 1798-1880. Chiefly known for his work on the Mollusca of the 'Crag.')

[The following letter is of interest in connection with the mention of Mr. Bentham in the last letter:]

G. BENTHAM TO FRANCIS DARWIN. 25 Wilton Place, S.W., May 30th, 1882.

My dear Sir,

In compliance with your note which I received last night, I send herewith the letters I have from your father. I should have done so on seeing the general request published in the papers, but that I did not think there were any among them which could be of any use to you. Highly flattered as I was by the kind and friendly notice with which Mr. Darwin occasionally honoured me, I was never admitted into his intimacy, and he therefore never made any communications to me in relation to his views and labours. I have been throughout one of his most sincere admirers, and fully adopted his theories and conclusions, notwithstanding the severe pain and disappointment they at first occasioned me. On the day that his celebrated paper was read at the Linnean Society, July 1st, 1858, a long paper of mine had been set down for reading, in which, in commenting on the British Flora, I had collected a number of observations and facts illustrating what I then believed to be a fixity in species, however difficult it might be to assign their limits, and showing a tendency of abnormal forms produced by cultivation or otherwise, to withdraw within those original limits when left to themselves. Most fortunately my paper had to give way to Mr. Darwin's and when once that was read, I felt bound to defer mine for reconsideration; I began to entertain doubts on the subject, and on the appearance of the 'Origin of Species,' I was forced, however reluctantly, to give up my long-cherished convictions, the results of much labour and study, and I cancelled all that part of my paper which urged original fixity, and published only portions of the remainder in another form, chiefly in the 'Natural History Review.' I have since acknowledged on various occasions my full adoption of Mr. Darwin's views, and chiefly in my Presidential Address of 1863, and in my thirteenth and last address, issued in the form of a report to the British Association at its meeting at Belfast in 1874.

I prize so highly the letters that I have of Mr. Darwin's, that I should feel obliged by your returning them to me when you have done with them. Unfortunately I have not kept the envelopes, and Mr. Darwin usually only dated them by the month not by the year, so that they are not in any chronological order.

Yours very sincerely, GEORGE BENTHAM.

CHARLES DARWIN TO C. LYELL. Down [March] 12th [1860].

My dear Lyell,

Thinking over what we talked about, the high state of intellectual development of the old Grecians with the little or no subsequent improvement, being an apparent difficulty, it has just occurred to me that in fact the case harmonises perfectly with our views. The case would be a decided difficulty on the Lamarckian or Vestigian doctrine of necessary progression, but on the view which I hold of progression depending on the conditions, it is no objection at all, and harmonises with the other facts of progression in the corporeal structure of other animals. For in a state of anarchy, or despotism, or bad government, or after irruption of barbarians, force, strength, or ferocity, and not intellect, would be apt to gain the day.

We have so enjoyed your and Lady Lyell's visit.

Good-night. C. DARWIN.

P.S. — By an odd chance (for I had not alluded even to the subject) the ladies attacked me this evening, and threw the high state of old Grecians into my teeth, as an unanswerable difficulty, but by good chance I had my answer all pat, and silenced them. Hence I have thought it worth scribbling to you...

CHARLES DARWIN TO J. PRESTWICH. (Now Professor of Geology in the University of Oxford.) Down, March 12th [1860].

... At some future time, when you have a little leisure, and when you have read my 'Origin of Species,' I should esteem it a SINGULAR favour if you would send me any general criticisms. I do not mean of unreasonable length, but such as you could include in a letter. I have always admired your various memoirs so much that I should be eminently glad to receive your opinion, which might be of real service to me.

Pray do not suppose that I expect to CONVERT or PERVERT you; if I could stagger you in ever so slight a degree I should be satisfied; nor fear to annoy me by severe criticisms, for I have had some hearty kicks from some of my best friends. If it would not be disagreeable to you to send me your opinion, I certainly should be truly obliged...

CHARLES DARWIN TO ASA GRAY. Down, April 3rd [1860].

... I remember well the time when the thought of the eye made me cold all over, but I have got over this stage of the complaint, and now small trifling particulars of structure often make me very uncomfortable. The sight of a feather in a peacock's tail, whenever I gaze at it, makes me sick!..

You may like to hear about reviews on my book. Sedgwick (as I and Lyell feel CERTAIN from internal evidence) has reviewed me savagely and unfairly in the "Spectator". (See the quotations which follow the present letter.) The notice includes much abuse, and is hardly fair in several respects. He would actually lead any one, who was ignorant of geology, to suppose that I had invented the great gaps between successive geological formations, instead of its being an almost universally admitted dogma. But my dear old friend Sedgwick, with his noble heart, is old, and is rabid with indignation. It is hard to please every one; you may remember that in my last letter I asked you to leave out about the Weald denudation: I told Jukes this (who is head man of the Irish geological survey), and he blamed me much, for he believed every word of it, and thought it not at all exaggerated! In fact, geologists have no means of gauging the infinitude of past time. There has been one prodigy of a review, namely, an OPPOSED one (by Pictet (Francois Jules Pictet, in the 'Archives des Sciences de la Bibliotheque Universelle,' Mars 1860. The article is written in a courteous and considerate tone, and concludes by saying that the 'Origin' will be of real value to naturalists, especially if they are not led away by its seductive arguments to believe in the dangerous doctrine of modification. A passage which seems to have struck my father as being valuable, and opposite which he has made double pencil marks and written the word "good," is worth quoting: "La theorie de M. Darwin s'accorde mal avec l'histoire des types a formes bien tranchees et definies qui paraissent n'avoir vecu que pendant un temps limite. On en pourrait citer des centaines d'exemples, tel que les reptiles volants, les ichthyosaures, les belemnites, les ammonites, etc." Pictet was born in 1809, died 1872; he was Professor of Anatomy and Zoology at Geneva.), the palaeontologist, in the Bib. Universelle of Geneva) which is PERFECTLY fair and just, and I agree to every word he says; our only difference being that he attaches less weight to arguments in favour, and more to arguments opposed, than I do. Of all the opposed reviews, I think this the only quite fair one, and I never expected to see one. Please observe that I do not class your review by any means as opposed, though you think so yourself! It has done me MUCH too good service ever to appear in that rank in my eyes. But I fear I shall weary you with so much about my book. I should rather think there was a good chance of my becoming the most egotistical man in all Europe! What a proud pre-eminence! Well, you have helped to make me so and therefore you must forgive me if you can.

My dear Gray, ever yours most gratefully, C. DARWIN.

[In a letter to Sir Charles Lyell reference is made to Sedgwick's review in the "Spectator", March 24:

"I now feel certain that Sedgwick is the author of the article in the "Spectator". No one else could use such abusive terms. And what a misrepresentation of my notions! Any ignoramus would suppose that I had FIRST broached the doctrine, that the breaks between successive formations marked long intervals of time. It is very unfair. But poor dear old Sedgwick seems rabid on the question. "Demoralised understanding!" If ever I talk with him I will tell him that I never could believe that an inquisitor could be a good man: but now I know that a man may roast another, and yet have as kind and noble a heart as Sedgwick's."

The following passages are taken from the review:

"I need hardly go on any further with these objections. But I cannot conclude without expressing my detestation of the theory, because of its unflinching materialism; — because it has deserted the inductive track, the only track that leads to physical truth; — because it utterly repudiates final causes, and thereby indicates a demoralised understanding on the part of its advocates."

"Not that I believe that Darwin is an atheist; though I cannot but regard his materialism as atheistical. I think it untrue, because opposed to the obvious course of nature, and the very opposite of inductive truth. And I think it intensely mischievous."

"Each series of facts is laced together by a series of assumptions, and repetitions of the one false principle. You cannot make a good rope out of a string of air bubbles."

"But any startling and (supposed) novel paradox, maintained very boldly and with something of imposing plausibility, produces in some minds a kind of pleasing excitement which predisposes them in its favour; and if they are unused to careful reflection, and averse to the labour of accurate investigation, they will be likely to conclude that what is (apparently) ORIGINAL, must be a production of original GENIUS, and that anything very much opposed to prevailing notions must be a grand DISCOVERY, — in short, that whatever comes from the 'bottom of a well' must be the 'truth' supposed to be hidden there."

In a review in the December number of 'Macmillan's Magazine,' 1860, Fawcett vigorously defended my father from the charge of employing a false method of reasoning; a charge which occurs in Sedgwick's review, and was made at the time ad nauseam, in such phrases as: "This is not the true Baconian method." Fawcett repeated his defence at the meeting of the British Association in 1861. (See an interesting letter from my father in Mr. Stephen's 'Life of Henry Fawcett,' 1886, page 101.)]

CHARLES DARWIN TO W.B CARPENTER. Down, April 6th [1860].

My dear Carpenter,

I have this minute finished your review in the 'Med. Chirurg. Review.' (April 1860.) You must let me express my admiration at this most able essay, and I hope to God it will be largely read, for it must produce a great effect. I ought not, however, to express such warm admiration, for you give my book, I fear, far too much praise. But you have gratified me extremely; and though I hope I do not care very much for the approbation of the non-scientific readers, I cannot say that this is at all so with respect to such few men as yourself. I have not a criticism to make, for I object to not a word; and I admire all, so that I cannot pick out one part as better than the rest. It is all so well balanced. But it is impossible not to be struck with your extent of knowledge in geology, botany, and zoology. The extracts which you give from Hooker seem to me EXCELLENTLY chosen, and most forcible. I am so much pleased in what you say also about Lyell. In fact I am in a fit of enthusiasm, and had better write no more. With cordial thanks,

Yours very sincerely, C. DARWIN.

CHARLES DARWIN TO C. LYELL. Down, April 10th [1860].

My dear Lyell,

Thank you much for your note of the 4th; I am very glad to hear that you are at Torquay. I should have amused myself earlier by writing to you, but I have had Hooker and Huxley staying here, and they have fully occupied my time, as a little of anything is a full dose for me... There has been a plethora of reviews, and I am really quite sick of myself. There is a very long review by Carpenter in the 'Medical and Chirurg. Review,' very good and well balanced, but not brilliant. He discusses Hooker's books at as great length as mine, and makes excellent extracts; but I could not get Hooker to feel the least interest in being praised.

Carpenter speaks of you in thoroughly proper terms. There is a BRILLIANT review by Huxley ('Westminster Review,' April 1860.), with capital hits, but I do not know that he much advances the subject. I THINK I have convinced him that he has hardly allowed weight enough to the case of varieties of plants being in some degrees sterile.

To diverge from reviews: Asa Gray sends me from Wyman (who will write), a good case of all the pigs being black in the Everglades of Virginia. On asking about the cause, it seems (I have got capital analogous cases) that when the BLACK pigs eat a certain nut their bones become red, and they suffer to a certain extent, but that the WHITE pigs lose their hoofs and perish, "and we aid by SELECTION, for we kill most of the young white pigs." This was said by men who could hardly read. By the way, it is a great blow to me that you cannot admit the potency of natural selection. The more I think of it, the less I doubt its power for great and small changes. I have just read the 'Edinburgh' ('Edinburgh Review,' April 1860.), which without doubt is by — . It is extremely malignant, clever, and I fear will be very damaging. He is atrociously severe on Huxley's lecture, and very bitter against Hooker. So we three ENJOYED it together. Not that I really enjoyed it, for it made me uncomfortable for one night; but I have got quite over it to-day. It requires much study to appreciate all the bitter spite of many of the remarks against me; indeed I did not discover all myself. It scandalously misrepresents many parts. He misquotes some passages, altering words within inverted commas...

It is painful to be hated in the intense degree with which — hates me.

Now for a curious thing about my book, and then I have done. In last Saturday's "Gardeners' Chronicle" (April 7th, 1860.), a Mr. Patrick Matthew publishes a long extract from his work on 'Naval Timber and Arboriculture,' published in 1831, in which he briefly but completely anticipates the theory of Natural Selection. I have ordered the book, as some few passages are rather obscure, but it is certainly, I think, a complete but not developed anticipation! Erasmus always said that surely this would be shown to be the case some day. Anyhow, one may be excused in not having discovered the fact in a work on Naval Timber.

I heartily hope that your Torquay work may be successful. Give my kindest remembrances to Falconer, and I hope he is pretty well. Hooker and Huxley (with Mrs. Huxley) were extremely pleasant. But poor dear Hooker is tired to death of my book, and it is a marvel and a prodigy if you are not worse tired — if that be possible. Farewell, my dear Lyell, Yours affectionately, C. DARWIN.

CHARLES DARWIN TO J.D. HOOKER. Down, [April 13th, 1860].

My dear Hooker,

Questions of priority so often lead to odious quarrels, that I should esteem it a great favour if you would read the enclosed. ((My father wrote ("Gardeners' Chronicle", 1860, page 362, April 21st): "I have been much interested by Mr. Patrick Matthew's communication in the number of your paper dated April 7th. I freely acknowledge that Mr. Matthew has anticipated by many years the explanation which I have offered of the origin of species, under the name of natural selection. I think that no one will feel surprised that neither I, nor apparently any other naturalist, had heard of Mr. Matthew's views, considering how briefly they are given, and that they appeared in the appendix to a work on Naval Timber and Arboriculture. I can do no more than offer my apologies to Mr. Matthew for my entire ignorance of this publication. If any other edition of my work is called for, I will insert to the foregoing effect." In spite of my father's recognition of his claims, Mr. Matthew remained unsatisfied, and complained that an article in the 'Saturday Analyst and Leader' was "scarcely fair in alluding to Mr. Darwin as the parent of the origin of species, seeing that I published the whole that Mr. Darwin attempts to prove, more than twenty-nine years ago." — "Saturday Analyst and Leader", November 24, 1860.) If you think it proper that I should send it (and of this there can hardly be any question), and if you think it full and ample enough, please alter the date to the day on which you post it, and let that be soon. The case in the "Gardeners' Chronicle" seems a LITTLE stronger than in Mr. Matthew's book, for the passages are therein scattered in three places; but it would be mere hair-splitting to notice that. If you object to my letter, please return it; but I do not expect that you will, but I thought that you would not object to run your eye over it. My dear Hooker, it is a great thing for me to have so good, true, and old a friend as you. I owe much for science to my friends.

Many thanks for Huxley's lecture. The latter part seemed to be grandly eloquent.

... I have gone over [the 'Edinburgh'] review again, and compared passages, and I am astonished at the misrepresentations. But I am glad I resolved not to answer. Perhaps it is selfish, but to answer and think more on the subject is too unpleasant. I am so sorry that Huxley by my means has been thus atrociously attacked. I do not suppose you much care about the gratuitous attack on you.

Lyell in his letter remarked that you seemed to him as if you were overworked. Do, pray, be cautious, and remember how many and many a man has done this — who thought it absurd till too late. I have often thought the same. You know that you were bad enough before your Indian journey.

CHARLES DARWIN TO C. LYELL. Down, April [1860].

My dear Lyell,

I was very glad to get your nice long letter from Torquay. A press of letters prevented me writing to Wells. I was particularly glad to hear what you thought about not noticing [the 'Edinburgh'] review. Hooker and Huxley thought it a sort of duty to point out the alteration of quoted citations, and there is truth in this remark; but I so hated the thought that I resolved not to do so. I shall come up to London on Saturday the 14th, for Sir B. Brodie's party, as I have an accumulation of things to do in London, and will (if I do not hear to the contrary) call about a quarter before ten on Sunday morning, and sit with you at breakfast, but will not sit long, and so take up much of your time. I must say one more word about our quasi-theological controversy about natural selection, and let me have your opinion when we meet in London. Do you consider that the successive variations in the size of the crop of the Pouter Pigeon, which man has accumulated to please his caprice, have been due to "the creative and sustaining powers of Brahma?" In the sense that an omnipotent and omniscient Deity must order and know everything, this must be admitted; yet, in honest truth, I can hardly admit it. It seems preposterous that a maker of a universe should care about the crop of a pigeon solely to please man's silly fancies. But if you agree with me in thinking such an interposition of the Deity uncalled for, I can see no reason whatever for believing in such interpositions in the case of natural beings, in which strange and admirable peculiarities have been naturally selected for the creature's own benefit.

Imagine a Pouter in a state of nature wading into the water and then, being buoyed up by its inflated crop, sailing about in search of food. What admiration this would have excited — adaptation to the laws of hydrostatic pressure, etc. etc. For the life of me I cannot see any difficulty in natural selection producing the most exquisite structure, IF SUCH STRUCTURE CAN BE ARRIVED AT BY GRADATION, and I know from experience how hard it is to name any structure towards which at least some gradations are not known.

Ever yours, C. DARWIN.

P.S. — The conclusion at which I have come, as I have told Asa Gray, is that such a question, as is touched on in this note, is beyond the human intellect, like "predestination and free will," or the "origin of evil."

CHARLES DARWIN TO J.D. HOOKER. Down, [April 18th, 1860].

My dear Hooker,

I return — 's letter... Some of my relations say it cannot POSSIBLY be — 's article (The 'Edinburgh Review.'), because the reviewer speaks so very highly of — . Poor dear simple folk! My clever neighbour, Mr. Norman, says the article is so badly written, with no definite object, that no one will read it. Asa Gray has sent me an article ('North American Review,' April, 1860. "By Professor Bowen," is written on my father's copy. The passage referred to occurs at page 488, where the author says that we ought to find "an infinite number of other varieties — gross, rude, and purposeless — the unmeaning creations of an unconscious cause.") from the United States, clever, and dead against me. But one argument is funny. The reviewer says, that if the doctrine were true, geological strata would be full of monsters which have failed! A very clear view this writer had of the struggle for existence!

... I am glad you like Adam Bede so much. I was charmed with it...

We think you must by mistake have taken with your own numbers of the 'National Review' my precious number. (This no doubt refers to the January number, containing Dr. Carpenter's review of the 'Origin.') I wish you would look.

CHARLES DARWIN TO ASA GRAY. Down, April 25th [1860].

My dear Gray,

I have no doubt I have to thank you for the copy of a review on the 'Origin' in the 'North American Review.' It seems to me clever, and I do not doubt will damage my book. I had meant to have made some remarks on it; but Lyell wished much to keep it, and my head is quite confused between the many reviews which I have lately read. I am sure the reviewer is wrong about bees' cells, i.e. about the distance; any lesser distance would do, or even greater distance, but then some of the places would lie outside the generative spheres; but this would not add much difficulty to the work. The reviewer takes a strange view of instinct: he seems to regard intelligence as a developed instinct; which I believe to be wholly false. I suspect he has never much attended to instinct and the minds of animals, except perhaps by reading.

My chief object is to ask you if you could procure for me a copy of the "New York Times" for Wednesday, March 28th. It contains A VERY STRIKING review of my book, which I should much like to keep. How curious that the two most striking reviews (i.e. yours and this) should have appeared in America. This review is not really useful, but somehow is impressive. There was a good review in the 'Revue des Deux Mondes,' April 1st, by M. Laugel, said to be a very clever man.

Hooker, about a fortnight ago, stayed here a few days, and was very pleasant; but I think he overworks himself. What a gigantic undertaking, I imagine, his and Bentham's 'Genera Plantarum' will be! I hope he will not get too much immersed in it, so as not to spare some time for Geographical Distribution and other such questions.

I have begun to work steadily, but very slowly as usual, at details on variation under domestication.

My dear Gray, Yours always truly and gratefully, C. DARWIN.

CHARLES DARWIN TO C. LYELL. Down, [May 8th, 1860].

... I have sent for the 'Canadian Naturalist.' If I cannot procure a copy I will borrow yours. I had a letter from Henslow this morning, who says that Sedgwick was, on last Monday night, to open a battery on me at the Cambridge Philosophical Society. Anyhow, I am much honoured by being attacked there, and at the Royal Society of Edinburgh.

I do not think it worth while to contradict single cases nor is it worth while arguing against those who do not attend to what I state. A moment's reflection will show you that there must be (on our doctrine) large genera not varying (see page 56 on the subject, in the second edition of the 'Origin'). Though I do not there discuss the case in detail.

It may be sheer bigotry for my own notions, but I prefer to the Atlantis, my notion of plants and animals having migrated from the Old to the New World, or conversely, when the climate was much hotter, by approximately the line of Behring's Straits. It is most important, as you say, to see living forms of plants going back so far in time. I wonder whether we shall ever discover the flora of the dry land of the coal period, and find it not so anomalous as the swamp or coal-making flora. I am working away over the blessed Pigeon Manuscript; but, from one cause or another, I get on very slowly...

This morning I got a letter from the Academy of Natural Sciences of Philadelphia, announcing that I am elected a correspondent... It shows that some Naturalists there do not think me such a scientific profligate as many think me here.

My dear Lyell, yours gratefully, C. DARWIN.

P.S. — What a grand fact about the extinct stag's horn worked by man!

CHARLES DARWIN TO J.D. HOOKER. Down, [May 13th, 1860].

My dear Hooker,

I return Henslow, which I was very glad to see. How good of him to defend me. (Against Sedgwick's attack before the Cambridge Philosophical Society.) I will write and thank him.

As you said you were curious to hear Thomson's (Dr. Thomas Thomson the Indian Botanist. He was a collaborateur in Hooker and Thomson's Flora Indica. 1855.) opinion, I send his kind letter. He is evidently a strong opposer to us...

CHARLES DARWIN TO J.D. HOOKER. Down, [May 15th, 1860].

... How paltry it is in such men as X, Y and Co. not reading your essay. It is incredibly paltry. (These remarks do not apply to Dr. Harvey, who was, however, in a somewhat similar position. See below.) They may all attack me to their hearts' content. I am got case-hardened. As for the old fogies in Cambridge, it really signifies nothing. I look at their attacks as a proof that our work is worth the doing. It makes me resolve to buckle on my armour. I see plainly that it will be a long uphill fight. But think of Lyell's progress with Geology. One thing I see most plainly, that without Lyell's, yours, Huxley's and Carpenter's aid, my book would have been a mere flash in the pan. But if we all stick to it, we shall surely gain the day. And I now see that the battle is worth fighting. I deeply hope that you think so. Does Bentham progress at all? I do not know what to say about Oxford. (His health prevented him from going to Oxford for the meeting of the British Association.) I should like it much with you, but it must depend on health...

Yours must affectionately, C. DARWIN.

CHARLES DARWIN TO C. LYELL. Down, May 18th [1860].

My dear Lyell,

I send a letter from Asa Gray to show how hotly the battle rages there. Also one from Wallace, very just in his remarks, though too laudatory and too modest, and how admirably free from envy or jealousy. He must be a good fellow. Perhaps I will enclose a letter from Thomson of Calcutta; not that it is much, but Hooker thinks so highly of him...

Henslow informs me that Sedgwick (Sedgwick's address is given somewhat abbreviated in "The Cambridge Chronicle", May 19th, 1860.) and then Professor Clarke [sic] (The late William Clark, Professor of Anatomy, my father seems to have misunderstood his informant. I am assured by Mr. J.W. Clark that his father (Prof. Clark) did not support Sedgwick in the attack.) made a regular and savage onslaught on my book lately at the Cambridge Philosophical Society, but Henslow seems to have defended me well, and maintained that the subject was a legitimate one for investigation. Since then Phillips (John Phillips, M.A., F.R.S., born 1800, died 1874, from the effects of a fall. Professor of Geology at King's College, London, and afterwards at Oxford. He gave the 'Rede' lecture at Cambridge on May 15th, 1860, on 'The Succession of Life on the earth.' The Rede Lecturer is appointed annually by the Vice-Chancellor, and is paid by an endowment left in 1524 by Sir Robert Rede, Lord Chief Justice, in the reign of Henry VIII.) has given lectures at Cambridge on the same subject, but treated it very fairly. How splendidly Asa Gray is fighting the battle. The effect on me of these multiplied attacks is simply to show me that the subject is worth fighting for, and assuredly I will do my best... I hope all the attacks make you keep up your courage, and courage you assuredly will require...

CHARLES DARWIN TO A.R. WALLACE. Down, May 18th, 1860.

My dear Mr. Wallace,

I received this morning your letter from Amboyna, dated February 16th, containing some remarks and your too high approval of my book. Your letter has pleased me very much, and I most completely agree with you on the parts which are strongest and which are weakest. The imperfection of the Geological Record is, as you say, the weakest of all; but yet I am pleased to find that there are almost more geological converts than of pursuers of other branches of natural science... I think geologists are more easily converted than simple naturalists, because more accustomed to reasoning. Before telling you about the progress of opinion on the subject, you must let me say how I admire the generous manner in which you speak of my book. Most persons would in your position have felt some envy or jealousy. How nobly free you seem to be of this common failing of mankind. But you speak far too modestly of yourself. You would, if you had my leisure, have done the work just as well, perhaps better, than I have done it...

... Agassiz sends me a personal civil message, but incessantly attacks me; but Asa Gray fights like a hero in defence. Lyell keeps as firm as a tower, and this Autumn will publish on the 'Geological History of Man,' and will then declare his conversion, which now is universally known. I hope that you have received Hooker's splendid essay... Yesterday I heard from Lyell that a German, Dr. Schaaffhausen (Hermann Schaaffhausen 'Ueber Bestandigkeit und Umwandlung der Arten.' Verhandl. d. Naturhist. Vereins, Bonn, 1853. See 'Origin,' Historical Sketch.), has sent him a pamphlet published some years ago, in which the same view is nearly anticipated; but I have not yet seen this pamphlet. My brother, who is a very sagacious man, always said, "you will find that some one will have been before you." I am at work at my larger work, which I shall publish in a separate volume. But from ill-health and swarms of letters, I get on very very slowly. I hope that I shall not have wearied you with these details. With sincere thanks for your letter, and with most deeply felt wishes for your success in science, and in every way, believe me,

Your sincere well-wisher, C. DARWIN.

CHARLES DARWIN TO ASA GRAY. Down, May 22nd 1860.

My dear Gray,

Again I have to thank you for one of your very pleasant letters of May 7th, enclosing a very pleasant remittance of 22 pounds. I am in simple truth astonished at all the kind trouble you have taken for me. I return Appleton's account. For the chance of your wishing for a formal acknowledgment I send one. If you have any further communication to the Appletons, pray express my acknowledgment for [their] generosity; for it is generosity in my opinion. I am not at all surprised at the sale diminishing; my extreme surprise is at the greatness of the sale. No doubt the public has been SHAMEFULLY imposed on! for they bought the book thinking that it would be nice easy reading. I expect the sale to stop soon in England, yet Lyell wrote to me the other day that calling at Murray's he heard that fifty copies had gone in the previous forty-eight hours. I am extremely glad that you will notice in 'Silliman' the additions in the 'Origin.' Judging from letters (and I have just seen one from Thwaites to Hooker), and from remarks, the most serious omission in my book was not explaining how it is, as I believe, that all forms do not necessarily advance, how there can now be SIMPLE organisms still existing... I hear there is a VERY severe review on me in the 'North British,' by a Rev. Mr. Dunns (This statement as to authorship was made on the authority of Robert Chambers.), a Free Kirk minister, and dabbler in Natural History. I should be very glad to see any good American reviews, as they are all more or less useful. You say that you shall touch on other reviews. Huxley told me some time ago that after a time he would write a review on all the reviews, whether he will I know not. If you allude to the 'Edinburgh,' pray notice SOME of the points which I will point out on a separate slip. In the "Saturday Review" (one of our cleverest periodicals) of May 5th, page 573, there is a nice article on [the 'Edinburgh'] review, defending Huxley, but not Hooker; and the latter, I think, [the 'Edinburgh' reviewer] treats most ungenerously. (In a letter to Mr. Huxley my father wrote: "Have you seen the last "Saturday Review"? I am very glad of the defence of you and of myself. I wish the reviewer had noticed Hooker. The reviewer, whoever he is, is a jolly good fellow, as this review and the last on me showed. He writes capitally, and understands well his subject. I wish he had slapped [the 'Edinburgh' reviewer] a little bit harder.") But surely you will get sick unto death of me and my reviewers.

With respect to the theological view of the question. This is always painful to me. I am bewildered. I had no intention to write atheistically. But I own that I cannot see as plainly as others do, and as I should wish to do, evidence of design and beneficence on all sides of us. There seems to me too much misery in the world. I cannot persuade myself that a beneficent and omnipotent God would have designedly created the Ichneumonidae with the express intention of their feeding within the living bodies of Caterpillars, or that a cat should play with mice. Not believing this, I see no necessity in the belief that the eye was expressly designed. On the other hand, I cannot anyhow be contented to view this wonderful universe, and especially the nature of man, and to conclude that everything is the result of brute force. I am inclined to look at everything as resulting from designed laws, with the details, whether good or bad, left to the working out of what we may call chance. Not that this notion AT ALL satisfies me. I feel most deeply that the whole subject is too profound for the human intellect. A dog might as well speculate on the mind of Newton. Let each man hope and believe what he can. Certainly I agree with you that my views are not at all necessarily atheistical. The lightning kills a man, whether a good one or bad one, owing to the excessively complex action of natural laws. A child (who may turn out an idiot) is born by the action of even more complex laws, and I can see no reason why a man, or other animal, may not have been aboriginally produced by other laws, and that all these laws may have been expressly designed by an omniscient Creator, who foresaw every future event and consequence. But the more I think the more bewildered I become; as indeed I probably have shown by this letter.

Most deeply do I feel your generous kindness and interest.

Yours sincerely and cordially, CHARLES DARWIN.

{Here follow my father's criticisms on the 'Edinburgh Review'}:

"What a quibble to pretend he did not understand what I meant by INHABITANTS of South America; and any one would suppose that I had not throughout my volume touched on Geographical Distribution. He ignores also everything which I have said on Classification, Geological Succession, Homologies, Embryology, and Rudimentary Organs — page 496.

He falsely applies what I said (too rudely) about "blindness of preconceived opinions" to those who believe in creation, whereas I exclusively apply the remark to those who give up multitudes of species as true species, but believe in the remainder — page 500.

He slightly alters what I say, — I ASK whether creationists really believe that elemental atoms have flashed into life. He says that I describe them as so believing, and this, surely, is a difference — page 501.

He speaks of my "clamouring against" all who believe in creation, and this seems to me an unjust accusation — page 501.

He makes me say that the dorsal vertebrae vary; this is simply false: I nowhere say a word about dorsal vertebrae — page 522.

What an illiberal sentence that is about my pretension to candour, and about my rushing through barriers which stopped Cuvier: such an argument would stop any progress in science — page 525.

How disingenuous to quote from my remark to you about my BRIEF letter [published in the 'Linn. Soc. Journal'], as if it applied to the whole subject — page 530.

How disingenuous to say that we are called on to accept the theory, from the imperfection of the geological record, when I over and over again [say] how grave a difficulty the imperfection offers — page 530."]

CHARLES DARWIN TO J.D. HOOKER. Down, May 30th [1860].

My dear Hooker,

I return Harvey's letter, I have been very glad to see the reason why he has not read your Essay. I feared it was bigotry, and I am glad to see that he goes a little way (VERY MUCH further than I supposed) with us...

I was not sorry for a natural opportunity of writing to Harvey, just to show that I was not piqued at his turning me and my book into ridicule (A "serio-comic squib," read before the 'Dublin University Zoological and Botanical Association,' February 17, 1860, and privately printed. My father's presentation copy is inscribed "With the writer's REPENTANCE, October 1860."), not that I think it was a proceeding which I deserved, or worthy of him. It delights me that you are interested in watching the progress of opinion on the change of Species; I feared that you were weary of the subject; and therefore did not send A. Gray's letters. The battle rages furiously in the United States. Gray says he was preparing a speech, which would take 1 1/2 hours to deliver, and which he "fondly hoped would be a stunner." He is fighting splendidly, and there seems to have been many discussions with Agassiz and others at the meetings. Agassiz pities me much at being so deluded. As for the progress of opinion, I clearly see that it will be excessively slow, almost as slow as the change of species... I am getting wearied at the storm of hostile reviews and hardly any useful...

CHARLES DARWIN TO C. LYELL. Down, Friday night [June 1st, 1860].

... Have you seen Hopkins (William Hopkins died in 1866, "in his sevent-third year." He began life with a farm in Suffolk, but ultimately entered, comparatively late in life, at Peterhouse, Cambridge; he took his degree in 1827, and afterward became an Esquire Bedell of the University. He was chiefly known as a mathematical "coach," and was eminently successful in the manufacture of Senior Wranglers. Nevertheless Mr. Stephen says ('Life of Fawcett,' page 26) that he "was conspicuous for inculcating" a "liberal view of the studies of the place. He endeavoured to stimulate a philosophical interest in the mathematical sciences, instead of simply rousing an ardour for competition." He contributed many papers on geological and mathematical subjects to the scientific journals. He had a strong influence for good over the younger men with whom he came in contact. The letter which he wrote to Henry Fawcett on the occasion of his blindness illustrates this. Mr. Stephen says ('Life of Fawcett,' page 48) that by "this timely word of good cheer," Fawcett was roused from "his temporary prostration," and enabled to take a "more cheerful and resolute tone.") in the new 'Fraser'? the public will, I should think, find it heavy. He will be dead against me, as you prophesied; but he is generally civil to me personally. ('Fraser's Magazine,' June 1860. My father, no doubt, refers to the following passage, page 752, where the Reviewer Expresses his "full participation in the high respect in which the author is universally held, both as a man and a naturalist; and the more so, because in the remarks which will follow in the second part of this Essay we shall be found to differ widely from him as regards many of his conclusions and the reasonings on which he has founded them, and shall claim the full right to express such differences of opinion with all that freedom which the interests of scientific truth demands, and which we are sure Mr. Darwin would be one of the last to refuse to any one prepared to exercise it with candour and courtesy." Speaking of this review, my father wrote to Dr. Asa Gray: "I have remonstrated with him [Hopkins] for so coolly saying that I base my views on what I reckon as great difficulties. Any one, by taking these difficulties alone, can make a most strong case against me. I could myself write a more damning review than has as yet appeared!" A second notice by Hopkins appeared in the July number of 'Fraser's Magazine.') On his standard of proof, NATURAL science would never progress, for without the making of theories I am convinced there would be no observation.

... I have begun reading the 'North British' (May 1860.), which so far strikes me as clever.

Phillips's Lecture at Cambridge is to be published.

All these reiterated attacks will tell heavily; there will be no more converts, and probably some will go back. I hope you do not grow disheartened, I am determined to fight to the last. I hear, however, that the great Buckle highly approves of my book.

I have had a note from poor Blyth (Edward Blyth, 1810-1873. His indomitable love of natural history made him neglect the druggist's business with which he started in life, and he soon got into serious difficulties. After supporting himself for a few years as a writer on Field Natural History, he ultimately went out to India as Curator of the Museum of the R. Asiatic Soc. of Bengal, where the greater part of his working life was spent. His chief publications were the monthly reports made as part of his duty to the Society. He had stored in his remarkable memory a wonderful wealth of knowledge, especially with regard to the mammalia and birds of India — knowledge of which he freely gave to those who asked. His letters to my father give evidence of having been carefully studied, and the long list of entries after his name in the index to 'Animals and Plants,' show how much help was received from him. His life was an unprosperous and unhappy one, full of money difficulties and darkened by the death of his wife after a few years of marriage.), of Calcutta, who is much disappointed at hearing that Lord Canning will not grant any money; so I much fear that all your great pains will be thrown away. Blyth says (and he is in many respects a very good judge) that his ideas on species are quite revolutionised...

CHARLES DARWIN TO J.D. HOOKER. Down, June 5th [1860].

My dear Hooker,

It is a pleasure to me to write to you, as I have no one to talk about such matters as we write on. But I seriously beg you not to write to me unless so inclined; for busy as you are, and seeing many people, the case is very different between us...

Have you seen — 's abusive article on me?.. It out does even the 'North British' and 'Edinburgh' in misapprehension and misrepresentation. I never knew anything so unfair as in discussing cells of bees, his ignoring the case of Melipona, which builds combs almost exactly intermediate between hive and humble bees. What has — done that he feels so immeasurably superior to all us wretched naturalists, and to all political economists, including that great philosopher Malthus? This review, however, and Harvey's letter have convinced me that I must be a very bad explainer. Neither really understand what I mean by Natural Selection. I am inclined to give up the attempt as hopeless. Those who do not understand, it seems, cannot be made to understand.

By the way, I think, we entirely agree, except perhaps that I use too forcible language about selection. I entirely agree, indeed would almost go further than you when you say that climate (i.e. variability from all unknown causes) is "an active handmaid, influencing its mistress most materially." Indeed, I have never hinted that Natural Selection is "the efficient cause to the exclusion of the other," i.e. variability from Climate, etc. The very term SELECTION implies something, i.e. variation or difference, to be selected...

How does your book progress (I mean your general sort of book on plants), I hope to God you will be more successful than I have been in making people understand your meaning. I should begin to think myself wholly in the wrong, and that I was an utter fool, but then I cannot yet persuade myself, that Lyell, and you and Huxley, Carpenter, Asa Gray, and Watson, etc., are all fools together. Well, time will show, and nothing but time. Farewell...

CHARLES DARWIN TO C. LYELL. Down, June 6th [1860].

... It consoles me that — sneers at Malthus, for that clearly shows, mathematician though he may be, he cannot understand common reasoning. By the way what a discouraging example Malthus is, to show during what long years the plainest case may be misrepresented and misunderstood. I have read the 'Future'; how curious it is that several of my reviewers should advance such wild arguments, as that varieties of dogs and cats do not mingle; and should bring up the old exploded doctrine of definite analogies... I am beginning to despair of ever making the majority understand my notions. Even Hopkins does not thoroughly. By the way, I have been so much pleased by the way he personally alludes to me. I must be a very bad explainer. I hope to Heaven that you will succeed better. Several reviews and several letters have shown me too clearly how little I am understood. I suppose "natural selection" was a bad term; but to change it now, I think, would make confusion worse confounded, nor can I think of a better; "Natural Preservation" would not imply a preservation of particular varieties, and would seem a truism, and would not bring man's and nature's selection under one point of view. I can only hope by reiterated explanations finally to make the matter clearer. If my MS. spreads out, I think I shall publish one volume exclusively on variation of animals and plants under domestication. I want to show that I have not been quite so rash as many suppose.

Though weary of reviews, I should like to see Lowell's (The late J.A. Lowell in the 'Christian Examiner' (Boston, U.S., May, 1860.) some time... I suppose Lowell's difficulty about instinct is the same as Bowen's; but it seems to me wholly to rest on the assumption that instincts cannot graduate as finely as structures. I have stated in my volume that it is hardly possible to know which, i.e. whether instinct or structure, change first by insensible steps. Probably sometimes instinct, sometimes structure. When a British insect feeds on an exotic plant, instinct has changed by very small steps, and their structures might change so as to fully profit by the new food. Or structure might change first, as the direction of tusks in one variety of Indian elephants, which leads it to attack the tiger in a different manner from other kinds of elephants. Thanks for your letter of the 2nd, chiefly about Murray. (N.B. Harvey of Dublin gives me, in a letter, the argument of tall men marrying short women, as one of great weight!)

I do not quite understand what you mean by saying, "that the more they prove that you underrate physical conditions, the better for you, as Geology comes in to your aid."

... I see in Murray and many others one incessant fallacy, when alluding to slight differences of physical conditions as being very important; namely, oblivion of the fact that all species, except very local ones, range over a considerable area, and though exposed to what the world calls considerable DIVERSITIES, yet keep constant. I have just alluded to this in the 'Origin' in comparing the productions of the Old and the New Worlds. Farewell, shall you be at Oxford? If H. gets quite well, perhaps I shall go there.

Yours affectionately, C. DARWIN.

CHARLES DARWIN TO C. LYELL. Down [June 14th, 1860].

... Lowell's review (J.A. Lowell in the 'Christian Examiner,' May 1860.) is pleasantly written, but it is clear that he is not a naturalist. He quite overlooks the importance of the accumulation of mere individual differences, and which, I think I can show, is the great agency of change under domestication. I have not finished Schaaffhausen, as I read German so badly. I have ordered a copy for myself, and should like to keep yours till my own arrives, but will return it to you instantly if wanted. He admits statements rather rashly, as I dare say I do. I see only one sentence as yet at all approaching natural selection.

There is a notice of me in the penultimate number of 'All the Year Round,' but not worth consulting; chiefly a well-done hash of my own words. Your last note was very interesting and consolatory to me.

I have expressly stated that I believe physical conditions have a more direct effect on plants than on animals. But the more I study, the more I am led to think that natural selection regulates, in a state of nature, most trifling differences. As squared stone, or bricks, or timber, are the indispensable materials for a building, and influence its character, so is variability not only indispensable, but influential. Yet in the same manner as the architect is the ALL important person in a building, so is selection with organic bodies...

[The meeting of the British Association at Oxford in 1860 is famous for two pitched battles over the 'Origin of Species.' Both of them originated in unimportant papers. On Thursday, June 28, Dr. Daubeny of Oxford made a communication to Section D: "On the final causes of the sexuality of plants, with particular reference to Mr. Darwin's work on the 'Origin of Species.'" Mr. Huxley was called on by the President, but tried (according to the "Athenaeum" report) to avoid a discussion, on the ground "that a general audience, in which sentiment would unduly interfere with intellect, was not the public before which such a discussion should be carried on." However, the subject was not allowed to drop. Sir R. Owen (I quote from the "Athenaeum", July 7, 1860), who "wished to approach this subject in the spirit of the philosopher," expressed his "conviction that there were facts by which the public could come to some conclusion with regard to the probabilities of the truth of Mr. Darwin's theory." He went on to say that the brain of the gorilla "presented more differences, as compared with the brain of man, than it did when compared with the brains of the very lowest and most problematical of the Quadrumana." Mr. Huxley replied, and gave these assertions a "direct and unqualified contradiction," pledging himself to "justify that unusual procedure elsewhere" ('Man's Place in Nature,' by T.H. Huxley, 1863, page 114.), a pledge which he amply fulfilled. (See the 'Nat. Hist. Review,' 1861.) On Friday there was peace, but on Saturday 30th, the battle arose with redoubled fury over a paper by Dr. Draper of New York, on the 'Intellectual development of Europe considered with reference to the views of Mr. Darwin.'

The following account is from an eye-witness of the scene.

"The excitement was tremendous. The Lecture-room, in which it had been arranged that the discussion should be held, proved far too small for the audience, and the meeting adjourned to the Library of the Museum, which was crammed to suffocation long before the champions entered the lists. The numbers were estimated at from 700 to 1000. Had it been term-time, or had the general public been admitted, it would have been impossible to have accommodated the rush to hear the oratory of the bold Bishop. Professor Henslow, the President of Section D, occupied the chair and wisely announced in limine that none who had not valid arguments to bring forward on one side or the other, would be allowed to address the meeting: a caution that proved necessary, for no fewer than four combatants had their utterances burked by him, because of their indulgence in vague declamation.

"The Bishop was up to time, and spoke for full half-an-hour with inimitable spirit, emptiness and unfairness. It was evident from his handling of the subject that he had been 'crammed' up to the throat, and that he knew nothing at first hand; in fact, he used no argument not to be found in his 'Quarterly' article. He ridiculed Darwin badly, and Huxley savagely, but all in such dulcet tones, so persuasive a manner, and in such well-turned periods, that I who had been inclined to blame the President for allowing a discussion that could serve no scientific purpose now forgave him from the bottom of my heart. Unfortunately the Bishop, hurried along on the current of his own eloquence, so far forgot himself as to push his attempted advantage to the verge of personality in a telling passage in which he turned round and addressed Huxley: I forgot the precise words, and quote from Lyell. 'The Bishop asked whether Huxley was related by his grandfather's or grandmother's side to an ape.' (Lyell's 'Letters,' vol. ii. page 335.) Huxley replied to the scientific argument of his opponent with force and eloquence, and to the personal allusion with a sel-restraint, that gave dignity to his crushing rejoinder."

Many versions of Mr. Huxley's speech were current: the following report of his conclusion is from a letter addressed by the late John Richard Green, then an undergraduate, to a fellow-student, now Professor Boyd Dawkins. "I asserted, and I repeat, that a man has no reason to be ashamed of having an ape for his grandfather. If there were an ancestor whom I should feel shame in recalling, it would be a MAN, a man of restless and versatile intellect, who, not content with an equivocal (Prof.

V. Carus, who has a distinct recollection of the scene, does not remember the word equivocal. He believes too that Lyell's version of the "ape" sentence is slightly incorrect.) success in his own sphere of activity, plunges into scientific questions with which he has no real acquaintance, only to obscure them by an aimless rhetoric, and distract the attention of his hearers from the real point at issue by eloquent digressions, and skilled appeals to religious prejudice."

The letter above quoted continues:

"The excitement was now at its height; a lady fainted and had to be carried out, and it was some time before the discussion was resumed. Some voices called for Hooker, and his name having been handed up, the President invited him to give his view of the theory from the Botanical side. This he did, demonstrating that the Bishop, by his own showing, had never grasped the principles of the 'Origin' (With regard to the Bishop's 'Quarterly Review,' my father wrote: "These very clever men think they can write a review with a very slight knowledge of the book reviewed or subject in question."), and that he was absolutely ignorant of the elements of botanical science. The Bishop made no reply, and the meeting broke up.

"There was a crowded conversazione in the evening at the rooms of the hospitable and genial Professor of Botany, Dr. Daubeny, where the almost sole topic was the battle of the 'Origin,' and I was much struck with the fair and unprejudiced way in which the black coats and white cravats of Oxford discussed the question, and the frankness with which they offered their congratulations to the winners in the combat.]

CHARLES DARWIN TO J.D. HOOKER. Sudbrook Park, Monday night [July 2nd, 1860].

My dear Hooker,

I have just received your letter. I have been very poorly, with almost continuous bad headache for forty-eight hours, and I was low enough, and thinking what a useless burthen I was to myself and all others, when your letter came, and it has so cheered me; your kindness and affection brought tears into my eyes. Talk of fame, honour, pleasure, wealth, all are dirt compared with affection; and this is a doctrine with which, I know, from your letter, that you will agree with from the bottom of your heart... How I should have liked to have wandered about Oxford with you, if I had been well enough; and how still more I should have liked to have heard you triumphing over the Bishop. I am astonished at your success and audacity. It is something unintelligible to me how any one can argue in public like orators do. I had no idea you had this power. I have read lately so many hostile views, that I was beginning to think that perhaps I was wholly in the wrong, and that — was right when he said the whole subject would be forgotten in ten years; but now that I hear that you and Huxley will fight publicly (which I am sure I never could do), I fully believe that our cause will, in the long-run, prevail. I am glad I was not in Oxford, for I should have been overwhelmed, with my [health] in its present state.

CHARLES DARWIN TO T.H. HUXLEY. Sudbrook Park, Richmond, July 3rd [1860].

... I had a letter from Oxford, written by Hooker late on Sunday night, giving me some account of the awful battles which have raged about species at Oxford. He tells me you fought nobly with Owen (but I have heard no particulars), and that you answered the B. of O. capitally. I often think that my friends (and you far beyond others) have good cause to hate me, for having stirred up so much mud, and led them into so much odious trouble. If I had been a friend of myself, I should have hated me. (How to make that sentence good English, I know not.) But remember, if I had not stirred up the mud, some one else certainly soon would. I honour your pluck; I would as soon have died as tried to answer the Bishop in such an assembly...

[On July 20th, my father wrote to Mr. Huxley:

"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."]

CHARLES DARWIN TO J.D. HOOKER. [July 1860].

... I have just read the 'Quarterly.' ('Quarterly Review,' July 1860. The article in question was by Wilberforce, Bishop of Oxford, and was afterwards published in his "Essays Contributed to the 'Quarterly Review,' 1874." The passage from the 'Anti-Jacobin' gives the history of the evolution of space from the "primaeval point or punctum saliens of the universe," which is conceived to have moved "forward in a right line ad infinitum, till it grew tired; after which the right line, which it had generated, would begin to put itself in motion in a lateral direction, describing an area of infinite extent. This area, as soon as it became conscious of its own existence, would begin to ascend or descend according as its specific gravity would determine it, forming an immense solid space filled with vacuum, and capable of containing the present universe."

The following (page 263) may serve as an example of the passages in which the reviewer refers to Sir Charles Lyell: — "That Mr. Darwin should have wandered from this broad highway of nature's works into the jungle of fanciful assumption is no small evil. We trust that he is mistaken in believing that he may count Sir C. Lyell as one of his converts. We know, indeed, that the strength of the temptations which he can bring to bear upon his geological brother... Yet no man has been more distinct and more logical in the denial of the transmutation of species than Sir C. Lyell, and that not in the infancy of his scientific life, but in its full vigour and maturity." The Bishop goes on to appeal to Lyell, in order that with his help "this flimsy speculation may be as completely put down as was what in spite of all denials we must venture to call its twin though less instructed brother, the 'Vestiges of Creation.'"

With reference to this article, Mr. Brodie Innes, my father's old friend and neighbour, writes: — "Most men would have been annoyed by an article written with the Bishop's accustomed vigour, a mixture of argument and ridicule. Mr. Darwin was writing on some parish matter, and put a postscript — 'If you have not seen the last 'Quarterly,' do get it; the Bishop of Oxford has made such capital fun of me and my grandfather.' By a curious coincidence, when I received the letter, I was staying in the same house with the Bishop, and showed it to him. He said, 'I am very glad he takes it in that way, he is such a capital fellow.'") It is uncommonly clever; it picks out with skill all the most conjectural parts, and brings forward well all the difficulties. It quizzes me quite splendidly by quoting the 'Anti-Jacobin' versus my Grandfather. You are not alluded to, nor, strange to say, Huxley; and I can plainly see, here and there, — 's hand. The concluding pages will make Lyell shake in his shoes. By Jove, if he sticks to us, he will be a real hero. Good-night. Your wel-quizzed, but not sorrowful, and affectionate friend.

C.D.

I can see there has been some queer tampering with the Review, for a page has been cut out and reprinted.

[Writing on July 22 to Dr. Asa Gray my father thus refers to Lyell's position: —

"Considering his age, his former views and position in society, I think his conduct has been heroic on this subject."]

CHARLES DARWIN TO ASA GRAY. [Hartfield, Sussex] July 22nd [1860].

My dear Gray,

Owing to absence from home at water-cure and then having to move my sick girl to whence I am now writing, I have only lately read the discussion in Proc. American Acad. (April 10, 1860. Dr. Gray criticised in detail "several of the positions taken at the preceding meeting by Mr. [J.A.] Lowell, Prof. Bowen and Prof. Agassiz." It was reprinted in the "Athenaeum", August 4, 1860.), and now I cannot resist expressing my sincere admiration of your most clear powers of reasoning. As Hooker lately said in a note to me, you are more than ANY ONE else the thorough master of the subject. I declare that you know my book as well as I do myself; and bring to the question new lines of illustration and argument in a manner which excites my astonishment and almost my envy! I admire these discussions, I think, almost more than your article in Silliman's Journal. Every single word seems weighed carefully, and tells like a 32-pound shot. It makes me much wish (but I know that you have not time) that you could write more in detail, and give, for instance, the facts on the variability of the American wild fruits. The "Athenaeum" has the largest circulation, and I have sent my copy to the editor with a request that he would republish the first discussion; I much fear he will not, as he reviewed the subject in so hostile a spirit... I shall be curious [to see] and will order the August number, as soon as I know that it contains your review of Reviews. My conclusion is that you have made a mistake in being a botanist, you ought to have been a lawyer.

... Henslow (Professor Henslow was mentioned in the December number of 'Macmillan's Magazine' as being an adherent of Evolution. In consequence of this he published, in the February number of the following year, a letter defining his position. This he did by means of an extract from a letter addressed to him by the Rev. L. Jenyns (Blomefield) which "very nearly," as he says, expressed his views. Mr. Blomefield wrote, "I was not aware that you had become a convert to his (Darwin's) theory, and can hardly suppose you have accepted it as a whole, though, like myself, you may go to the length of imagining that many of the smaller groups, both of animals and plants, may at some remote period have had a common parentage. I do not with some say that the whole of his theory cannot be true — but that it is very far from proved; and I doubt its ever being possible to prove it.") and Daubeny are shaken. I hear from Hooker that he hears from Hochstetter that my views are making very considerable progress in Germany, and the good workers are discussing the question. Bronn at the end of his translation has a chapter of criticism, but it is such difficult German that I have not yet read it. Hopkins's review in 'Fraser' is thought the best which has appeared against us. I believe that Hopkins is so much opposed because his course of study has never led him to reflect much on such subjects as geographical distribution, classification, homologies, etc., so that he does not feel it a relief to have some kind of explanation.

CHARLES DARWIN TO C. LYELL. Hartfield [Sussex], July 30th [1860].

... I had lots of pleasant letters about the British Association, and our side seems to have got on very well. There has been as much discussion on the other side of the Atlantic as on this. No one I think understands the whole case better than Asa Gray, and he has been fighting nobly. He is a capital reasoner. I have sent one of his printed discussions to our "Athenaeum", and the editor says he will print it. The 'Quarterly' has been out some time. It contains no malice, which is wonderful... It makes me say many things which I do not say. At the end it quotes all your conclusions against Lamarck, and makes a solemn appeal to you to keep firm in the true faith. I fancy it will make you quake a little. — has ingeniously primed the Bishop (with Murchison) against you as head of the uniformitarians. The only other review worth mentioning, which I can think of, is in the third No. of the 'London Review,' by some geologist, and favorable for a wonder. It is very ably done, and I should like much to know who is the author. I shall be very curious to hear on your return whether Bronn's German translation of the 'Origin' has drawn any attention to the subject. Huxley is eager about a 'Natural History Review,' which he and others are going to edit, and he has got so many first-rate assistants, that I really believe he will make it a first-rate production. I have been doing nothing, except a little botanical work as amusement. I shall hereafter be very anxious to hear how your tour has answered. I expect your book on the geological history of Man will, with a vengeance, be a bomb-shell. I hope it will not be very long delayed. Our kindest remembrances to Lady Lyell. This is not worth sending, but I have nothing better to say.

Yours affectionately, C. DARWIN.

CHARLES DARWIN TO F. WATKINS. (See Volume I.) Down, July 30th, [1860?].

My dear Watkins,

Your note gave me real pleasure. Leading the retired life which I do, with bad health, I oftener think of old times than most men probably do; and your face now rises before me, with the pleasant old expression, as vividly as if I saw you.

My book has been well abused, praised, and splendidly quizzed by the Bishop of Oxford; but from what I see of its influence on really good workers in science, I feel confident that, IN THE

MAIN, I am on the right road. With respect to your question, I think the arguments are valid, showing that all animals have descended from four or five primordial forms; and that analogy and weak reasons go to show that all have descended from some single prototype.

Farewell, my old friend. I look back to old Cambridge days with unalloyed pleasure.

Believe me, yours most sincerely, CHARLES DARWIN.

T.H. HUXLEY TO CHARLES DARWIN. August 6th, 1860.

My dear Darwin,

I have to announce a new and great ally for you...

Von Baer writes to me thus: — Et outre cela, je trouve que vous ecrivez encore des redactions. Vous avez ecrit sur l'ouvrage de M. Darwin une critique dont je n'ai trouve que des debris dans un journal allemand. J'ai oublie le nom terrible du journal anglais dans lequel se trouve votre recension. En tout cas aussi je ne peux pas trouver le journal ici. Comme je m'interesse beaucoup pour les idees de M. Darwin, sur lesquelles j'ai parle publiquement et sur lesquelles je ferai peut-etre imprimer quelque chose — vous m'obligeriez infiniment si vous pourriez me faire parvenir ce que vous avez ecrit sur ces idees.

"J'ai enonce les memes idees sur la transformation des types ou origine d'especes que M. Darwin. (See Vol. I.) Mais c'est seulement sur la geographie zoologique que je m'appuie. Vous trouverez, dans le dernier chapitre du traite 'Ueber Papuas und Alfuren,' que j'en parle tres decidement sans savoir que M. Darwin s'occupait de cet objet."

The treatise to which Von Baer refers he gave me when over here, but I have not been able to lay hands on it since this letter reached me two days ago. When I find it I will let you know what there is in it.

Ever yours faithfully, T.H. HUXLEY.

CHARLES DARWIN TO T.H. HUXLEY. Down, August 8 [1860].

My dear Huxley,

Your note contained magnificent news, and thank you heartily for sending it me. Von Baer weighs down with a vengeance all the virulence of [the 'Edinburgh' reviewer] and weak arguments of Agassiz. If you write to Von Baer, for heaven's sake tell him that we should think one nod of approbation on our side, of the greatest value; and if he does write anything, beg him to send us a copy, for I would try and get it translated and published in the "Athenaeum" and in 'Silliman' to touch up Agassiz... Have you seen Agassiz's weak metaphysical and theological attack on the 'Origin' in the last 'Silliman'? (The 'American Journal of Science and Arts' (commonly called 'Silliman's Journal'), July 1860. Printed from advanced sheets of vol. iii. of 'Contributions to the Nat. Hist. of the U.S.' My father's copy has a pencilled "Truly" opposite the following passage: — "Unless Darwin and his followers succeed in showing that the struggle for life tends to something beyond favouring the existence of certain individuals over that of other individuals, they will soon find that they are following a shadow.") I would send it you, but apprehend it would be less trouble for you to look at it in London than return it to me. R. Wagner has sent me a German pamphlet ('Louis Agassiz's Prinzipien der Classification, etc., mit Rucksicht auf Darwins Ansichten. Separat-Abdruck aus den Gottingischen gelehrten Anzeigen,' 1860.), giving an abstract of Agassiz's 'Essay on Classification,' "mit Rucksicht auf Darwins Ansichten," etc. etc. He won't go very "dangerous lengths," but thinks the truth lies half-way between Agassiz and the 'Origin.' As he goes thus far he will, nolens volens, have to go further. He says he is going to review me in [his] yearly Report. My good and kind agent for the propagation of the Gospel — i.e. the devil's gospel.

Ever yours, C. DARWIN.

CHARLES DARWIN TO C. LYELL. Down, August 11th [1860].

... I have laughed at Woodward thinking that you were a man who could be influenced in your judgment by the voice of the public; and yet after mortally sneering at him, I was obliged to confess to myself, that I had had fears, what the effect might be of so many heavy guns fired by great men.

As I have (sent by Murray) a spare 'Quarterly Review,' I send it by this post, as it may amuse you. The Anti-Jacobin part amused me. It is full of errors, and Hooker is thinking of answering it. There has been a cancelled page; I should like to know what gigantic blunder it contained. Hooker says that — has played on the Bishop, and made him strike whatever note he liked; he has wished to make the article as disagreeable to you as possible. I will send the "Athenaeum" in a day or two.

As you wish to hear what reviews have appeared, I may mention that Agassiz has fired off a shot in the last 'Silliman,' not good at all, denies variations and rests on the perfection of Geological evidence. Asa Gray tells me that a very clever friend has been almost converted to our side by this review of Agassiz's... Professor Parsons (Theophilus Parsons, Professor of Law in Harvard University.) has published in the same 'Silliman' a speculative paper correcting my notions, worth nothing. In the 'Highland Agricultural Journal' there is a review by some Entomologist, not worth much. This is all that I can remember... As Huxley says, the platoon firing must soon cease. Hooker and Huxley, and Asa Gray, I see, are determined to stick to the battle and not give in; I am fully convinced that whenever you publish, it will produce a great effect on all TRIMMERS, and on many others. By the way I forgot to mention Daubeny's pamphlet ('Remarks on the final causes of the sexuality of plants with particular reference to Mr. Darwin's work on the "Origin of Species."' — British Association Report, 1860.), very liberal and candid, but scientifically weak. I believe Hooker is going nowhere this summer; he is excessively busy... He has written me many, most nice letters. I shall be very curious to hear on your return some account of your Geological doings. Talking of Geology, you used to be interested about the "pipes" in the chalk. About three years ago a perfectly circular hole suddenly appeared in a flat grass field to everyone's astonishment, and was filled up with many waggon loads of earth; and now two or three days ago, again it has circularly subsided about two feet more. How clearly this shows what is still slowly going on. This morning I recommenced work, and am at dogs; when I have written my short discussion on them, I will have it copied, and if you like, you can then see how the argument stands, about their multiple origin. As you seemed to think this important, it might be worth your reading; though I do not feel sure that you will come to the same probable conclusion that I have done. By the way, the Bishop makes a very telling case against me, by accumulating several instances where I speak very doubtfully; but this is very unfair, as in such cases as this of the dog, the evidence is and must be very doubtful...

CHARLES DARWIN TO ASA GRAY. Down, August 11 [1860].

My dear Gray,

On my return home from Sussex about a week ago, I found several articles sent by you. The first article, from the 'Atlantic Monthly,' I am very glad to possess. By the way, the editor of the "Athenaeum" (August 4, 1860.) has inserted your answer to Agassiz, Bowen, and Co., and when I therein read them, I admired them even more than at first. They really seemed to be admirable in their condensation, force, clearness and novelty.

I am surprised that Agassiz did not succeed in writing something better. How absurd that logical quibble — "if species do not exist, how can they vary?" As if any one doubted their temporary existence. How coolly he assumes that there is some clearly defined distinction between individual differences and varieties. It is no wonder that a man who calls identical forms, when found in two countries, distinct species, cannot find variation in nature. Again, how unreasonable to suppose that domestic varieties selected by man for his own fancy should resemble natural varieties or species. The whole article seems to me poor; it seems to me hardly worth a detailed answer (even if I could do it, and I much doubt whether I possess your skill in picking out salient points and driving a nail into them), and indeed you have already answered several points. Agassiz's name, no doubt, is a heavy weight against us...

If you see Professor Parsons, will you thank him for the extremely liberal and fair spirit in which his Essay ('Silliman's Journal,' July, 1860.) is written. Please tell him that I reflected much on the chance of favourable monstrosities (i.e. great and sudden variation) arising. I have, of course, no objection to this, indeed it would be a great aid, but I do not allude to the subject, for, after much labour, I could find nothing which satisfied me of the probability of such occurrences. There seems to me in almost every case too much, too complex, and too beautiful adaptation, in every structure, to believe in its sudden production. I have alluded under the head of beautifully hooked seeds to such possibility. Monsters are apt to be sterile, or NOT to transmit monstrous peculiarities. Look at the fineness of gradation in the shells of successive SUB-STAGES of the same great formation; I could give many other considerations which made me doubt such view. It holds, to a certain extent, with domestic productions no doubt, where man preserves some abrupt change in structure. It amused me to see Sir R. Murchison quoted as a judge of affinities of animals, and it gave me a cold shudder to hear of any one speculating about a true crustacean giving birth to a true fish! (Parson's, loc. cit. page 5, speaking of Pterichthys and Cephalaspis, says: — "Now is it too much to infer from these facts that either of these animals, if a crustacean, was so nearly a fish that some of its ova may have become fish; or, if itself a fish, was so nearly a crustacean that it may have been born from the ovum of a crustacean?")

Yours most truly, C. DARWIN.

CHARLES DARWIN TO C. LYELL. Down, September 1st [1860].

My dear Lyell,

I have been much interested by your letter of the 28th, received this morning. It has DELIGHTED me, because it demonstrates that you have thought a good deal lately on Natural Selection. Few things have surprised me more than the entire paucity of objections and difficulties new to me in the published reviews. Your remarks are of a different stamp and new to me. I will run through them, and make a few pleadings such as occur to me.

I put in the possibility of the Galapagos having been CONTINUOUSLY joined to America, out of mere subservience to the many who believe in Forbes's doctrine, and did not see the danger of admission, about small mammals surviving there in such case. The case of the Galapagos, from certain facts on littoral sea-shells (viz. Pacific Ocean and South American littoral species), in fact convinced me more than in any other case of other islands, that the Galapagos had never been continuously united with the mainland; it was mere base subservience, and terror of Hooker and Co.

With respect to atolls, I think mammals would hardly survive VERY LONG, even if the main islands (for as I have said in the Coral Book, the outline of groups of atolls do not look like a former CONTINENT) had been tenanted by mammals, from the extremely small area, the very peculiar conditions, and the probability that during subsidence all or nearly all atolls have been breached and flooded by the sea many times during their existence as atolls.

I cannot conceive any existing reptile being converted into a mammal. From homologies I should look at it as certain that all mammals had descended from some single progenitor. What its nature was, it is impossible to speculate. More like, probably, the Ornithorhynchus or Echidna than any known form; as these animals combine reptilian characters (and in a less degree bird character) with mammalian. We must imagine some form as intermediate, as is Lepidosiren now, between reptiles and fish, between mammals and birds on the one hand (for they retain longer the same embryological character) and reptiles on the other hand. With respect to a mammal not being developed on any island, besides want of time for so prodigious a development, there must have arrived on the island the necessary and peculiar progenitor, having a character like the embryo of a mammal; and not an ALREADY DEVELOPED reptile, bird or fish.

# Конец ознакомительного фрагмента.