C. D. to J. D. Hooker. [April 14th, 1855.]

. . . You are a good man to confess that you expected the cress would be killed in a week, for this gives me a nice little triumph. The children at first were tremendously eager, and asked me often, "whether I should beat Dr. Hooker!" The cress and lettuce have just vegetated well after twenty-one days' immersion. But I will write no more, which is a great virtue in me; for it is to me a very great pleasure telling you everything I do.

. . . If you knew some of the experiments (if they may be so called) which I am trying, you would have a good right to sneer, for they are so *absurd* even in *my* opinion that I dare not tell you.

Have not some men a nice notion of experimentising? I have had a letter telling me that seeds *must* have *great* power of resisting salt water, for otherwise how could they get to islands? This is the true way to solve a problem?

Experiments on the transportal of seeds through the agency of animals, also gave him much labour. He wrote to Fox (1855):—

"All nature is perverse and will not do as I wish it; and just at present I wish I had my old barnacles to work at, and nothing new."

And to Hooker:—

"Everything has been going wrong with me lately: the fish at the Zoolog. Soc. ate up lots of soaked seeds, and in imagination they had in my mind been swallowed, fish and all, by a heron, had been carried a hundred miles, been voided on the banks of some other lake and germinated splendidly, when lo and behold, the fish ejected vehemently, and with disgust equal to my own, *all* the seeds from their mouths."

THE UNFINISHED BOOK.

In his Autobiographical sketch (p. 41) my father wrote:— "Early in 1856 Lyell advised me to write out my views pretty fully, and I began at once to do so on a scale three or four times as extensive as that which was afterwards followed in my *Origin of Species;* yet it was only an abstract of the materials which I had collected." The remainder of the present chapter is chiefly concerned with the preparation of this unfinished book.

The work was begun on May 14th, and steadily continued up to June 1858, when it was interrupted by the arrival of Mr.

Wallace's MS. During the two years which we are now considering, he wrote ten chapters (that is about one-half) of the projected book.

C. D. to J. D. Hooker. May 9th [1856].

. . . I very much want advice and *truthful* consolation if you can give it. I had a good talk with Lyell about my species work, and he urges me strongly to publish something. I am fixed against any periodical or Journal, as I positively will *not* expose myself to an Editor or a Council allowing a publication for which they might be abused. If I publish anything it must be a *very thin* and little volume, giving a sketch of my views and difficulties; but it is really dreadfully unphilosophical to give a *résumé*, without exact references, of an unpublished work. But Lyell seemed to think I might do this, at the suggestion of friends, and on the ground, which I I might state, that I had been at work for eighteen * years, and yet could not publish for several years, and especially as I could point out difficulties which seemed to me to require especial investigation. Now what think you? I should be really grateful for advice. I thought of giving up a couple of months and writing such a sketch, and trying to keep my judgment open whether or no to publish it when completed. It will be simply impossible for me to give exact references; anything important I should state on the authority of the author generally; and instead of giving all the facts on which I ground my opinion, I could give by memory only one or two. In the Preface I would state that the work could not be considered strictly scientific, but a mere sketch or outline of a future work in which full references, &c., should be given. Eheu, eheu, I believe I should sneer at any one else doing this, and my only comfort is, that I *truly* never dreamed of it, till Lyell suggested it, and seems deliberately to think it advisable.

I am in a peck of troubles, and do pray forgive me for troubling you.

Yours affectionately.

He made an attempt at a sketch of his views, but as he wrote to Fox in October 1856 :—

"I found it such unsatisfactory work that I have desisted,

* The interval of eighteen years, from 1837 when he began to collect facts, would bring the date of this letter to 1855, not 1856, nevertheless the latter seems the more probable date.

and am now drawing up my work as perfect as my materials of nineteen years' collecting suffice, but do not intend to stop to perfect any line of investigation beyond current work."

And in November he wrote to Sir Charles Lyell:—

"I am working very steadily at my big book; I have found it quite impossible to publish any preliminary essay or sketch; but am doing my work as completely as my present materials allow without waiting to perfect them. And this much acceleration I owe to you."

Again to Mr. Fox, in February, 1857:—

"I am got most deeply interested in my subject; though I wish I could set less value on the bauble fame, either present or posthumous, than I do, but not I think, to any extreme degree: yet, if I know myself, I would work just as hard, though with less gusto, if I knew that my book would be published for ever anonymously."

C. D. to A. R. Wallace. Moor Park, May 1st, 1857.

My dear Sir—I am much obliged for your letter of October 10th, from Celebes, received a few days ago; in a laborious undertaking, sympathy is a valuable and real encouragement. By your letter and even still more by your paper* in the Annals, a year or more ago, I can plainly see that we have thought much alike and to a certain extent have come to similar conclusions. In regard to the Paper in the Annals, I agree to the truth of almost every word of your paper; and I dare say that you will agree with me that it is very rare to find oneself agreeing pretty closely with any theoretical paper; for it is lamentable how each man draws his own different conclusions from the very same facts. This summer will make the 20th year (!) since I opened my first note-book, on the question how and in what way do species and varieties differ from each other. I am now preparing my work for publication, but I find the subject so very large, that though I have written many chapters, I do not suppose I shall go to press for two years. I have never heard how long you intend staying in the Malay Archipelago; I wish I might profit by the publication of your Travels there before my work appears, for no doubt you will reap a large harvest of facts. [I have acted already in accordance with your advice of keeping domestic varieties, and those appearing in a state of nature,

* "On the Law that has regulated the Introduction of New Species." —*Ann. Nat. Hist.*, 1855.

distinct; but I have sometimes doubted of the wisdom of this, and therefore I am glad to be backed by your opinion. I must confess, however, I rather doubt the truth of the now very prevalent doctrine of all our domestic animals having descended from several wild stocks; though I do not doubt that it is so in some cases. I think there is rather better evidence on the sterility of hybrid animals than you seem to admit: and in regard to plants the collection of carefully recorded facts by Kölreuter and Gaertner (and Herbert) is *enormous*. I most entirely agree with you on the little effects of "climatal conditions," which one sees referred to *ad nauseam* in all books: I suppose some very little effect must be attributed to such influences, but I fully believe that they are very slight. It is really *impossible* to explain my views (in the compass of a letter), on the causes and means of variation in a state of nature; but I have slowly adopted a distinct and tangible idea,—whether true or false others must judge; for the firmest conviction of the truth of a doctrine by its author, seems, alas, not to be the slightest guarantee of truth! . . .

In December 1857 he wrote to the same correspondent:—

"You ask whether I shall discuss 'man.' I think I shall avoid the whole subject, as so surrounded with prejudices; though I fully admit that it is the highest and most interesting problem for the naturalist. My work, on which I have now been at work more or less for twenty years, will not fix or settle anything; but I hope it will aid by giving a large collection of facts, with one definite end. I get on very slowly, partly from ill-health, partly from being a very slow worker. I have got about half written; but I do not suppose I shall publish under a couple of years. I have now been three whole months on one chapter on Hybridism!

"I am astonished to see that you expect to remain out three or four years more. What a wonderful deal you will have seen, and what interesting areas—the grand Malay Archipelago and the richest parts of South America! I infinitely admire and honour your zeal and courage in the good cause of Natural Science; and you have my very sincere and cordial good wishes for success of all kinds, and may all your theories succeed, except that on Oceanic Islands, on which subject I will do battle to the death."

And to Fox in February 1858:—

"I am working very hard at my book, perhaps too hard. It will be very big, and I am become most deeply interested in the way facts fall into groups. I am like Crœsus overwhelmed with my riches in facts, and I mean to make my book as perfect as ever I can. I shall not go to press at soonest for a couple of years."

The letter which follows, written from his favourite resting place, the Water-Cure Establishment at Moor Park, comes in like a lull before the storm,—the upset of all his plans by the arrival of Mr. Wallace's manuscript, a phase in the history of his life to which the next chapter is devoted.

C. D. to Mrs. Darwin. Moor Park, April [1858].

The weather is quite delicious. Yesterday, after writing to you, I strolled a little beyond the glade for an hour and a half, and enjoyed myself—the fresh yet dark green of the grand Scotch firs, the brown of the catkins of the old birches, with their white stems, and a fringe of distant green from the larches, made an excessively pretty view. At last I fell fast asleep on the grass, and awoke with a chorus of birds singing around me, and squirrels running up the trees, and some woodpeckers laughing, and it was as pleasant and rural a scene as ever I saw, and I did not care one penny how any of the beasts or birds had been formed. I sat in the drawing-room till after eight, and then went and read the Chief Justice's summing up, and thought Bernard * guilty, and then read a bit of my novel, which is feminine, virtuous, clerical, philanthropical, and all that sort of thing, but very decidedly flat. I say feminine, for the author is ignorant about money matters, and not much of a lady—for she makes her men say, "My Lady." I like Miss Craik very much, though we have some battles, and differ on every subject. I like also the Hungarian; a thorough gentleman, formerly attaché at Paris, and then in the Austrian cavalry, and now a pardoned exile, with broken health. He does not seem to like Kossuth, but says, he is certain [he is] a sincere patriot, most clever and eloquent, but weak, with no determination of character. . . .

* Simon Bernard was tried in April 1858 as an accessory to Orsini's attempt on the life of the Emperor of the French. The verdict was "not guilty."

CHAPTER XI.

THE WRITING OF THE 'ORIGIN OF SPECIES.'

"I have done my best. If you had all my material I am sure you would have made a splendid book."—*From a letter to Lyell, June* 21, 1859.

JUNE 18, 1858, TO NOVEMBER 1859.

C. D. to C. Lyell. Down, 18th [June 1858].

MY DEAR LYELL—Some year or so ago you recommended me to read a paper by Wallace in the *Annals*,* which had interested you, and as I was writing to him, I knew this would please him much, so I told him. He has to-day sent me the enclosed, and asked me to forward it to you. It seems to me well worth reading. Your words have come true with a vengeance—that I should be forestalled. You said this, when I explained to you here very briefly my views of 'Natural Selection' depending on the struggle for existence. I never saw a more striking coincidence; if Wallace had my MS. sketch written out in 1842, he could not have made a better short abstract! Even his terms now stand as heads of my chapters. Please return me the MS., which he does not say he wishes me to publish, but I shall, of course, at once write and offer to send to any journal. So all my originality, whatever it may amount to, will be smashed, though my book, if it will ever have any value, will not be deteriorated; as all the labour consists in the application of the theory.

I hope you will approve of Wallace's sketch, that I may tell him what you say.

My dear Lyell, yours most truly.

C. D. to C. Lyell. Down [June 25, 1858].

MY DEAR LYELL—I am very sorry to trouble you, busy as you are, in so merely personal an affair; but if you will give me your deliberate opinion, you will do me as great a service

* *Annals and Mag. of Nat. Hist.*, 1855.

er man did, for I have entire confidence in your judgment and honour. . . .

There is nothing in Wallace's sketch which is not written out much fuller in my sketch, copied out in 1844, and read by Hooker some dozen years ago. About a year ago I sent a short sketch, of which I have a copy, of my views (owing to correspondence on several points) to Asa Gray, so that I could most truly say and prove that I take nothing from Wallace. I should be extremely glad now to publish a sketch of my general views in about a dozen pages or so; but I cannot persuade myself that I can do so honourably. Wallace says nothing about publication, and I enclose his letter. But as I had not intended to publish any sketch, can I do so honourably, because Wallace has sent me an outline of his doctrine? I would far rather burn my whole book, than that he or any other man should think that I had behaved in a paltry spirit. Do you not think his having sent me this sketch ties my hands? . . . . If I could honourably publish, I would state that I was induced now to publish a sketch (and I should be very glad to be permitted to say, to follow your advice long ago given) from Wallace having sent me an outline of my general conclusions. We differ only, [in] that I was led to my views from what artificial selection has done for domestic animals. I would send Wallace a copy of my letter to Asa Gray, to show him that I had not stolen his doctrine. But I cannot tell whether to publish now would not be base and paltry. This was my first impression, and I should have certainly acted on it had it not been for your letter.

This is a trumpery affair to trouble you with, but you cannot tell how much obliged I should be for your advice.

By the way, would you object to send this and your answer to Hooker to be forwarded to me? for then I shall have the opinion of my two best and kindest friends. This letter is miserably written, and I write it now, that I may for a time banish the whole subject; and I am worn out with musing. . . .

My good dear friend, forgive me. This is a trumpery letter, influenced by trumpery feelings.

Yours most truly.

I will never trouble you or Hooker on the subject again.

C. D. to C. Lyell. Down, 26th [June 1858].

MY DEAR LYELL—Forgive me for adding a P.S. to make the case as strong as possible against myself.

Wallace might say, "You did not intend publishing an abstract of your views till you received my communication. Is it fair to take advantage of my having freely, though unasked, communicated to you my ideas, and thus prevent me forestalling you ?" The advantage which I should take being that I am induced to publish from privately knowing that Wallace is in the field. It seems hard on me that I should be thus compelled to lose my priority of many years' standing, but I cannot feel at all sure that this alters the justice of the case. First impressions are generally right, and I at first thought it would be dishonourable in me now to publish.

Yours most truly.

P.S.—I have always thought you would make a first-rate Lord Chancellor; and I now appeal to you as a Lord Chancellor.

C. D. to J. D. Hooker. Tuesday night [June 29, 1858].

MY DEAR HOOKER—I have just read your letter, and see you want the papers at once. I am quite prostrated,* and can do nothing, but I send Wallace, and the abstract † of my letter to Asa Gray, which gives most imperfectly only the means of change, and does not touch on reasons for believing that species do change. I dare say all is too late. I hardly care about it. But you are too generous to sacrifice so much time and kindness. It is most generous, most kind. I send my sketch of 1844 solely that you may see by your own handwriting that you did read it. I really cannot bear to look at it. Do not waste much time. It is miserable in me to care at all about priority.

The table of contents will show what it is.

I would make a similar, but shorter and more accurate sketch for the *Linnean Journal.*

I will do anything. God bless you, my dear kind friend.

I can write no more. I send this by my servant to Kew.

The joint paper ‡ of Mr. Wallace and my father was read at the Linnean Society on the evening of July 1st. Mr.

* After the death, from scarlet fever, of his infant child.

† "Abstract" is here used in the sense of "extract;" in this sense also it occurs in the *Linnean Journal*, where the sources of my father's paper are described.

‡ "On the tendency of Species to form Varieties and on the Perpetuation of Varieties and Species by Natural Means of Selection."—*Linnean Society's Journal*, iii. p. 53.

Wallace's Essay bore the title, "On the Tendency of Varieties to depart indefinitely from the Original Type."

My father's contribution to the paper consisted of (1) Extracts from the sketch of 1844; (2) part of a letter addressed to Dr. Asa Gray, dated September 5, 1857. The paper was "communicated" to the Society by Sir Charles Lyell and Sir Joseph Hooker, in whose prefatory letter a clear account of the circumstances of the case is given.

Referring to Mr. Wallace's Essay, they wrote:—

"So highly did Mr. Darwin appreciate the value of the views therein set forth, that he proposed, in a letter to Sir Charles Lyell, to obtain Mr. Wallace's consent to allow the Essay to be published as soon as possible. Of this step we highly approved, provided Mr. Darwin did not withhold from the public, as he was strongly inclined to do (in favour of Mr. Wallace), the memoir which he had himself written on the same subject, and which, as before stated, one of us had perused in 1844, and the contents of which we had both of us been privy to for many years. On representing this to Mr. Darwin, he gave us permission to make what use we thought proper of his memoir, &c.; and in adopting our present course, of presenting it to the Linnean Society, we have explained to him that we are not solely considering the relative claims to priority of himself and his friend, but the interests of science generally."

Sir Charles Lyell and Sir J. D. Hooker were present at the reading of the paper, and both, I believe, made a few remarks, chiefly with a view of impressing on those present the necessity of giving the most careful consideration to what they had heard. There was, however, no semblance of a discussion. Sir Joseph Hooker writes to me: "The interest excited was intense, but the subject was too novel and too ominous for the old school to enter the lists, before armouring. After the meeting it was talked over with bated breath: Lyell's approval and perhaps in a small way mine, as his lieutenant in the affair, rather overawed the Fellows, who would otherwise have flown out against the doctrine. We had, too, the vantage ground of being familiar with the authors and their theme."

Mr. Wallace has, at my request, been so good as to allow me to publish the following letter. Professor Newton, to whom the letter is addressed, had submitted to Mr. Wallace his recollections of what the latter had related to him many years before, and had asked Mr. Wallace for a fuller version of the story. Hence the few corrections in Mr. Wallace's letter, for instance *bed* for *hammock*.

A. R. Wallace to A. Newton. Frith Hill, Godalming, Dec. 3rd, 1887.

MY DEAR NEWTON—I had hardly heard of Darwin before going to the East, except as connected with the voyage of the *Beagle*, which I *think* I had read. I saw him *once* for a few minutes in the British Museum before I sailed. Through Stevens, my agent, I heard that he wanted curious *varieties* which he was studying. I *think* I wrote to him about some varieties of ducks I had sent, and he must have written once to me. I find on looking at his "Life" that his *first* letter to me is given in vol. ii. p. 95, and another at p. 109, both after the publication of my first paper. I must have heard from some notices in the *Athenæum*, I think (which I had sent me), that he was studying varieties and species, and as I was continually thinking of the subject, I wrote to him giving some of my notions, and making some suggestions. But at that time I had not the remotest notion that he had already arrived at a definite theory—still less that it was the same as occurred to me, suddenly, in Ternate in 1858. The most interesting coincidence in the matter, I think, is, that I, *as well as Darwin*, was led to the theory itself through Malthus—in my case it was his elaborate account of the action of "preventive checks" in keeping down the population of savage races to a tolerably fixed but scanty number. This had strongly impressed me, and it suddenly flashed upon me that all animals are necessarily thus kept down—"the struggle for existence"—while *variations*, on which I was always thinking, must necessarily often be *beneficial*, and would then cause those varieties to increase while the injurious variations diminished.* You are quite at liberty to mention the circumstances, but I think you have coloured them a little highly, and introduced some slight errors. I was lying on my bed (no hammocks in the East) in the hot fit of intermittent fever, when the idea suddenly came to me. I thought it almost all out before the fit was over, and

* This passage was published as a footnote in a review of the *Life and Letters of Charles Darwin* which appeared in the *Quarterly Review*, Jan. 1888. In the new edition (1891) of *Natural Selection and Tropical Nature* (p. 20), Mr. Wallace has given the facts above narrated. There is a slight and quite unimportant discrepancy between the two accounts, viz. that in the narrative of 1891 Mr. Wallace speaks of the "cold fit" instead of the "hot fit" of his ague attack.

the moment I got up began to write it down, and I believe finished the first draft the next day.

I had no idea whatever of "dying,"—as it was not a serious illness,—but I *had* the idea of working it out, so far as I was able, when I returned home, not at all expecting that Darwin had so long anticipated me. I can truly say *now*, as I said many years ago, that I am glad it was so; for I have not the love of *work*, *experiment* and *detail* that was so pre-eminent in Darwin, and without which anything I could have written would never have convinced the world. If you do refer to me at any length, can you send me a proof and I will return it to you at once?

Yours faithfully
ALFRED R. WALLACE.

C. D. to J. D. Hooker. Miss Wedgwood's, Hartfield, Tunbridge Wells [July 13th, 1858].

MY DEAR HOOKER—Your letter to Wallace seems to me perfect, quite clear and most courteous. I do not think it could possibly be improved, and I have to-day forwarded it with a letter of my own. I always thought it very possible that I might be forestalled, but I fancied that I had a grand enough soul not to care; but I found myself mistaken and punished; I had, however, quite resigned myself, and had written half a letter to Wallace to give up all priority to him, and should certainly not have changed had it not been for Lyell's and your quite extraordinary kindness. I assure you I feel it, and shall not forget it. I am *more* than satisfied at what took place at the Linnean Society. I had thought that your letter and mine to Asa Gray were to be only an appendix to Wallace's paper.

We go from here in a few days to the sea-side, probably to the Isle of Wight, and on my return (after a battle with pigeon skeletons) I will set to work at the abstract, though how on earth I shall make anything of an abstract in thirty pages of the Journal, I know not, but will try my best. . . .

I must try and see you before your journey; but do not think I am fishing to ask you to come to Down, for you will have no time for that.

You cannot imagine how pleased I am that the notion of Natural Selection has acted as a purgative on your bowels of immutability. Whenever naturalists can look at species changing as certain, what a magnificent field will be open,—

on all the laws of variation,—on the genealogy of all living beings,—on their lines of migration, &c. &c. Pray thank Mrs. Hooker for her very kind little note, and pray say how truly obliged I am, and in truth ashamed to think that she should have had the trouble of copying my ugly MS. It was extraordinarily kind in her. Farewell, my dear kind friend.

Yours affectionately.

P.S.—I have had some fun here in watching a slave-making ant; for I could not help rather doubting the wonderful stories, but I have now seen a defeated marauding party, and I have seen a migration from one nest to another of the slave-makers, carrying their slaves (who are *house*, and not field niggers) in their mouths!

C. D. to C. Lyell. King's Head Hotel, Sandown, Isle of Wight. July 18th [1858].

. . . We are established here for ten days, and then go on to Shanklin, which seems more amusing to one, like myself, who cannot walk. We hope much that the sea may do H. and L. good. And if it does, our expedition will answer, but not otherwise.

I have never half thanked you for all the extraordinary trouble and kindness you showed me about Wallace's affair. Hooker told me what was done at the Linnean Society, and I am far more than satisfied, and I do not think that Wallace can think my conduct unfair in allowing you and Hooker to do whatever you thought fair. I certainly was a little annoyed to lose all priority, but had resigned myself to my fate. I am going to prepare a longer abstract; but it is really impossible to do justice to the subject, except by giving the facts on which each conclusion is grounded, and that will, of course, be absolutely impossible. Your name and Hooker's name appearing as in any way the least interested in my work will, I am certain, have the most important bearing in leading people to consider the subject without prejudice. I look at this as so very important, that I am almost glad of Wallace's paper for having led to this.

My dear Lyell, yours most gratefully.

The following letter refers to the proof-sheets of the Linnean paper. The 'introduction' means the prefatory letter signed by Sir C. Lyell and Sir J. D. Hooker.

C. D. to J. D. Hooker. King's Head Hotel, Sandown, Isle of Wight. July 21st [1858].

MY DEAR HOOKER—I received only yesterday the proof-sheets, which I now return. I think your introduction cannot be improved.

I am disgusted with my bad writing. I could not improve it, without rewriting all, which would not be fair or worth while, as I have begun on a better abstract for the Linnean Society. My excuse is that it *never* was intended for publication. I have made only a few corrections in the style; but I cannot make it decent, but I hope moderately intelligible. I suppose some one will correct the revise. (Shall I?)

Could I have a clean proof to send to Wallace?

I have not yet fully considered your remarks on big genera (but your general concurrence is of the *highest possible* interest to me); nor shall I be able till I re-read my MS.; but you may rely on it that you never make a remark to me which is lost from *inattention.* I am particularly glad you do not object to my stating your objections in a modified form, for they always struck me as very important, and as having much inherent value, whether or no they were fatal to my notions. I will consider and reconsider all your remarks. . . .

I am very glad at what you say about my Abstract, but you may rely on it that I will condense to the utmost. I would aid in money if it is too long.* In how many ways you have aided me!

Yours affectionately.

The "Abstract" mentioned in the last sentence of the preceding letter was in fact the *Origin of Species,* on which he now set to work. In his *Autobiography* (p. 41) he speaks of beginning to write in September, but in his Diary he wrote, "July 20 to Aug. 12, at Sandown, began Abstract of Species book." "Sep. 16, Recommenced Abstract." The book was begun with the idea that it would be published as a paper, or series of papers, by the Linnean Society, and it was only in the late autumn that it became clear that it must take the form of an independent volume.

* That is to say, he would help to pay for the printing, if it should prove too long for the Linnean Society.

C. D. to J. D. Hooker. Norfolk House, Shanklin, Isle of Wight. [August 1858.]

MY DEAR HOOKER,—I write merely to say that the MS. came safely two or three days ago. I am much obliged for the correction of style: I find it unutterably difficult to write clearly. When we meet I must talk over a few points on the subject.

You speak of going to the sea-side somewhere; we think this the nicest sea-side place which we have ever seen, and we like Shanklin better than other spots on the south coast of the island, though many are charming and prettier, so that I would suggest your thinking of this place. We are on the actual coast; but tastes differ so much about places.

If you go to Broadstairs, when there is a strong wind from the coast of France and in fine, dry, warm weather, look out and you will *probably* (!) see thistle-seeds blown across the Channel. The other day I saw one blown right inland, and then in a few minutes a second one and then a third; and I said to myself, God bless me, how many thistles there must be in France; and I wrote a letter in imagination to you. But I then looked at the *low* clouds, and noticed that they were not coming inland, so I feared a screw was loose, I then walked beyond a headland and found the wind parallel to the coast, and on this very headland a noble bed of thistles, which by every wide eddy were blown far out to sea, and then came right in at right angles to the shore! One day such a number of insects were washed up by the tide, and I brought to life thirteen species of Coleoptera; not that I suppose these came from France. But do you watch for thistle-seed as you saunter along the coast. . . .

C. D. to J. D. Hooker. [Down] Oct. 6th, 1858.

. . . If you have or can make leisure, I should very much like to hear news of Mrs. Hooker, yourself, and the children. Where did you go, and what did you do and are doing? There is a comprehensive text.

You cannot tell how I enjoyed your little visit here. It did me much good. If Harvey* is still with you, pray remember me very kindly to him.

. . . I am working most steadily at my Abstract [*Origin of Species*], but it grows to an inordinate length; yet fully to

* W. H. Harvey, born 1811, died 1866: a well-known botanist.

make my view clear (and never giving briefly more than a fact or two, and slurring over difficulties), I cannot make it shorter. It will yet take me three or four months; so slow do I work, though never idle. You cannot imagine what a service you have done me in making me make this Abstract; for though I thought I had got all clear, it has clarified my brains very much, by making me weigh the relative importance of the several elements.

He was not so fully occupied but that he could find time to help his boys in their collecting. He sent a short notice to the *Entomologists' Weekly Intelligencer*, June 25th, 1859, recording the capture of *Licinus silphoides*, *Clytus mysticus*, *Panagæus 4-pustulatus*. The notice begins with the words, "We three very young collectors having lately taken in the parish of Down," &c., and is signed by three of his boys, but was clearly not written by them. I have a vivid recollection of the pleasure of turning out my bottle of dead beetles for my father to name, and the excitement, in which he fully shared, when any of them proved to be uncommon ones. The following letter to Mr. Fox (Nov. 13th, 1858), illustrates this point:—

"I am reminded of old days by my third boy having just begun collecting beetles, and he caught the other day *Brachinus crepitans*, of immortal Whittlesea Mere memory. My blood boiled with old ardour when he caught a Licinus—a prize unknown to me."

And again to Sir John Lubbock:—

"I feel like an old war-horse at the sound of the trumpet when I read about the capturing of rare beetles—is not this a magnanimous simile for a decayed entomologist?—It really almost makes me long to begin collecting again. Adios.

"'Floreat Entomologia'!—to which toast at Cambridge I have drunk many a glass of wine. So again, 'Floreat Entomologia.'—N.B. I have *not* now been drinking any glasses full of wine."

C. D. to J. D. Hooker. Down, Jan. 23rd, 1859.

. . . I enclose letters to you and me from Wallace. I admire extremely the spirit in which they are written. I never felt very sure what he would say. He must be an amiable man. Please return that to me, and Lyell ought to be told how well satisfied he is. These letters have vividly brought before me how much I owe to your and Lyell's most kind and generous conduct in all this affair.

. . . How glad I shall be when the Abstract is finished, and I can rest! . . .

C. D. to A. R. Wallace. Down, Jan. 25th [1859].

MY DEAR SIR,—I was extremely much pleased at receiving three days ago your letter to me and that to Dr. Hooker. Permit me to say how heartily I admire the spirit in which they are written. Though I had absolutely nothing whatever to do in leading Lyell and Hooker to what they thought a fair course of action, yet I naturally could not but feel anxious to hear what your impression would be. I owe indirectly much to you and them; for I almost think that Lyell would have proved right, and I should never have completed my larger work, for I have found my Abstract [*Origin of Species*] hard enough with my poor health, but now, thank God, I am in my last chapter but one. My Abstract will make a small volume of 400 or 500 pages. Whenever published, I will, of course, send you a copy, and then you will see what I mean about the part which I believe selection has played with domestic productions. It is a very different part, as you suppose, from that played by "Natural Selection." I sent off, by the same address as this note, a copy of the *Journal of the Linnean Society*, and subsequently I have sent some half-dozen copies of the paper. I have many other copies at your disposal. . . .

I am glad to hear that you have been attending to birds' nests. I have done so, though almost exclusively under one point of view, viz. to show that instincts vary, so that selection could work on and improve them. Few other instincts, so to speak, can be preserved in a Museum.

Many thanks for your offer to look after horses' stripes; if there are any donkeys, pray add them. I am delighted to hear that you have collected bees' combs. . . . . This is an especial hobby of mine, and I think I can throw a light on the subject. If you can collect duplicates at no very great expense, I should be glad of some specimens for myself with some bees of each kind. Young, growing, and irregular combs, and those which have not had pupæ, are most valuable for measurements and examination. Their edges should be well protected against abrasion.

Every one whom I have seen has thought your paper very well written and interesting. It puts my extracts (written in 1839,* now just twenty years ago!), which I must say in apology were never for an instant intended for publication, into the shade.

* See a discussion on the date of the earliest sketch of the *Origin* in the *Life and Letters*, ii. p. 10.

You ask about Lyell's frame of mind. I think he is somewhat staggered, but does not give in, and speaks with horror, often to me, of what a thing it would be, and what a job it would be for the next edition of *The Principles*, if he were "perverted." But he is most candid and honest, and I think will end by being perverted. Dr. Hooker has become almost as heterodox as you or I, and I look at Hooker as *by far* the most capable judge in Europe.

Most cordially do I wish you health and entire success in all your pursuits, and, God knows, if admirable zeal and energy deserve success, most amply do you deserve it. I look at my own career as nearly run out. If I can publish my Abstract and perhaps my greater work on the same subject, I shall look at my course as done.

Believe me, my dear Sir, yours very sincerely.

In March 1859 the work was telling heavily on him. He wrote to Fox:—

"I can see daylight through my work, and am now finally correcting my chapters for the press; and I hope in a month or six weeks to have proof-sheets. I am weary of my work. It is a very odd thing that I have no sensation that I overwork my brain; but facts compel me to conclude that my brain was never formed for much thinking. We are resolved to go for two or three months, when I have finished, to Ilkley, or some such place, to see if I can anyhow give my health a good start, for it certainly has been wretched of late, and has incapacitated me for everything. You do me injustice when you think that I work for fame; I value it to a certain extent; but, if I know myself, I work from a sort of instinct to try to make out truth."

C. D. to C. Lyell. Down, March 28th [1859].

MY DEAR LYELL,—If I keep decently well, I hope to be able to go to press with my volume early in May. This being so, I want much to beg a little advice from you. From an expression in Lady Lyell's note, I fancy that you have spoken to Murray. Is it so? And is he willing to publish my Abstract? * If you will tell me whether anything, and what has passed, I will then write to him. Does he know at all of the subject of the book? Secondly, can you advise me whether I had better state what terms of publication I should prefer, or

* *The Origin of Species.*

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.

He sent copies of the *Origin*, accompanied by letters similar to the last, to M. De Candolle, Dr. Asa Gray, Falconer and Mr. Jenyns (Blomefield).

To Henslow he wrote (Nov. 11th, 1859):—

"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 ever 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."

C. D. 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, &c. I have heard from Mr. Sclater 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 * 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.

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

C. Darwin to W. B. Carpenter. November 19th [1859].

. . . . 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 monomaniacs.

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

Yours very sincerely.

* Mr. Wallace was in the Malay Archipelago.

I am indebted to Messrs. Smith and Elder for the information that these articles were written by Mr. G. H. Lewes.

The following extract from a letter (Feb. 1870) to his friend Professor Newton, the well-known ornithologist, shows how much he valued the appreciation of his colleagues.

"I suppose it would be universally held extremely wrong for a defendant to write to a Judge to express his satisfaction at a judgment in his favour; and yet I am going thus to act. I have just read what you have said in the 'Record' * about my pigeon chapters, and it has gratified me beyond measure. I have sometimes felt a little disappointed that the labour of so many years seemed to be almost thrown away, for you are the first man capable of forming a judgment (excepting partly Quatrefages), who seems to have thought anything of this part of my work. The amount of labour, correspondence, and care, which the subject cost me, is more than you could well suppose. I thought the article in the *Athenæum* was very unjust; but now I feel amply repaid, and I cordially thank you for your sympathy and too warm praise."

WORK ON MAN.

In February 1867, when the manuscript of *Animals and Plants* had been sent to Messrs. Clowes to be printed, and before the proofs began to come in, he had an interval of spare time, and began a "Chapter on Man," but he soon found it growing under his hands, and determined to publish it separately as a "very small volume."

It is remarkable that only four years before this date, namely in 1864, he had given up hope of being able to work out this subject. He wrote to Mr. Wallace:—

"I have collected a few notes on man, but I do not suppose that I shall ever use them. Do you intend to follow out your views, and if so, would you like at some future time to have my few references and notes? I am sure I hardly know whether they are of any value, and they are at present in a state of chaos. There is much more that I should like to write, but I have not strength." But this was at a period of ill-health; not long before, in 1863, he had written in the same depressed tone about his future work generally:—

"I have been so steadily going downhill, I cannot help doubting whether I can ever crawl a little uphill again. Unless

* *Zoological Record.* The volume for 1868, published December, 1869.

I can, enough to work a little, I hope my life may be very short, for to lie on a sofa all day and do nothing but give trouble to the best and kindest of wives and good dear children is dreadful."

The "Chapter on Man," which afterwards grew into the *Descent of Man*, was interrupted by the necessity of correcting the proofs of *Animals and Plants*, and by some botanical work, but was resumed with unremitting industry on the first available day in the following year. He could not rest, and he recognised with regret the gradual change in his mind that rendered continuous work more and more necessary to him as he grew older. This is expressed in a letter to Sir J. D. Hooker, June 17, 1868, which repeats to some extent what is given in the *Autobiography*:—

"I am glad you were at the *Messiah*, it is the one thing that I should like to hear again, but I dare say I should find my soul too dried up to appreciate it as in old days; and then I should feel very flat, for it is a horrid bore to feel as I constantly do, that I am a withered leaf for every subject except Science. It sometimes makes me hate Science, though God knows I ought to be thankful for such a perennial interest, which makes me forget for some hours every day my accursed stomach.'

The Descent of Man (and this is indicated on its title-page) consists of two separate books, namely on the pedigree of mankind, and on sexual selection in the animal kingdom generally. In studying this latter part of the subject he had to take into consideration the whole subject of colour. I give the two following characteristic letters, in which the reader is as it were present at the birth of a theory.

C. D. to A. R. Wallace. Down, February 23 [1867].

DEAR WALLACE,—I much regretted that I was unable to call on you, but after Monday I was unable even to leave the house. On Monday evening I called on Bates, and put a difficulty before him, which he could not answer, and, as on some former similar occasion, his first suggestion was, "You had better ask Wallace." My difficulty is, why are caterpillars sometimes so beautifully and artistically coloured? Seeing that many are coloured to escape danger, I can hardly attribute their bright colour in other cases to mere physical conditions. Bates says the most gaudy caterpillar he ever saw in Amazonia (of a sphinx) was conspicuous at the distance of yards, from its black and red colours, whilst feeding on large green leaves. If any one objected to male butterflies having been made beautiful by sexual selection, and asked why should they not have been made beautiful as well as their caterpillars, what would you answer? I could not answer, but should maintain my ground. Will you think over this, and some time, either by letter or when we meet, tell me what you think? . . . .

He seems to have received an explanation by return of post, for a day or two afterwards he could write to Wallace:—

"Bates was quite right; you are the man to apply to in a difficulty. I never heard anything more ingenious than your suggestion, and I hope you may be able to prove it true. That is a splendid fact about the white moths; it warms one's very blood to see a theory thus almost proved to be true."

Mr. Wallace's suggestion was that conspicuous caterpillars or perfect insects (*e.g.* white butterflies), which are distasteful to birds, benefit by being promptly recognised and therefore easily avoided.*

The letter from Darwin to Wallace goes on: "The reason of my being so much interested just at present about sexual selection is, that I have almost resolved to publish a little essay on the origin of Mankind, and I still strongly think (though I failed to convince you, and this, to me, is the heaviest blow possible) that sexual selection has been the main agent in forming the races of man.

"By the way, there is another subject which I shall introduce in my essay, namely, expression of countenance. Now, do you happen to know by any odd chance a very good-natured and acute observer in the Malay Archipelago, who you think would make a few easy observations for me on the expression of the Malays when excited by various emotions?"

The reference to the subject of expression in the above letter is explained by the fact, that my father's original intention was to give his essay on this subject as a chapter in the *Descent of Man*, which in its turn grew, as we have seen, out of a proposed chapter in *Animals and Plants.*

He got much valuable help from Dr. Günther, of the Natural History Museum, to whom he wrote in May 1870:—

"As I crawl on with the successive classes I am astonished to find how similar the rules are about the nuptial or 'wedding

* Mr. Jenner Weir's observations published in the *Transactions of the Entomological Society* (1869 and 1870) give strong support to the theory in question.

those which at least cannot be assailed by mere efforts of imagination."

In the fifth edition of the *Origin*, my father altered a passage in the Historical Sketch (fourth edition, p. xviii.). He thus practically gave up the difficult task of understanding whether or not Sir R. Owen claims to have discovered the principle of Natural Selection. Adding, "As far as the mere enunciation of the principle of Natural Selection is concerned, it is quite immaterial whether or not Professor Owen preceded me, for both of us . . . were long ago preceded by Dr. Wells and Mr. Matthew."

The desire that his views might spread in France was always strong with my father, and he was therefore justly annoyed to find that in 1869 the publisher of the French edition had brought out a third edition without consulting the author. He was accordingly glad to enter into an arrangement for a French translation of the fifth edition; this was undertaken by M. Reinwald, with whom he continued to have pleasant relations as the publisher of many of his books into French.

He wrote to Sir J. D. Hooker:—

"I must enjoy myself and tell you about Mdlle. C. Royer, who translated the *Origin* into French, and for whose second edition I took infinite trouble. She has now just brought out a third edition without informing me, so that all the corrections, &c., in the fourth and fifth English editions are lost. Besides her enormously long preface to the first edition, she has added a second preface abusing me like a pickpocket for Pangenesis, which of course has no relation to the *Origin*. So I wrote to Paris; and Reinwald agrees to bring out at once a new translation from the fifth English edition, in competition with her third edition. . . . This fact shows that 'evolution of species' must at last be spreading in France."

It will be well perhaps to place here all that remains to be said about the *Origin of Species*. The sixth or final edition was published in January 1872 in a smaller and cheaper form than its predecessors. The chief addition was a discussion suggested by Mr. Mivart's *Genesis of Species*, which appeared in 1871, before the publication of the *Descent of Man*. The following quotation from a letter to Wallace (July 9, 1871) may serve to show the spirit and method in which Mr. Mivart dealt with the subject. "I grieve to see the omission of the words by Mivart, detected by Wright.* I complained to

* The late Chauncey Wright, in an article published in the *North American Review*, vol. cxiii. pp. 83, 84. Wright points out that the words omitted are "essential to the point on which he [Mr. Mivart] cites Mr.

Mivart that in two cases he quotes only the commencement of sentences by me, and thus modifies my meaning; but I never supposed he would have omitted words. There are other cases of what I consider unfair treatment."

My father continues, with his usual charity and moderation:—

"I conclude with sorrow that though he means to be honourable, he is so bigoted that he cannot act fairly."

In July 1871, my father wrote to Mr. Wallace:—

"I feel very doubtful how far I shall succeed in answering Mivart, it is so difficult to answer objections to doubtful points, and make the discussion readable. I shall make only a selection. The worst of it is, that I cannot possibly hunt through all my references for isolated points, it would take me three weeks of intolerably hard work. I wish I had your power of arguing clearly. At present I feel sick of everything, and if I could occupy my time and forget my daily discomforts, or rather miseries, I would never publish another word. But I shall cheer up, I dare say, soon, having only just got over a bad attack. Farewell; God knows why I bother you about myself. I can say nothing more about missing-links than what I have said. I should rely much on pre-silurian times; but then comes Sir W. Thomson like an odious spectre.* Farewell.

". . . There is a most cutting review of me in the [July] *Quarterly*; I have only read a few pages. The skill and style make me think of Mivart. I shall soon be viewed as the most despicable of men. This *Quarterly Review* tempts me to republish Ch. Wright,† even if not read by any one, just to show some one will say a word against Mivart, and that his (*i.e.* Mivart's) remarks ought not to be swallowed without some reflection. . . . God knows whether my strength and spirit will last out to write a chapter versus Mivart and others; I do so hate controversy and feel I shall do it so badly."

The *Quarterly* review was the subject of an article by Mr. Huxley in the November number of the *Contemporary Review*. Here, also, are discussed Mr. Wallace's *Contribution to the Theory of Natural Selection*, and the second edition of Mr.

Darwin's authority." It should be mentioned that the passage from which words are omitted is not given within inverted commas by Mr. Mivart.

* My father, as an Evolutionist, felt that he required more time than Sir W. Thomson's estimate of the age of the world allows.

† Chauncey Wright's review was published as a pamphlet in the autumn of 1871.

Mivart's *Genesis of Species*. What follows is taken from Mr. Huxley's article. The *Quarterly* reviewer, though to some extent an evolutionist, believes that Man "differs more from an elephant or a gorilla, than do these from the dust of the earth on which they tread." The reviewer also declares that Darwin has "with needless opposition, set at naught the first principles of both philosophy and religion." Mr. Huxley passes from the *Quarterly* reviewer's further statement, that there is no necessary opposition between evolution and religion, to the more definite position taken by Mr. Mivart, that the orthodox authorities of the Roman Catholic Church agree in distinctly asserting derivative creation, so that "their teachings harmonize with all that modern science can possibly require." Here Mr. Huxley felt the want of that "study of Christian philosophy" (at any rate, in its Jesuitic garb), which Mr. Mivart speaks of, and it was a want he at once set to work to fill up. He was then staying at St. Andrews, whence he wrote to my father:—

"By great good luck there is an excellent library here, with a good copy of Suarez,* in a dozen big folios. Among these I dived, to the great astonishment of the librarian, and looking into them 'as careful robins eye the delver's toil' (*vide Idylls*), I carried off the two venerable clasped volumes which were most promising." Even those who know Mr. Huxley's unrivalled power of tearing the heart out of a book must marvel at the skill with which he has made Suarez speak on his side. "So I have come out," he wrote, "in the new character of a defender of Catholic orthodoxy, and upset Mivart out of the mouth of his own prophet."

The remainder of Mr. Huxley's critique is largely occupied with a dissection of the *Quarterly* reviewer's psychology, and his ethical views. He deals, too, with Mr. Wallace's objections to the doctrine of Evolution by natural causes when applied to the mental faculties of Man. Finally, he devotes a couple of pages to justifying his description of the *Quarterly* reviewer's treatment of Mr. Darwin as alike "unjust and unbecoming." †

* The learned Jesuit on whom Mr. Mivart mainly relies.

† The same words may be applied to Mr. Mivart's treatment of my father. The following extract from a letter to Mr. Wallace (June 17th, 1874) refers to Mr. Mivart's statement (*Lessons from Nature*, p. 144) that Mr. Darwin at first studiously disguised his views as to the "bestiality of man":—

"I have only just heard of and procured your two articles in the *Academy*. I thank you most cordially for your generous defence of me against Mr. Mivart. In the *Origin* I did not discuss the derivation of any one species; but that I might not be accused of concealing my opinion,