DARWIN CHARLES

MORE LETTERS OF CHARLES DARWIN — VOLUME 1

Charles Darwin

More Letters of Charles Darwin — Volume 1

http://www.litres.ru/pages/biblio_book/?art=25091732
More Letters of Charles Darwin — Volume 1 / A Record of His Work in a Series of Hitherto Unpublished Letters:

Содержание

Charles Darwin
More Letters of Charles Darwin — Volume 1 / A Record of His Work in a Series of Hitherto Unpublished Letters

VOLUME I

DEDICATED WITH
AFFECTION AND RESPECT, TO

SIR JOSEPH HOOKER

IN REMEMBRANCE OF HIS LIFELONG
FRIENDSHIP WITH CHARLES DARWIN

PREFACE

The "Life and Letters of Charles Darwin" was published in 1887. Since that date, through the kindness of various correspondents, additional letters have been received; among them may be mentioned those written by Mr. Darwin to Mr. Belt, Lady Derby, Hugh Falconer, Mr. Francis Galton, Huxley, Lyell, Mr. John Morley, Max Muller, Owen, Lord Playfair, John Scott, Thwaites, Sir William Turner, John Jenner Weir. But the material for our work consisted in chief part of a mass of letters which, for want of space or for other reasons, were not printed in the "Life and Letters." We would draw particular attention to the correspondence with Sir Joseph Hooker. To him Mr. Darwin wrote with complete freedom, and this has given something of a personal charm to the most technical of his letters. There is also much correspondence, hardly inferior in biographical interest, with Sir Charles Lyell, Fritz Muller, Mr. Huxley, and Mr. Wallace. From this unused material we have been able to compile an almost complete record of Mr. Darwin's work in a series of letters now published for the first time. We have, however, in a few instances, repeated paragraphs, or in one or two cases whole letters, from the "Life and Letters," where such repetition seemed necessary for the sake of clearness or continuity.

Our two volumes contain practically all the matter that it now seems desirable to publish. But at some future time others may find interesting data in what remains unprinted; this is certainly true of a short series of letters dealing with the Cirripedes, which are omitted solely for want of space. (Preface/1. Those addressed to the late Albany Hancock have already appeared in the "Transactions of the Tyneside Nat. Field Club," VIII., page 250.)

We are fortunate in being permitted, by Sir Joseph Hooker and by Mr. Wallace, to publish certain letters from them to Mr. Darwin. We have also been able to give a few letters from Sir Charles Lyell, Hugh Falconer, Edward Forbes, Dr. Asa Gray, Professor Hyatt, Fritz Muller, Mr. Francis Galton, and Sir T. Lauder Brunton. To the two last named, also to Mrs. Lyell (the biographer of Sir Charles), Mrs. Asa Gray and Mrs. Hyatt, we desire to express our grateful acknowledgments.

The present volumes have been prepared, so as to give as full an idea as possible of the course of Mr. Darwin's work. The volumes therefore necessarily contain many letters of a highly technical character, but none, we hope, which are not essentially interesting. With a view to saving space, we have confined ourselves to elucidating the letters by full annotations, and have for the same reason — though with some regret — omitted in most cases the beginnings and endings of the letters. For the main facts of Mr. Darwin's life, we refer our readers to the abstract of his private Diary, given in the present volume.

Mr. Darwin generally wrote his letters when he was tired or hurried, and this often led to the omission of words. We have usually inserted the articles, and this without any indication of their absence in the originals. Where there seemed any possibility of producing an alteration of meaning (and in many cases where there is no such possibility) we have placed the introduced words in square brackets. We may say once for all that throughout the book square brackets indicate words not found in the originals. (Preface/2. Except in a few places where brackets are used to indicate passages previously published. In all such cases the meaning of the symbol is explained.) Dots indicate omissions, but many omissions are made without being so indicated.

The selection and arrangement of the letters have not been easy. Our plan has been to classify the letters according to subject — into such as deal with Evolution, Geographical Distribution, Botany, etc., and in each group to place the letters chronologically. But in several of the chapters we have adopted sectional headings, which we believe will be a help to the reader. The great difficulty lay in deciding in which of the chief groups a given letter should be placed. If the MS. had been cut up into paragraphs, there would have been no such difficulty; but we feel strongly that a letter should as far as possible be treated as a whole. We have in fact allowed this principle to interfere with an accurate classification, so that the reader will find, for instance, in the chapters on Evolution, questions considered which might equally well have come under Geographical Distribution or Geology, or questions in the chapter on Man which might have been placed under the heading Evolution. In the same way, to avoid mutilation, we have allowed references to one branch of science to remain in letters mainly concerned with another subject. For these irregularities we must ask the reader's patience, and beg him to believe that some pains have been devoted to arrangement.

Mr. Darwin, who was careful in other things, generally omitted the date in familiar correspondence, and it is often only by treating a letter as a detective studies a crime that we can make sure of its date. Fortunately, however, Sir Joseph Hooker and others of Darwin's correspondents were accustomed to add the date on which the letters were received. This sometimes leads to an inaccuracy which needs a word of explanation. Thus a letter which Mr. Darwin dated "Wednesday" might be headed by us "Wednesday {January 3rd, 1867}," the latter half being the date on which the letter was received; if it had been dated by the writer it would have been "Wednesday, January 2nd, 1867."

In thanking those friends — especially Sir Joseph Hooker and Mr. Wallace — who have looked through some of our proof-sheets, we wish to make it clear that they are not in the smallest degree responsible for our errors or omissions; the weight of our shortcomings rests on us alone.

We desire to express our gratitude to those who have so readily supplied us with information, especially to Sir Joseph Hooker, Professor Judd, Professor Newton, Dr. Sharp, Mr. Herbert Spencer, and Mr. Wallace. And we have pleasure in

mentioning Mr. H.W. Rutherford, of the University Library, to whose conscientious work as a copyist we are much indebted.

Finally, it is a pleasure to express our obligation to those who have helped us in the matter of illustrations. The portraits of Dr. Asa Gray, Mr. Huxley, Sir Charles Lyell, Mr. Romanes, are from their respective Biographies, and for permission to make use of them we have to thank Mrs. Gray, Mr. L. Huxley, Mrs. Lyell, and Mrs. Romanes, as well as the publishers of the books in question. For the reproduction of the early portrait of Mr. Darwin we are indebted to Miss Wedgwood; for the interesting portraits of Hugh Falconer and Edward Forbes we have to thank Mr. Irvine Smith, who obtained for us the negatives; these being of paper, and nearly sixty years old, rendered their reproduction a work of some difficulty. We also thank Messrs. Elliott & Fry for very kindly placing at our disposal a negative of the fine portrait, which forms the frontispiece to Volume II. For the opportunity of making facsimiles of diagrams in certain of the letters, we are once more indebted to Sir Joseph Hooker, who has most generously given the original letters to Mr. Darwin's family.

Cambridge, October, 1902.

MORE LETTERS OF CHARLES DARWIN

VOLUME I. OUTLINE OF CHARLES DARWIN'S LIFE

BASED ON HIS DIARY, DATED AUGUST 1838

References to the Journals in which Mr. Darwin's papers were published will be found in his "Life and Letters" III., Appendix II. We are greatly indebted to Mr. C.F. Cox, of New York, for calling our attention to mistakes in the Appendix, and we take this opportunity of correcting them.

Appendix II., List ii. — Mr. Romanes spoke on Mr. Darwin's essay on Instinct at a meeting of the Linnean Society, December 6th, 1883, and some account of it is given in "Nature" of the same date. But it was not published by the Linnean Society.

Appendix II., List iii. — "Origin of saliferous deposits. Salt lakes of Patagonia and La Plata" (1838). This is the heading of an extract from Darwin's volume on South America reprinted in the "Quarterly Journal of the Geological Society," Volume II., Part ii., "Miscellanea," pages 127-8, 1846.

The paper on "Analogy of the Structure of some Volcanic Rocks, etc." was published in 1845, not in 1851.

A paper "On the Fertilisation of British Orchids by Insect Agency," in the "Entomologist's Weekly Intelligencer" viii., and "Gardeners' Chronicle," June 9th, 1860, should be inserted in the bibliography.

1809. February 12th: Born at Shrewsbury.

1817. Death of his mother.

1818. Went to Shrewsbury School.

1825. Left Shrewsbury School.

1826.

October: Went to Edinburgh University. Read two papers before the Plinian Society of Edinburgh "at the close of 1826 or early in 1827."

1827. Entered at Christ's College, Cambridge.

1828. Began residence at Cambridge.

1831.

January: Passed his examination for B.A., and kept the two following terms.

August: Geological tour with Sedgwick.

September 11th: Went to Plymouth to see the "Beagle."

October 2nd: "Took leave of my home."

December 27th: "Sailed from England on our circumnavigation."

1832.

January 16th: "First landed on a tropical shore" (Santiago).

1833.

December 6th: "Sailed for last time from Rio Plata."

1834.

June 10th: "Sailed for last time from Tierra del Fuego."

1835.

September 5th: "Sailed from west shores of South America."

November 16th: Letters to Professor Henslow, read at a meeting of the Cambridge Philosophical Society.

November 18th: Paper read before the Geological Society on Notes made during a Survey of the East and West Coasts of South America in years 1832-35.

1836.

May 31st: Anchored at the Cape of Good Hope.

October 2nd: Anchored at Falmouth.

October 4th: Reached Shrewsbury after an absence of five years and two days.

December 13th: Went to live at Cambridge.

1837.

January 4th: Paper on Recent Elevation in Chili read.

March 13th: Settled at 36, Great Marlborough Street.

March 14th: Paper on "Rhea" read.

May: Read papers on Coral Formation, and on the Pampas, to the Geological Society.

July: Opened first note-book on Transmutation of Species.

March 13th to November: Occupied with his Journal.

October and November: Preparing the scheme for the Zoology of the Voyage of the "Beagle." Working at Geology of South America.

November 1st: Read the paper on Earthworms before the Geological Society.

1838.

Worked at the Geology of South America and Zoology of Voyage. "Some little species theory."

March 7th: Read paper on the Connexion of certain Volcanic Phenomena and on the Formation of Mountain Chains, to the Geological Society.

May: Health began to break down.

June 23rd: Started for Glen Roy. The paper on Glen Roy was written in August and September.

October 5th: Began Coral paper.

November 11th: Engaged to be married to his cousin, Emma Wedgwood.

December 31st: "Entered 12 Upper Gower Street."

1839.

January 29th: Married at Maer.

February and March: Some work on Corals and on Species Theory.

March (part) and April: Working at Coral paper. Papers on a Rock seen on an Iceberg, and on the Parallel Roads of Glen Roy. Published "Journal and Remarks," being volume iii. of the

"Narrative of the Surveying Voyages of H.M.S. 'Adventure' and 'Beagle,' etc." For the rest of the year, Corals and Zoology of the Voyage. Publication of the "Zoology of the Voyage of H.M.S. 'Beagle,'" Part II. (Mammalia).

1840.

Worked at Corals and the Zoology of the Voyage. Contributed Geological introduction to Part I. of the "Zoology of the Voyage" (Fossil Mammalia by Owen).

1841.

Publication of Part III. of the "Zoology of the Voyage" (Birds). Read paper on Boulders and Glacial Deposits of South America, to Geological Society. Published paper on a remarkable bar of Sandstone off Pernambuco, on the coast of Brazil. Publication of Part IV. of "Zoology of the Voyage" (Fish).

1842.

May 6th: Last proof of the Coral book corrected.

June: Examined Glacier action in Wales. "Wrote pencil sketch of my Species Theory."

July: Wrote paper on Glaciers of Caernarvonshire.

October: Began his book on Volcanic Islands.

1843.

Working at "Volcanic Islands" and "some Species work."

1844.

February 13th: Finished "Volcanic Islands."

July to September: Wrote an enlarged version of Species Theory. Papers on Sagitta, and on Planaria.

July 27th: Began his book on the Geology of South America.

1845.

Paper on the Analogy of the Structure of Volcanic Rocks with that of Glaciers. "Proc. R. Soc. Edin."

April 25th to August 25th: Working at second edition of "Naturalist's Voyage."

1846.

October 1st: Finished last proof of "Geological Observations on South America." Papers on Atlantic Dust, and on Geology of Falkland Islands, communicated to the Geological Society. Paper on Arthrobalanus.

1847.

Working at Cirripedes. Review of Waterhouse's "Natural History of the Mammalia."

1848.

March 20th: Finished Scientific Instructions in Geology for the Admiralty Manual. Working at Cirripedes. Paper on Erratic Boulders.

1849.

Health especially bad. Working at Cirripedes.

March-June: Water-cure at Malvern.

1850.

Working at Cirripedes. Published Monographs of Recent and Fossil Lepadidae.

1852.

Working at Cirripedes.

1853.

November 30th: "Royal Medal given to me."

1854.

Published Monographs on Recent and on Fossil Balanidae and Verrucidae.

September 9th: Finished packing up all my Cirripedes. "Began sorting notes for Species Theory."

1855.

March-April: Experiments on the effect of salt water on seeds. Papers on Icebergs and on Vitality of Seeds.

1856.

May 14th: "Began, by Lyell's advice, writing Species Sketch" (described in "Life and Letters" as the "Unfinished Book").

December 16th: Finished Chapter III. Paper read to Linnean Society, On Sea-water and the Germination of Seeds.

1857.

September 29th: Finished Chapters VII. and VIII.

September 30th to December 29th: Working on Hybridism. Paper on the Agency of Bees in the Fertilisation of Papilionaceous Flowers.

1858.

March 9th: "Finished Instinct chapter."

June 18th: Received Mr. Wallace's sketch of his evolutionary theory.

July 1st: Joint paper of Darwin and Wallace read at the

Linnean Society.

July 20th to July 27th: "Began Abstract of Species book," i.e., the "Origin of Species," at Sandown, I.W. Paper on Bees and Fertilisation of Flowers.

1859.

May 25th: Began proof-sheets of the "Origin of Species."

November 24th: Publication of the "Origin": 1250 copies printed.

October 2nd to December 9th: At the water-cure establishment, Ilkley, Yorkshire.

1860.

January 7th: Publication of Edition II. of "Origin" (3000 copies).

January 9th: "Looking over MS. on Variation." Paper on the Fertilisation of British Orchids.

July and again in September: Made observations on Drosera. Paper on Moths and Flowers. Publication of "A Naturalist's Voyage."

1861.

Up to July at work on "Variation under Domestication."

April 30th: Publication of Edition III. of "Origin" (2000 copies).

July to the end of year: At work on Orchids.

November: Primula paper read at Linnean Society. Papers on Pumilio and on Fertilisation of Vinca.

1862.

May 15th: Orchid book published. Working at Variation. Paper on Catasetum (Linnean Society). Contribution to Chapter III. of Jenyns' Memoir of Henslow.

1863.

Working at "Variation under Domestication." Papers on Yellow Rain, the Pampas, and on Cirripedes. A review of Bates' paper on Mimetic Butterflies. Severe illness to the end of year.

1864.

Illness continued until April. Paper on Linum published by the Linnean Society.

May 25th: Paper on Lythrum finished.

September 13th: Paper on Climbing Plants finished. Work on "Variation under Domestication."

November 30th: Copley medal awarded to him.

1865.

January 1st: Continued at work on Variation until April 22nd. The work was interrupted by illness until late in the autumn.

February: Read paper on Climbing Plants.

December 25th: Began again on Variation.

1866.

Continued work at "Variation under Domestication."

March 1st to May 10th: At work on Edition IV. of the "Origin." Published June (1250 copies). Read paper on Cytisus scoparius to the Linnean Society.

December 22nd: Began the last chapter of "Variation under Domestication."

1867.

November 15th: Finished revises of "Variation under Domestication."

December: Began papers on Illegitimate Unions of Dimorphic and Trimorphic Plants, and on Primula.

1868.

January 30th: Publication of "Variation under Domestication."

February 4th: Began work on Man.

February 10th: New edition of "Variation under Domestication." Read papers on Illegitimate Unions of Dimorphic and Trimorphic Plants, and on Verbascum.

1869.

February 10th: "Finished fifth edition of 'Origin'; has taken me forty-six days."

Edition V. published in May.

Working at the "Descent of Man." Papers on the Fertilisation of Orchids, and on the Fertilisation of Winter-flowering Plants.

1870.

Working at the "Descent of Man." Paper on the Pampas Woodpecker.

1871.

January 17th: Began the "Expression of the Emotions."

February 24th: "Descent of Man" published (2500 copies).

April 27th: Finished the rough copy of "Expression."

June 18th: Began Edition VI. of "Origin." Paper on the

Fertilisation of Leschenaultia.

1872.

January 10th: Finished proofs of Edition VI. of the "Origin," and "again rewriting 'Expression.'"

August 22nd: Finished last proofs of "Expression."

August 23rd: Began working at Drosera.

November: "Expression" published (7000 copies, and 2000 more printed at the end of the year.)

November 8th: "At Murray's sale 5267 copies sold to London booksellers."

1873.

January: Correcting the Climbing Plants paper for publication as a book.

February 3rd: At work on "Cross-fertilisation."

February to September: Contributions to "Nature."

June 14th: "Began Drosera again."

November 20th: Began "Descent of Man," Edition II.

1874.

"Descent of Man," Edition II, in one volume, published (Preface dated September). "Coral Reefs," Edition II., published.

April 1st: Began "Insectivorous Plants."

February to May: Contributed notes to "Nature."

1875.

July 2nd: "Insectivorous Plants" published (3000 copies); 2700 copies sold immediately.

July 6th: "Correcting 2nd edition of 'Variation under

Domestication.'" It was published in the autumn.

September 1st (approximately): Began on "Cross and Self-Fertilisation."

November: Vivisection Commission.

1876.

May 5th: "Finished MS., first time over, of 'Cross and Self-Fertilisation.'"

May to June: Correction of "Fertilisation of Orchids," Edition II. Wrote his Autobiographical Sketch.

May and November: Contributions to "Nature."

August 19th: First proofs of "Cross and Self-Fertilisation."

November 10th: "Cross and Self-Fertilisation" published (1500 copies).

1877.

"All the early part of summer at work on 'Different Forms of Flowers.'"

July: Publication of "Different Forms of Flowers" (1250 copies). During the rest of the year at work on the bloom on leaves, movements of plants, "and a little on worms."

November: LL.D. at Cambridge. Second edition of "Fertilisation of Orchids" published. Contributions to "Nature," "Gardeners' Chronicle," and "Mind."

1878.

The whole year at work on movements of plants, and on the bloom on leaves.

May: Contribution to "Nature." Second edition of "Different

Forms of Flowers." Wrote prefatory letter to Kerner's "Flowers and their Unbidden Guests."

1879.

The whole year at work on movements of plants, except for "about six weeks" in the spring and early summer given to the "Life of Erasmus Darwin," which was published in the autumn. Contributions to "Nature."

1880. "All spring finishing MS. of 'Power of Movement in Plants' and proof sheets." "Began in autumn on Worms." Prefatory notice written for Meldola's translation of Weismann's book.

November 6th: 1500 copies of "Power of Movement" sold at Murray's sale. Contributions to "Nature."

1881.

During all the early part of the year at work on the "Worm book." Several contributions to "Nature."

October 10th: The book on "Earthworms" published: 2000 copies sold at once.

November: At work on the action of carbonate of ammonia on plants.

1882.

No entries in the Diary.

February: At work correcting the sixth thousand of the "Earthworms."

March 6th and March 16th: Papers on the action of Carbonate of Ammonia on roots, etc., read at the Linnean Society.

April 6th: Note to "Nature" on Dispersal of Bivalves.

April 18th: Van Dyck's paper on Syrian Dogs, with a preliminary notice by Charles Darwin, read before the Zoological Society.

April 19th: Charles Darwin died at Down.

CHARLES DARWIN

CHAPTER 1.I. — AN AUTOBIOGRAPHICAL FRAGMENT, AND EARLY LETTERS

1809-1842

(Chapter I./1. In the process of removing the remainder of Mr. Darwin's books and papers from Down, the following autobiographical notes, written in 1838, came to light. They seem to us worth publishing — both as giving some new facts, and also as illustrating the interest which he clearly felt in his own development. Many words are omitted in the manuscript, and some names incorrectly spelled; the corrections which have been made are not always indicated.)

My earliest recollection, the date of which I can approximately tell, and which must have been before I was four years old, was when sitting on Caroline's (Caroline Darwin) knee in the drawing room, whilst she was cutting an orange for me, a cow ran by the window which made me jump, so that I received a bad cut, of which I bear the scar to this day. Of this scene I recollect the place where I sat and the cause of the fright, but not the cut itself, and I think my memory is real, and not as often happens in similar cases, {derived} from hearing the thing often repeated, {when} one obtains so vivid an image, that it cannot be separated from memory: because I clearly remember which way the cow ran, which would not probably have been told me. My memory here is an obscure picture, in which from not recollecting any pain I am scarcely conscious of its reference to myself.

1813.

When I was four years and a half old I went to the sea, and stayed there some weeks. I remember many things, but with the exception of the maidservants (and these are not individualised) I recollect none of my family who were there. I remember either myself or Catherine being naughty, and being shut up in a room and trying to break the windows. I have an obscure picture of a house before my eyes, and of a neighbouring small shop, where the owner gave me one fig, but which to my great joy turned out to be two: this fig was given me that the man might kiss the maidservant. I remember a common walk to a kind of well, on the road to which was a cottage shaded with damascene (Chapter I./2. Damson is derived from Damascene; the fruit was formerly known as a "Damask Prune.") trees, inhabited by an old man, called a hermit, with white hair, who used to give us damascenes. I know not whether the damascenes, or the reverence and indistinct fear for this old man produced the greatest effect on my memory. I remember when going there crossing in the carriage a broad ford, and fear and astonishment of white foaming water has made a vivid impression. I think memory of events commences abruptly; that is, I remember these earliest things quite as clearly as others very much later in life, which were equally impressed on me. Some very early recollections are connected with fear at Parkfield and with poor Betty Harvey. I remember with horror her story of people being pushed into the canal by the towing rope, by going the wrong side of the horse. I had the greatest horror of this story — keen instinct against death. Some other recollections are those of vanity — namely, thinking that people were admiring me, in one instance for perseverance and another for boldness in climbing a low tree, and what is odder, a consciousness, as if instinctive, that I was vain, and contempt of myself. My supposed admirer was old Peter Haile the bricklayer, and the tree the mountain ash on the lawn. All my recollections seem to be connected most closely with myself; now Catherine (Catherine Darwin) seems to recollect scenes where others were the chief actors. When my mother died I was 8 1/2 years old, and {Catherine} one year less, yet she remembers all particulars and events of each day whilst I scarcely recollect anything (and so with very many other cases) except being sent for, the memory of going into her room, my father meeting me — crying afterwards. I recollect my mother's gown and scarcely anything of her appearance, except one or two walks with her. I have no distinct remembrance of any conversation, and those only of a very trivial nature. I remember her saying "if she did ask me to do something," which I said she had, "it was solely for my good."

Catherine remembers my mother crying, when she heard of my grandmother's death. Also when at Parkfield how Aunt Sarah and Aunt Kitty used to receive her. Susan, like me, only remembers affairs personal. It is sufficiently odd this {difference} in subjects remembered. Catherine says she does not remember the impression made upon her by external things, as scenery, but for things which she reads she has an excellent memory, i.e., for ideas. Now her sympathy being ideal, it is part of her character, and shows how easily her kind of memory was stamped, a vivid thought is repeated, a vivid impression forgotten.

I remember obscurely the illumination after the battle of Waterloo, and the Militia exercising about that period, in the field opposite our house.

1817.

At 8 1/2 years old I went to Mr. Case's School. (Chapter I/3. A day-school at Shrewsbury kept by Rev. G. Case, minister of the Unitarian Chapel ("Life and Letters," Volume I., page 27 et seq.)) I remember how very much I was afraid of meeting the dogs in Barker Street, and how at school I could not get up my courage to fight. I was very timid by nature. I remember I took great delight at school in fishing for newts in the quarry pool. I had thus young formed a strong taste for collecting, chiefly seals, franks, etc., but also pebbles and minerals — one which was given me by some boy decided this taste. I believe shortly after this, or before, I had smattered in botany, and certainly when at Mr. Case's School I was very fond of gardening, and invented some great falsehoods about being able to colour crocuses as I liked. (Chapter I./4. The story is given in the "Life and Letters," I., page 28, the details being slightly different.) At this time I felt very strong friendship for some boys. It was soon after I began collecting stones, i.e., when 9 or 10, that I distinctly recollect the desire I had of being able to know something about every pebble in front of the hall door — it was my earliest and only geological aspiration at that time. I was in those days a very great story-teller — for the pure pleasure of exciting attention and surprise. I stole fruit and hid it for these same motives, and injured trees by barking them for similar ends. I scarcely ever went out walking without saying I had seen a pheasant or some strange bird (natural history taste); these lies, when not detected, I presume, excited my attention, as I recollect them vividly, not connected with shame, though some I do, but as something which by having produced a great effect on my mind, gave pleasure like a tragedy. I recollect when I was at Mr. Case's inventing a whole fabric to show how fond I was of speaking the TRUTH! My invention is still so vivid in my mind, that I could almost fancy it was true, did not memory of former shame tell me it was false. I have no particularly happy or unhappy recollections of this time or earlier periods of my life. I remember well a walk I took with a boy named Ford across some fields to a farmhouse on the Church Stretton road. I do not remember any mental pursuits excepting those of collecting stones, etc., gardening, and about this time often going with my father in his carriage, telling him of my lessons, and seeing game and other wild birds, which was a great delight to me. I was born a naturalist.

When I was 9 1/2 years old (July 1818) I went with Erasmus to see Liverpool: it has left no impressions on my mind, except most trifling ones — fear of the coach upsetting, a good dinner, and an extremely vague memory of ships.

In Midsummer of this year I went to Dr. Butler's School. (Chapter I./5. Darwin entered Dr. Butler's school in Shrewsbury in the summer of 1818, and remained there till 1825 ("Life and Letters," I., page 30).) I well recollect the first going there, which oddly enough I cannot of going to Mr. Case's, the first school of all. I remember the year 1818 well, not from having first gone to a public school, but from writing those figures in my school book, accompanied with obscure thoughts, now fulfilled, whether I should recollect in future life that year.

In September (1818) I was ill with the scarlet fever. I well remember the wretched feeling of being delirious.

1819, July (10 1/2 years old).

Went to the sea at Plas Edwards and stayed there three weeks, which now appears to me like three months. (Chapter I./6. Plas Edwards, at Towyn, on the Welsh coast.) I remember a certain shady green road (where I saw a snake) and a waterfall, with a degree of pleasure, which must be connected with the pleasure from scenery, though not directly recognised as such. The sandy plain before the house has left a strong impression, which is obscurely connected with an indistinct remembrance of curious insects, probably a Cimex mottled with red, and Zygaena, the burnet-moth. I was at that time very passionate (when I swore like a trooper) and quarrelsome. The former passion has I think nearly wholly but slowly died away. When journeying there by stage coach I remember a recruiting officer (I think I should know his face to this day) at tea time, asking the maid-servant for toasted bread and butter. I was convulsed with laughter and thought it the quaintest and wittiest speech that ever passed from the mouth of man. Such is wit at 10 1/2 years old. The memory now flashes across me of the pleasure I had in the evening on a blowy day walking along the beach by myself and seeing the gulls and cormorants wending their way home in a wild and irregular course. Such poetic pleasures, felt so keenly in after years, I should not have expected so early in life.

1820, July.

Went a riding tour (on old Dobbin) with Erasmus to Pistyll Rhiadr (Chapter I./7. Pistyll Rhiadr proceeds from Llyn Pen Rhiadr down the Llyfnant to the Dovey.); of this I recollect little, an indistinct picture of the fall, but I well remember my astonishment on hearing that fishes could jump up it.

(Chapter I./8. The autobiographical fragment here comes to an end. The next letters give some account of Darwin as an

Edinburgh student. He has described ("Life and Letters," I., pages 35-45) his failure to be interested in the official teaching of the University, his horror at the operating theatre, and his gradually increasing dislike of medical study, which finally determined his leaving Edinburgh, and entering Cambridge with a view to taking Orders.)

LETTER 1. TO R.W. DARWIN. Sunday Morning {Edinburgh, October, 1825}.

My dear Father

As I suppose Erasmus (Erasmus Darwin) has given all the particulars of the journey, I will say no more about it, except that altogether it has cost me 7 pounds. We got into our lodgings yesterday evening, which are very comfortable and near the College. Our Landlady, by name Mrs. Mackay, is a nice clean old body — exceedingly civil and attentive. She lives in "11, Lothian Street, Edinburgh" (1/1. In a letter printed in the "Edinburgh Evening Despatch" of May 22nd, 1888, the writer suggested that a tablet should be placed on the house, 11, Lothian Street. This suggestion was carried out in 1888 by Mr. Ralph Richardson (Clerk of the Commissary Court, Edinburgh), who obtained permission from the proprietors to affix a tablet to the house, setting forth that Charles Darwin resided there as an Edinburgh University student. We are indebted to Mr. W.K. Dickson for obtaining for us this information, and to Mr. Ralph Richardson for kindly supplying us with particulars. See Mr. Richardson's Inaugural Address, "Trans. Edinb. Geol. Soc." 1894-95; also

"Memorable Edinburgh Houses," by Wilmot Harrison, 1898.), and only four flights of steps from the ground-floor, which is very moderate to some other lodgings that we were nearly taking. The terms are 1 pound 6 shillings for two very nice and LIGHT bedrooms and a sitting-room; by the way, light bedrooms are very scarce articles in Edinburgh, since most of them are little holes in which there is neither air nor light. We called on Dr. Hanley the first morning, whom I think we never should have found, had it not been for a good-natured Dr. of Divinity who took us into his library and showed us a map, and gave us directions how to find him. Indeed, all the Scotchmen are so civil and attentive, that it is enough to make an Englishman ashamed of himself. I should think Dr. Butler or any other fat English Divine would take two utter strangers into his library and show them the way! When at last we found the Doctor, and having made all the proper speeches on both sides, we all three set out and walked all about the town, which we admire excessively; indeed Bridge Street is the most extraordinary thing I ever saw, and when we first looked over the sides, we could hardly believe our eyes, when instead of a fine river, we saw a stream of people. We spend all our mornings in promenading about the town, which we know pretty well, and in the evenings we go to the play to hear Miss Stephens (Probably Catherine Stephens), which is quite delightful; she is very popular here, being encored to such a degree, that she can hardly get on with the play. On Monday we are going to Der F (I do not know how to spell the rest of the word). (1/2. "Der F" is doubtless "Der Freischutz," which appeared in 1820, and of which a selection was given in London, under Weber's direction, in 1825. The last of Weber's compositions, "From Chindara's warbling fount," was written for Miss Stephens, who sang it to his accompaniment "the last time his fingers touched the key-board." (See "Dict. of Music," "Stephens" and "Weber.")) Before we got into our lodgings, we were staying at the Star Hotel in Princes St., where to my surprise I met with an old schoolfellow, whom I like very much; he is just come back from a walking tour in Switzerland and is now going to study for his {degree?} The introductory lectures begin next Wednesday, and we were matriculated for them on Saturday; we pay 10s., and write our names in a book, and the ceremony is finished; but the Library is not free to us till we get a ticket from a Professor. We just have been to Church and heard a sermon of only 20 minutes. I expected, from Sir Walter Scott's account, a soul-cutting discourse of 2 hours and a half.

I remain your affectionate son, C. DARWIN.

LETTER 2. TO CAROLINE DARWIN. January 6th, 1826. Edinburgh.

Many thanks for your very entertaining letter, which was a great relief after hearing a long stupid lecture from Duncan on Materia Medica, but as you know nothing either of the Lectures or Lecturers, I will give you a short account of them. Dr. Duncan is so very learned that his wisdom has left no room for his sense, and he lectures, as I have already said, on the Materia Medica, which cannot be translated into any word expressive enough of its stupidity. These few last mornings, however, he has shown signs of improvement, and I hope he will "go on as well as can be expected." His lectures begin at eight in the morning. Dr. Hope begins at ten o'clock, and I like both him and his lectures VERY much (after which Erasmus goes to "Mr. Sizars on Anatomy," who is a charming Lecturer). At 12 the Hospital, after which I attend Monro on Anatomy. I dislike him and his lectures so much, that I cannot speak with decency about them. Thrice a week we have what is called Clinical lectures, which means lectures on the sick people in the Hospital — these I like very much. I said this account should be short, but I am afraid it has been too long, like the lectures themselves.

I will be a good boy and tell something about Johnson again (not but what I am very much surprised that Papa should so forget himself as call me, a Collegian in the University of Edinburgh, a boy). He has changed his lodgings for the third time; he has got very cheap ones, but I am afraid it will not answer, for they must make up by cheating. I hope you like Erasmus' official news, he means to begin every letter so. You mentioned in your letter that Emma was staying with you: if she is not gone, ask her to tell Jos that I have not succeeded in getting any titanium, but that I will try again...I want to know how old I shall be next birthday — I believe 17, and if so, I shall be forced to go abroad for one year, since it is necessary that I shall have completed my 21st year before I take my degree. Now you have no business to be frowning and puzzling over this letter, for I did not promise to write a good hand to you.

LETTER 3. TO J.S. HENSLOW.

(3/1. Extracts from Darwin's letters to Henslow were read before the Cambridge Philosophical Society on November 16th, 1835. Some of the letters were subsequently printed, in an 8vo pamphlet of 31 pages, dated December 1st, 1835, for private distribution among the members of the Society. A German translation by W. Preyer appeared in the "Deutsche Rundschau," June 1891.)

{15th August, 1832. Monte Video.}

We are now beating up the Rio Plata, and I take the opportunity of beginning a letter to you. I did not send off the specimens from Rio Janeiro, as I grudged the time it would take to pack them up. They are now ready to be sent off and most probably go by this packet. If so they go to Falmouth (where Fitz-Roy has made arrangements) and so will not trouble your brother's agent in London. When I left England I was not fully aware how essential a kindness you offered me when you undertook to receive my boxes. I do not know what I should do without such head-quarters. And now for an apologetical prose about my collection: I am afraid you will say it is very small, but I have not been idle, and you must recollect what a very small show hundreds of species make. The box contains a good many geological specimens; I am well aware that the greater number are too small. But I maintain that no person has a right to accuse me, till he has tried carrying rocks under a tropical sun. I have endeavoured to get specimens of every variety of rock, and have written notes upon all. If you think it worth your while to examine any of them I shall be very glad of some mineralogical information, especially on any numbers between 1 and 254 which include Santiago rocks. By my catalogue I shall know which you may refer to. As for my plants, "pudet pigetque mihi." All I can say is that when objects are present which I can observe and particularise about, I cannot summon resolution to collect when I know nothing.

It is positively distressing to walk in the glorious forest amidst such treasures and feel they are all thrown away upon one. My collection from the Abrolhos is interesting, as I suspect it nearly contains the whole flowering vegetation — and indeed from extreme sterility the same may almost be said of Santiago. I have sent home four bottles with animals in spirits, I have three more, but would not send them till I had a fourth. I shall be anxious to hear how they fare. I made an enormous collection of Arachnidae at Rio, also a good many small beetles in pill boxes, but it is not the best time of year for the latter. Amongst the lower animals nothing has so much interested me as finding two species of elegantly coloured true Planaria inhabiting the dewy forest! The false relation they bear to snails is the most extraordinary thing of the kind I have ever seen. In the same genus (or more truly family) some of the marine species possess an organisation so marvellous that I can scarcely credit my eyesight. Every one has heard of the discoloured streaks of water in the equatorial regions. One I examined was owing to the presence of such minute Oscillariae that in each square inch of surface there must have been at least one hundred thousand present. After this I had better be silent, for you will think me a Baron Munchausen amongst naturalists. Most assuredly I might collect a far greater number of specimens of Invertebrate animals if I took less time over each; but I have come to the conclusion that two animals with their original colour and shape noted down will be more valuable to naturalists than six with only dates and place. I hope you will send me your criticisms about my collection; and it will be my endeavour that nothing you say shall be lost on me. I would send home my writings with my specimens, only I find I have so repeatedly occasion to refer back that it would be a serious loss to me. I cannot conclude about my collection without adding that I implicitly trust in your keeping an exact account against all the expense of boxes, etc., etc. At this present minute we are at anchor in the mouth of the river, and such a strange scene as it is. Everything is in flames — the sky with lightning, the water with luminous particles, and even the very masts are pointed with a blue flame. I expect great interest in scouring over the plains of Monte Video, yet I look back with regret to the Tropics, that magic lure to all naturalists. The delight of sitting on a decaying trunk amidst the quiet gloom of the forest is unspeakable and never to be forgotten. How often have I then wished for you. When I see a banana I well recollect admiring them with you in

Cambridge — little did I then think how soon I should eat their fruit.

August 15th. In a few days the box will go by the "Emulous" packet (Capt. Cooke) to Falmouth and will be forwarded to you. This letter goes the same way, so that if in course of due time you do not receive the box, will you be kind enough to write to Falmouth? We have been here (Monte Video) for some time; but owing to bad weather and continual fighting on shore, we have scarcely ever been able to walk in the country. I have collected during the last month nothing, but to-day I have been out and returned like Noah's Ark with animals of all sorts. I have to-day to my astonishment found two Planariae living under dry stones: ask L. Jenyns if he has ever heard of this fact. I also found a most curious snail, and spiders, beetles, snakes, scorpions ad libitum, and to conclude shot a Cavia weighing a cwt. — On Friday we sail for the Rio Negro, and then will commence our real wild work. I look forward with dread to the wet stormy regions of the south, but after so much pleasure I must put up with some sea-sickness and misery.

LETTER 4. TO J.S. HENSLOW. Monte Video, 24th November 1832.

We arrived here on the 24th of October, after our first cruise on the coast of Patagonia. North of the Rio Negro we fell in with some little schooners employed in sealing: to save the loss of time in surveying the intricate mass of banks, Capt. Fitz-Roy has hired two of them and has put officers on them. It took us nearly a month fitting them out; as soon as this was finished we came back here, and are now preparing for a long cruise to the south. I expect to find the wild mountainous country of Terra del Fuego very interesting, and after the coast of Patagonia I shall thoroughly enjoy it. — I had hoped for the credit of Dame Nature, no such country as this last existed; in sad reality we coasted along 240 miles of sand hillocks; I never knew before, what a horrid ugly object a sand hillock is. The famed country of the Rio Plata in my opinion is not much better: an enormous brackish river, bounded by an interminable green plain is enough to make any naturalist groan. So Hurrah for Cape Horn and the Land of Storms. Now that I have had my growl out, which is a privilege sailors take on all occasions, I will turn the tables and give an account of my doing in Nat. History. I must have one more growl: by ill luck the French Government has sent one of its collectors to the Rio Negro, where he has been working for the last six months, and is now gone round the Horn. So that I am very selfishly afraid he will get the cream of all the good things before me. As I have nobody to talk to about my luck and ill luck in collecting, I am determined to vent it all upon you. I have been very lucky with fossil bones; I have fragments of at least 6 distinct animals: as many of them are teeth, I trust, shattered and rolled as they have been, they will be recognised. I have paid all the attention I am capable of to their geological site; but of course it is too long a story for here. 1st, I have the tarsi and metatarsi very perfect of a Cavia; 2nd, the upper jaw and head of some very large animal with four square hollow molars and the head greatly protruded in front. I at first thought it belonged either to the Megalonyx or Megatherium (4/1). The animal may probably have been Grypotherium Darwini, Ow. The osseous plates mentioned below must have belonged to one of the Glyptodontidae, and not to Megatherium. We are indebted to Mr. Kerr for calling our attention to a passage in Buckland's "Bridgewater Treatise" (Volume II., page 20, note), where bony armour is ascribed to Megatherium.); in confirmation of this in the same formation I found a large surface of the osseous polygonal plates, which "late observations" (what are they?) show belong to the Megatherium. Immediately I saw this I thought they must belong to an enormous armadillo, living species of which genus are so abundant here. 3rd, The lower jaw of some large animal which, from the molar teeth, I should think belonged to the Edentata; 4th, some large molar teeth which in some respects would seem to belong to an enormous rodent; 5th, also some smaller teeth belonging to the same order. If it interests you sufficiently to unpack them, I shall be very curious to hear something about them. Care must be taken in this case not to confuse the tallies. They are mingled with marine shells which appear to me identical with what now exist. But since they were deposited in their beds several geological changes have taken place in the country. So much for the dead, and now for the living: there is a poor specimen of a bird which to my unornithological eyes appears to be a happy mixture of a lark, pigeon and snipe (No. 710). Mr. MacLeay himself never imagined such an inosculating creature: I suppose it will turn out to be some well-known bird, although it has quite baffled me. I have taken some interesting Amphibia; a new Trigonocephalus beautifully connecting in its habits Crotalus and the Viperidae, and plenty of new (as far as my knowledge goes) saurians. As for one little toad, I hope it may be new, that it may be christened "diabolicus." Milton must allude to this very individual when he talks of "squat like a toad" (4/2. "...him {Satan} there they {Ithuriel and Zephon} found, Squat like a toad, close at the ear of Eve" ("Paradise Lost," Book IV., line 800).

"Formerly Milton's "Paradise Lost" had been my chief favourite, and in my excursions during the voyage of the 'Beagle,' when I could take only a single volume, I always chose Milton" ("Autobiography," page 69).); its colours are by Werner (4/3. Werner's "Nomenclature of Colours," Edinburgh, 1821.) ink black, vermilion red and buff orange. It has been a splendid cruise for me in Nat. History. Amongst the Pelagic Crustacea, some new and curious genera. In the Zoophytes some interesting animals. As for one Flustra, if I had not the specimen to back me up nobody would believe in its most anomalous structure. But as for novelty all this is nothing to a family of pelagic animals which at first sight appear like Medusae but are really highly organised. I have examined them repeatedly, and certainly from their structure it would be impossible to place them in any existing order. Perhaps Salpa is the nearest animal, although the transparency of the body is nearly the only character they have in common. I think the dried plants nearly contain all which were then (Bahia Blanca) flowering. All the specimens will be packed in casks. I think there will be three (before sending this letter I will specify dates, etc., etc.). I am afraid you will groan or rather the floor of the lecture room will when the casks arrive. Without you I should be utterly undone. The small cask contains fish: will you open it to see how the spirit has stood the evaporation of the Tropics. On board the ship everything goes on as well as possible; the only drawback is the fearful length of time between this and the day of our return. I do not see any limits to it. One year is nearly completed and the second will be so, before we even leave the east coast of S. America. And then our voyage may be said really to have commenced. I know not how I shall be able to endure it. The frequency with which I think of all the happy hours I have spent at Shrewsbury and Cambridge is rather ominous — I trust everything to time and fate and will feel my way as I go on.

November 24th. — We have been at Buenos Ayres for a week; it is a fine large city, but such a country, everything is mud, you can go nowhere, you can do nothing for mud. In the city I obtained much information about the banks of the Uruguay — I hear of limestone with shells, and beds of shells in every direction. I hope when we winter in the Plata to have a most interesting geological excursion into that country: I purchased fragments (Nos. 837-8) of some enormous bones, which I was assured belonged to the former giants!! I also procured some seeds — I do not know whether they are worth your accepting; if you think so I will get some more. They are in the box. I have sent to you by the "Duke of York" packet, commanded by Lieut. Snell, to Falmouth two large casks containing fossil bones, a small cask with fish and a box containing skins, spirit bottle, etc., and pill-boxes with beetles. Would you be kind enough to open these latter as they are apt to become mouldy. With the exception of the bones the rest of my collection looks very scanty. Recollect how great a proportion of time is spent at sea. I am always anxious to hear in what state the things come and any criticisms about quantity or kind of specimens. In the smaller cask is part of a large head, the anterior portions of which are in the other large one. The packet has arrived and I am in a great bustle. You will not hear from me for some months.

LETTER 5. TO J.S. HENSLOW. Valparaiso, July 24th 1834.

A box has just arrived in which were two of your most kind and affectionate letters. You do not know how happy they have made me. One is dated December 15th, 1833, the other January 15th of the same year! By what fatality it did not arrive sooner I cannot conjecture; I regret it much, for it contains the information I most wanted, about manner of packing, etc., etc.: roots with specimens of plants, etc., etc. This I suppose was written after the reception of my first cargo of specimens. Not having heard from you until March of this year I really began to think that my collections were so poor, that you were puzzled what to say; the case is now quite on the opposite tack; for you are guilty of exciting all my vain feelings to a most comfortable pitch; if hard work will atone for these thoughts, I vow it shall not be spared. It is rather late, but I will allude to some remarks in the January letter; you advise me to send home duplicates of my notes; I have been aware of the advantage of doing so; but then at sea to this day, I am invariably sick, excepting on the finest days, at which times with pelagic animals around me, I could never bring myself to the task — on shore the most prudent person could hardly expect such a sacrifice of time. My notes are becoming bulky. I have about 600 small quarto pages full; about half of this is Geology — the other imperfect descriptions of animals; with the latter I make it a rule only to describe those parts or facts, which cannot be seen in specimens in spirits. I keep my private Journal distinct from the above. (N.B. this letter is a most untidy one, but my mind is untidy with joy; it is your fault, so you must take the consequences.) With respect to the land Planariae, unquestionably they are not molluscous animals. I read your letters last night, this morning I took a little walk; by a curious coincidence, I found a new white species of Planaria, and a new to me Vaginulus (third species which I have found in S. America) of Cuvier. Amongst the marine mollusques I have seen a good many genera, and at Rio found one quite new one. With respect to the December letter, I am very glad to hear the four casks arrived safe; since which time you have received another cargo, with the bird skins about which you did not understand me. Have any of the B. Ayrean seeds produced plants? From the Falklands I acknowledged a box and letter from you; with the letter were a few seeds from Patagonia. At present I have specimens enough to make a heavy cargo, but shall wait as much longer as possible, because opportunities are not now so good as before. I have just got scent of some fossil bones of a MAMMOTH; what they may be I do not know, but if gold or galloping will get them they shall be mine. You tell me you like hearing how I am going on and what doing, and you well may imagine how much I enjoy speaking to any one upon subjects which I am always thinking about, but never have any one to talk to {about}. After leaving the Falklands we proceeded to the Rio S. Cruz, following up the river till within twenty miles of the Cordilleras. Unfortunately want of provisions compelled us to return. This expedition was most important to me as it was a transverse section of the great Patagonian formation. I conjecture (an accurate examination of fossils may possibly determine the point) that the main bed is somewhere about the Miocene period (using Mr. Lyell's expression); I judge from what I have seen of the present shells of Patagonia. This bed contains an ENORMOUS field of lava. This is of some interest, as being a rude approximation to the age of the volcanic part of the great range of the Andes. Long before this it existed as a slate and porphyritic line of hills. I have collected a tolerable quantity of information respecting the period and forms of elevations of these plains. I think these will be interesting to Mr. Lyell; I had deferred reading his third volume till my return: you may guess how much pleasure it gave me; some of his woodcuts came so exactly into play that I have only to refer to them instead of redrawing similar ones. I had my barometer with me, I only wish I had used it more in these plains. The valley of S. Cruz appears to me a very curious one; at first it quite baffled me. I believe I can show good reasons for supposing it to have been once a northern straits like to that of Magellan. When I return to England you will have some hard work in winnowing my Geology; what little I know I have learnt in such a curious fashion that I often feel very doubtful about the number of grains {of value?}. Whatever number they may turn out, I have enjoyed extreme pleasure in collecting them. In T. del Fuego I collected and examined some corallines; I have observed one fact which quite startled me: it is that in the genus Sertularia (taken in its most restricted form as {used} by Lamoureux) and in two species which, excluding comparative expressions, I should find much difficulty in describing as different, the polypi quite and essentially differed in all their most important and evident parts of structure. I have already seen enough to be convinced that the present families of corallines as arranged by Lamarck, Cuvier, etc., are highly artificial. It appears that they are in the same state {in} which shells were when Linnaeus left them for Cuvier to rearrange. I do so wish I was a better hand at dissecting, I find I can do very little in the minute parts of structure; I am forced to take a very rough examination as a type for different classes of structure. It is most extraordinary I can nowhere see in my books one single description of the polypus of any one coralline excepting Alcyonium Lobularia of Savigny. I found a curious little stony Cellaria (5/1. Cellaria, a genus of Bryozoa, placed in the section Flustrina of the Suborder Chilostomata.) (a new genus) each cell provided with long toothed bristle, these are capable of various and rapid motions. This motion is often simultaneous, and can be produced by irritation. This fact, as far as I can see, is quite isolated in the history of zoophytes (excepting the Flustra with an organ like a vulture's head); it points out a much more intimate relation between the polypi than Lamarck is willing to allow. I forgot whether I mentioned having seen something of the manner of propagation in that most ambiguous family, the corallines; I feel pretty well convinced if they are not plants they are not zoophytes. The "gemmule" of a Halimeda contained several articulations united, ready to burst their envelope, and become attached to some basis. I believe in zoophytes universally the gemmule produces a single polypus, which afterwards or at the same time grows with its cell or single articulation.

The "Beagle" left the Sts. of Magellan in the middle of winter; she found her road out by a wild unfrequented channel; well might Sir J. Narborough call the west coast South Desolation, "because it is so desolate a land to behold." We were driven into Chiloe by some very bad weather. An Englishman gave me three specimens of that very fine Lucanoidal insect which is described in the "Camb. Phil. Trans." (5/2. "Description of Chiasognathus Grantii, a new Lucanideous Insect, etc." by J.F. Stephens ("Trans. Camb. Phil. Soc." Volume IV., page 209, 1833.)), two males and one female. I find Chiloe is composed of lava and recent deposits. The lavas are curious from abounding in, or rather being in parts composed of pitchstone. If we go to Chiloe in the summer, I shall reap an entomological harvest. I suppose the Botany both there and in Chili is well-known.

I forgot to state that in the four cargoes of specimens there have been sent three square boxes, each containing four glass bottles. I mention this in case they should be stowed beneath geological specimens and thus escape your notice, perhaps some spirit may be wanted in them. If a box arrives from B. Ayres with a Megatherium head the other unnumbered specimens, be kind enough to tell me, as I have strong fears for its safety. We arrived here the day before yesterday; the views of the distant mountains are most sublime and the climate delightful; after our long cruise in the damp gloomy climates of the south, to breathe a clear dry air and feel honest warm sunshine, and eat good fresh roast beef must be the summum bonum of human life. I do not like the look of the rocks half so much as the beef, there is too much of those rather insipid ingredients, mica, quartz and feldspar. Our plans are at present undecided; there is a good deal of work to the south of Valparaiso and to the north an indefinite quantity. I look forward to every part with interest. I have sent you in this letter a sad dose of egotism, but recollect I look up to you as my father in

Natural History, and a son may talk about himself to his father. In your paternal capacity as proproctor what a great deal of trouble you appear to have had. How turbulent Cambridge is become. Before this time it will have regained its tranquillity. I have a most schoolboy-like wish to be there, enjoying my holidays. It is a most comfortable reflection to me, that a ship being made of wood and iron, cannot last for ever, and therefore this voyage must have an end.

October 28th. This letter has been lying in my portfolio ever since July; I did not send it away because I did not think it worth the postage; it shall now go with a box of specimens. Shortly after arriving here I set out on a geological excursion, and had a very pleasant ramble about the base of the Andes. The whole country appears composed of breccias (and I imagine slates) which universally have been modified and oftentimes completely altered by the action of fire. The varieties of porphyry thus produced are endless, but nowhere have I yet met with rocks which have flowed in a stream; dykes of greenstone are very numerous. Modern volcanic action is entirely shut up in the very central parts (which cannot now be reached on account of the snow) of the Cordilleras. In the south of the R. Maypu I examined the Tertiary plains, already partially described by M. Gay. (5/3. "Rapport fait a l'Academie Royale des Sciences, sur les Travaux Geologiques de M. Gay," by Alex. Brongniart ("Ann. Sci. Nat." Volume XXVIII., page 394, 1833.) The fossil shells appear to me to be far more different from the recent ones than in the great Patagonian formation; it will be curious if an Eocene and Miocene (recent there is abundance of) could be proved to exist in S. America as well as in Europe. I have been much interested by finding abundance of recent shells at an elevation of 1,300 feet; the country in many places is scattered over with shells but these are all littoral ones. So that I suppose the 1,300 feet elevation must be owing to a succession of small elevations such as in 1822. With these certain proofs of the recent residence of the ocean over all the lower parts of Chili, the outline of every view and the form of each valley possesses a high interest. Has the action of running water or the sea formed this deep ravine? was a question which often arose in my mind and generally was answered by finding a bed of recent shells at the bottom. I have not sufficient arguments, but I do not believe that more than a small fraction of the height of the Andes has been formed within the Tertiary period. The conclusion of my excursion was very unfortunate, I became unwell and could hardly reach this place. I have been in bed for the last month, but am now rapidly getting well. I had hoped during this time to have made a good collection of insects but it has been impossible: I regret the less because Chiloe fairly swarms with collectors; there are more naturalists in the country, than carpenters or shoemakers or any other honest trade.

In my letter from the Falkland Islands I said I had fears about a box with a Megatherium. I have since heard from B. Ayres that it went to Liverpool by the brig "Basingwaithe." If you have not received it, it is I think worth taking some trouble about. In October two casks and a jar were sent by H.M.S. "Samarang" via Portsmouth. I have no doubt you have received them. With this letter I send a good many bird skins; in the same box with them, there is a paper parcel containing pill boxes with insects. The other pill boxes require no particular care. You will see in two of these boxes some dried Planariae (terrestrial), the only method I have found of preserving them (they are exceedingly brittle). By examining the white species I understand some little of the internal structure. There are two small parcels of seeds. There are some plants which I hope may interest you, or at least those from Patagonia where I collected every one in flower. There is a bottle clumsily but I think securely corked containing water and gas from the hot baths of Cauquenes seated at foot of Andes and long celebrated for medicinal properties. I took pains in filling and securing both water and gas. If you can find any one who likes to analyze them, I should think it would be worth the trouble. I have not time at present to copy my few observations about the locality, etc., etc., {of} these springs. Will you tell me how the Arachnidae which I have sent home, for instance those from Rio, appear to be preserved. I have doubts whether it is worth while collecting them.

We sail the day after to-morrow: our plans are at last limited and definite; I am delighted to say we have bid an eternal adieu to T. del Fuego. The "Beagle" will not proceed further south than C. Tres Montes; from which point we survey to the north. The

Chonos Archipelago is delightfully unknown: fine deep inlets running into the Cordilleras — where we can steer by the light of a volcano. I do not know which part of the voyage now offers the most attractions. This is a shamefully untidy letter, but you must forgive me.

LETTER 6. TO J.S. HENSLOW. April 18th, 1835. Valparaiso.

I have just returned from Mendoza, having crossed the Cordilleras by two passes. This trip has added much to my knowledge of the geology of the country. Some of the facts, of the truth of which I in my own mind feel fully convinced, will appear to you quite absurd and incredible. I will give a very short sketch of the structure of these huge mountains. In the Portillo pass (the more southern one) travellers have described the Cordilleras to consist of a double chain of nearly equal altitude separated by a considerable interval. This is the case; and the same structure extends to the northward to Uspallata; the little elevation of the eastern line (here not more than 6,000-7,000 feet.) has caused it almost to be overlooked. To begin with the western and principal chain, we have, where the sections are best seen, an enormous mass of a porphyritic conglomerate resting on granite. This latter rock seems to form the nucleus of the whole mass, and is seen in the deep lateral valleys, injected amongst, upheaving, overturning in the most extraordinary manner, the overlying strata. The stratification in all the mountains is beautifully distinct and from a variety in the colour can be seen at great distances. I cannot imagine any part of the world presenting a more extraordinary scene of the breaking up of the crust of the globe than the very central parts of the Andes. The upheaval has taken place by a great number of (nearly) N. and S. lines; which in most cases have formed as many anticlinal and synclinal ravines; the strata in the highest pinnacles are almost universally inclined at an angle from 70 deg to 80 deg. I cannot tell you how I enjoyed some of these views — it is worth coming from England, once to feel such intense delight; at an elevation from 10 to 12,000 feet there is a transparency in the air, and a confusion of distances and a sort of stillness which gives the sensation of being in another world, and when to this is joined the picture so plainly drawn of the great epochs of violence, it causes in the mind a most strange assemblage of ideas.

The formation I call Porphyritic Conglomerates is the most important and most developed one in Chili: from a great number of sections I find it a true coarse conglomerate or breccia, which by every step in a slow gradation passes into a fine claystone-porphyry; the pebbles and cement becoming porphyritic till at last all is blended in one compact rock. The porphyries are excessively abundant in this chain. I feel sure at least 4/5ths of them have been thus produced from sedimentary beds in situ. There are porphyries which have been injected from below amongst strata, and others ejected, which have flowed in streams; it is remarkable, and I could show specimens of this rock produced in these three methods, which cannot be distinguished. It is a great mistake considering the Cordilleras here as composed of rocks which have flowed in streams. In this range I nowhere saw a fragment, which I believe to have thus originated, although the road passes at no great distance from the active volcanoes. The porphyries, conglomerate, sandstone and quartzose sandstone and limestones alternate and pass into each other many times, overlying (where not broken through by the granite) clay-slate. In the upper parts, the sandstone begins to alternate with gypsum, till at last we have this substance of a stupendous thickness. I really think the formation is in some places (it varies much) nearly 2,000 feet thick, it occurs often with a green (epidote?) siliceous sandstone and snow-white marble; it resembles that found in the Alps in containing large concretions of a crystalline marble of a blackish grey colour. The upper beds which form some of the higher pinnacles consist of layers of snow-white gypsum and red compact sandstone, from the thickness of paper to a few feet, alternating in an endless round. The rock has a most curiously painted appearance. At the pass of the Peuquenes in this formation, where however a black rock like clay-slate, without many laminae, occurring with a pale limestone, has replaced the red sandstone, I found abundant impressions of shells. The elevation must be between 12 and 13,000 feet. A shell which I believe is the Gryphaea is the most abundant — an Ostrea, Turratella, Ammonites, small bivalves, Terebratulae (?). Perhaps some good conchologist (6/1. Some of these genera are mentioned by Darwin ("Geol. Obs." page

181) as having been named for him by M. D'Orbigny.) will be able to give a guess, to what grand division of the formations of Europe these organic remains bear most resemblance. They are exceedingly imperfect and few. It was late in the season and the situation particularly dangerous for snow-storms. I did not dare to delay, otherwise a grand harvest might have been reaped. So much for the western line; in the Portillo pass, proceeding eastward, we meet an immense mass of conglomerate, dipping to the west 45 deg, which rest on micaceous sandstone, etc., etc., upheaved and converted into quartz-rock penetrated by dykes from the very grand mass of protogine (large crystals of quartz, red feldspar, and occasional little chlorite). Now this conglomerate which reposes on and dips from the protogene 45 deg consists of the peculiar rocks of the first described chain, pebbles of the black rock with shells, green sandstone, etc., etc. It is hence manifest that the upheaval (and deposition at least of part) of the grand eastern chain is entirely posterior to the western. To the north in the Uspallata pass, we have also a fact of the same class. Bear this in mind: it will help to make you believe what follows. I have said the Uspallata range is geologically, although only 6,000-7,000 feet, a continuation of the grand eastern chain. It has its nucleus of granite, consists of grand beds of various crystalline rocks, which I can feel no doubt are subaqueous lavas alternating with sandstone, conglomerates and white aluminous beds (like decomposed feldspar) with many other curious varieties of sedimentary deposits. These lavas and

sandstones alterate very many times, and are quite conformable one to the other. During two days of careful examination I said to myself at least fifty times, how exactly like (only rather harder) these beds are to those of the upper Tertiary strata of Patagonia, Chiloe and Concepcion, without the possible identity ever having occurred to me. At last there was no resisting the conclusion. I could not expect shells, for they never occur in this formation; but lignite or carbonaceous shale ought to be found. I had previously been exceedingly puzzled by meeting in the sandstone, thin layers (few inches to feet thick) of a brecciated pitchstone. I strongly suspect the underlying granite has altered such beds into this pitchstone. The silicified wood (particularly characteristic) was yet absent. The conviction that I was on the Tertiary strata was so strong by this time in my mind, that on the third day in the midst of lavas and {? masses} of granite I began my apparently forlorn hunt. How do you think I succeeded? In an escarpement of compact greenish sandstone, I found a small wood of petrified trees in a vertical position, or rather the strata were inclined about 20-30 deg to one point and the trees 70 deg to the opposite one. That is, they were before the tilt truly vertical. The sandstone consists of many layers, and is marked by the concentric lines of the bark (I have specimens); 11 are perfectly silicified and resemble the dicotyledonous wood which I have found at Chiloe and Concepcion (6/2. "Geol. Obs." page 202. Specimens of the silicified wood were examined by Robert Brown, and determined by him as coniferous, "partaking of the characters of the Araucarian tribe, with some curious points of affinity with the yew."); the others (30-40) I only know to be trees from the analogy of form and position; they consist of snow-white columns (like Lot's wife) of coarsely crystalline carb. of lime. The largest shaft is 7 feet. They are all close together, within 100 yards, and about the same level: nowhere else could I find any. It cannot be doubted that the layers of fine sandstone have quietly been deposited between a clump of trees which were fixed by their roots. The sandstone rests on lava, is covered by a great bed apparently about 1,000 feet thick of black augitic lava, and over this there are at least 5 grand alternations of such rocks and aqueous sedimentary deposits, amounting in thickness to several thousand feet. I am quite afraid of the only conclusion which I can draw from this fact, namely that there must have been a depression in the surface of the land to that amount. But neglecting this consideration, it was a most satisfactory support of my presumption of the Tertiary (I mean by Tertiary, that the shells of the period were closely allied, or some identical, to those which now live, as in the lower beds of Patagonia) age of this eastern chain. A great part of the proof must remain upon my ipse dixit of a mineralogical resemblance with those beds whose age is known, and the character of which resemblance is to be subject to infinite variation, passing from one variety to another by a concretionary structure. I hardly expect you to believe me, when it is a consequence of this view that granite, which forms peaks of a height probably of 14,000 feet, has been fluid in the

Tertiary period; that strata of that period are altered by its heat, and are traversed by dykes from the mass. That these strata have also probably undergone an immense depression, that they are now inclined at high angles and form regular or complicated anticlinal lines. To complete the climax and seal your disbelief, these same sedimentary strata and lavas are traversed by VERY NUMEROUS, true metallic veins of iron, copper, arsenic, silver and gold, and these can be traced to the underlying granite. A gold mine has been worked close to the clump of silicified trees. If when you see my specimens, sections and account, you should think that there is pretty strong presumptive evidence of the above facts, it appears very important; for the structure, and size of this chain will bear comparison with any in the world, and that this all should have been produced in so very recent a period is indeed wonderful. In my own mind I am quite convinced of the reality of this. I can anyhow most conscientiously say that no previously formed conjecture warped my judgment. As I have described so did I actually observe the facts. But I will have some mercy and end this most lengthy account of my geological trip.

On some of the large patches of perpetual snow, I found the famous red snow of the Arctic countries; I send with this letter my observations and a piece of paper on which I tried to dry some specimens. If the fact is new and you think it worth while, either yourself examine them or send them to whoever has described the specimens from the north and publish a notice in any of the periodicals. I also send a small bottle with two lizards, one of them is viviparous as you will see by the accompanying notice. A M. Gay — a French naturalist — has already published in one of the newspapers of this country a similar statement and probably has forwarded to Paris some account; as the fact appears singular would it not be worth while to hand over the specimens to some good lizardologist and comparative anatomist to publish an account of their internal structure? Do what you think fit.

This letter will go with a cargo of specimens from Coquimbo. I shall write to let you know when they are sent off. In the box there are two bags of seeds, one {from the} valleys of the Cordilleras 5,000-10,000 feet high, the soil and climate exceedingly dry, soil very light and stony, extremes in temperature; the other chiefly from the dry sandy Traversia of Mendoza 3,000 feet more or less. If some of the bushes should grow but not be healthy, try a slight sprinkling of salt and saltpetre. The plain is saliferous. All the flowers in the Cordilleras appear to be autumnal flowerers — they were all in blow and seed, many of them very pretty. I gathered them as I rode along on the hill sides. If they will but choose to come up, I have no doubt many would be great rarities. In the Mendoza bag there are the seeds or berries of what appears to be a small potato plant with a whitish flower. They grow many leagues from where any habitation could ever have existed owing to absence of water. Amongst the Chonos dried plants, you will see a fine specimen of the wild potato, growing under a most opposite climate, and unquestionably a true wild potato. It must be a distinct species from that of the Lower Cordilleras one. Perhaps as with the banana, distinct species are now not to be distinguished in their varieties produced by cultivation. I cannot copy out the few remarks about the Chonos potato. With the specimens there is a bundle of old papers and notebooks. Will you take care of them; in case I should lose my notes, these might be useful. I do not send home any insects because they must be troublesome to you, and now so little more of the voyage remains unfinished I can well take charge of them. In two or three days I set out for Coquimbo by land; the "Beagle" calls for me in the beginning of June. So that I have six weeks more to enjoy geologising over these curious mountains of Chili. There is at present a bloody revolution in Peru. The Commodore has gone there, and in the hurry has carried our letters with him; perhaps amongst them there will be one from you. I wish I had the old Commodore here, I would shake some consideration for others into his old body. From Coquimbo you will again hear from me.

LETTER 7. TO J.S. HENSLOW. Lima, July 12th, 1835.

This is the last letter which I shall ever write to you from the shores of America, and for this reason I send it. In a few days time the "Beagle" will sail for the Galapagos Islands. I look forward with joy and interest to this, both as being somewhat nearer to England and for the sake of having a good look at an active volcano. Although we have seen lava in abundance, I have never yet beheld the crater. I sent by H.M.S. "Conway" two large boxes of specimens. The "Conway" sailed the latter end of June. With them were letters for you, since that time I have travelled by land from Valparaiso to Copiapo and seen something more of the Cordilleras. Some of my geological views have been, subsequently to the last letter, altered. I believe the upper mass of strata is not so very modern as I supposed. This last journey has explained to me much of the ancient history of the Cordilleras. I feel sure they formerly consisted of a chain of volcanoes from which enormous streams of lava were poured forth at the bottom of the sea. These alternate with sedimentary beds to a vast thickness; at a subsequent period these volcanoes must have formed islands, from which have been produced strata of several thousand feet thick of coarse conglomerate. (7/1. See "Geological Observations on South America" (London, 1846), Chapter VII.: "Central Chile; Structure of the Cordillera.") These islands were covered with fine trees; in the conglomerate, I found one 15 feet in circumference perfectly silicified to the very centre. The alternations of compact crystalline rocks (I cannot doubt subaqueous lavas), and sedimentary beds, now upheaved fractured and indurated, form the main range of the Andes. The formation was produced at the time when ammonites, gryphites, oysters, Pecten, Mytilus, etc., etc., lived. In the central parts of Chili the structure of the lower beds is rendered very obscure by the metamorphic action which has rendered even the coarsest conglomerates porphyritic. The Cordilleras of the Andes so worthy of admiration from the grandeur of their dimensions, rise in dignity when it is considered that since the period of ammonites, they have formed a marked feature in the geography of the globe. The geology of these mountains pleased me in one respect; when reading Lyell, it had always struck me that if the crust of the world goes on changing in a circle, there ought to be somewhere found formations which, having the age of the great European Secondary beds, should possess the structure of Tertiary rocks or those formed amidst islands and in limited basins. Now the alternations of lava and coarse sediment which form the upper parts of the Andes, correspond exactly to what would accumulate under such circumstances. In consequence of this, I can only very roughly separate into three divisions the varying strata (perhaps 8,000 feet thick) which compose these mountains. I am afraid you will tell me to learn my ABC to know quartz from feldspar before I indulge in such speculations. I lately got hold of a report on M. Dessalines D'Orbigny's labours in S. America (7/2. "Voyage dans l'Amerique Meridionale, etc." (A. Dessalines D'Orbigny).); I experienced rather a debasing degree of vexation to find he has described the Geology of the Pampas, and that I have had some hard riding for nothing, it was however gratifying that my conclusions are the same, as far as I can collect, with his results. It is also capital that the whole of Bolivia will be described. I hope to be able to connect his geology of that country with mine of Chili. After leaving Copiapo, we touched at Iquique. I visited but do not quite understand the position of the nitrate of soda beds. Here in Peru, from the state of anarchy, I can make no expedition.

I hear from home, that my brother is going to send me a box with books, and a letter from you. It is very unfortunate that I cannot receive this before we reach Sydney, even if it ever gets safely so far. I shall not have another opportunity for many months of again writing to you. Will you have the charity to send me one more letter (as soon as this reaches you) directed to the C. of Good Hope. Your letters besides affording me the greatest delight always give me a fresh stimulus for exertion. Excuse this geological prosy letter, and farewell till you hear from me at Sydney, and see me in the autumn of 1836.

LETTER 8. TO JOSIAH WEDGWOOD. {Shrewsbury, October 5th, 1836.}

My dear Uncle

The "Beagle" arrived at Falmouth on Sunday evening, and I reached home late last night. My head is quite confused with so much delight, but I cannot allow my sisters to tell you first how happy I am to see all my dear friends again. I am obliged to return in three or four days to London, where the "Beagle" will be paid off, and then I shall pay Shrewsbury a longer visit. I am most anxious once again to see Maer, and all its inhabitants, so that in the course of two or three weeks, I hope in person to thank you, as being my first Lord of the Admiralty. (8/1.) Readers of the "Life and Letters" will remember that it was to Josiah Wedgwood that Darwin owed the great opportunity of his life ("Life and Letters," Volume I., page 59), and it was fitting that he should report himself to his "first Lord of the Admiralty." The present letter clears up a small obscurity to which Mr. Poulton has called attention ("Charles Darwin and the Theory of Natural Selection," "Century" Series, 1896, page 25). Writing to Fitz-Roy from Shrewsbury on October 6th, Darwin says, "I arrived here yesterday morning at breakfast time." This refers to his arrival at his father's house, after having slept at the inn. The date of his arrival in Shrewsbury was, therefore, October 4th, as given in the "Life and Letters," I., page 272.) The entries in his Diary are: — October 2, 1831. Took leave of my home. October 4, 1836. Reached Shrewsbury after absence of 5 years and 2 days.) I am so very happy I hardly know what I am writing. Believe me your most affectionate nephew,

CHAS. DARWIN.

LETTER 9. TO C. LYELL. Shrewsbury, Monday {November 12th, 1838}.

My dear Lyell

I suppose you will be in Hart St. (9/1. Sir Charles Lyell lived at 16, Hart Street, Bloomsbury.) to-morrow {or} the 14th. I write because I cannot avoid wishing to be the first person to tell Mrs. Lyell and yourself, that I have the very good, and shortly since {i.e. until lately} very unexpected fortune of going to be married! The lady is my cousin Miss Emma Wedgwood, the sister of Hensleigh Wedgwood, and of the elder brother who married my sister, so we are connected by manifold ties, besides on my part, by the most sincere love and hearty gratitude to her for accepting such a one as myself.

I determined when last at Maer to try my chance, but I hardly expected such good fortune would turn up for me. I shall be in town in the middle or latter end of the ensuing week. (9/2. Mr. Darwin was married on January 29th, 1839 (see "Life and Letters," I., page 299). The present letter was written the day after he had become engaged.) I fear you will say I might very well have left my story untold till we met. But I deeply feel your kindness and friendship towards me, which in truth I may say, has been one chief source of happiness to me, ever since my return to England: so you must excuse me. I am well sure that Mrs. Lyell, who has sympathy for every one near her, will give me her hearty congratulations.

Believe me my dear Lyell Yours most truly obliged CHAS. DARWIN.

(PLATE: MRS. DARWIN. Walker and Cockerell, ph. sc.)

LETTER 10. TO EMMA WEDGWOOD. Sunday Night. Athenaeum. {January 20th, 1839.}

...I cannot tell you how much I enjoyed my Maer visit, — I felt in anticipation my future tranquil life: how I do hope you may be as happy as I know I shall be: but it frightens me, as often as I think of what a family you have been one of. I was thinking this morning how it came, that I, who am fond of talking and am scarcely ever out of spirits, should so entirely rest my notions of happiness on quietness, and a good deal of solitude: but I believe the explanation is very simple and I mention it because it will give you hopes, that I shall gradually grow less of a brute, it is that during the five years of my voyage (and indeed I may add these two last) which from the active manner in which they have been passed, may be said to be the commencement of my real life, the whole of my pleasure was derived from what passed in my mind, while admiring views by myself, travelling across the wild deserts or glorious forests or pacing the deck of the poor little "Beagle" at night. Excuse this much egotism, — I give it you because I think you will humanize me, and soon teach me there is greater happiness than building theories and accumulating facts in silence and solitude. My own dearest Emma, I earnestly pray, you may never regret the great, and I will add very good, deed, you are to perform on the Tuesday: my own dear future wife, God bless you...The Lyells called on me to-day after church; as Lyell was so full of geology he was obliged to disgorge, — and I dine there on Tuesday for an especial confidence. I was quite ashamed of myself to-day, for we talked for half an hour, unsophisticated geology, with poor Mrs. Lyell sitting by, a monument of patience. I want practice in ill-treatment the female sex, — I did not observe Lyell had any compunction; I hope to harden my conscience in time: few husbands seem to find it difficult to effect this. Since my return I have taken several looks, as you will readily believe, into the drawing-room; I suppose my taste {for} harmonious colours is already deteriorated, for I declare the room begins to look less ugly. I take so much pleasure in the house (10/1. No. 12, Upper

Gower Street, is now No. 110, Gower Street, and forms part of a block inhabited by Messrs. Shoolbred's employes. We are indebted, for this information, to Mr. Wheatley, of the Society of Arts.), I declare I am just like a great overgrown child with a new toy; but then, not like a real child, I long to have a co-partner and possessor.

(10/2. The following passage is taken from the MS. copy of the "Autobiography;" it was not published in the "Life and Letters" which appeared in Mrs. Darwin's lifetime: —)

You all know your mother, and what a good mother she has ever been to all of you. She has been my greatest blessing, and I can declare that in my whole life I have never heard her utter one word I would rather have been unsaid. She has never failed in kindest sympathy towards me, and has borne with the utmost patience my frequent complaints of ill-health and discomfort. I do not believe she has ever missed an opportunity of doing a kind action to any one near her. I marvel at my good fortune that she, so infinitely my superior in every single moral quality, consented to be my wife. She has been my wise adviser and cheerful comforter throughout life, which without her would have been during a very long period a miserable one from ill-health. She has earned the love of every soul near her.

LETTER 11. C. LYELL TO C. DARWIN. {July?, 1841?}.

(11/1. Lyell started on his first visit to the United States in July, 1841, and was absent thirteen months. Darwin returned to London July 23rd, 1841, after a prolonged absence; he may, therefore, have missed seeing Lyell. Assuming the date 1841 to be correct, it would seem that the plan of living in the country was formed a year before it was actually carried out.)

I have no doubt that your father did rightly in persuading you to stay {at Shrewsbury}, but we were much disappointed in not seeing you before our start for a year's absence. I cannot tell you how often since your long illness I have missed the friendly intercourse which we had so frequently before, and on which I built more than ever after your marriage. It will not happen easily that twice in one's life, even in the large world of London, a congenial soul so occupied with precisely the same pursuits and with an independence enabling him to pursue them will fall so nearly in my way, and to have had it snatched from me with the prospect of your residence somewhat far off is a privation I feel as a very great one. I hope you will not, like Herschell, get far off from a railway.

LETTER 12. TO CATHERINE DARWIN.

(12/1. The following letter was written to his sister Catherine about two months before Charles Darwin settled at Down: —)

Sunday {July 1842}.

You must have been surprised at not having heard sooner about the house. Emma and I only returned yesterday afternoon from sleeping there. I will give you in detail, as my father would like, MY opinion on it — Emma's slightly differs. Position: — about 1/4 of a mile from the small village of Down in Kent — 16 miles from St. Paul's — 8 1/2 miles from station (with many trains) which station is only 10 from London. This is bad, as the drive from {i.e. on account of} the hills is long. I calculate we are two hours going from London Bridge. Village about forty houses with old walnut trees in the middle where stands an old flint church and the lanes meet. Inhabitants very respectable — infant school — grown up people great musicians — all touch their hats as in Wales and sit at their open doors in the evening; no high road leads through the village. The little pot-house where we slept is a grocer's shop, and the landlord is the carpenter — so you may guess the style of the village. There are butcher and baker and post-office. A carrier goes weekly to London and calls anywhere for anything in London and takes anything anywhere. On the road {from London} to the village, on a fine day the scenery is absolutely beautiful: from close to our house the view is very distant and rather beautiful, but the house being situated on a rather high tableland has somewhat of a desolate air. There is a most beautiful old farm-house, with great thatched barns and old stumps of oak trees, like that of Skelton, one field off. The charm of the place to me is that almost every field is intersected (as alas is ours) by one or more foot-paths. I never saw so many walks in any other county. The country is extraordinarily rural and quiet with narrow lanes and high hedges and hardly any ruts. It is really surprising to think London is only 16 miles off. The house stands very badly, close to a tiny lane and near another man's field. Our field is 15 acres and flat, looking into flat-bottomed valleys on both sides, but no view from the drawing-room, which faces due south, except on our flat field and bits of rather ugly distant horizon. Close in front there are some old (very productive) cherry trees, walnut trees, yew, Spanish chestnut, pear, old larch, Scotch fir and silver fir and old mulberry trees, {which} make rather a pretty group. They give the ground an old look, but from not flourishing much they also give it rather a desolate look. There are quinces and medlars and plums with plenty of fruit, and Morello cherries; but few apples. The purple magnolia flowers against the house. There is a really fine beech in view in our hedge. The kitchen garden is a detestable slip and the soil looks wretched from the quantity of chalk flints, but I really believe it is productive. The hedges grow well all round our field, and it is a noted piece of hayland. This year the crop was bad, but was bought, as it stood, for 2 pounds per acre — that is 30 pounds — the purchaser getting it in. Last year it was sold for 45 pounds — no manure was put on in the interval. Does not this sound well? Ask my father. Does the mulberry and magnolia show it is not very cold in winter, which I fear is the case? Tell Susan it is 9 miles from Knole Park and 6 from Westerham, at which places I hear the scenery is beautiful. There are many very odd views round our house — deepish flat-bottomed valley and nice farm-house, but big, white, ugly, fallow fields; — much wheat grown here. House ugly, looks neither old nor new — walls two feet thick — windows rather small — lower story rather low. Capital study 18 x 18. Dining-room 21 x 18. Drawing-room can easily be added to: is 21 x 15. Three stories, plenty of bedrooms. We could hold the Hensleighs and you and Susan and Erasmus all together. House in good repair. Mr. Cresy a few years ago laid out for the owner 1,500 pounds and made a new roof. Water-pipes over house — two bath-rooms — pretty good offices and good stable-yard, etc., and a cottage. I believe the price is about 2,200 pounds, and I have no doubt I shall get it for one year on lease first to try, so that I shall do nothing to the house at first (last owner kept three cows, one horse, and one donkey, and sold some hay annually from one field). I have no doubt if we complete the purchase I shall at least save 1,000 pounds over Westcroft, or any other house we have seen. Emma was at first a good deal disappointed, and at the country round the house; the day was gloomy and cold with N.E. wind. She likes the actual field and house better than I; the house is just situated as she likes for retirement, not too near or too far from other houses, but she thinks the country looks desolate. I think all chalk countries do, but I am used to Cambridgeshire, which is ten times worse. Emma is rapidly coming round. She was dreadfully bad with toothache and headache in the evening and Friday, but in coming back yesterday she was so delighted with the scenery for the first few miles from Down, that it has worked a great change in her. We go there again the first fine day Emma is able, and we then finally settle what to do.

(12/2. The following fragmentary "Account of Down" was found among Mr. Darwin's papers after the publication of the "Life and Letters." It gives the impression that he intended to write a natural history diary after the manner of Gilbert White, but there is no evidence that this was actually the case.)

1843. May 15th. — The first peculiarity which strikes a stranger unaccustomed to a hilly chalk country is the valleys, with their steep rounded bottoms — not furrowed with the smallest rivulet. On the road to Down from Keston a mound has been thrown across a considerable valley, but even against this mound there is no appearance of even a small pool of water having collected after the heaviest rains. The water all percolates straight downwards. Ascertain average depth of wells, inclination of strata, and springs. Does the water from this country crop out in springs in Holmsdale or in the valley of the Thames? Examine the fine springs in Holmsdale.

The valleys on this platform sloping northward, but exceedingly even, generally run north and south; their sides near the summits generally become suddenly more abrupt, and are fringed with narrow strips, or, as they are here called, "shaws" of wood, sometimes merely by hedgerows run wild. The sudden steepness may generally be perceived, as just before ascending to Cudham Wood, and at Green Hill, where one of the lanes crosses these valleys. These valleys are in all probability ancient sea-bays, and I have sometimes speculated whether this sudden steepening of the sides does not mark the edges of vertical cliffs formed when these valleys were filled with sea-water, as would naturally happen in strata such as the chalk.

In most countries the roads and footpaths ascend along the bottoms of valleys, but here this is scarcely ever the case. All the villages and most of the ancient houses are on the platforms or narrow strips of flat land between the parallel valleys. Is this owing to the summits having existed from the most ancient times as open downs and the valleys having been filled up with brushwood? I have no evidence of this, but it is certain that most of the farmhouses on the flat land are very ancient. There is one peculiarity which would help to determine the footpaths to run along the summits instead of the bottom of the valleys, in that these latter in the middle are generally covered, even far more thickly than the general surface, with broken flints. This bed of flints, which gradually thins away on each side, can be seen from a long distance in a newly ploughed or fallow field as a whitish band. Every stone which ever rolls after heavy rain or from the kick of an animal, ever so little, all tend to the bottom of the valleys; but whether this is sufficient to account for their number I have sometimes doubted, and have been inclined to apply to the case Lyell's theory of solution by rain-water, etc., etc.

The flat summit-land is covered with a bed of stiff red clay, from a few feet in thickness to as much, I believe, as twenty feet: this {bed}, though lying immediately on the chalk, and abounding with great, irregularly shaped, unrolled flints, often with the colour and appearance of huge bones, which were originally embedded in the chalk, contains not a particle of carbonate of lime. This bed of red clay lies on a very irregular surface, and often descends into deep round wells, the origin of which has been explained by Lyell. In these cavities are patches of sand like sea-sand, and like the sand which alternates with the great beds of small pebbles derived from the wear-and-tear of chalk-flints, which form Keston, Hayes and Addington Commons. Near Down a rounded chalk-flint is a rarity, though some few do occur; and I have not yet seen a stone of distant origin, which makes a difference — at least to geological eyes — in the very aspect of the country, compared with all the northern counties.

The chalk-flints decay externally, which, according to Berzelius ("Edin. New Phil. Journal," late number), is owing to the flints containing a small proportion of alkali; but, besides this external decay, the whole body is affected by exposure of a few years, so that they will not break with clean faces for building.

This bed of red clay, which renders the country very slippery in the winter months from October to April, does not cover the sides of the valleys; these, when ploughed, show the white chalk, which tint shades away lower in the valley, as insensibly as a colour laid on by a painter's brush.

Nearly all the land is ploughed, and is often left fallow, which gives the country a naked red look, or not unfrequently white, from a covering of chalk laid on by the farmers. Nobody seems at all aware on what principle fresh chalk laid on land abounding with lime does it any good. This, however, is said to have been the practice of the country ever since the period of the Romans, and at present the many white pits on the hill sides, which so frequently afford a picturesque contrast with the overhanging yew trees, are all quarried for this purpose.

The number of different kinds of bushes in the hedgerows, entwined by traveller's joy and the bryonies, is conspicuous compared with the hedges of the northern counties.

March 25th {1844?}. — The first period of vegetation, and the banks are clothed with pale-blue violets to an extent I have never seen equalled, and with primroses. A few days later some of the copses were beautifully enlivened by Ranunculus auricomus, wood anemones, and a white Stellaria. Again, subsequently, large areas were brilliantly blue with bluebells. The flowers are here very beautiful, and the number of flowers; {and} the darkness of the blue of the common little Polygala almost equals it to an alpine gentian.

There are large tracts of woodland, {cut down} about once every ten years; some of these enclosures seem to be very ancient. On the south side of Cudham Wood a beech hedge has grown to Brobdignagian size, with several of the huge branches crossing each other and firmly grafted together.

Larks abound here, and their songs sound most agreeably on all sides; nightingales are common. Judging from an odd cooing note, something like the purring of a cat, doves are very common in the woods.

June 25th. — The sainfoin fields are now of the most beautiful pink, and from the number of hive-bees frequenting them the humming noise is quite extraordinary. This humming is rather deeper than the humming overhead, which has been continuous and loud during all these last hot days over almost every field. The labourers here say it is made by "air-bees," and one man, seeing a wild bee in a flower different from the hive kind, remarked: "That, no doubt, is an air-bee." This noise is considered as a sign of settled fair weather.

CHAPTER 1.II. — EVOLUTION, 1844-1858

(Chapter II./1. Since the publication of the "Life and Letters," Mr. Huxley's obituary notice of Charles Darwin has appeared. (Chapter II./2. "Proc. R. Soc." volume 44, 1888, and "Collected Essays (Darwiniana)," page 253, 1899.) This masterly paper is, in our opinion, the finest of the great series of Darwinian essays which we owe to Mr. Huxley. We would venture to recommend it to our readers as the best possible introduction to these pages. There is, however, one small point in which we differ from Mr. Huxley. In discussing the growth of Mr. Darwin's evolutionary views, Mr. Huxley quotes from the autobiography (Chapter II./3. "Life and Letters," I., page 82. Some account of the origin of his evolutionary views is given in a letter to Jenyns (Blomefield), "Life and Letters," II. page 34.) a passage in which the writer describes the deep impression made on his mind by certain groups of facts observed in South America. Mr. Huxley goes on: "The facts to which reference is here made were, without doubt, eminently fitted to attract the attention of a philosophical thinker; but, until the relations of the existing with the extinct species, and of the species of the different geographical areas with one another, were determined with some exactness, they afforded but an unsafe foundation for speculation. It was not possible that this determination should have been effected before the return of the "Beagle" to England; and thus the date (Chapter II./4. The date in question is July 1837, when he "opened first note-book on Transmutation of Species.') which Darwin (writing in 1837) assigns to the dawn of the new light which was rising in his mind, becomes intelligible." This seems to us inconsistent with Darwin's own statement that it was especially the character of the "species on Galapagos Archipelago" which had impressed him. (Chapter II./5. See "Life and Letters," I., page 276.) This must refer to the zoological specimens: no doubt he was thinking of the birds, but these he had himself collected in 1835 (Chapter II./6. He wrote in his "Journal," page 394, "My attention was first thoroughly aroused, by comparing together the numerous specimens shot by myself and several other parties on board," etc.), and no accurate determination of the forms was necessary to impress on him the remarkable characteristic species of the different islands. We agree with Mr. Huxley that 1837 is the date of the "new light which was rising in his mind." That the dawn did not come sooner seems to us to be accounted for by the need of time to produce so great a revolution in his conceptions. We do not see that Mr. Huxley's supposition as to the effect of the determination of species, etc., has much weight. Mr. Huxley quotes a letter from Darwin to Zacharias, "But I did not become convinced that species were mutable until, I think, two or three years {after 1837} had elapsed" (see Letter 278). This passage, which it must be remembered was written in 1877, is all but irreconcilable with the direct evidence of the 1837 note-book. A series of passages are quoted from it in the "Life and Letters," Volume II., pages 5 et seq., and these it is impossible to read without feeling that he was convinced of immutability. He had not yet attained to a clear idea of Natural Selection, and therefore his views may not have had, even to himself, the irresistible convincing power they afterwards gained; but that he was, in the ordinary sense of the word, convinced of the truth of the doctrine of evolution we cannot doubt. He thought it "almost useless" to try to prove the truth of evolution until the cause of change was discovered. And it is natural that in later life he should have felt that conviction was wanting till that cause was made out. (Chapter II./7. See "Charles Darwin, his Life told, etc." 1892, page 165.) For the purposes of the present chapter the point is not very material. We know that in 1842 he wrote the first sketch of his theory, and that it was greatly amplified in 1844. So that, at the date of the first letters of this chapter, we know that he had a working hypothesis of evolution which did not differ in essentials from that given in the "Origin of Species."

To realise the amount of work that was in progress during the period covered by Chapter II., it should be remembered that during part of the time — namely, from 1846 to 1854 — he was largely occupied by his work on the Cirripedes. (Chapter II./8. "Life and Letters," I. page 346.) This research would have fully occupied a less methodical workman, and even to those who saw him at work it seemed his whole occupation. Thus (to quote a story of Lord Avebury's) one of Mr. Darwin's children is said to have asked, in regard to a neighbour, "Then where does he do his barnacles?" as though not merely his father, but all other men, must be occupied on that group.

Sir Joseph Hooker, to whom the first letter in this chapter is addressed, was good enough to supply a note on the origin of his intimacy with Mr. Darwin, and this is published in the "Life and Letters." (Chapter II./9. Ibid., II., page 19. See also "Nature," 1899, June 22nd, page 187, where some reminiscences are published, which formed part of Sir Joseph's speech at the unveiling of Darwin's statue in the Oxford Museum.) The close intercourse that sprang up between them was largely carried on by correspondence, and Mr. Darwin's letters to Sir Joseph have supplied most valuable biographical material. But it should not be forgotten that, quite apart from this, science owes much to this memorable friendship, since without Hooker's aid Darwin's great work would hardly have been carried out on the botanical side. And Sir Joseph did far more than supply knowledge and guidance in technical matters: Darwin owed to him a sympathetic and inspiriting comradeship which cheered and refreshed him to the end of his life.

A sentence from a letter to Hooker written in 1845 shows, quite as well as more serious utterances, how quickly the acquaintance grew into friendship.

"Farewell! What a good thing is community of tastes! I feel as if I had known you for fifty years. Adios." And in illustration of the permanence of the sympathetic bond between them, we quote a letter of 1881 written forty-two years after the first meeting with Sir Joseph in Trafalgar Square (see "Life and Letters," II., page 19). Mr. Darwin wrote: "Your letter has cheered me, and the world does not look a quarter so black this morning as it did when I wrote before. Your friendly words are worth their weight in gold.")

LETTER 13. TO J.D. HOOKER. Down, Thursday {January 11th, 1844}.

My dear Sir

I must write to thank you for your last letter, and to tell you how much all your views and facts interest me. I must be allowed to put my own interpretation on what you say of "not being a good arranger of extended views" — which is, that you do not indulge in the loose speculations so easily started by every smatterer and wandering collector. I look at a strong tendency to generalise as an entire evil.

What you say of Mr. Brown is humiliating; I had suspected it, but would not allow myself to believe in such heresy. Fitz-Roy gave him a rap in his preface (13/1. In the preface to the "Surveying Voyages of the 'Adventure' and the 'Beagle,' 1826-30, forming Volume I of the work, which includes the later voyage of the "Beagle," Captain Fitz-Roy wrote (March, 1839): "Captain King took great pains in forming and preserving a botanical collection, aided by a person embarked solely for that purpose. He placed this collection in the British Museum, and was led to expect that a first-rate botanist would have examined and described it; but he has been disappointed." A reference to Robert Brown's dilatoriness over King's collection occurs in the "Life and Letters," I., page 274, note.), and made him very indignant, but it seems a much harder one would not have been wasted. My cryptogamic collection was sent to Berkeley; it was not large. I do not believe he has yet published an account, but he wrote to me some year ago that he had described {the specimens} and mislaid all his descriptions. Would it not be well for you to put yourself in communication with him, as otherwise something will perhaps be twice laboured over? My best (though poor) collection of the cryptogams was from the Chonos Islands.

Would you kindly observe one little fact for me, whether any species of plant, peculiar to any island, as Galapagos, St. Helena, or New Zealand, where there are no large quadrupeds, have hooked seeds — such hooks as, if observed here, would be thought with justness to be adapted to catch into wool of animals.

Would you further oblige me some time by informing me (though I forget this will certainly appear in your "Antarctic Flora") whether in islands like St. Helena, Galapagos, and New Zealand, the number of families and genera are large compared with the number of species, as happens in coral islands, and as, I believe, in the extreme Arctic land. Certainly this is the case with marine shells in extreme Arctic seas. Do you suppose the fewness of species in proportion to number of large groups in coral islets is owing to the chance of seeds from all orders getting drifted to such new spots, as I have supposed. Did you collect sea-shells in

Kerguelen-land? I should like to know their character.

Your interesting letters tempt me to be very unreasonable in asking you questions; but you must not give yourself any trouble about them, for I know how fully and worthily you are employed. (13/2. The rest of the letter has been previously published in "Life and Letters," II., page 23.)

Besides a general interest about the southern lands, I have been now ever since my return engaged in a very presumptuous work, and I know no one individual who would not say a very foolish one. I was so struck with the distribution of the Galapagos organisms, etc., and with the character of the American fossil mammifers, etc., that I determined to collect blindly every sort of fact which could bear any way on what are species. I have read heaps of agricultural and horticultural books, and have never ceased collecting facts. At last gleams of light have come, and I am almost convinced (quite contrary to the opinion I started with) that species are not (it is like confessing a murder) immutable. Heaven forfend me from Lamarck nonsense of a "tendency to progression," "adaptations from the slow willing of animals," etc.! But the conclusions I am led to are not widely different from his; though the means of change are wholly so. I think I have found out (here's presumption!) the simple way by which species become exquisitely adapted to various ends. You will now groan, and think to yourself, "on what a man have I been wasting my time and writing to." I should, five years ago, have thought so...(13/3. On the questions here dealt with see the interesting letter to Jenyns in the "Life and Letters," II., page 34.)

LETTER 14. TO J.D. HOOKER. {November} 1844.

...What a curious, wonderful case is that of the Lycopodium! (14/1. Sir J.D. Hooker wrote, November 8, 1844: "I am firmly convinced (but not enough to print it) that L. Selago varies in Van Diemen's Land into L. varium. Two more different SPECIES (as they have hitherto been thought), per se cannot be conceived, but nowhere else do they vary into one another, nor does Selago vary at all in England.")...I suppose you would hardly have expected them to be more varying than a phanerogamic plant. I trust you will work the case out, and, even if unsupported, publish it, for you can surely do this with due caution. I have heard of some analogous facts, though on the smallest scale, in certain insects being more variable in one district than in another, and I think the same holds with some land-shells. By a strange chance I had noted to ask you in this letter an analogous question, with respect to genera, in lieu of individual species, — that is, whether you know of any case of a genus with most of its species being variable (say Rubus) in one continent, having another set of species in another continent non-variable, or not in so marked a manner. Mr. Herbert (14/2. No doubt Dean Herbert, the horticulturist. See "Life and Letters," I., page 343.) incidentally mentioned in a letter to me that the heaths at the Cape of Good Hope were very variable, whilst in Europe they are (?) not so; but then the species here are few in comparison, so that the case, even if true, is not a good one. In some genera of insects the variability appears to be common in distant parts of the world. In shells, I hope hereafter to get much light on this question through fossils. If you can help me, I should be very much obliged: indeed, all your letters are most useful to me.

MONDAY: — Now for your first long letter, and to me quite as interesting as long. Several things are quite new to me in it — viz., for one, your belief that there are more extra-tropical than intra-tropical species. I see that my argument from the Arctic regions is false, and I should not have tried to argue against you, had I not fancied that you thought that equability of climate was the direct cause of the creation of a greater or lesser number of species. I see you call our climate equable; I should have thought it was the contrary. Anyhow, the term is vague, and in England will depend upon whether a person compares it with the United States or Tierra del Fuego. In my Journal (page 342) I see I state that in South Chiloe, at a height of about 1,000 feet, the forests had a Fuegian aspect: I distinctly recollect that at the sea-level in the middle of Chiloe the forest had almost a tropical aspect. I should like much to hear, if you make out, whether the N. or S. boundaries of a plant are the most restricted; I should have expected that the S. would be, in the temperate regions, from the number of antagonist species being greater. N.B. Humboldt, when in London, told me of some river (14/3. The Obi (see "Flora Antarctica," page 211, note). Hooker writes: "Some of the most conspicuous trees attain either of its banks, but do not cross them.") in N.E. Europe, on the opposite banks of which the flora was, on the same soil and under same climate, widely different!

I forget (14/4. The last paragraph is published in "Life and Letters," II., page 29.) my last letter, but it must have been a very silly one, as it seems I gave my notion of the number of species being in great degree governed by the degree to which the area had been often isolated and divided. I must have been cracked to have written it, for I have no evidence, without a person be willing to admit all my views, and then it does follow.

(14/5. The remainder of the foregoing letter is published in the "Life and Letters," II., page 29. It is interesting as giving his views on the mutability of species. Thus he wrote: "With respect to books on this subject, I do not know any systematical ones, except Lamarck's, which is veritable rubbish; but there are plenty, as Lyell, Pritchard, etc., on the view of the immutability." By "Pritchard" is no doubt intended James Cowles "Prichard," author of the "Physical History of Mankind." Prof. Poulton has given in his paper, "A remarkable Anticipation of Modern Views on Evolution" (14/6. "Science Progress," Volume I., April 1897, page 278.), an interesting study of Prichard's work. He shows that Prichard was in advance of his day in his views on the non-transmission of acquired characters. Prof. Poulton also tries to show that Prichard was an evolutionist. He allows that Prichard wrote with hesitation, and that in the later editions of his book his views became weaker. But, even with these qualifications, we think that Poulton has unintentionally exaggerated the degree to which Prichard believed in evolution.

One of Prichard's strongest sentences is quoted by Poulton (loc. cit., page 16); it occurs in the "Physical History of Mankind," Ed. 2, Volume II., page 570: —

"Is it not probable that the varieties which spring up within the limits of particular species are further adaptations of structure to the circumstances under which the tribe is destined to exist? Varieties branch out from the common form of a species, just as the forms of species deviate from the common type of a genus. Why should the one class of phenomena be without end or utility, a mere effect of contingency or chance, more than the other?"

If this passage, and others similar to it, stood alone, we might agree with Prof. Poulton; but this is impossible when we find in Volume I. of the same edition, page 90, the following uncompromising statement of immutability: —

"The meaning attached to the term species, in natural history, is very simple and obvious. It includes only one circumstance — namely, an original distinctness and constant transmission of any character. A race of animals, or plants, marked by any peculiarities of structure which have always been constant and undeviating, constitutes a species."

On page 91, in speaking of the idea that the species which make up a genus may have descended from a common form, he says: —

"There must, indeed, be some principle on which the phenomena of resemblance, as well as those of diversity, may be explained; and the reference of several forms to a common type seems calculated to suggest the idea of some original affinity; but, as this is merely a conjecture, it must be kept out of sight when our inquiries respect matters of fact only."

This view is again given in Volume II., page 569, where he asks whether we should believe that "at the first production of a genus, when it first grew into existence, some slight modification in the productive causes stamped it originally with all these specific diversities? Or is it most probable that the modification was subsequent to its origin, and that the genus at its first creation was one and uniform, and afterwards became diversified by the influence of external agents?" He concludes that "the former of these suppositions is the conclusion to which we are led by all that can be ascertained respecting the limits of species, and the extent of variation under the influence of causes at present existing and operating."

In spite of the fact that Prichard did not carry his ideas to their logical conclusion, it may perhaps excite surprise that Mr. Darwin should have spoken of him as absolutely on the side of immutability.

We believe it to be partly accounted for (as Poulton suggests) by the fact that Mr. Darwin possessed only the third edition (1836 and 1837) and the fourth edition (1841-51). (14/7. The edition of 1841-51 consists of reprints of the third edition and three additional volumes of various dates. Volumes I. and II. are described in the title-page as the fourth edition; Volumes III. and IV. as the third edition, and Volume V. has no edition marked in the title.) In neither of these is the evolutionary point of view so strong as in the second edition.

We have gone through all the passages marked by Mr. Darwin for future reference in the third and fourth editions, and have been only able to find the following, which occurs in the third edition (Volume I., 1836, page 242) (14/8. There is also (ed. 1837, Volume II., page 344) a vague reference to Natural Selection, of which the last sentence is enclosed in pencil in inverted commas, as though Mr. Darwin had intended to quote it: "In other parts of Africa the xanthous variety {of man} often appears, but does not multiply. Individuals thus characterised are like seeds which perish in an uncongenial soil.")

"The variety in form, prevalent among all organised productions of nature, is found to subsist between individual beings of whatever species, even when they are offspring of the same parents. Another circumstance equally remarkable is the tendency which exists in almost every tribe, whether of animals or of plants, to transmit to their offspring and to perpetuate in their race all individual peculiarities which may thus have taken their rise. These two general facts in the economy of organised beings lay a foundation for the existence of diversified races, originating from the same primitive stock and within the limits of identical species."

On the following page (page 243) a passage (not marked by Mr. Darwin) emphasises the limitation which Prichard ascribed to the results of variation and inheritance: —

"Even those physiologists who contend for what is termed the indefinite nature of species admit that they have limits at present and under ordinary circumstances. Whatever diversities take place happen without breaking in upon the characteristic type of the species. This is transmitted from generation to generation: goats produce goats, and sheep, sheep."

The passage on page 242 occurs in the reprint of the 1836-7 edition which forms part of the 1841-51 edition, but is not there marked by Mr. Darwin. He notes at the end of Volume I. of the 1836-7 edition: "March, 1857. I have not looked through all these {i.e. marked passages}, but I have gone through the later edition"; and a similar entry is in Volume II. of the third edition. It is therefore easy to understand how he came to overlook the passage on page 242 when he began the fuller statement of his species theory which is referred to in the "Life and Letters" as the "unfinished book." In the historical sketch prefixed to the "Origin of Species" writers are named as precursors whose claims are less strong than Prichard's, and it is certain that Mr. Darwin would have given an account of him if he had thought of him as an evolutionist.

The two following passages will show that Mr. Darwin was, from his knowledge of Prichard's books, justified in classing him among those who did not believe in the mutability of species:

"The various tribes of organised beings were originally placed by the Creator in certain regions, for which they are by their nature peculiarly adapted. Each species had only one beginning in a single stock: probably a single pair, as Linnaeus supposed, was first called into being in some particular spot, and the progeny left to disperse themselves to as great a distance from the original centre of their existence as the locomotive powers bestowed on them, or their capability of bearing changes of climate and other physical agencies, may have enabled them to wander." (14/9. Prichard, third edition, 1836-7, Volume I., page 96.)

The second passage is annotated by Mr. Darwin with a shower of exclamation marks:

"The meaning attached to the term SPECIES in natural history is very definite and intelligible. It includes only the following conditions — namely, separate origin and distinctness of race, evinced by the constant transmission of some characteristic peculiarity of organisation. A race of animals or of plants marked by any peculiar character which has always been constant and undeviating constitutes a species; and two races are considered as specifically different, if they are distinguished from each other by some characteristic which one cannot be supposed to have acquired, or the other to have lost through any known operation of physical causes; for we are hence led to conclude that the tribes thus distinguished have not descended from the same original stock." (14/10. Prichard, ed. 1836-7, Volume I., page 106. This passage is almost identical with that quoted from the second edition, Volume I., page 90. The latter part, from "and two races..." occurs in the second edition, though not quoted above.)

As was his custom, Mr. Darwin pinned at the end of the first volume of the 1841-51 edition a piece of paper containing a list of the pages where marked passages occur. This paper bears, written in pencil, "How like my book all this will be!" The words appear to refer to Prichard's discussion on the dispersal of animals and plants; they certainly do not refer to the evolutionary views to be found in the book.)

LETTER 15. TO J.D. HOOKER. Down {1844}.

Thank you exceedingly for your long letter, and I am in truth ashamed of the time and trouble you have taken for me; but I must some day write again to you on the subject of your letter. I will only now observe that you have extended my remark on the range of species of shells into the range of genera or groups. Analogy from shells would only go so far, that if two or three species...were found to range from America to India, they would be found to extend through an unusual thickness of strata — say from the Upper Cretaceous to its lowest bed, or the Neocomian. Or you may reverse it and say those species which range throughout the whole Cretaceous, will have wide ranges: viz., from America through Europe to India (this is one actual case with shells in the Cretaceous period).

LETTER 16. TO J.D. HOOKER. Down {1845}.

I ought to have written sooner to say that I am very willing to subscribe 1 pound 1 shilling to the African man (though it be murder on a small scale), and will send you a Post-office-order payable to Kew, if you will be so good as to take charge of it. Thanks for your information about the Antarctic Zoology; I got my numbers when in Town on Thursday: would it be asking your publisher to take too much trouble to send your Botany {"Flora Antarctica," by J.D. Hooker, 1844} to the Athenaeum Club? he might send two or three numbers together. I am really ashamed to think of your having given me such a valuable work; all I can say is that I appreciate your present in two ways — as your gift, and for its great use to my species-work. I am very glad to hear that you mean to attack this subject some day. I wonder whether we shall ever be public combatants; anyhow, I congratulate myself in a most unfair advantage of you, viz., in having extracted more facts and views from you than from any one other person. I daresay your explanation of polymorphism on volcanic islands may be the right one; the reason I am curious about it is, the fact of the birds on the Galapagos being in several instances very fine-run species — that is, in comparing them, not so much one with another, as with their analogues from the continent. I have somehow felt, like you, that an alpine form of a plant is not a true variety; and yet I cannot admit that the simple fact of the cause being assignable ought to prevent its being called a variety; every variation must have some cause, so that the difference would rest on our knowledge in being able or not to assign the cause. Do you consider that a true variety should be produced by causes acting through the parent? But even taking this definition, are you sure that alpine forms are not inherited from one, two, or three generations? Now, would not this be a curious and valuable experiment (16/1. For an account of work of this character, see papers by G. Bonnier in the "Revue Generale," Volume II., 1890; "Ann. Sc. Nat." Volume XX.; "Revue Generale," Volume VII.), viz., to get seeds of some alpine plant, a little more hairy, etc., etc., than its lowland fellow, and raise seedlings at Kew: if this has not been done, could you not get it done? Have you anybody in Scotland from whom you could get the seeds?

I have been interested by your remarks on Senecia and Gnaphalium: would it not be worth while (I should be very curious to hear the result) to make a short list of the generally considered variable or polymorphous genera, as Rosa, Salix, Rubus, etc., etc., and reflect whether such genera are generally mundane, and more especially whether they have distinct or identical (or closely allied) species in their different and distant habitats.

Don't forget me, if you ever stumble on cases of the same species being MORE or LESS variable in different countries.

With respect to the word "sterile" as used for male or polleniferous flowers, it has always offended my ears dreadfully; on the same principle that it would to hear a potent stallion, ram or bull called sterile, because they did not bear, as well as beget, young.

With respect to your geological-map suggestion, I wish with all my heart I could follow it; but just reflect on the number of measurements requisite; why, at present it could not be done even in England, even with the assumption of the land having simply risen any exact number of feet. But subsidence in most cases has hopelessly complexed the problem: see what Jordanhill-Smith (16/2. James Smith, of Jordan Hill, author of a paper "On the Geology of Gibraltar" ("Quart. Journ. Geol. Soc." Volume II., page 41, 1846).) says of the dance up and down, many times, which Gibraltar has had all within the recent period. Such maps as Lyell (16/3. "Principles of Geology," 1875, Volume I., Plate I, page 254.) has published of sea and land at the beginning of the Tertiary period must be excessively inaccurate: it assumes that every part on which Tertiary beds have not been deposited, must have then been dry land, — a most doubtful assumption.

I have been amused by Chambers v. Hooker on the K. Cabbage. I see in the "Explanations" (the spirit of which, though not the facts, ought to shame Sedgwick) that "Vestiges" considers all land-animals and plants to have passed from marine forms; so Chambers is quite in accordance. Did you hear Forbes, when here, giving the rather curious evidence (from a similarity in error) that Chambers must be the author of the "Vestiges": your case strikes me as some confirmation. I have written an unreasonably long and dull letter, so farewell. (16/4. "Explanations: A Sequel to the Vestiges of the Natural History of Creation" was published in 1845, after the appearance of the fourth edition of the "Vestiges," by way of reply to the criticisms on the original book. The "K. cabbage" referred to at the beginning of the paragraph is Pringlea antiscorbutica," the "Kerguelen Cabbage" described by Sir J.D. Hooker in his "Flora Antarctica." What Chambers wrote on this subject we have not discovered. The mention of Sedgwick is a reference to his severe review of the "Vestiges" in the "Edinburgh Review," 1845, volume 82, page 1. Darwin described it as savouring "of the dogmatism of the pulpit" ("Life and Letters," I., page 344). Mr. Ireland's edition of the "Vestiges" (1844), in which Robert Chambers was first authentically announced as the author, contains (page xxix) an extract from a letter written by Chambers in 1860, in which the following passage occurs, "The April number of the 'Edinburgh Review"' (1860) makes all but a direct amende for the abuse it poured upon my work a number of years ago." This is the well-known review by Owen, to which references occur in the "Life and Letters," II., page 300. The amende to the "Vestiges" is not so full as the author felt it to be; but it was clearly in place in a paper intended to belittle the "Origin"; it also gave the reviewer (page 511) an opportunity for a hit at Sedgwick and his 1845 review.)

LETTER 17. TO L. BLOMEFIELD {JENYNS}. Down. February 14th {1845}.

I have taken my leisure in thanking you for your last letter and discussion, to me very interesting, on the increase of species. Since your letter, I have met with a very similar view in Richardson, who states that the young are driven away by the old into unfavourable districts, and there mostly perish. When one meets with such unexpected statistical returns on the increase and decrease and proportion of deaths and births amongst mankind, and in this well-known country of ours, one ought not to be in the least surprised at one's ignorance, when, where, and how the endless increase of our robins and sparrows is checked.

Thanks for your hints about terms of "mutation," etc.; I had some suspicions that it was not quite correct, and yet I do not see my way to arrive at any better terms. It will be years before I publish, so that I shall have plenty of time to think of better words. Development would perhaps do, only it is applied to the changes of an individual during its growth. I am, however, very glad of your remark, and will ponder over it.

We are all well, wife and children three, and as flourishing as this horrid, house-confining, tempestuous weather permits.

LETTER 18. TO J.D. HOOKER. Down {1845}.

I hope you are getting on well with your lectures, and that you have enjoyed some pleasant walks during the late delightful weather. I write to tell you (as perhaps you might have had fears on the subject) that your books have arrived safely. I am exceedingly obliged to you for them, and will take great care of them; they will take me some time to read carefully.

I send to-day the corrected MS. of the first number of my "Journal" (18/1. In 1842 he had written to his sister: "Talking of money, I reaped the other day all the profit which I shall ever get from my "Journal" {"Journal of Researches, etc."} which consisted in paying Mr. Colburn 21 pounds 10 shillings for the copies which I presented to different people; 1,337 copies have been sold. This is a comfortable arrangement, is it not?" He was proved wrong in his gloomy prophecy, as the second edition was published by Mr. Murray in 1845.) in the Colonial Library, so that if you chance to know of any gross mistake in the first 214 pages (if you have my "Journal"), I should be obliged to you to tell me.

Do not answer this for form's sake; for you must be very busy. We have just had the Lyells here, and you ought to have a wife to stop your working too much, as Mrs. Lyell peremptorily stops Lyell.

LETTER 19. TO J.D. HOOKER.

(19/1. Sir J.D. Hooker's letters to Mr. Darwin seem to fix the date as 1845, while the reference to Forbes' paper indicates 1846.)

Down {1845-1846}.

I am particularly obliged for your facts about solitary islands having several species of peculiar genera; it knocks on the head some analogies of mine; the point stupidly never occurred to me to ask about. I am amused at your anathemas against variation and co.; whatever you may be pleased to say, you will never be content with simple species, "as they are." I defy you to steel your mind to technicalities, like so many of our brother naturalists. I am much pleased that I thought of sending you Forbes' article. (19/2. E. Forbes' celebrated paper "Memoirs of the Geological Survey of Great Britain," Volume I., page 336, 1846. In Lyell's "Principles," 7th Edition, 1847, page 676, he makes a temperate claim of priority, as he had already done in a private letter of October 14th, 1846, to Forbes ("Life of Sir Charles Lyell," 1881, Volume II., page 106) both as regards the Sicilian flora and the barrier effect of mountain-chains. See Letter 20 for a note on Forbes.) I confess I cannot make out the evidence of his time-notions in distribution, and I cannot help suspecting that they are rather vague. Lyell preceded Forbes in one class of speculation of this kind: for instance, in his explaining the identity of the Sicily Flora with that of South Italy, by its having been wholly upraised within the recent period; and, so I believe, with mountain-chains separating floras. I do not remember Humboldt's fact about the heath regions. Very curious the case of the broom; I can tell you something analogous on a small scale. My father, when he built his house, sowed many broom-seeds on a wild bank, which did not come up, owing, as it was thought, to much earth having been thrown over them. About thirty-five years afterwards, in cutting a terrace, all this earth was thrown up, and now the bank is one mass of broom. I see we were in some degree talking to cross-purposes; when I said I did {not} much believe in hybridising to any extent, I did not mean at all to exclude crossing. It has long been a hobby of mine to see in how many flowers such crossing is probable; it was, I believe, Knight's view, originally, that every plant must be occasionally crossed. (19/3. See an article on "The Knight-Darwin law" by Francis Darwin in "Nature," October 27th, 1898, page 630.) I find, however, plenty of difficulty in showing even a vague probability of this; especially in the

Leguminosae, though their {structure?} is inimitably adapted to favour crossing, I have never yet met with but one instance of a NATURAL MONGREL (nor mule?) in this family.

I shall be particularly curious to hear some account of the appearance and origin of the Ayrshire Irish Yew. And now for the main object of my letter: it is to ask whether you would just run your eye over the proof of my Galapagos chapter (19/4. In the second edition of the "Naturalist's Voyage."), where I mention the plants, to see that I have made no blunders, or spelt any of the scientific names wrongly. As I daresay you will so far oblige me, will you let me know a few days before, when you leave Edinburgh and how long you stay at Kinnordy, so that my letter might catch you. I am not surprised at my collection from James Island differing from others, as the damp upland district (where I slept two nights) is six miles from the coast, and no naturalist except myself probably ever ascended to it. Cuming had never even heard of it. Cuming tells me that he was on Charles, James, and Albemarle Islands, and that he cannot remember from my description the Scalesia, but thinks he could if he saw a specimen. I have no idea of the origin of the distribution of the Galapagos shells, about which you ask. I presume (after Forbes' excellent remarks on the facilities by which embryo-shells are transported) that the Pacific shells have been borne thither by currents; but the currents all run the other way.

(PLATE: EDWARD FORBES 1844? From a photograph by Hill & Adamson.)

LETTER 20. EDWARD FORBES TO C. DARWIN.

(20/1. Edward Forbes was at work on his celebrated paper in the "Geological Survey Memoirs" for 1846. We have not seen the letter of Darwin's to which this is a reply, nor, indeed, any of his letters to Forbes. The date of the letter is fixed by Forbes's lecture given at the Royal Institution on February 27th, 1846 (according to L. Horner's privately printed "Memoirs," II., page 94.))

Wednesday. 3, Southwark Street, Hyde Park. {1846}.

Dear Darwin

To answer your very welcome letter, so far from being a waste of time, is a gain, for it obliges me to make myself clear and understood on matters which I have evidently put forward imperfectly and with obscurity. I have devoted the whole of this week to working and writing out the flora question, for I now feel strong enough to give my promised evening lecture on it at the Royal Institution on Friday, and, moreover, wish to get it in printable form for the Reports of our Survey. Therefore at no time can I receive or answer objections with more benefit than now. From the hurry and pressure which unfortunately attend all my movements and doings I rarely have time to spare, in preparing for publication, to do more than give brief and unsatisfactory abstracts, which I fear are often extremely obscure.

Now for your objections — which have sprung out of my own obscurities.

I do not argue in a circle about the Irish case, but treat the botanical evidence of connection and the geological as distinct. The former only I urged at Cambridge; the latter I have not yet publicly maintained.

My Cambridge argument (20/2. "On the Distribution of Endemic Plants," by E. Forbes, "Brit. Assoc. Rep." 1845 (Cambridge), page 67.) was this: That no known currents, whether of water or air, or ordinary means of transport (20/3. Darwin's note on transportation (found with Forbes' letter): "Forbes' arguments, from several Spanish plants in Ireland not being transported, not sound, because sea-currents and air ditto and migration of birds in SAME LINES. I have thought not-transportation the greatest difficulty. Now we see how many seeds every plant and tree requires to be regularly propagated in its own country, for we cannot think the great number of seeds superfluous, and therefore how small is the chance of here and there a solitary seedling being preserved in a well-stocked country."), would account for the little group of Asturian plants — few as to species, but playing a conspicuous part in the vegetation — giving a peculiar botanical character to the south of Ireland; that, as I had produced evidence of the other floras of our islands, i.e. the Germanic, the Cretaceous, and the Devonian (these terms used topographically, not geologically) having been acquired by migration over continuous land (the glacial or alpine flora I except for the present — as ice-carriage might have played a great part in its introduction) — I considered it most probable, and maintained, that the introduction of that

Irish flora was also effected by the same means. I held also that the character of this flora was more southern and more ancient than that of any of the others, and that its fragmentary and limited state was probably due to the plants composing it having (from their comparative hardiness — heaths, saxifrages, etc.) survived the destroying influence of the glacial epoch.

My geological argument now is as follows: half the Mediterranean islands, or more, are partly — in some cases (as Malta) wholly — composed of the upheaved bed of the Miocene sea; so is a great part of the south of France from Bordeaux to Montpellier; so is the west of Portugal; and we find the corresponding beds with the same fossils (Pecten latissimus, etc.) in the Azores. So general an upheaval seems to me to indicate the former existence of a great post-Miocene land {in} the region of what is usually called the Mediterranean flora. (Everywhere these Miocene islands, etc., bear a flora of true type.) If this land existed, it did not extend to America, for the fossils of the Miocene of America are representative and not identical. Where, then, was the edge or coast-line of it, Atlantic-wards? Look at the form and constancy of the great fucus-bank, and consider that it is a Sargassum bank, and that the Sargassum there is in an abnormal condition, and that the species of this genus of fuci are essentially ground-growers, and then see the probability of this bank having originated on a line of ancient coast.

Now, having thus argued independently, first on my flora and second on the geological evidences of land in the quarter required, I put the two together to bear up my Irish case.

I cannot admit the Sargassum case to be parallel with that of Confervae or Oscillatoria.

I think I have evidence from the fossils of the boulder formations in Ireland that if such Miocene land existed it must have been broken up or partially broken up at the epoch of the glacial or boulder period.

All objections thankfully received.

Ever most sincerely,

EDWARD FORBES.

LETTER 21. TO L. JENYNS (BLOMEFIELD). Down. {1846}.

I am much obliged for your note and kind intended present of your volume. (21/1. No doubt the late Mr. Blomefield's "Observations in Natural History." See "Life and Letters," II., page 31.) I feel sure I shall like it, for all discussions and observations on what the world would call trifling points in Natural History always appear to me very interesting. In such foreign periodicals as I have seen, there are no such papers as White, or Waterton, or some few other naturalists in Loudon's and Charlesworth's Journal, would have written; and a great loss it has always appeared to me. I should have much liked to have met you in London, but I cannot leave home, as my wife is recovering from a rather sharp fever attack, and I am myself slaving to finish my S. American Geology (21/2. "Geological Observations in South America" (London), 1846.), of which, thanks to all Plutonic powers, two-thirds are through the press, and then I shall feel a comparatively free man. Have you any thoughts of Southampton? (21/3. The British Association met at Southampton in 1846.) I have some vague idea of going there, and should much enjoy meeting you.

LETTER 22. TO J.D. HOOKER. Shrewsbury {end of February 1846}.

I came here on account of my father's health, which has been sadly failing of late, but to my great joy he has got surprisingly better...I had not heard of your botanical appointment (22/1. Sir Joseph was appointed Botanist to the Geological Survey in 1846.), and am very glad of it, more especially as it will make you travel and give you change of work and relaxation. Will you some time have to examine the Chalk and its junction with London Clay and Greensand? If so our house would be a good central place, and my horse would be at your disposal. Could you not spin a long week out of this examination? it would in truth delight us, and you could bring your papers (like Lyell) and work at odd times. Forbes has been writing to me about his subsidence doctrines; I wish I had heard his full details, but I have expressed to him in my ignorance my objections, which rest merely on its too great hypothetical basis; I shall be curious, when I meet him, to hear what he says. He is also speculating on the gulf-weed. I confess I cannot appreciate his reasoning about his Miocene continent, but I daresay it is from want of knowledge.

You allude to the Sicily flora not being peculiar, and this being caused by its recent elevation (well established) in the main part: you will find Lyell has put forward this very clearly and well. The Apennines (which I was somewhere lately reading about) seems a very curious case.

I think Forbes ought to allude a little to Lyell's (22/2. See Letter 19.) work on nearly the same subject as his speculations; not that I mean that Forbes wishes to take the smallest credit from him or any man alive; no man, as far as I see, likes so much to give credit to others, or more soars above the petty craving for self-celebrity.

If you come to any more conclusions about polymorphism, I should be very glad to hear the result: it is delightful to have many points fermenting in one's brain, and your letters and conclusions always give one plenty of this same fermentation. I wish I could even make any return for all your facts, views, and suggestions.

LETTER 23. TO J.D. HOOKER.

(23/1. The following extract gives the germ of what developed into an interesting discussion in the "Origin" (Edition I., page 147). Darwin wrote, "I suspect also that some cases of compensation which have been advanced and likewise some other facts, may be merged under a more general principle: namely, that natural selection is continually trying to economise in every part of the organism." He speaks of the general belief of botanists in compensation, but does not quote any instances.)

{September 1846}.

Have you ever thought of G. St. Hilaire's "loi de balancement" (23/2. According to Darwin ("Variation of Animals and Plants," 2nd edition, II., page 335) the law of balancement was propounded by Goethe and Geoffroy Saint-Hilaire (1772-1844) nearly at the same time, but he gives no reference to the works of these authors. It appears, however, from his son Isidore's "Vie, Travaux etc., d'Etienne Geoffroy Saint-Hilaire," Paris 1847, page 214, that the law was given in his "Philosophie Anatomique," of which the first part was published in 1818. Darwin (ibid.) gives some instances of the law holding good in plants.), as applied to plants? I am well aware that some zoologists quite reject it, but it certainly appears to me that it often holds good with animals. You are no doubt aware of the kind of facts I refer to, such as great development of canines in the carnivora apparently causing a diminution — a compensation or balancement — in the small size of premolars, etc. I have incidentally noticed some analogous remarks on plants, but have never seen it discussed by botanists. Can you think of cases in any one species in genus, or genus in family, with certain parts extra developed, and some adjoining parts reduced? In varieties of the same species double flowers and large fruits seem something of this — want of pollen and of seeds balancing with the increased number of petals and development of fruit. I hope we shall see you here this autumn.

(24/1. In this year (1847) Darwin wrote a short review of Waterhouse's "Natural History of the Mammalia," of which the first volume had appeared. It was published in "The Annals and Magazine of Natural History," Volume XIX., page 53. The following sentence is the only one which shows even a trace of evolution: "whether we view classification as a mere contrivance to convey much information in a single word, or as something more than a memoria technica, and as connected with the laws of creation, we cannot doubt that where such important differences in the generative and cerebral systems, as distinguish the Marsupiata from the Placentata, run through two series of animals, they ought to be arranged under heads of equal value."

A characteristic remark occurs in reference to Geographical Distribution, "that noble subject of which we as yet but dimly see the full bearing."

The following letter seems to be of sufficient interest to be published in spite of the obscurities caused by the want of date. It seems to have been written after 1847, in which year a dispute involving Dr. King and several "arctic gentlemen" was carried on in the "Athenaeum." Mr. Darwin speaks of "Natural History Instructions for the present expedition." This may possibly refer to the "Admiralty Manual of Scientific Enquiry" (1849), for it is clear, from the prefatory memorandum of the Lords of the Admiralty, that they believed the manual would be of use in the forthcoming expeditions in search of Sir John Franklin.)

LETTER 24. TO E. CRESY.

(24/2. Mr. Cresy was, we believe, an architect: his friendship with Mr. Darwin dates from the settlement at Down.)

Down {after 1847}.

Although I have never particularly attended to the points in dispute between Dr. (Richard) King and the other Arctic gentlemen, yet I have carefully read all the articles in the "Athenaeum," and took from them much the same impression as you convey in your letter, for which I thank you. I believe that old sinner, Sir J. Barrow (24/3. Sir John Barrow, (1764-1848): Secretary to the Admiralty. has been at the bottom of all the money wasted over the naval expeditions. So strongly have I felt on this subject, that, when I was appointed on a committee for Nat. Hist. instructions for the present expedition, had I been able to attend I had resolved to express my opinion on the little advantage, comparatively to the expense, gained by them. There have been, I believe, from the beginning eighteen expeditions; this strikes me as monstrous, considering how little is known, for instance, on the interior of Australia. The country has paid dear for Sir John's hobbyhorse. I have very little doubt that Dr. King is quite right in the advantage of land expeditions as far as geography is concerned; and that is now the chief object. (24/4. This sentence would imply that Darwin thought it hopeless to rescue Sir J. Franklin's expedition. If so, the letter must be, at least, as late as 1850. If the eighteen expeditions mentioned above are "search expeditions," it would also bring the date of the letter to 1850.)

LETTER 25. TO RICHARD OWEN. Down {March 26th, 1848}.

My dear Owen

I do not know whether your MS. instructions are sent in; but even if they are not sent in, I daresay what I am going to write will be absolutely superfluous (25/1. The results of Mr. Darwin's experience given in the above letter were embodied by Prof. Owen in the section "On the Use of the Microscope on Board Ship," forming part of the article "Zoology" in the "Manual of Scientific Enquiry, Prepared for the Use of Her Majesty's Navy" (London, 1849).), but I have derived such infinitely great advantage from my new simple microscope, in comparison with the one which I used on board the "Beagle," and which was recommended to me by R. Brown ("Life and Letters," I., page 145.), that I cannot forego the mere chance of advantage of urging this on you. The leading point of difference consists simply in having the stage for saucers very large and fixed. Mine will hold a saucer three inches in inside diameter. I have never seen such a microscope as mine, though Chevalier's (from whose plan many points of mine are taken), of Paris, approaches it pretty closely. I fully appreciate the utter ABSURDITY of my giving you advice about means of dissecting; but I have appreciated myself the enormous disadvantage of having worked with a bad instrument, though thought a few years since the best. Please to observe that without you call especial attention to this point, those ignorant of Natural History will be sure to get one of the fiddling instruments sold in shops. If you thought fit, I would point out the differences, which, from my experience, make a useful microscope for the kind of dissection of the invertebrates which a person would be likely to attempt on board a vessel. But pray again believe that I feel the absurdity of this letter, and I write merely from the chance of yourself, possessing great skill and having worked with good instruments, {not being} possibly fully aware what an astonishing difference the kind of microscope makes for those who have not been trained in skill for dissection under water. When next I come to town (I was prevented last time by illness) I must call on you, and report, for my own satisfaction, a really (I think) curious point I have made out in my beloved barnacles. You cannot tell how much I enjoyed my talk with you here.

Ever, my dear Owen, Yours sincerely, C. DARWIN.

P.S. — If I do not hear, I shall understand that my letter is superfluous. Smith and Beck were so pleased with the simple microscope they made for me, that they have made another as a model. If you are consulted by any young naturalists, do recommend them to look at this. I really feel quite a personal gratitude to this form of microscope, and quite a hatred to my old one.

LETTER 26. TO J.S. HENSLOW. Down {April 1st, 1848.}

Thank you for your note and giving me a chance of seeing you in town; but it was out of my power to take advantage of it, for I had previously arranged to go up to London on Monday. I should have much enjoyed seeing you. Thanks also for your address (26/1. An introductory lecture delivered in March 1848 at the first meeting of a Society "for giving instructions to the working classes in Ipswich in various branches of science, and more especially in natural history" ("Memoir of the Rev. J.S. Henslow," by Leonard Jenyns, page 150.), which I like very much. The anecdote about Whewell and the tides I had utterly forgotten; I believe it is near enough to the truth. I rather demur to one sentence of yours — viz., "However delightful any scientific pursuit may be, yet, if it should be wholly unapplied, it is of no more use than building castles in the air." Would not your hearers infer from this that the practical use of each scientific discovery ought to be immediate and obvious to make it worthy of admiration? What a beautiful instance chloroform is of a discovery made from purely scientific researches, afterwards coming almost by chance into practical use! For myself I would, however, take higher ground, for I believe there exists, and I feel within me, an instinct for truth, or knowledge or discovery, of something of the same nature as the instinct of virtue, and that our having such an instinct is reason enough for scientific researches without any practical results ever ensuing from them. You will wonder what makes me run on so, but I have been working very hard for the last eighteen months on the anatomy, etc., of the Cirripedia (on which I shall publish a monograph), and some of my friends laugh at me, and I fear the study of the Cirripedia will ever remain "wholly unapplied," and yet I feel that such study is better than castle-building.

LETTER 27. TO J.D. HOOKER, at Dr. Falconer's, Botanic Garden, Calcutta. Down, May 10th, 1848.

I was indeed delighted to see your handwriting; but I felt almost sorry when I beheld how long a letter you had written. I know that you are indomitable in work, but remember how precious your time is, and do not waste it on your friends, however much pleasure you may give them. Such a letter would have cost me half-a-day's work. How capitally you seem going on! I do envy you the sight of all the glorious vegetation. I am much pleased and surprised that you have been able to observe so much in the animal world. No doubt you keep a journal, and an excellent one it will be, I am sure, when published. All these animal facts will tell capitally in it. I can quite comprehend the difficulty you mention about not knowing what is known zoologically in India; but facts observed, as you will observe them, are none the worse for reiterating. Did you see Mr. Blyth in Calcutta? He would be a capital man to tell you what is known about Indian Zoology, at least in the Vertebrata. He is a very clever, odd, wild fellow, who will never do what he could do, from not sticking to any one subject. By the way, if you should see him at any time, try not to forget to remember me very kindly to him; I liked all I saw of him. Your letter was the very one to charm me, with all its facts for my Species-book, and truly obliged I am for so kind a remembrance of me. Do not forget to make enquiries about the origin, even if only traditionally known, of any varieties of domestic quadrupeds, birds, silkworms, etc. Are there domestic bees? if so hives ought to be brought home. Of all the facts you mention, that of the wild {illegible}, when breeding with the domestic, producing offspring somewhat sterile, is the most surprising: surely they must be different species. Most zoologists would absolutely disbelieve such a statement, and consider the result as a proof that they were distinct species. I do not go so far as that, but the case seems highly improbable. Blyth has studied the Indian Ruminantia. I have been much struck about what you say of lowland plants ascending mountains, but the alpine not descending. How I do hope you will get up some mountains in Borneo; how curious the result will be! By the way, I never heard from you what affinity the Maldive flora has, which is cruel, as you tempted me by making me guess. I sometimes groan over your Indian journey, when I think over all your locked up riches. When shall I see a memoir on Insular floras, and on the Pacific? What a grand subject Alpine floras of the world (27/1. Mr. William Botting Hemsley, F.R.S., of the Royal Gardens, Kew, is now engaged on a monograph of the high-level Alpine plants of the world.) would be, as far as known; and then you have never given a coup d'oeil on the similarity and dissimilarity of Arctic and Antarctic floras. Well, thank heavens, when you do come back you will be nolens volens a fixture. I am particularly glad you have been at the Coal; I have often since you went gone on maundering on the subject, and I shall never rest easy in Down churchyard without the problem be solved by some one before I die. Talking of dying makes me tell you that my confounded stomach is much the same; indeed, of late has been rather worse, but for the last year, I think, I have been able to do more work. I have done nothing besides the barnacles, except, indeed, a little theoretical paper on erratic boulders (27/2. "On the Transportal of Erratic Boulders from a Lower to a Higher Level" ("Quart. Journ. Geol. Soc." Volume IV., pages 315-23. 1848). In this paper Darwin favours the view that the transport of boulders was effected by coast-ice. An earlier paper entitled "Notes on the Effects produced by the ancient Glaciers of Caernarvonshire, and on the Boulders transported by floating Ice" ("Phil. Mag." 1842, page 352) is spoken of by Sir Archibald Geikie as standing "almost at the top of the long list of English contributions to the history of the Ice Age" ("Charles Darwin," "Nature" Series, page 23).), and Scientific Geological Instructions for the Admiralty Volume (27/3. "A manual of Scientific Enquiry, prepared for the use of Her Majesty's Navy, and adapted for Travellers in General." Edited by Sir John F.W. Herschel, Bart. Section VI. — Geology — by Charles Darwin. London, 1849. See "Life and Letters," pages 328-9.), which cost me some trouble. This work, which is edited by Sir J. Herschel, is a very good job, inasmuch as the captains of men-of-war will now see that the Admiralty cares for science, and so will favour naturalists on board. As for a man who is not scientific by nature, I do not believe instructions will do him any good; and if he be scientific and good for anything the instructions will be superfluous. I do not know who does the Botany; Owen does the Zoology, and I have sent him an account of my new simple microscope, which

I consider perfect, even better than yours by Chevalier. N.B. I have got a 1/8 inch object-glass, and it is grand. I have been getting on well with my beloved Cirripedia, and get more skilful in dissection. I have worked out the nervous system pretty well in several genera, and made out their ears and nostrils (27/4. For the olfactory sacs see Darwin's "Monograph of the Cirripedia," 1851, page 52.), which were quite unknown. I have lately got a bisexual cirripede, the male being microscopically small and parasitic within the sack of the female. I tell you this to boast of my species theory, for the nearest closely allied genus to it is, as usual, hermaphrodite, but I had observed some minute parasites adhering to it, and these parasites I now can show are supplemental males, the male organs in the hermaphrodite being unusually small, though perfect and containing zoosperms: so we have almost a polygamous animal, simple females alone being wanting. I never should have made this out, had not my species theory convinced me, that an hermaphrodite species must pass into a bisexual species by insensibly small stages; and here we have it, for the male organs in the hermaphrodite are beginning to fail, and independent males ready formed. But I can hardly explain what I mean, and you will perhaps wish my barnacles and species theory al Diavolo together. But I don't care what you say, my species theory is all gospel. We have had only one party here: viz., of the Lyells, Forbes, Owen, and Ramsay, and we both missed you and Falconer very much...I know more of your history than you will suppose, for Miss Henslow most goodnaturedly sent me a packet of your letters, and she wrote me so nice a little note that it made me quite proud. I have not heard of anything in the scientific line which would interest you. Sir H. De la Beche (27/5. The Presidential Address delivered by De la Beche before the Geological Society in 1848 ("Quart. Journ. Geol. Soc." Volume IV., "Proceedings," page xxi, 1848).) gave a very long and rather dull address; the most interesting part was from Sir J. Ross. Mr. Beete Jukes figured in it very prominently: it really is a very nice quality in Sir Henry, the manner in which he pushes forward his subordinates. Jukes has since read what was considered a very valuable paper. The man, not content with moustaches, now sports an entire beard, and I am sure thinks himself like Jupiter tonans. There was a short time since a not very creditable discussion at a meeting of the Royal Society, where Owen fell foul of Mantell with fury and contempt about belemnites. What wretched doings come from the order of fame; the love of truth alone would never make one man attack another bitterly. My paper is full, so I must wish you with all my heart farewell. Heaven grant that your health may keep good.

LETTER 28. TO J.S. HENSLOW. The Lodge, Malvern, May 6th, 1849.

Your kind note has been forwarded to me here. You will be surprised to hear that we all — children, servants, and all — have been here for nearly two months. All last autumn and winter my health grew worse and worse: incessant sickness, tremulous hands, and swimming head. I thought I was going the way of all flesh. Having heard of much success in some cases from the cold-water cure, I determined to give up all attempts to do anything and come here and put myself under Dr. Gully. It has answered to a considerable extent: my sickness much checked and considerable strength gained. Dr. G., moreover (and I hear he rarely speaks confidently), tells me he has little doubt but that he can cure me in the course of time — time, however, it will take. I have experienced enough to feel sure that the cold-water cure is a great and powerful agent and upsetter of all constitutional habits. Talking of habits, the cruel wretch has made me leave off snuff — that chief solace of life. We thank you most sincerely for your prompt and early invitation to Hitcham for the British Association for 1850 (28/1. The invitation was probably not for 1850, but for 1851, when the Association met at Ipswich.): if I am made well and strong, most gladly will I accept it; but as I have been hitherto, a drive every day of half a dozen miles would be more than I could stand with attending any of the sections. I intend going to Birmingham (28/2. The Association met at Birmingham in 1849.) if able; indeed, I am bound to attempt it, for I am honoured beyond all measure in being one of the Vice-Presidents. I am uncommonly glad you will be there; I fear, however, we shall not have any such charming trips as Nuneham and Dropmore. (28/3. In a letter to Hooker (October 12th, 1849) Darwin speaks of "that heavenly day at Dropmore." ("Life and Letters," I., page 379.)) We shall stay here till at least June 1st, perhaps till July 1st; and I shall have to go on with the aqueous treatment at home for several more months. One most singular effect of the treatment is that it induces in most people, and eminently in my case, the most complete stagnation of mind. I have ceased to think even of barnacles! I heard some time since from Hooker...How capitally he seems to have succeeded in all his enterprises! You must be very busy now. I happened to be thinking the other day over the Gamlingay trip to the Lilies of the Valley (28/4. The Lily of the Valley (Convallaria majalis) is recorded from Gamlingay by Professor Babington in his "Flora of Cambridgeshire," page 234. (London, 1860.)): ah, those were delightful days when one had no such organ as a stomach, only a mouth and the masticating appurtenances. I am very much surprised at what you say, that men are beginning to work in earnest {at} Botany. What a loss it will be for Natural History that you have ceased to reside all the year in Cambridge!

LETTER 29. TO J.F. ROYLE. Down, September 1st {184-?}.

I return you with very many thanks your valuable work. I am sure I have not lost any slip or disarranged the loose numbers. I have been interested by looking through the volumes, though I have not found quite so much as I had thought possible about the varieties of the Indian domestic animals and plants, and the attempts at introduction have been too recent for the effects (if any) of climate to have been developed. I have, however, been astonished and delighted at the evidence of the energetic attempts to do good by such numbers of people, and most of them evidently not personally interested in the result. Long may our rule flourish in India. I declare all the labour shown in these transactions is enough by itself to make one proud of one's countrymen...

LETTER 30. TO HUGH STRICKLAND.

(30/1. The first paragraph of this letter is published in the "Life and Letters," I., page 372, as part of a series of letters to Strickland, beginning at page 365, where a biographical note by Professor Newton is also given. Professor Newton wrote: "In 1841 he brought the subject of Natural History Nomenclature before the British Association, and prepared the code of rules for Zoological Nomenclature, now known by his name — the principles of which are very generally accepted." Mr. Darwin's reasons against appending the describer's name to that of the species are given in "Life and Letters," page 366. The present letter is of interest as giving additional details in regard to Darwin's difficulties.)

Down, February 10th {1849}.

I have again to thank you cordially for your letter. Your remarks shall fructify to some extent, and I will try to be more faithful to rigid virtue and priority; but as for calling Balanus "Lepas" (which I did not think of) I cannot do it, my pen won't write it — it is impossible. I have great hopes some of my difficulties will disappear, owing to wrong dates in Agassiz and to my having to run several genera into one; for I have as yet gone, in but few cases, to original sources. With respect to adopting my own notions in my Cirripedia book, I should not like to do so without I found others approved, and in some public way; nor indeed is it well adapted, as I can never recognise a species without I have the original specimen, which fortunately I have in many cases in the British Museum. Thus far I mean to adopt my notion, in never putting mihi or Darwin after my own species, and in the anatomical text giving no authors' names at all, as the systematic part will serve for those who want to know the history of the species as far as I can imperfectly work it out.

I have had a note from W. Thompson (30/2. Mr. Thompson is described in the preface to the Lepadidae as "the distinguished Natural Historian of Ireland.") this morning, and he tells me Ogleby has some scheme identical almost with mine. I feel pretty sure there is a growing general aversion to the appendage of author's name, except in cases where necessary. Now at this moment I have seen specimens ticketed with a specific name and no reference — such are hopelessly inconvenient; but I declare I would rather (as saving time) have a reference to some second systematic work than to the original author, for I have cases of this which hardly help me at all, for I know not where to look amongst endless periodical foreign papers. On the other hand, one can get hold of most systematic works and so follow up the scent, and a species does not long lie buried exclusively in a paper.

I thank you sincerely for your very kind offer of occasionally assisting me with your opinion, and I will not trespass much. I have a case, but {it is one} about which I am almost sure; and so to save you writing, if I conclude rightly, pray do not answer, and I shall understand silence as assent.

Olfers in 1814 made Lepas aurita Linn. into the genus Conchoderma; {Oken} in 1815 gave the name Branta to Lepas aurita and vittata, and by so doing he alters essentially Olfers' generic definition. Oken was right (as it turns out), and Lepas aurita and vittata must form together one genus. (30/3. In the "Monograph on the Cirripedia" (Lepadidae) the names used are Conchoderma aurita and virgata.) (I leave out of question a multitude of subsequent synonyms.) Now I suppose I must retain Conchoderma of Olfers. I cannot make out a precise rule in the "British Association Report" for this. When a genus is cut into two I see that the old name is retained for part and altered to it; so I suppose the definition may be enlarged to receive another species — though the cases are somewhat different. I should have had no doubt if Lepas aurita and vittata had been made into two genera, for then when run together the oldest of the two would have been retained. Certainly to put Conchoderma Olfers is not quite correct when applied to the two species, for such was not Olfers' definition and opinion. If I do not hear, I shall retain Conchoderma for the two species...

P.S. — Will you by silence give consent to the following?

Linnaeus gives no type to his genus Lepas, though L. balanus comes first. Several oldish authors have used Lepas exclusively for the pedunculate division, and the name has been given to the family and compounded in sub-generic names. Now, this shows that old authors attached the name Lepas more particularly to the pedunculate division. Now, if I were to use Lepas for Anatifera (30/4. Anatifera and Anatifa were used as generic names for what Linnaeus and Darwin called Lepas anatifera.) I should get rid of the difficulty of the second edition of Hill and of the difficulty of Anatifera vel Anatifa. Linnaeus's generic description is equally applicable to Anatifera and Balanus, though the latter stands first. Must the mere precedence rigorously outweigh the apparent opinion of many old naturalists? As for using Lepas in place of Balanus, I cannot. Every one will understand what is meant by Lepas Anatifera, so that convenience would be wonderfully thus suited. If I do not hear, I shall understand I have your consent.

LETTER 31. J.D. HOOKER TO CHARLES DARWIN.

(31/1. In the "Life and Letters," I., page 392, is a letter to Sir J.D. Hooker from Mr. Darwin, to whom the former had dedicated his "Himalayan Journals." Mr. Darwin there wrote: "Your letter, received this morning, has interested me extremely, and I thank you sincerely for telling me your old thoughts and aspirations." The following is the letter referred to, which at our request Sir Joseph has allowed us to publish.)

Kew, March 1st, 1854.

Now that my book (31/2. "Himalayan Journals," 2 volumes. London, 1854.) has been publicly acknowledged to be of some value, I feel bold to write to you; for, to tell you the truth, I have never been without a misgiving that the dedication might prove a very bad compliment, however kindly I knew you would receive it. The idea of the dedication has been present to me from a very early date: it was formed during the Antarctic voyage, out of love for your own "Journal," and has never deserted me since; nor would it, I think, had I never known more of you than by report and as the author of the said "Naturalist's Journal." Short of the gratification I felt in getting the book out, I know no greater than your kind, hearty acceptation of the dedication; and, had the reviewers gibbeted me, the dedication would alone have given me real pain. I have no wish to assume a stoical indifference to public opinion, for I am well alive to it, and the critics might have irritated me sorely, but they could never have caused me the regret that the association of your name with a bad book of mine would have.

You will laugh when I tell you that, my book out, I feel past the meridian of life! But you do not know how from my earliest childhood I nourished and cherished the desire to make a creditable journey in a new country, and write such a respectable account of its natural features as should give me a niche amongst the scientific explorers of the globe I inhabit, and hand my name down as a useful contributor of original matter. A combination of most rare advantages has enabled me to gain as much of my object as contents me, for I never wished to be greatest amongst you, nor did rivalry ever enter my thoughts. No ulterior object has ever been present to me in this pursuit. My ambition is fully gratified by the satisfactory completion of my task, and I am now happy to go on jog-trot at Botany till the end of my days — downhill, in one sense, all the way. I shall never have such another object to work for, nor shall I feel the want of it...As it is, the craving of thirty years is satisfied, and I now look back on life in a way I never could previously. There never was a past hitherto to me. The phantom was always in view; mayhap it is only a "ridiculus mus" after all, but it is big enough for me...

(PLATE: T.H. HUXLEY, 1857. Maull & Polyblank photo., Walker & Cockerell ph. sc.)

(32/1. The story of Huxley's life has been fully given in the interesting biography edited by Mr. Leonard Huxley. (32/2. "Life and Letters of Thomas Henry Huxley." London 1900.) Readers of this book and of the "Life and Letters of Charles Darwin" gain an insight into the relationship between this pair of friends to which any words of ours can add but little. Darwin realised to the full the essential strength of Mr. Huxley's nature; he knew, as all the world now knows, the delicate sense of honour of his friend, and he was ever inclined to lean on his guidance in practical matters, as on an elder brother. Of Mr. Huxley's dialectical and literary skill he was an enthusiastic admirer, and he never forgot what his theories owed to the fighting powers of his "general agent." (32/3. Ibid., I., page 171.) Huxley's estimate of Darwin is very interesting: he valued him most highly for what was so strikingly characteristic of himself — the love of truth. He spoke of finding in him "something bigger than ordinary humanity — an unequalled simplicity and directness of purpose

— a sublime unselfishness." (32/4. Ibid., II., page 94. Huxley is speaking of Gordon's death, and goes on: "Of all the people whom I have met with in my life, he and Darwin are the two in whom I have found," etc.) The same point of view comes out in Huxley's estimate of Darwin's mental power. (32/5. Ibid., II., page 39.) "He had a clear, rapid intelligence, a great memory, a vivid imagination, and what made his greatness was the strict subordination of all these to his love of truth." This, as an analysis of Darwin's mental equipment, seems to us incomplete, though we do not pretend to mend it. We do not think it is possible to dissect and label the complex qualities which go to make up that which we all recognise as genius. But, if we may venture to criticise, we would say that Mr. Huxley's words do not seem to cover that supreme power of seeing and thinking what the rest of the world had overlooked, which was one of Darwin's most striking characteristics. As throwing light on the quality of their friendship, we give below a letter which has already appeared in the "Life and Letters of T.H. Huxley," I., page 366. Mr. L. Huxley gives an account of the breakdown in health which convinced Huxley's friends that rest and relief from anxiety must be found for him. Mr. L. Huxley aptly remarks of the letter, "It is difficult to say whether it does more honour to him who sent it or to him who received it." (32/6. Huxley's "Life," I., page 366. Mr. Darwin left to Mr. Huxley a legacy of 1,000 pounds, "as a slight memorial of my lifelong affection and respect for him."))

LETTER 32. TO T.H. HUXLEY. Down, April 23rd, 1873.

My dear Huxley

I have been asked by some of your friends (eighteen in number) to inform you that they have placed, through Robarts, Lubbock & Co., the sum of 2,100 pounds to your account at your bankers. We have done this to enable you to get such complete rest as you may require for the re-establishment of your health; and in doing this we are convinced that we act for the public interest, as well as in accordance with our most earnest desires. Let me assure you that we are all your warm personal friends, and that there is not a stranger or mere acquaintance amongst us. If you could have heard what was said, or could have read what was, as I believe, our inmost thoughts, you would know that we all feel towards you, as we should to an honoured and much loved brother. I am sure that you will return this feeling, and will therefore be glad to give us the opportunity of aiding you in some degree, as this will be a happiness to us to the last day of our lives. Let me add that our plan occurred to several of your friends at nearly the same time and quite independently of one another.

My dear Huxley, Your affectionate friend, CHARLES DARWIN.

LETTER 33. TO T.H. HUXLEY.

(33/1. The following letter is one of the earliest of the long series addressed to Mr. Huxley.)

Down, April 23rd {1854}.

My dear Sir

I have got out all the specimens, which I have thought could by any possibility be of any use to you; but I have not looked at them, and know not what state they are in, but should be much pleased if they are of the smallest use to you. I enclose a catalogue of habitats: I thought my notes would have turned out of more use. I have copied out such few points as perhaps would not be apparent in preserved specimens. The bottle shall go to Mr. Gray on Thursday next by our weekly carrier.

I am very much obliged for your paper on the Mollusca (33/2. The paper of Huxley's is "On the Morphology of the Cephalous Mollusca, etc." ("Phil. Trans. R. Soc." Volume 143, Part I., 1853, page 29.)); I have read it all with much interest: but it would be ridiculous in me to make any remarks on a subject on which I am so utterly ignorant; but I can see its high importance. The discovery of the type or "idea" (33/3. Huxley defines his use of the word "archetype" at page 50: "All that I mean is the conception of a form embodying the most general propositions that can be affirmed respecting the Cephalous Mollusca, standing in the same relation to them as the diagram to a geometrical theorem, and like it, at once, imaginary and true.") (in your sense, for I detest the word as used by Owen, Agassiz & Co.) of each great class, I cannot doubt, is one of the very highest ends of Natural History; and certainly most interesting to the worker-out. Several of your remarks have interested me: I am, however, surprised at what you say versus "anamorphism" (33/4. The passage referred to is at page 63: "If, however, all Cephalous Mollusks...be only modifications by excess or defect of the parts of a definite archetype, then, I think, it follows as a necessary consequence, that no anamorphism takes place in this group. There is no progression from a lower to a higher type, but merely a more or less complete evolution of one type." Huxley seems to use the term anamorphism in a sense differing from that of some writers. Thus in Jourdan's "Dictionnaire des Termes Usites dans les Sciences Naturelles," 1834, it is defined as the production of an atypical form either by arrest or excess of development.), I should have thought that the archetype in imagination was always in some degree embryonic, and therefore capable {of} and generally undergoing further development.

Is it not an extraordinary fact, the great difference in position of the heart in different species of Cleodora? (33/5. A genus of Pteropods.) I am a believer that when any part, usually constant, differs considerably in different allied species that it will be found in some degree variable within the limits of the same species. Thus, I should expect that if great numbers of specimens of some of the species of Cleodora had been examined with this object in view, the position of the heart in some of the species would have been found variable. Can you aid me with any analogous facts?

I am very much pleased to hear that you have not given up the idea of noticing my cirripedial volume. All that I have seen since confirms everything of any importance stated in that volume — more especially I have been able rigorously to confirm in an anomalous species, by the clearest evidence, that the actual cellular contents of the ovarian tubes, by the gland-like action of a modified portion of the continuous tube, passes into the cementing stuff: in fact cirripedes make glue out of their own unformed eggs! (33/6. On Darwin's mistake in this point see "Life and Letters," III., page 2.)

Pray believe me, Yours sincerely, C. DARWIN.

I told the above case to Milne Edwards, and I saw he did not place the smallest belief in it.

LETTER 34. TO T.H. HUXLEY. Down, September 2nd, {1854}.

My second volume on the everlasting barnacles is at last published (34/1. "A Monograph of the Sub-class Cirripedia. II. The Balanidae, the Verrucidae." Ray Society, 1854.), and I will do myself the pleasure of sending you a copy to Jermyn Street next Thursday, as I have to send another book then to Mr. Baily.

And now I want to ask you a favour — namely, to answer me two questions. As you are so perfectly familiar with the doings, etc., of all Continental naturalists, I want you to tell me a few names of those whom you think would care for my volume. I do not mean in the light of puffing my book, but I want not to send copies to those who from other studies, age, etc., would view it as waste paper. From assistance rendered me, I consider myself bound to send copies to: (1) Bosquet of Maestricht, (2) Milne Edwards, (3) Dana, (4) Agassiz, (5) Muller, (6) W. Dunker of Hesse Cassel. Now I have five or six other copies to distribute, and will you be so very kind as to help me? I had thought of

Von Siebold, Loven, d'Orbigny, Kolliker, Sars, Kroyer, etc., but I know hardly anything about any of them.

My second question, it is merely a chance whether you can answer, — it is whether I can send these books or any of them (in some cases accompanied by specimens), through the Royal Society: I have some vague idea of having heard that the Royal Society did sometimes thus assist members.

I have just been reading your review of the "Vestiges" (34/2. In his chapter on the "Reception of the Origin of Species" ("Life and Letters," II., pages 188-9), Mr. Huxley wrote: "and the only review I ever have qualms of conscience about, on the ground of needless savagery, is one I wrote on the 'Vestiges.'" The article is in the "British and Foreign Medico-chirurgical Review," XIII., 1854, page 425. The "great man" referred to below is Owen: see Huxley's review, page 439, and Huxley's "Life." I., page 94.), and the way you handle a great Professor is really exquisite and inimitable. I have been extremely interested in other parts, and to my mind it is incomparably the best review I have read on the "Vestiges"; but I cannot think but that you are rather hard on the poor author. I must think that such a book, if it does no other good, spreads the taste for Natural Science.

But I am perhaps no fair judge, for I am almost as unorthodox about species as the "Vestiges" itself, though I hope not quite so unphilosophical. How capitally you analyse his notion about law. I do not know when I have read a review which interested me so much. By Heavens, how the blood must have gushed into the capillaries when a certain great man (whom with all his faults I cannot help liking) read it!

I am rather sorry you do not think more of Agassiz's embryological stages (34/3. See "Origin," Edition VI., page 310: also Letter 40, Note.), for though I saw how exceedingly weak the evidence was, I was led to hope in its truth.

LETTER 35. TO J.D. HOOKER. Down {1854}.

With respect to "highness" and "lowness," my ideas are only eclectic and not very clear. It appears to me that an unavoidable wish to compare all animals with men, as supreme, causes some confusion; and I think that nothing besides some such vague comparison is intended, or perhaps is even possible, when the question is whether two kingdoms such as the Articulata or Mollusca are the highest. Within the same kingdom I am inclined to think that "highest" usually means that form which has undergone most "morphological differentiation" from the common embryo or archetype of the class; but then every now and then one is bothered (as Milne Edwards has remarked) by "retrograde development," i.e., the mature animal having fewer and less important organs than its own embryo. The specialisation of parts to different functions, or "the division of physiological labour" (35/1. A slip of the pen for "physiological division of labour.") of Milne Edwards exactly agrees (and to my mind is the best definition, when it can be applied) with what you state is your idea in regard to plants. I do not think zoologists agree in any definite ideas on this subject; and my ideas are not clearer than those of my brethren.

LETTER 36. TO J.D. HOOKER. Down, July 2nd {1854}.

I have had the house full of visitors, and when I talk I can do absolutely nothing else; and since then I have been poorly enough, otherwise I should have answered your letter long before this, for I enjoy extremely discussing such points as those in your last note. But what a villain you are to heap gratuitous insults on my ELASTIC theory: you might as well call the virtue of a lady elastic, as the virtue of a theory accommodating in its favours. Whatever you may say, I feel that my theory does give me some advantages in discussing these points. But to business: I keep my notes in such a way, viz., in bulk, that I cannot possibly lay my hand on any reference; nor as far as the vegetable kingdom is concerned do I distinctly remember having read any discussion on general highness or lowness, excepting Schleiden (I fancy) on Compositae being highest. Ad. de Jussieu (36/1. "Monographie de la Famille des Malpighiacees," by Adrien de Jussieu, "Arch. du Museum." Volume III., page 1, 1843.), in "Arch. du Museum," Tome 3, discusses the value of characters of degraded flowers in the Malpighiaceae, but I doubt whether this at all concerns you. Mirbel somewhere has discussed some such question.

Plants lie under an enormous disadvantage in respect to such discussions in not passing through larval stages. I do not know whether you can distinguish a plant low from non-development from one low from degradation, which theoretically, at least, are very distinct. I must agree with Forbes that a mollusc may be higher than one articulate animal and lower than another; if one was asked which was highest as a whole, the Molluscan or Articulate Kingdom, I should look to and compare the highest in each, and not compare their archetypes (supposing them to be known, which they are not.)

But there are, in my opinion, more difficult cases than any we have alluded to, viz., that of fish — but my ideas are not clear enough, and I do not suppose you would care to hear what I obscurely think on this subject. As far as my elastic theory goes, all I care about is that very ancient organisms (when different from existing) should tend to resemble the larval or embryological stages of the existing.

I am glad to hear what you say about parallelism: I am an utter disbeliever of any parallelism more than mere accident. It is very strange, but I think Forbes is often rather fanciful; his "Polarity" (36/2. See Letter 41, Note.) makes me sick — it is like "magnetism" turning a table.

If I can think of any one likely to take your "Illustrations" (36/3. "Illustrations of Himalayan Plants from Drawings made by J.F. Cathcart." Folio, 1855.), I will send the advertisement. If you want to make up some definite number so as to go to press, I will put my name down with PLEASURE (and I hope and believe that you will trust me in saying so), though I should not in the course of nature subscribe to any horticultural work: — act for me.

LETTER 37. TO J.D. HOOKER. Down, {May} 29th, 1854.

I am really truly sorry to hear about your {health}. I entreat you to write down your own case, — symptoms, and habits of life, — and then consider your case as that of a stranger; and I put it to you, whether common sense would not order you to take more regular exercise and work your brain less. (N.B. Take a cold bath and walk before breakfast.) I am certain in the long run you would not lose time. Till you have a thoroughly bad stomach, you will not know the really great evil of it, morally, physically, and every way. Do reflect and act resolutely. Remember your troubled heart-action formerly plainly told how your constitution was tried. But I will say no more — excepting that a man is mad to risk health, on which everything, including his children's inherited health, depends. Do not hate me for this lecture. Really I am not surprised at your having some headache after Thursday evening, for it must have been no small exertion making an abstract of all that was said after dinner. Your being so engaged was a bore, for there were several things that I should have liked to have talked over with you. It was certainly a first-rate dinner, and I enjoyed it extremely, far more than I expected. Very far from disagreeing with me, my London visits have just lately taken to suit my stomach admirably; I begin to think that dissipation, high-living, with lots of claret, is what I want, and what I had during the last visit. We are going to act on this same principle, and in a very profligate manner have just taken a pair of season-tickets to see the Queen open the Crystal Palace. (37/1. Queen

Victoria opened the Crystal Palace at Sydenham on June 10th, 1854.) How I wish there was any chance of your being there! The last grand thing we were at together answered, I am sure, very well, and that was the Duke's funeral.

Have you seen Forbes' introductory lecture (37/2. Edward Forbes was appointed to a Professorship at Edinburgh in May, 1854.) in the "Scotsman" (lent me by Horner)? it is really ADMIRABLY done, though without anything, perhaps, very original, which could hardly be expected: it has given me even a higher opinion than I before had, of the variety and polish of his intellect. It is, indeed, an irreparable loss to London natural history society. I wish, however, he would not praise so much that old brown dry stick Jameson. Altogether, to my taste, it is much the best introductory lecture I have ever read. I hear his anniversary address is very good.

Adios, my dear Hooker; do be wise and good, and be careful of your stomach, within which, as I know full well, lie intellect, conscience, temper, and the affections.

LETTER 38. TO J.D. HOOKER. Down, December 2nd {1854}.

You are a pretty fellow to talk of funking the returning thanks at the dinner for the medal. (38/1. The Royal medal was given to Sir Joseph in 1854.) I heard that it was decidedly the best speech of the evening, given "with perfect fluency, distinctness, and command of language," and that you showed great self-possession: was the latter the proverbially desperate courage of a coward? But you are a pretty fellow to be so desperately afraid and then to make the crack speech. Many such an ordeal may you have to go through! I do not know whether Sir William {Hooker} would be contented with Lord Rosse's (38/2. President of the Royal Society 1848-54.) speech on giving you the medal; but I am very much pleased with it, and really the roll of what you have done was, I think, splendid. What a great pity he half spoiled it by not having taken the trouble just to read it over first. Poor Hofmann (38/3. August Wilhelm Hofmann, the other medallist of 1854.) came off in this respect even worse. It is really almost arrogant insolence against every one not an astronomer.

The next morning I was at a very pleasant breakfast party at Sir R. Inglis's. (38/4. Sir Robert Inglis, President of the British Association in 1847. Apparently Darwin was present at the afternoon meeting, but not at the dinner.) I have received, with very many thanks, the aberrant genera; but I have not had time to consider them, nor your remarks on Australian botanical geography.

LETTER 39. TO T.H. HUXLEY.

(39/1. The following letter shows Darwin's interest in the adjudication of the Royal medals. The year 1855 was the last during which he served on the Council of the Society. He had previously served in 1849-50.)

Down, March 31st, 1855.

I have thought and enquired much about Westwood, and I really think he amply deserves the gold medal. But should you think of some one with higher claim I am quite ready to give up. Indeed, I suppose without I get some one to second it, I cannot propose him.

Will you be so kind as to read the enclosed, and return it to me? Should I send it to Bell? That is, without you demur or convince me. I had thought of Hancock, a higher class of labourer; but, as far as I can weigh, he has not, as yet, done so much as Westwood. I may state that I read the whole "Classification" (39/2. Possibly Westwood's "Introduction to the Modern Classification of Insects" (1839).) before I was on the Council, and ever thought on the subject of medals. I fear my remarks are rather lengthy, but to do him justice I could not well shorten them. Pray tell me frankly whether the enclosed is the right sort of thing, for though I was once on the Council of the Royal, I never attended any meetings, owing to bad health.

With respect to the Copley medal (39/3. The Copley Medal was given to Lyell in 1858.), I have a strong feeling that Lyell has a high claim, but as he has had the Royal Medal I presume that it would be thought objectionable to propose him; and as I intend (you not objecting and converting me) to propose W. for the Royal, it would, of course, appear intolerably presumptuous to propose for the Copley also.

LETTER 40. TO T.H. HUXLEY. Down, June 10th, 1855.

Shall you attend the Council of the Royal Society on Thursday next? I have not been very well of late, and I doubt whether I can attend; and if I could do anything (pray conceal the scandalous fact), I want to go to the Crystal Palace to meet the Horners, Lyells, and a party. So I want to know whether you will speak for me most strongly for Barrande. You know better than I do his admirable labours on the development of trilobites, and his most important work on his Lower or Primordial Zone. I enclose an old note of Lyell's to show what he thinks. With respect to Dana, whom I also proposed, you know well his merits. I can speak most highly of his classificatory work on crustacea and his Geographical Distribution. His Volcanic Geology is admirable, and he has done much good work on coral reefs.

If you attend, do not answer this; but if you cannot be at the Council, please inform me, and I suppose I must, if I can, attend.

Thank you for your abstract of your lecture at the Royal Institution, which interested me much, and rather grieved me, for I had hoped things had been in a slight degree otherwise. (40/1. "On certain Zoological Arguments commonly adduced in favour of the hypothesis of the Progressive Development of Animal Life," Discourse, Friday, April 20, 1855: "Proceedings R.I." (1855). Published also in "Huxley's Scientific Memoirs." The lecturer dwelt chiefly on the argument of Agassiz, which he summarises as follows: "Homocercal fishes have in their embryonic state heterocercal tails; therefore heterocercality is, so far, a mark of an embryonic state as compared with homocercality, and the earlier heterocercal fish are embryonic as compared with the later homocercal." He shows that facts do not support this view, and concludes generally "that there is no real parallel between the successive forms assumed in the development of the life of the individual at present and those which have appeared at different epochs in the past.") I heard some time ago that before long I might congratulate you on becoming a married man. (40/2. Mr. Huxley was married July 21st, 1855.) From my own experience of some fifteen years, I am very sure that there is nothing in this wide world which more deserves congratulation, and most sincerely and heartily do I congratulate you, and wish you many years of as much happiness as this world can afford.

LETTER 41. TO J.D. HOOKER.

(41/1. The following letter illustrates Darwin's work on aberrant genera. In the "Origin," Edition I., page 429, he wrote: "The more aberrant any form is, the greater must be the number of connecting forms which, on my theory, have been exterminated and utterly lost. And we have some evidence of aberrant forms having suffered severely from extinction, for they are generally represented by extremely few species; and such species as do occur are generally very distinct from each other, which again implies extinction.")

Down, November 15th {1855?}.

In Schoenherr's Catalogue of Curculionidae (41/2. "Genera et Species Curculionidum." (C.J. Schoenherr: Paris, 1833-38.)), the 6,717 species are on an average 10.17 to a genus. Waterhouse (who knows the group well, and who has published on fewness of species in aberrant genera) has given me a list of 62 aberrant genera, and these have on an average 7.6 species; and if one single genus be removed (and which I cannot yet believe ought to be considered aberrant), then the 61 aberrant genera would have only 4.91 species on an average. I tested these results in another way. I found in Schoenherr 9 families, including only 11 genera, and these genera (9 of which were in Waterhouse's list) I found included only 3.36 species on an average.

This last result led me to Lindley's "Vegetable Kingdom," in which I found (excluding thallogens and acrogens) that the genera include each 10.46 species (how near by chance to the Curculionidae), and I find 21 orders including single genera, and these 21 genera have on average 7.95 species; but if Lindley is right that Erythroxylon (with its 75 species) ought to be amongst the Malpighiads, then the average would be only 4.6 per genus.

But here comes, as it appears to me, an odd thing (I hope I shall not quite weary you out). There are 29 other orders, each with 2 genera, and these 58 genera have on an average 15.07 species: this great number being owing to the 10 genera in the Smilaceae, Salicaceae (with 220 species), Begoniaceae, Balsaminaceae, Grossulariaceae, without which the remaining 48 genera have on an average only 5.91 species.

This case of the orders with only 2 genera, the genera notwithstanding having 15.07 species each, seems to me very perplexing and upsets, almost, the conclusion deducible from the orders with single genera.

I have gone higher, and tested the alliances with 1, 2, and 3 orders; and in these cases I find both the genera few in each alliance, and the species, less than the average of the whole kingdom, in each genus.

All this has amused me, but I daresay you will have a good sneer at me, and tell me to stick to my barnacles. By the way, you agree with me that sometimes one gets despondent — for instance, when theory and facts will not harmonise; but what appears to me even worse, and makes me despair, is, when I see from the same great class of facts, men like Barrande deduce conclusions, such as his "Colonies" (41/3. Lyell briefly refers to Barrande's Bohemian work in a letter (August 31st, 1856) to Fleming ("Life of Sir Charles Lyell," II., page 225): "He explained to me on the spot his remarkable discovery of a 'colony' of Upper Silurian fossils, 3,400 feet deep, in the midst of the Lower Silurian group. This has made a great noise, but I think I can explain away the supposed anomaly by, etc." (See Letter 40, Note.) and his agreement with E. de Beaumont's lines of Elevation, or such men as Forbes with his Polarity (41/4. Edward Forbes "On the Manifestation of Polarity in the Distribution of Organised Beings in Time" ("Edinburgh New Phil. Journal," Volume LVII., 1854, page 332). The author points out that "the maximum development of generic types during the Palaeozoic period was during its earlier epochs; that during the Neozoic period towards its later periods." Thus the two periods of activity are conceived to be at the two opposite poles of a sphere which in some way represents for him the system of Nature.); I have not a doubt that before many months are over I shall be longing for the most dishonest species as being more honest than the honestest theories. One remark more. If you feel any interest, or can get any one else to feel any interest on the aberrant genera question, I should think the most interesting way would be to take aberrant genera in any great natural family, and test the average number of species to the genera in that family.

How I wish we lived near each other! I should so like a talk with you on geographical distribution, taken in its greatest features. I have been trying from land productions to take a very general view of the world, and I should so like to see how far it agrees with plants.

LETTER 42. TO MRS. LYELL.

(42/1. Mrs. Lyell is a daughter of the late Mr. Leonard Horner, and widow of Lieut. — Col. Lyell, a brother of Sir Charles.)

Down, January 26th {1856}.

I shall be very glad to be of any sort of use to you in regard to the beetles. But first let me thank you for your kind note and offer of specimens to my children. My boys are all butterfly hunters; and all young and ardent lepidopterists despise, from the bottom of their souls, coleopterists.

The simplest plan for your end and for the good of entomology, I should think, would be to offer the collection to Dr. J.E. Gray for the British Museum on condition that a perfect set was made out for you. If the collection was at all valuable, I should think he would be very glad to have this done. Whether any third set would be worth making out would depend on the value of the collection. I do not suppose that you expect the insects to be named, for that would be a most serious labour. If you do not approve of this scheme, I should think it very likely that Mr. Waterhouse would think it worth his while to set a series for you, retaining duplicates for himself; but I say this only on a venture. You might trust Mr. Waterhouse implicitly, which I fear, as {illegible} goes, is more than can be said for all entomologists. I presume, if you thought of either scheme, Sir Charles Lyell could easily see the gentlemen and arrange it; but, if not, I could do so when next I come to town, which, however, will not be for three or four weeks.

With respect to giving your children a taste for Natural History, I will venture one remark — viz., that giving them specimens in my opinion would tend to destroy such taste. Youngsters must be themselves collectors to acquire a taste; and if I had a collection of English lepidoptera, I would be systematically most miserly, and not give my boys half a dozen butterflies in the year. Your eldest has the brow of an observer, if there be the least truth in phrenology. We are all better, but we have been of late a poor household.

LETTER 43. TO J.D. HOOKER. Down {1855}.

I should have less scruple in troubling you if I had any confidence what my work would turn out. Sometimes I think it will be good, at other times I really feel as much ashamed of myself as the author of the "Vestiges" ought to be of himself.

I know well that your kindness and friendship would make you do a great deal for me, but that is no reason that I should be unreasonable. I cannot and ought not to forget that all your time is employed in work certain to be valuable. It is superfluous in me to say that I enjoy exceedingly writing to you, and that your answers are of the greatest possible service to me. I return with many thanks the proof on Aquilegia (43/1. This seems to refer to the discussion on the genus Aquilegia in Hooker and Thomson's "Flora Indica," 1855, Volume I., Systematic Part, page 44. The authors' conclusion is that "all the European and many of the Siberian forms generally recognised belong to one very variable species." With regard to cirripedes, Mr. Darwin spoke of "certain just perceptible differences which blend together and constitute varieties and not species" ("Life and Letters," I., page 379).): it has interested me much. It is exactly like my barnacles; but for my particular purpose, most unfortunately, both Kolreuter and Gartner have worked chiefly on A. vulgaris and canadensis and atro-purpurea, and these are just the species that you seem not to have studied. N.B. Why do you not let me buy the Indian Flora? You are too magnificent.

Now for a short ride on my chief (at present) hobbyhorse, viz. aberrant genera. What you say under your remarks on Lepidodendron seems just the case that I want, to give some sort of evidence of what we both believe in, viz. how groups came to be anomalous or aberrant; and I think some sort of proof is required, for I do not believe very many naturalists would at all admit our view.

Thank you for the caution on large anomalous genera first catching attention. I do not quite agree with your "grave objection to the whole process," which is "that if you multiply the anomalous species by 100, and divide the normal by the same, you will then reverse the names..." For, to take an example, Ornithorhynchus and Echidna would not be less aberrant if each had a dozen (I do not say 100, because we have no such cases in the animal kingdom) species instead of one. What would really make these two genera less anomalous would be the creation of many genera and sub-families round and radiating from them on all sides. Thus if Australia were destroyed, Didelphys in S. America would be wonderfully anomalous (this is your case with Proteaceae), whereas now there are so many genera and little sub-families of Marsupiata that the group cannot be called aberrant or anomalous. Sagitta (and the earwig) is one of the most anomalous animals in the world, and not a bit the less because there are a dozen species. Now, my point (which, I think is a slightly new point of view) is, if it is extinction which has made the genus anomalous, as a general rule the same causes of extinction would allow the existence of only a few species in such genera. Whenever we meet (which will be on the 23rd {at the} Club) I shall much like to hear whether this strikes you as sound. I feel all the time on the borders of a circle of truism. Of course I could not think of such a request, but you might possibly: — if Bentham does not think the whole subject rubbish, ask him some time to pick out the dozen most anomalous genera in the Leguminosae, or any great order of which there is a monograph by which I could calculate the ordinary percentage of species to genera. I am the more anxious, as the more I enquire, the fewer are the cases in which it can be done. It cannot be done in birds, or, I fear, in mammifers. I doubt much whether in any other class of insects {other than Curculionidae}.

I saw your nice notice of poor Forbes in the "Gardeners' Chronicle," and I see in the "Athenaeum" a notice of meeting on last Saturday of his friends. Of course I shall wish to subscribe as soon as possible to any memorial...

I have just been testing practically what disuse does in reducing parts. I have made {skeletons} of wild and tame duck (oh the smell of well-boiled, high duck!), and I find the tame duck ought, according to scale of wild prototype, to have its two wings 360 grains in weight; but it has only 317, or 43 grains too little, or 1/7 of {its} own two wings too little in weight. This seems rather interesting to me. (43/2. On the conclusions drawn from these researches, see Mr. Platt Ball, "The Effects of Use and Disuse" (Nature Series), 1890, page 55. With regard to his pigeons, Darwin wrote, in November 1855: "I love them to that extent that I cannot bear to kill and skeletonise them.")

P.S. — I do not know whether you will think this worth reading over. I have worked it out since writing my letter, and tabulate the whole.

21 orders with 1 genus, having 7.95 species (or 4.6?).

29 orders with 2 genera, having 15.05 species on an average.

23 orders each with 3 genera, and these genera include on an average 8.2 species.

20 orders each with 4 genera, and these genera include on an average 12.2 species.

27 orders each with above 50 genera (altogether 4716 genera), and these genera on an average have 9.97 species.

From this I conclude, whether there be many or few genera in an order, the number of species in a genus is not much affected; but perhaps when {there is} only one genus in an order it will be affected, and this will depend whether the {genus} Erythroxylon be made a family of.

LETTER 44. TO J.D. HOOKER. Down, April 8th {1856}.

I have been particularly glad to get your splendid eloge of Lindley. His name had been lately passing through my head, and I had hoped that Miers would have proposed him for the Royal medal. I most entirely agree that the Copley (44/1. The late Professor Lindley never attained the honour of the Copley medal. The Royal medal was awarded to him in 1857.) is more appropriate, and I daresay he would not have valued the Royal. From skimming through many botanical books, and from often consulting the "Vegetable Kingdom," I had (ignorant as I am) formed the highest opinion of his claims as a botanist. If Sharpey will stick up strong for him, we should have some chance; but the natural sciences are but feebly represented in the Council. Sir P. Egerton, I daresay, would be strong for him. You know Bell is out. Now, my only doubt is, and I hope that you will consider this, that the natural sciences being weak on the Council, and (I fancy) the most powerful man in the Council, Col. S{abine}, being strong against Lindley, whether we should have any chance of succeeding. It would be so easy to name some eminent man whose name would be well-known to all the physicists. Would Lindley hear of and dislike being proposed for the Copley and not succeeding? Would it not be better on this view to propose him for the Royal? Do think of this. Moreover, if Lindley is not proposed for the Royal, I fear both Royal medals would go {to} physicists; for I, for one, should not like to propose another zoologist, though Hancock would be a very good man, and I fancy there would be a feeling against medals to two botanists. But for whatever Lindley is proposed, I will do my best. We will talk this over here.

LETTER 45. TO J.D. HOOKER. Down, May 9th {1856}.

...With respect to Huxley, I was on the point of speaking to Crawford and Strezlecki (who will be on Committee of the Athenaeum) when I bethought me of how Owen would look and what he would say. Cannot you fancy him, with slow and gentle voice, asking "Will Mr. Crawford tell me what Mr. Huxley has done, deserving this honour; I only know that he differs from, and disputes the authority of Cuvier, Ehrenberg, and Agassiz as of no weight at all." And when I began to tell Mr. Crawford what to say, I was puzzled, and could refer him only to some excellent papers in the "Phil. Trans." for which the medal had been awarded. But I doubt, with an opposing faction, whether this would be considered enough, for I believe real scientific merit is not thought enough, without the person is generally well known. Now I want to hear what you deliberately think on this head: it would be bad to get him proposed and then rejected; and Owen is very powerful.

LETTER 46. TO J.D. HOOKER. Down {1856}.

I have got the Lectures, and have read them. (46/1. The reference is presumably to the Royal Institution Lectures given in 1854-56. Those which we have seen — namely, those reprinted in the "Scientific Memoirs," Volume I. — "On the Common Plan of Animal Form," page 281; "On certain Zoological Arguments, etc." page 300; "On Natural History as Knowledge, Discipline, and Power," page 305, do not seem to us to contain anything likely to offend; but Falconer's attack in the "Ann. and Mag. of Nat. Hist." June 1856, on the last-named lecture, shows strong feeling. A reply by Mr. Huxley appeared in the July number of the same Journal. The most heretical discussion from a modern standpoint is at page 311, where he asks how it is conceivable that the bright colours of butterflies and shells or the elegant forms of Foraminifera can possibly be of service to their possessors; and it is this which especially struck Darwin, judging by the pencil notes on his copy of the Lecture.) Though I believe, as far as my knowledge goes, that Huxley is right, yet I think his tone very much too vehement, and I have ventured to say so in a note to Huxley. I had not thought of these lectures in relation to the Athenaeum (46/2. Mr. Huxley was in 1858 elected to the Athenaeum Club under Rule 2, which provides for the annual election of "a certain number of persons of distinguished eminence in science, literature, or the arts, or for public services."), but I am inclined quite to agree with you, and that we had better pause before anything is said...(N.B. I found Falconer very indignant at the manner in which Huxley treated Cuvier in his Royal Institution lectures; and I have gently told Huxley so.) I think we had better do nothing: to try in earnest to get a great naturalist into the Athenaeum and fail, is far worse than doing nothing.

How strange, funny, and disgraceful that nearly all (Faraday and Sir J. Herschel at least exceptions) our great men are in quarrels in couplets; it never struck me before...

LETTER 47. C. LYELL TO CHARLES DARWIN.

(47/1. In the "Life and Letters," II., page 72, is given a letter (June 16th, 1856) to Lyell, in which Darwin exhales his indignation over the "extensionists" who created continents ad libitum to suit the convenience of their theories. On page 74 a fuller statement of his views is given in a letter dated June 25th. We have not seen Lyell's reply to this, but his reply to Darwin's letter of June 16th is extant, and is here printed for the first time.)

53, Harley Street, London, June 17th, 1856.

I wonder you did not also mention D. Sharpe's paper (47/2. "On the Last Elevation of the Alps, etc." ("Quart. Journ. Geol. Soc." Volume XII., 1856, page 102.), just published, by which the Alps were submerged as far as 9,000 feet of their present elevation above the sea in the Glacial period and then since uplifted again. Without admitting this, you would probably convey the alpine boulders to the Jura by marine currents, and if so, make the Alps and Jura islands in the glacial sea. And would not the Glacial theory, as now very generally understood, immerse as much of Europe as I did in my original map of Europe, when I simply expressed all the area which at some time or other had been under water since the commencement of the Eocene period? I almost suspect the glacial submergence would exceed it.

But would not this be a measure of the movement in every other area, northern (arctic), antarctic, or tropical, during an equal period — oceanic or continental? For the conversion of sea into land would always equal the turning of much land into sea.

But all this would be done in a fraction of the Pliocene period; the Glacial shells are barely 1 per cent. extinct species. Multiply this by the older Pliocene and Miocene epochs.

You also forget an author who, by means of atolls, contrived to submerge archipelagoes (or continents?), the mountains of which must originally have differed from each other in height 8,000 (or 10,000?) feet, so that they all just rose to the surface at one level, or their sites are marked by buoys of coral. I could never feel sure whether he meant this tremendous catastrophe, all brought about by what Sedgwick called "Lyell's niggling operations," to have been effected during the era of existing species of corals.

Perhaps you can tell me, for I am really curious to know...(47/3. The author referred to is of course Darwin.)

Now, although there is nothing in my works to warrant the building up of continents in the Atlantic and Pacific even since the Eocene period, yet, as some of the rocks in the central Alps are in part Eocene, I begin to think that all continents and oceans may be chiefly, if not all, post-Eocene, and Dana's "Atlantic Ocean" of the Lower Silurian is childish (see the Anniversary Address, 1856). (47/4. Probably Dana's Anniversary Address to the "American Association for the Advancement of Science," published in the "Proceedings" 1856.) But how far you are at liberty to call up continents from "the vasty deep" as often as you want to convey a Helix from the United States to Europe in Miocene or Pliocene periods is a question; for the ocean is getting deeper of late, and Haughton says the mean depth is eleven miles! by his late paper on tides. (47/5. "On the Depth of the Sea deducible from Tidal Observations" ("Proc. Irish Acad." Volume VI., page 354, 1853-54).) I shall be surprised if this turns out true by soundings.

I thought your mind was expanding so much in regard to time that you would have been going ahead in regard to the possibility of mountain-chains being created in a fraction of the period required to convert a swan into a goose, or vice versa. Nine feet did the Rimutaka chain of New Zealand gain in height in January, 1855, and a great earthquake has occurred in New Zealand every seven years for half a century nearly.

The "Washingtonia" (Californian conifer) (47/6. Washingtonia, or Wellingtonia, better known as Sequoia. Asa Gray, writing in 1872, states his belief that "no Sequoia now alive can sensibly antedate the Christian era" ("Scientific Papers," II., page 144).) lately exhibited was four thousand years old, so that one individual might see a chain of hills rise, and rise with it, much {more} a species — and those islands which J. Hooker describes as covered with New Zealand plants three hundred (?) miles to the N.E. (?) of New Zealand may have been separated from the mainland two or three or four generations of Washingtonia ago.

If the identity of the land-shells of all the hundreds of British Isles be owing to their having been united since the Glacial period, and the discordance, almost total, of the shells of Porto Santo and Madeira be owing to their having been separated {during} all the newer and possibly older Pliocene periods, then it gives us a conception of time which will aid you much in your conversion of species, if immensity of time will do all you require; for the Glacial period is thus shown, as we might have anticipated, to be contemptible in duration or in distance from us, as compared to the older Pliocene, let alone the Miocene, when our contemporary species were, though in a minority, already beginning to flourish.

The littoral shells, according to MacAndrew, imply that Madeira and the Canaries were once joined to the mainland of Europe or Africa, but that those isles were disjoined so long ago that most of the species came in since. In short, the marine shells tell the same story as the land shells. Why do the plants of Porto Santo and Madeira agree so nearly? And why do the shells which are the same as European or African species remain quite unaltered, like the Crag species, which returned unchanged to the British seas after being expelled from them by glacial cold, when two millions (?) of years had elapsed, and after such migration to milder seas? Be so good as to explain all this in your next letter.

LETTER 48. TO J.D. HOOKER. Down, July 5th {1856}.

I write this morning in great tribulation about Tristan d'Acunha. (48/1. See "Flora Antarctica," page 216. Though Tristan d'Acunha is "only 1,000 miles distant from the Cape of Good Hope, and 3,000 from the Strait of Magalhaens, the botany of this island is far more intimately allied to that of Fuegia than Africa.") The more I reflect on your Antarctic flora the more I am astounded. You give all the facts so clearly and fully, that it is impossible to help speculating on the subject; but it drives me to despair, for I cannot gulp down your continent; and not being able to do so gives, in my eyes, the multiple creationists an awful triumph. It is a wondrous case, and how strange that A. De Candolle should have ignored it; which he certainly has, as it seems to me. I wrote Lyell a long geological letter (48/2. "Life and Letters," II., page 74.) about continents, and I have had a very long and interesting answer; but I cannot in the least gather his opinion about all your continental extensionists; and I have written again beseeching a verdict. (48/3. In the tenth edition of the "Principles," 1872, Lyell added a chapter (Chapter XLI., page 406) on insular floras and faunas in relation to the origin of species; he here (page 410) gives his reasons against Forbes as an extensionist.) I asked him to send to you my letter, for as it was well copied it would not be troublesome to read; but whether worth reading I really do not know; I have given in it the reasons which make me strongly opposed to continental extensions.

I was very glad to get your note some days ago: I wish you would think it worth while, as you intend to have the Laburnum case translated, to write to "Wien" (that unknown place) (48/4. There is a tradition that Darwin once asked Hooker where "this place Wien is, where they publish so many books."), and find out how the Laburnum has been behaving: it really ought to be known.

The Entada is a beast. (48/5. The large seeds of Entada scandens are occasionally floated across the Atlantic and cast on the shores of Europe.); I have never differed from you about the growth of a plant in a new island being a FAR harder trial than transportal, though certainly that seems hard enough. Indeed I suspect I go even further than you in this respect; but it is too long a story.

Thank you for the Aristolochia and Viscum cases: what species were they? I ask, because oddly these two very genera I have seen advanced as instances (I forget at present by whom, but by good men) in which the agency of insects was absolutely necessary for impregnation. In our British dioecious Viscum I suppose it must be necessary. Was there anything to show that the stigma was ready for pollen in these two cases? for it seems that there are many cases in which pollen is shed long before the stigma is ready. As in our Viscum, insects carry, sufficiently regularly for impregnation, pollen from flower to flower, I should think that there must be occasional crosses even in an hermaphrodite Viscum. I have never heard of bees and butterflies, only moths, producing fertile eggs without copulation.

With respect to the Ray Society, I profited so enormously by its publishing my Cirrepedia, that I cannot quite agree with you on confining it to translations; I know not how else I could possibly have published.

I have just sent in my name for 20 pounds to the Linnaean Society, but I must confess I have done it with heavy groans, whereas I daresay you gave your 20 pounds like a light-hearted gentleman...

P.S. Wollaston speaks strongly about the intermediate grade between two varieties in insects and mollusca being often rarer than the two varieties themselves. This is obviously very important for me, and not easy to explain. I believe I have had cases from you. But, if you believe in this, I wish you would give me a sentence to quote from you on this head. There must, I think, be a good deal of truth in it; otherwise there could hardly be nearly distinct varieties under any species, for we should have instead a blending series, as in brambles and willows.

LETTER 49. TO J.D. HOOKER. July 13th, 1856.

What a book a devil's chaplain might write on the clumsy, wasteful, blundering, low, and horribly cruel works of nature! With respect to crossing, from one sentence in your letter I think you misunderstand me. I am very far from believing in hybrids: only in crossing of the same species or of close varieties. These two or three last days I have been observing wheat, and have convinced myself that L. Deslongchamps is in error about impregnation taking place in closed flowers; i.e., of course, I can judge only from external appearances. By the way, R. Brown once told me that the use of the brush on stigma of grasses was unknown. Do you know its use?..

You say most truly about multiple creations and my notions. If any one case could be proved, I should be smashed; but as I am writing my book, I try to take as much pains as possible to give the strongest cases opposed to me, and often such conjectures as occur to me. I have been working your books as the richest (and vilest) mine against me; and what hard work I have had to get up your New Zealand Flora! As I have to quote you so often, I should like to refer to Muller's case of the Australian Alps. Where is it published? Is it a book? A correct reference would be enough for me, though it is wrong even to quote without looking oneself. I should like to see very much Forbes's sheets, which you refer to; but I must confess (I hardly know why) I have got rather to mistrust poor dear Forbes.

There is wonderful ill logic in his famous and admirable memoir on distribution, as it appears to me, now that I have got it up so as to give the heads in a page. Depend on it, my saying is a true one — viz. that a compiler is a great man, and an original man a commonplace man. Any fool can generalise and speculate; but oh, my heavens, to get up at second hand a New Zealand Flora, that is work...

And now I am going to beg almost as great a favour as a man can beg of another: and I ask some five or six weeks before I want the favour done, that it may appear less horrid. It is to read, but well copied out, my pages (about forty!!) on Alpine floras and faunas, Arctic and Antarctic floras and faunas, and the supposed cold mundane period. It would be really an enormous advantage to me, as I am sure otherwise to make botanical blunders. I would specify the few points on which I most want your advice. But it is quite likely that you may object on the ground that you might be publishing before me (I hope to publish in a year at furthest), so that it would hamper and bother you; and secondly you may object to the loss of time, for I daresay it would take an hour and a half to read. It certainly would be of immense advantage to me; but of course you must not think of doing it if it would interfere with your own work.

I do not consider this request in futuro as breaking my promise to give no more trouble for some time.

From Lyell's letters, he is coming round at a railway pace on the mutability of species, and authorises me to put some sentences on this head in my preface.

I shall meet Lyell on Wednesday at Lord Stanhope's, and will ask him to forward my letter to you; though, as my arguments have not struck him, they cannot have force, and my head must be crotchety on the subject; but the crotchets keep firmly there. I have given your opinion on continuous land, I see, too strongly.

LETTER 50. TO S.P. WOODWARD. Down, July 18th {1856}.

Very many thanks for your kindness in writing to me at such length, and I am glad to say for your sake that I do not see that I shall have to beg any further favours. What a range and what a variability in the Cyrena! (50/1. A genus of Lamellibranchs ranging from the Lias to the present day.) Your list of the ranges of the land and fresh-water shells certainly is most striking and curious, and especially as the antiquity of four of them is so clearly shown.

I have got Harvey's seaside book, and liked it; I was not particularly struck with it, but I will re-read the first and last chapters.

I am growing as bad as the worst about species, and hardly have a vestige of belief in the permanence of species left in me; and this confession will make you think very lightly of me, but I cannot help it. Such has become my honest conviction, though the difficulties and arguments against such heresy are certainly most weighty.

LETTER 51. TO C. LYELL. November 10th {1856}.

I know you like all cases of negative geological evidence being upset. I fancied that I was a most unwilling believer in negative evidence; but yet such negative evidence did seem to me so strong that in my "Fossil Lepadidae" I have stated, giving reasons, that I did not believe there could have existed any sessile cirripedes during the Secondary ages. Now, the other day Bosquet of Maestricht sends me a perfect drawing of a perfect Chthamalus (a recent genus) from the Chalk! (51/1. Chthamalus, a genus of Cirripedia. ("A Monograph on the Sub-class Cirripedia," by Charles Darwin, page 447. London, 1854.) A fossil species of this genus of Upper Cretaceous age was named by Bosquet Chthamalus Darwini. See "Origin," Edition VI., page 284; also Zittel, "Traite de Paleontologie," Traduit par Dr. C. Barrois, Volume II., page 540, figure 748. Paris, 1887.) Indeed, it is stretching a point to make it specifically distinct from our living British species. It is a genus not hitherto found in any Tertiary bed.

LETTER 52. TO T.H. HUXLEY. Down, July 9th, 1857.

I am extremely much obliged to you for having so fully entered on my point. I knew I was on unsafe ground, but it proves far unsafer than I had thought. I had thought that Brulle (52/1. This no doubt refers to A. Brulle's paper in the "Comptes rendus" 1844, of which a translation is given in the "Annals and Mag. of Natural History," 1844, page 484. In speaking of the development of the Articulata, the author says "that the appendages are manifested at an earlier period of the existence of an Articulate animal the more complex its degree of organisation, and vice versa that they make their appearance the later, the fewer the number of transformations which it has to undergo.") had a wider basis for his generalisation, for I made the extract several years ago, and I presume (I state it as some excuse for myself) that I doubted it, for, differently from my general habit, I have not extracted his grounds. It was meeting with Barneoud's paper which made me think there might be truth in the doctrine. (52/2. Apparently Barneoud "On the Organogeny of Irregular Corollas," from the "Comptes rendus," 1847, as given in "Annals and Mag. of Natural History," 1847, page 440. The paper chiefly deals with the fact that in their earliest condition irregular flowers are regular. The view attributed to Barneoud does not seem so definitely given in this paper as in a previous one ("Ann. Sc. Nat." Bot., Tom. VI., page 268.) Your instance of heart and brain of fish seems to me very good. It was a very stupid blunder on my part not thinking of the posterior part of the time of development. I shall, of course, not allude to this subject, which I rather grieve about, as I wished it to be true; but, alas! a scientific man ought to have no wishes, no affections — a mere heart of stone.

There is only one point in your letter which at present I cannot quite follow you in: supposing that Barneoud's (I do not say Brulle's) remarks were true and universal — i.e., that the petals which have to undergo the greatest amount of development and modification begin to change the soonest from the simple and common embryonic form of the petal — if this were a true law, then I cannot but think that it would throw light on Milne

Edwards' proposition that the wider apart the classes of animals are, the sooner do they diverge from the common embryonic plan — which common embryonic {plan} may be compared with the similar petals in the early bud, the several petals in one flower being compared to the distinct but similar embryos of the different classes. I much wish that you would so far keep this in mind, that whenever we meet I might hear how far you differ or concur in this. I have always looked at Barneoud's and Brulle's proposition as only in some degree analogous.

P.S. I see in my abstract of Milne Edwards' paper, he speaks of "the most perfect and important organs" as being first developed, and I should have thought that this was usually synonymous with the most developed or modified.

LETTER 53. TO J.D. HOOKER.

(53/1. The following letter is chiefly of interest as showing the amount and kind of work required for Darwin's conclusions on "large genera varying," which occupy no more than two or three pages in the "Origin" (Edition I., page 55). Some correspondence on the subject is given in the "Life and Letters," II., pages 102-5.)

Down, August 22nd {1857}.

Your handwriting always rejoices the cockles of my heart; though you have no reason to be "overwhelmed with shame," as I did not expect to hear.

I write now chiefly to know whether you can tell me how to write to Hermann Schlagenheit (is this spelt right?) (53/2. Schlagintweit.), for I believe he is returned to England, and he has poultry skins for me from W. Elliot of Madras.

I am very glad to hear that you have been tabulating some Floras about varieties. Will you just tell me roughly the result? Do you not find it takes much time? I am employing a laboriously careful schoolmaster, who does the tabulating and dividing into two great cohorts, more carefully than I can. This being so, I should be very glad some time to have Koch, Webb's Canaries, and Ledebour, and Grisebach, but I do not know even where Rumelia is. I shall work the British flora with three separate Floras; and I intend dividing the varieties into two classes, as Asa Gray and Henslow give the materials, and, further, A. Gray and H.C. Watson have marked for me the forms, which they consider real species, but yet are very close to others; and it will be curious to compare results. If it will all hold good it is very important for me; for it explains, as I think, all classification, i.e. the quasi-branching and sub-branching of forms, as if from one root, big genera increasing and splitting up, etc., as you will perceive. But then comes in, also, what I call a principle of divergence, which I think I can explain, but which is too long, and perhaps you would not care to hear. As you have been on this subject, you might like to hear what very little is complete (for my schoolmaster has had three weeks' holidays) — only three cases as yet, I see.

BABINGTON — British Flora.

593 species in genera of 5 and upwards have in a thousand species presenting vars. 134/1000.*	593 (odd chance equal) in genera of 3 and downwards have in a thousand presenting vars. 37/1000.

(*53/3. This sentence may be interpreted as follows: The number of species which present varieties are 134 per thousand in genera of 5 species and upwards. The result is obtained from tabulation of 593 species.)

HOOKER — New Zealand.

Genera with 4 species and upwards, 150/1000.	With 3 species and downwards 114/1000.

GODRON — Central France.

With 5 species and upwards 160/1000.	With 3 species and downwards 105/1000.

I do not enter into details on omitting introduced plants and very varying genera, as Rubus, Salix, Rosa, etc., which would make the result more in favour.

I enjoyed seeing Henslow extremely, though I was a good way from well at the time. Farewell, my dear Hooker: do not forget your visit here some time.

LETTER 54. TO J.D. HOOKER. Down, November 14th {1857}.

On Tuesday I will send off from London, whither I go on that day, Ledebour's three remaining volumes, Grisebach and Cybele, i.e., all that I have, and most truly am I obliged to you for them. I find the rule, as yet, of the species varying most in the large genera universal, except in Miquel's very brief and therefore imperfect list of the Holland flora, which makes me very anxious to tabulate a fuller flora of Holland. I shall remain in London till Friday morning, and if quite convenient to send me two volumes of D.C. Prodromus, I could take them home and tabulate them. I should think a volume with a large best known natural family, and a volume with several small broken families would be best, always supposing that the varieties are conspicuously marked in both. Have you the volume published by Lowe on Madeira? If so and if any varieties are marked I should much like to see it, to see if I can make out anything about habitats of vars. in so small an area — a point on which I have become very curious. I fear there is no chance of your possessing Forbes and Hancock "British Shells," a grand work, which I much wish to tabulate.

Very many thanks for seed of Adlumia cirrhosa, which I will carefully observe. My notice in the G. Ch. on Kidney Beans (54.1 "On the Agency of Bees in the Fertilisation of Papilionaceous Flowers" ("Gardeners' Chronicle," 1857, page 725).) has brought me a curious letter from an intelligent gardener, with a most remarkable lot of beans, crossed in a marvellous manner IN THE FIRST GENERATION, like the peas sent to you by

Berkeley and like those experimentalised on by Gartner and by Wiegmann. It is a very odd case; I shall sow these seeds and see what comes up. How very odd that pollen of one form should affect the outer coats and size of the bean produced by pure species!..

LETTER 55. TO J.D. HOOKER. Down {1857?}.

You know how I work subjects: namely, if I stumble on any general remark, and if I find it confirmed in any other very distinct class, then I try to find out whether it is true, — if it has any bearing on my work. The following, perhaps, may be important to me. Dr. Wight remarks that Cucurbitaceae (55/1. Wight, "Remarks on the Fruit of the Natural Order Cucurbitaceae" ("Ann. Mag. Nat. Hist." VIII., page 261). R. Wight, F.R.S. (1796-1872) was Superintendent of the Madras Botanic Garden.) is a very isolated family, and has very diverging affinities. I find, strongly put and illustrated, the very same remark in the genera of hymenoptera. Now, it is not to me at first apparent why a very distinct and isolated group should be apt to have more divergent affinities than a less isolated group. I am aware that most genera have more affinities than in two ways, which latter, perhaps, is the commonest case. I see how infinitely vague all this is; but I should very much like to know what you and Mr. Bentham (if he will read this), who have attended so much to the principles of classification, think of this. Perhaps the best way would be to think of half a dozen most isolated groups of plants, and then consider whether the affinities point in an unusual number of directions. Very likely you may think the whole question too vague to be worth consideration.

LETTER 56. TO J.D. HOOKER. Down, April 8th {1857}.

I now want to ask your opinion, and for facts on a point; and as I shall often want to do this during the next year or two, so let me say, once for all, that you must not take trouble out of mere good nature (of which towards me you have a most abundant stock), but you must consider, in regard to the trouble any question may take, whether you think it worth while — as all loss of time so far lessens your original work — to give me facts to be quoted on your authority in my work. Do not think I shall be disappointed if you cannot spare time; for already I have profited enormously from your judgment and knowledge. I earnestly beg you to act as I suggest, and not take trouble solely out of good-nature.

My point is as follows: Harvey gives the case of Fucus varying remarkably, and yet in same way under most different conditions. D. Don makes same remark in regard to Juncus bufonius in England and India. Polygala vulgaris has white, red, and blue flowers in Faroe, England, and I think Herbert says in Zante. Now such cases seem to me very striking, as showing how little relation some variations have to climatal conditions.

Do you think there are many such cases? Does Oxalis corniculata present exactly the same varieties under very different climates?

How is it with any other British plants in New Zealand, or at the foot of the Himalaya? Will you think over this and let me hear the result?

One other question: do you remember whether the introduced Sonchus in New Zealand was less, equally, or more common than the aboriginal stock of the same species, where both occurred together? I forget whether there is any other case parallel with this curious one of the Sonchus...

I have been making good, though slow, progress with my book, for facts have been falling nicely into groups, enlightening each other.

LETTER 57. TO T.H. HUXLEY. Moor Park, Farnham, Surrey {1857?}.

Your letter has been forwarded to me here, where I am profiting by a few weeks' rest and hydropathy. Your letter has interested and amused me much. I am extremely glad you have taken up the Aphis (57/1. Professor Huxley's paper on the organic reproduction of Aphis is in the "Trans. Linn. Soc." XXII. (1858), page 193. Prof. Owen had treated the subject in his introductory Hunterian lecture "On Parthenogenesis" (1849). His theory cannot be fully given here. Briefly, he holds that parthenogenesis is due to the inheritance of a "remnant of spermatic virtue": when the "spermatic force" or "virtue" is exhausted fresh impregnation occurs. Huxley severely criticises both Owen's facts and his theory.) question, but, for Heaven's sake, do not come the mild Hindoo (whatever he may be) to Owen; your father confessor trembles for you. I fancy Owen thinks much of this doctrine of his; I never from the first believed it, and I cannot but think that the same power is concerned in producing aphides without fertilisation, and producing, for instance, nails on the amputated stump of a man's fingers, or the new tail of a lizard. By the way, I saw somewhere during the last week or so a statement of a man rearing from the same set of eggs winged and wingless aphides, which seemed new to me. Does not some Yankee say that the American viviparous aphides are winged? I am particularly glad that you are ruminating on the act of fertilisation: it has long seemed to me the most wonderful and curious of physiological problems. I have often and often speculated for amusement on the subject, but quite fruitlessly. Do you not think that the conjugation of the Diatomaceae will ultimately throw light on the subject? But the other day I came to the conclusion that some day we shall have cases of young being produced from spermatozoa or pollen without an ovule. Approaching the subject from the side which attracts me most, viz., inheritance, I have lately been inclined to speculate, very crudely and indistinctly, that propagation by true fertilisation will turn out to be a sort of mixture, and not true fusion, of two distinct individuals, or rather of innumerable individuals, as each parent has its parents and ancestors. I can understand on no other view the way in which crossed forms go back to so large an extent to ancestral forms. But all this, of course, is infinitely crude. I hope to be in London in the course of this month, and there are two or three points which, for my own sake, I want to discuss briefly with you.

LETTER 58. TO T.H. HUXLEY. Down, September 26th {1857}.

Thanks for your very pleasant note. It amuses me to see what a bug-bear I have made myself to you; when having written some very pungent and good sentence it must be very disagreeable to have my face rise up like an ugly ghost. (58/1. This probably refers to Darwin's wish to moderate a certain pugnacity in Huxley.) I have always suspected Agassiz of superficiality and wretched reasoning powers; but I think such men do immense good in their way. See how he stirred up all Europe about glaciers. By the way, Lyell has been at the glaciers, or rather their effects, and seems to have done good work in testing and judging what others have done...

In regard to classification and all the endless disputes about the "Natural System," which no two authors define in the same way, I believe it ought, in accordance to my heterodox notions, to be simply genealogical. But as we have no written pedigrees you will, perhaps, say this will not help much; but I think it ultimately will, whenever heterodoxy becomes orthodoxy, for it will clear away an immense amount of rubbish about the value of characters, and will make the difference between analogy and homology clear. The time will come, I believe, though I shall not live to see it, when we shall have very fairly true genealogical trees of each great kingdom of Nature.

LETTER 59. TO T.H. HUXLEY. Down, December 16th {1857}.

In my opinion your Catalogue (59/1. It appears from a letter to Sir J.D. Hooker (December 25th, 1857) that the reference is to the proofs of Huxley's "Explanatory Preface to the Catalogue of the Palaeontological Collection in the Museum of Practical Geology," by T.H. Huxley and R. Etheridge, 1865. Mr. Huxley appends a note at page xlix: "It should be noted that these pages were written before the appearance of Mr. Darwin's book on 'The Origin of Species' — a work which has effected a revolution in biological speculation.") is simply the very best resume, by far, on the whole science of Natural History, which I have ever seen. I really have no criticisms: I agree with every word. Your metaphors and explanations strike me as admirable. In many parts it is curious how what you have written agrees with what I have been writing, only with the melancholy difference for me that you put everything in twice as striking a manner as I do. I append, more for the sake of showing that I have attended to the whole than for any other object, a few most trivial criticisms.

I was amused to meet with some of the arguments, which you advanced in talk with me, on classification; and it pleases me, {that} my long proses were so far not thrown away, as they led you to bring out here some good sentences. But on classification (59/2. This probably refers to Mr. Huxley's discussion on "Natural Classification," a subject hardly susceptible of fruitful treatment except from an evolutionary standpoint.) I am not quite sure that I yet wholly go with you, though I agree with every word you have here said. The whole, I repeat, in my opinion is admirable and excellent.

LETTER 60. TO J.D. HOOKER. Down, February 28th {1858}.

Hearty thanks for De Candolle received. I have put the big genera in hand. Also many thanks for your valuable remarks on the affinities of the species in great genera, which will be of much use to me in my chapter on classification. Your opinion is what I had expected from what little I knew, but I much wanted it confirmed, and many of your remarks were more or less new to me and all of value.

You give a poor picture of the philosophy of Botany. From my ignorance, I suppose, I can hardly persuade myself that things are quite as bad as you make them, — you might have been writing remarks on Ornithology! I shall meditate much on your remarks, which will also come in very useful when I write and consider my tables of big and small genera. I grieve for myself to say that Watson agrees with your view, but with much doubt. I gave him no guide what your opinion was. I have written to A. Gray and to X., who — i.e. the latter — on this point may be looked at as S. Smith's Foolometer.

I am now working several of the large local Floras, with leaving out altogether all the smallest genera. When I have done this, and seen what the sections of the largest genera say, and seen what the results are of range and commonness of varying species, I must come to some definite conclusion whether or not entirely to give up the ghost. I shall then show how my theory points, how the facts stand, then state the nature of your grievous assault and yield entirely or defend the case as far as I can honestly.

Again I thank you for your invaluable assistance. I have not felt the blow {Hooker's criticisms} so much of late, as I have been beyond measure interested on the constructive instinct of the hive-bee. Adios, you terrible worrier of poor theorists!

LETTER 61. TO J.D. HOOKER. Down {1858?}

Many thanks for Ledebour and still more for your letter, with its admirable resume of all your objections. It is really most kind of you to take so very much trouble about what seems to you, and probably is, mere vagaries.

I will earnestly try and be cautious. I will write out my tables and conclusion, and (when well copied out) I hope you will be so kind as to read it. I will then put it by and after some months look at it with fresh eyes. I will briefly work in all your objections and Watson's. I labour under a great difficulty from feeling sure that, with what very little systematic work I have done, small genera were more interesting and therefore more attracted my attention.

One of your remarks I do not see the bearing of under your point of view — namely, that in monotypic genera "the variation and variability" are "much more frequently noticed" than in polytypic genera. I hardly like to ask, but this is the only one of your arguments of which I do not see the bearing; and I certainly should be very glad to know. I believe I am the slowest (perhaps the worst) thinker in England; and I now consequently fully admit the full hostility of Urticaceae, which I will give in my tables.

I will make no remarks on your objections, as I do hope you will read my MS., which will not cost you much trouble when fairly copied out. From my own experience, I hardly believe that the most sagacious observers, without counting, could have predicted whether there were more or fewer recorded varieties in large or small genera; for I found, when actually making the list, that I could never strike a balance in my mind, — a good many varieties occurring together, in small or in large genera, always threw me off the balance...

P.S. — I have just thought that your remark about the much variation of monotypic genera was to show me that even in these, the smallest genera, there was much variability. If this be so, then do not answer; and I will so understand it.

LETTER 62. TO J.D. HOOKER. February 23rd {1858}.

Will you think of some of the largest genera with which you are well acquainted, and then suppose 4/5 of the species utterly destroyed and unknown in the sections (as it were) as much as possible in the centre of such great genera. Then would the remaining 1/5 of the species, forming a few sections, be, according to the general practice of average good Botanists, ranked as distinct genera? Of course they would in that case be closely related genera. The question, in fact, is, are all the species in a gigantic genus kept together in that genus, because they are really so very closely similar as to be inseparable? or is it because no chasms or boundaries can be drawn separating the many species? The question might have been put for Orders.

LETTER 63. TO J.D. HOOKER. Down, February 9th {1858}.

I should be very much obliged for your opinion on the enclosed. You may remember in the three first volumes tabulated, all orders went right except Labiatae. By the way, if by any extraordinary chance you have not thrown away the scrap of paper with former results, I wish you would return it, for I have lost my copy, and I shall have all the division to do again; but DO NOT hunt for it, for in any case I should have gone over the calculation again.

Now I have done the three other volumes. You will see that all species in the six volumes together go right, and likewise all orders in the three last volumes, except Verbenaceae. Is not Verbenaceae very closely allied to Labiatae? If so, one would think that it was not mere chance, this coincidence. The species in Labiatae and Verbenaceae together are between 1/5 and 1/6 of all the species (15,645), which I have now tabulated.

Now, bearing in mind the many local Floras which I have tabulated (belting the whole northern hemisphere), and considering that they (and authors of D.C. Prodromus) would probably take different degrees of care in recording varieties, and the genera would be divided on different principles by different men, etc., I am much surprised at the uniformity of the result, and I am satisfied that there must be truth in the rule that the small genera vary less than the large. What do you think? Hypothetically I can conjecture how the Labiatae might fail —

namely, if some small divisions of the Order were now coming into importance in the world and varying much and making species. This makes me want to know whether you could divide the Labiatae into a few great natural divisions, and then I would tabulate them separately as sub-orders. I see Lindley makes so many divisions that there would not be enough in each for an average. I send the table of the Labiatae for the chance of your being able to do this for me. You might draw oblique lines including and separating both large and small genera. I have also divided all the species into two equal masses, and my rule holds good for all the species in a mass in the six volumes; but it fails in several (four) large Orders — viz. Labiatae, Scrophulariaceae, Acanthaceae, and Proteaceae. But, then, when the species are divided into two almost exactly equal divisions, the divisions with large genera are so very few: for instance, in Solanaceae, Solanum balances all others. In Labiatae seven gigantic genera balance all others (viz. 113), and in Proteaceae five genera balance all others. Now, according to my hypothetical notions, I am far from supposing that all genera go on increasing forever, and therefore I am not surprised at this result, when the division is so made that only a very few genera are on one side. But, according to my notions, the sections or sub-genera of the gigantic genera ought to obey my rule (i.e., supposing a gigantic genus had come to its maximum, whatever increase was still going on ought to be going on in the larger sub-genera). Do you think that the sections of the gigantic genera in D.C.

Prodromus are generally NATURAL: i.e. not founded on mere artificial characters? If you think that they are generally made as natural as they can be, then I should like very much to tabulate the sub-genera, considering them for the time as good genera. In this case, and if you do not think me unreasonable to ask it, I should be very glad of the loan of Volumes X., XI., XII., and XIV., which include Acanthaceae, Scrophulariaceae, Labiatae, and Proteaceae, — that is, the orders which, when divided quite equally, do not accord with my rule, and in which a very few genera balance all the others.

I have written you a tremendous long prose.

LETTER 64. TO J.D. HOOKER. Down, June 8th {1858}.

I am confined to the sofa with boils, so you must let me write in pencil. You would laugh if you could know how much your note pleased me. I had the firmest conviction that you would say all my MS. was bosh, and thank God, you are one of the few men who dare speak the truth. Though I should not have much cared about throwing away what you have seen, yet I have been forced to confess to myself that all was much alike, and if you condemned that you would condemn all my life's work, and that I confess made me a little low; but I could have borne it, for I have the conviction that I have honestly done my best. The discussion comes in at the end of the long chapter on variation in a state of nature, so that I have discussed, as far as I am able, what to call varieties. I will try to leave out all allusion to genera coming in and out in this part, till when I discuss the "Principle of Divergence," which, with "Natural Selection," is the keystone of my book; and I have very great confidence it is sound. I would have this discussion copied out, if I could really think it would not bore you to read, — for, believe me, I value to the full every word of criticism from you, and the advantage which I have derived from you cannot be told...

I am glad to hear that poor old Brown is dying so easily...

You will think it paltry, but as I was asked to pay for printing the Diploma {from a Society of which he had been made an honorary member}, I did not like to refuse, so I send 1 pound. But I think it a shabby proceeding. If a gentleman did me some service, though unasked to do it, and then demanded payment, I should pay him, and think him a shabby dog; and on this principle I send my 1 pound.

(65/1. The following four letters refer to an inquiry instituted in 1858 by the Trustees of the British Museum as to the disposal of the Natural History Collections. The inquiry was one of the first steps towards the establishment of the Cromwell Road Museum, which was effected in 1875.)

LETTER 65. TO R.I. MURCHISON. Down, June 19th {1858}.

I have just received your note. Unfortunately I cannot attend at the British Museum on Monday. I do not suppose my opinion on the subject of your note can be of any value, as I have not much considered the subject, or had the advantage of discussing it with other naturalists. But my impression is, that there is much weight in what you say about not breaking up the natural history collection of the British Museum. I think a national collection ought to be in London. I can, however, see that some weighty arguments might be advanced in favour of Kew, owing to the immense value of Sir W. Hooker's collection and library; but these are private property, and I am not aware that there is any certainty of their always remaining at Kew. Had this been the case, I should have thought that the botanical collection might have been removed there without endangering the other branches of the collections. But I think it would be the greatest evil which could possibly happen to natural science in this country if the other collections were ever to be removed from the British Museum and Library.

Конец ознакомительного фрагмента.