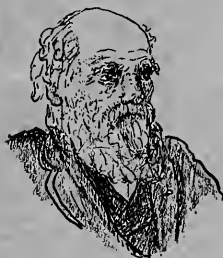


Calendar
of the
Letters of Charles Robert Darwin
to
Asa Gray

Prepared by
The Historical Records Survey
Division of Professional and Service Projects
Work Projects Administration



The Historical Records Survey
Boston, Massachusetts
December, 1939

1. Introduction

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Calendar
of the
Letters of Charles Robert Darwin
to
Asa Gray

With an Introduction
by
Bert James Loewenberg
Assistant Professor of History, University of South Dakota

Prepared by
The Historical Records Survey
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* * * * *

The Historical Records Survey
Boston, Massachusetts
December, 1939

The Historical Records Survey

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PREFACE

By authority of a Presidential Letter, the Historical Records Survey was established in January, 1936, under the national direction of Dr. Luther H. Evans, as a federally sponsored project of the Works Progress Administration (now the Work Projects Administration). Since federal sponsorship ceased on August 31, 1939, the sponsorship of the Massachusetts unit of the survey has been undertaken by Frederic W. Cook, Secretary of the Commonwealth.

The purpose of the project is to survey, preserve and render accessible historical source materials of all kinds. Its work has fallen naturally into the following main divisions: public records, private manuscripts, church records, early American imprints, historical portraits and newspapers. Practically all historical material falls under one or another of these divisions. In bringing this material under control certain techniques have been found practicable, depending on the nature of the subject matter, and using variously the methods of the inventory, the guide, the calendar, the check list or the index in the publication of the result. For public records, church records and portraits, the method of the inventory has worked best; for historical manuscripts, the guide or, in rare cases where the material was of unusual importance, the calendar; for imprints, the check list; for newspapers and court records, the index; and so on.

The actual work of gathering information concerning historical materials at their place of storage or custody has in most cases been preceded by a most necessary and, for both the custodian and posterity, important task, that of putting records in order; of cleaning, dusting, re-filing, and treating them; and, in short, doing everything possible to ensure their preservation. This function of the project, often performed by its workers under almost indescribable conditions of dust, filth, dampness, poor ventilation, and even vermin may well be regarded by future generations as a most important contribution of the survey.

Scarcely less important, however, are the editorial processes to which all field information must be subjected before publication. Here gaps and inadequacies are spotted, inconsistencies reconciled, and order brought out of chaos. In the field of public records it has been found necessary not only to sketch briefly the history of the county or town and its government but also to preface the inventory of each subordinate office or institution with an outline of its development, based upon its own records or upon statutory or other sources. In the inventories of church records, similarly, the preparation of the history of each church constitutes a task equally arduous with that of locating and listing its records. In Massachusetts two broader works have also been undertaken. The general historical background, statutory origin and functioning of county, city, or town offices have been studied with a view to providing satisfactory accounts of the development of county and municipal government generally. These latter undertakings are now happily nearing completion.

In the field of county records the surveys of eight of the fourteen counties of Massachusetts are nearing completion. In that of municipal records, approximately sixty of 350 cities and towns have been covered to date including several of the more populous. Editorial work is now also proceeding on six of an estimated ten volumes of the inventory of the records of the city of Boston. Some 200 manuscript depositories, large and small, have been surveyed and a preliminary guide to them published. An inventory of the records of Universalist churches in Massachusetts will soon be published, and field work is being carried on in other denominations, particularly in the Unitarian, Congregational, Baptist, and Jewish bodies. A catalogue of portraits painted before 1825 in Massachusetts has been published and editorial work is proceeding on similar listings for the other New England states and New York State. A listing of the publications of the Massachusetts unit of the survey follows at the end of this volume.

This Calendar of the Letters of Charles Robert Darwin to Asa Gray, the originals of which are in the possession of the Gray Herbarium at Harvard University, was undertaken because of the importance of the material. The 138 letters here condensed are, because of Darwin's handwriting, difficult and often well-nigh impossible to read. Some passages, because of their content, admit of no compression. Many have been published and a marginal indication has been made of that fact. A very complete index has been provided.

The calendar is primarily the work of Miss Cora F. Holbrook to whose careful and painstaking scholarship its accuracy and usefulness will be due. The field work which called for the reading of each one of the letters was largely done by Miss Marion R. Spreadby. The work has been done under the general supervision of Mr. Aron S. Gilmartin, who has charge of the manuscript and church survey division of the project.

The Historical Records Survey is indebted to the officials of the Gray Herbarium at Harvard University especially to Charles Alfred Weatherby, curator of the Gray Herbarium and to Professor Merritt Lyndon Fernald, Bishop Professor of Natural History at Harvard and director of the Herbarium, for their cooperation and assistance. It is also indebted to Dr. Bert J. Loewenberg, assistant professor in the department of history at the University of South Dakota for the introduction contributed by him and for his very helpful criticism of the manuscript.

Finally, it wishes to express its obligation to the national editorial office of the survey, especially to Dr. Luther H. Evans, director, and Mrs. Margaret Sherburne Eliot for advice and assistance; and to Secretary of the Commonwealth, Frederic W. Cook, for the sponsorship which makes this publication possible.

Carl J. Wennerblad
State Supervisor
Historical Records Survey

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INTRODUCTION

Letters in the hand of Charles Robert Darwin awaken an enthusiasm in collectors of original manuscripts scarcely less fervid than that inspired by other literary remains. But without the carefully drawn boundaries of certain academic specialties, there is little interest in the thought of Asa Gray whether in manuscript or in published form. Charles Robert Darwin, famed before the publication of the Origin of Species in 1859, thereafter became the most widely known (and most widely discussed) scientist of the nineteenth century. The reputation of Asa Gray, on the other hand, spread further and deeper in Europe than in the United States until the controversy which the Origin of Species initiated, directed public notice to his activity as an apostle of reasonableness. It is, therefore, the distinguished recipient rather than the famous sender of these letters who requires some brief elucidation.

Asa Gray was born in Oneida, New York, in 1810 and early indicated a fondness for study. While still a medical student (1830), Gray made the acquaintance of Dr. John Torrey, a leading American botanist (of the College of Physicians and Surgeons of New York) who exerted a profound influence on the later course of his life. Association with Torrey caused Gray to forsake medical practice for scientific research but the difficulties confronting professional botanists in the America of the thirties were real indeed. Botany was an avocation cultivated by doctors, ministers and the independently wealthy; it was not an accepted method of earning a living. Remunerative posts for botanists were few and Gray was forced to exist by teaching in various capacities in Utica (New York) and elsewhere. Long winters of unrequited effort, however, were followed by rich summers spent in field trips which yielded treasures of experience. Gray's ability was matched by Torrey's zeal in his behalf which was responsible for Gray's first major appointment as curator of the botanical collections of the New York Lyceum of Natural History (1836). Torrey's continuing patronage—and Gray's continuing success—was further demonstrated by an invitation to collaborate on the Flora of North America, an invitation for which all disciples hope but which few receive. Finally in 1838, Gray was appointed professor of Natural History at the University of Michigan then an outpost on the American cultural frontier. But Gray was convinced that until he had studied the North American plants in European herbaria he could not progress with the Flora. Accordingly, he sought and secured leave from the University to visit Europe.¹ It was during these European Wanderjahre that he

¹ The connection with Michigan never materialized for, when Gray returned to the United States after intermittent trips in 1842, he became Fisher Professor of Natural History at Harvard, a position he retained until 1873. He continued at Harvard after 1873 as director of the Herbarium.

Introduction

became acquainted with many of the most eminent scientific men and, among others, he met Charles Robert Darwin to whom he was presented, appropriately enough, in the gardens at Kew

The Harvard professorship marks a turning point not only in the history of Asa Gray but in the history of botanical science in America. Here Gray worked for forty-six years in the triple role of scholar, educator and critic. During this period he laid the foundations for systematic botany in the United States and made many an excursus into the realms of biological philosophy. Gray became the foremost American botanist while Harvard became the clearing house for scholarship in this and related fields.

Gray was a great teacher as well as a great scholar. His personal influence on students, while considerable, was exceeded by the influence of his writings. The famous Manual (first edition, 1848) is simply the best known title in a staggering bibliography yet it was not his books and articles alone which stimulated American scientific men. He was a prolific writer of critical reviews, which he considered one of his most vital functions, and it was partly through these reviews that American students were informed of European developments in botany and its contiguous provinces. Of these reviews a distinguished naturalist later said, "a chronological collection of this great series of reviews would form a most instructive commentary on the history of botany for a half a century."

Gray's teaching was not bounded by the classroom or restricted to critical and monographic writing. The Darwinian debate, seldom confined to the issue of the variability of species, impelled Gray to take a leading part on the side of tolerance. This was Gray's largest class and many an untrained but intelligent layman learned something of the basic intellectual values which the controversy seemed momentarily to threaten. Gray also conducted by correspondence a seminar in the problems of biology. He was an ardent and accomplished exponent of the art of letter writing as a medium of teaching and criticism. Among the students who sought his counsel was Charles Robert Darwin.

These letters are noteworthy for what they reveal of Gray as well as for what they reveal of Darwin. They illustrate Darwin's reliance upon Gray's learning and his dependence upon Gray's opinion of the larger issues of biological theory. The correspondence is especially valuable because it coincides with an epoch crucial in the history of intellectual development and with a period equally crucial in the life of Charles Robert Darwin. Although the formative era in Darwin's thinking came in the thirties and forties, the years from 1855 to 1859 were

Introduction

vital since this was the time that the final mental drafts of the Origin of Species were composed. Thereafter, from 1859 to 1881, Darwin labored unceasingly on revisions of the Origin and his other publications, all integral parts of the mosaic of evolution. Advice of men like Asa Gray was second in importance only to Darwin's own thought and Darwin's letters are one of the grandest testimonials of the interdependence of scholarship. The period from 1859 to 1881, moreover, synchronized with the first stage of the evolution controversy in America in which Gray's activity and strategic suggestions were essential elements of Darwin's ultimate victory.

Bert James Loewenberg

University of South Dakota

EXPLANATORY NOTES

Most of the letters are written from Down, Bromley, Kent; unless otherwise indicated, this may be assumed by the reader.

Dates supplied from Life and Letters of Charles Darwin or from More Letters of Charles Darwin have been enclosed in brackets. An asterisk used with the date denotes one which has been supplied in pencil on the original letters in a handwriting other than Darwin's.

A vertical line in the margin indicates that a portion of the letter appears in Life and Letters [L. L.] and More Letters [M. L.]. Although other publications also include quotations, such are not indicated.

The number immediately following the dimensions of each letter is that used on the original manuscript at the Gray Herbarium.

In condensing the text, generous quotations have been made from the letters themselves. In addition, Darwin's works, phrases or style of writing have been retained wherever they best express the thought. Such passages, however, have not always been enclosed in quotation marks.

Throughout the letters, Darwin refers to his several publishers: John Murray, 3d, of England; Karl J. Trübner, of Germany, and to D. Appleton and Co., and Ticknor and Fields, of the United States. Full identifications are not given in the entries.

The full title of the British Association for the Advancement of Science has not been given in every instance.

The American Journal of Science and Arts is commonly referred to by Darwin, Gray and in publications as Silliman's Journal. This shorter form of reference has also been retained in the calendar.

First names of persons mentioned have been supplied in most instances. First names are omitted, however, in some cases where it has been impossible to identify them with certainty; and in other instances, where identification would necessitate more extended research than was possible.

Biographical notes have been appended for most of the people mentioned in the correspondence. When it has not been possible to identify with certainty, no note appears.



CALENDAR OF CHARLES ROBERT DARWIN LETTERS
TO ASA GRAY, 1855-81.

[1855]
Apr. 17¹

Down, Farnborough, Kent.

Hopes Gray remembers being introduced to him [Darwin] at Kew: asks if Gray would answer some questions for him as he is "no botanist" and would like to test his animal "variation" facts on plants; he has the "greatest curiosity about Alpine Flora of the United States" and asks Gray to give him "other habitats or range of those plants, appending 'Indig.' for such as are confined to the mountains of the U.S.-'Arctic Am.' to such as are also found in Arctic America,-'Arctic Eu.' to those also found in Arctic Europe,-'Alps' to those found in any mountains of Europe, and 'Arct. Asia'." Notices there are 22 species common to the White Mountains and the mountains of New York; asks how wide a space of low land, on which these Alpine plants cannot grow, separates these mountains; hopes Gray will forgive one who is "not a botanist" for being so "presumptuous" as to make "even the most trifling suggestion to such a botanist" as Gray.

A. L. S. 2 pp. 25 cm x 20 cm. No. 1 [1]

L L
I, 420²

L.L.
I, 420

[1855]
June 8

Down, Farnborough, Kent.

Thanks Gray for the list of Alpine plants; can now picture, to some degree, the plants of American Alpine summits; the New Edition of Gray's Manual of the Botany of the Northern U. S. is "capital" news; knows from the preface how "pressed" Gray is for room, but it would take no space to append "Eu." in a bracket to every European plant, and this would answer every purpose; from his own experience while "making out" English plants in English manuals, it has often "struck" him how much interest it would give if some notice of their range had been indicated, and so he cannot doubt that American inquirers and beginners "would much like to know which of their plants were indigenous and which European"; asks if it would not be well in the Alpine plants to append the very same additions which Gray has now sent in the manuscript; suggests giving the habitats of those plants found west of the Rocky Mountains and those found in Eastern Asia, which, if he remembers correctly, is the main partition line of Siberia; "Perhaps Siberia more concerns the northern Flora of N. America. The ranges of the plants, to the East and West, viz. whether most found are in Greenland and Western Europe, or in E. Asia appear to me a very interesting point as tending to show whether the migration has been Eastward or Westward." Is "conscious that the only use of these remarks is to show a Botanist what points a non-Botanist is curious to learn; for I think everyone who studies profoundly a subject often becomes unaware what points the ignorant require information." Is glad Gray thinks of "drawing up some notice on geographical distribution, for the area of the Manual strikes me as in some

L.L.
I, 420

1. Life and Letters dates this letter Apr. 25, 1855.
2. The vertical line indicates what portion of the letter has appeared in print; the marginal note "L.L." refers to Life and Letters, 1887 edition, and "M.L." to More Letters, 1902 edition.

points better adapted for comparison with Europe than that of the whole of N. America." Cannot state, as Gray has asked him to do, definite points on which he wishes information as "they are so vague", wishes to see what results will come out from comparisons; "I presume . . . you would give for your area, the proportions (leaving out introduced plants) to the whole of the great leading families; this is one point I had intended (and indeed have done roughly) to tabulate from your Book, but of course I could have done it only very imperfectly. I should also . . . have ascertained the proportion to the whole Flora of the European plants (leaving out introduced) and of the separate great families, in order to speculate on means of transportal " Sent Gray a copy of the Gardeners' Chronicle with his short report of some "trifling" experiments he has been trying on the power of seeds to withstand sea water; "Has it struck you . . . that it would be advisable for Botanists to give in whole numbers, as well as in the lowest fraction, the proportional numbers of the Families Then I make out from your Manual that of the indigenous plants the proportion of the Umbelliferae are $36/1798 = 1/49$, for without one knows the whole numbers, one cannot judge how really close the numbers of the plants of the same family are in two distant countries . . . I may give an instance of the sort of points, and how vague and futile they often are, which I attempt to work out, that reflecting on [Robert] Brown's and [Sir Joseph] Hooker's remark, that near identity of proportional number of the great Families in two countries, shows probably that they were once continuously united. I thought I would calculate the proportions of, for instance, the introduced Compositae in Grt Britain to all the introduced plants, and the result was $10/92 = 1/9.2$ In our aboriginal or indigenous flora the proportion is $1/10$, and in many other cases I found an equally striking correspondence; I then took your Manual and worked out the same question, here I found in the Compositae an almost equally striking correspondence, viz: $24/206 = 1/8$ in the introduced plants and $223/1798 = 1/8$ in the indigenous; but when I came to the other Families, I found the proportions entirely different showing that the coincidences in the British Flora were probably accidental! You will, I presume, give the proportion of the species to the genera " Suggests dividing species into 3 groups - (a) species common to the Old World, stating numbers common to Europe and Asia, (b) indigenous species, but belonging to genera found in the Old World, (c) species belonging to genera confined to America or the New World; would like to have marked the close species in a Flora, so as to compare in different Floras whether the same genera have close species, and "for other purposes too vague to enumerate", has attempted, with Hooker's help, to "ascertain" in a similar way whether the different species of the same genera in distant quarters of the globe are variable or present varieties; "The definition I should give of a 'close species' was one that you thought specifically distinct, but which you could conceive that some other good Botanist might think only a race or variety; or again a species that gives bad trouble, though having opportunities of knowing it well, in discriminating from some other species."

Thanks Gray for answering questions about the distance of the Alpine summits; from his map he cannot make "tally" what Gray says about the distance of the White Mts. from Green Mts., and Green Mts. from those of N.Y.

A. L. S. 18 pp. 20 cm x 13 cm. No. 2

[2]

[1856?]
May 2

"It is preposterous in me to give you hints, but", as [Sir Joseph] Hooker says, "my questions are sometimes suggestive owing to my comparing the ranges etc. in different kingdoms of nature." Forgets whether Gray includes Arctic America, but, if so, for comparison with other parts of the world, suggests he exclude Arctic and Alpine-Arctic as belonging to a quite distinct category; when excluding the naturalized, thinks [Alphonse Louis Pierre] de Candolle "must be right in advising the exclusion, giving list, of plants exclusively found in cultivated land, even when it is not known they have been introduced by man. I would give list of temperate plants, if any, found in Eastern Asia, China, and Japan, and not elsewhere. Nothing would give me a better idea of Flora of U S than the proportion of the genera to all the genera, which are confined to America, and the proportion of genera confined to America and Eastern Asia with Japan; the remaining genera would be common to America and Europe and rest of the world; I presume it would be impossible to show any especial affinity in genera . . . between America and Western Europe; America might be related to Eastern Asia, (always excluding Arctic forms) by a genus having the same species confined to these two regions; or it might be related by the genus having different species, the genus itself not being found elsewhere. The relation of the genera, (excluding identical species) seems to me a most important element in geographical distribution often ignored, and I presume of more difficult application in plants than in animals, owing to the wider ranges of plants, but I find in N Zealand (from Hooker) that the consideration of genera with representative species tells the story of relationship even plainer than the identity of the species with different parts of the world. I should like to see the genera of the U. States, say 500 (excluding Arctic and Alpine) divided into 3 classes, with the proportions given, thus, 100/500 American genera, 200/500 Old World genera, but not having any identical species in common; 200/500 Old World genera, but having some identical species in common; supposing that these 200 genera included 600 U. S. plants, then the 600 would be the denominator to the fraction of the species in common to the Old World." Refers to a "discussion" in De Candolle's book on the relation of the size of families to the average range of the individual species, and "cannot but think" from facts he collected "long before De Candolle appeared" that he is on the "wrong scent" in having taken families, owing to their including too great a diversity in the constitution of the species, but that if he had taken genera he would have found that the individual species in large genera range over a greater area than do the species in small genera; thinks, if Gray has materials, this would be well worth working out, for it is a "very singular relation"; asks if any naturalized plants in the United States are social which are not so in their parent country; is surprised the importance of this has not struck De Candolle; asks if many of these naturalized plants are more variable than the average of United States plants;

M L
I, 422

"De Candolle has stated that the naturalized plants do not present varieties; but being very variable and presenting distinct varieties seems to me rather a different case." Asks if individuals of naturalized plants, which have their southern limits within Gray's area, are ever or often stunted in growth or unhealthy; has endeavored in vain to find any botanist who has observed this point, but has seen some remarks by Barton on the trees in the United States; "Trees seem in this respect to behave rather differently from other plants." Believes it would be a very curious point, "but I fear you would think it out of your Essay, to compare the list of European plants in Tierra del Fuego (in Hooker) with those in North America; for without multiple creation, I think we must admit that all now in Tierra del Fuego must have travelled through North America and so far they do concern you." De Candolle's discussion on social plants "strikes" him as the best he has ever seen; "two points strike me as eminently remarkable in them: that they should ever be social close to their extreme limits; and secondly that species having an extremely confined range, yet should be social where they do occur." Would be obliged for "any cases on these heads, more especially in regard to a species remaining or ceasing to be social on the confines of its range." Asks if there are any cases of the same species being more variable in the United States than in other countries in which it is found, or in different parts of the United States; quotes [George ?] Wahlenberg as saying that the same species in going south becomes more variable than in the extreme north; asks whether any of the genera, which have most of their species "horribly" variable in Europe or other parts of the world, are less variable in the United States, or the reverse case; "I suppose your Flora is too great, but a simple list in close column in small type of all the species, genera, and families, each consecutively numbered, has always struck me as most useful; and Hooker regrets that he did not give such a list in the introduction to N. Zealand and other Flora." It "appears" to him that the 6 heads of Gray's essay include every point which could be desired.

A. L. S. 10 pp. 21 cm x 16 cm. No. 4

M L
I, 422

[3]

[1856]
July 20

What Gray says about extinction in regard to "such" genera being hypothetical seems very "just"; thinks something direct, however, could be advanced on this head from fossil shells; "but hypothetical such notions must remain. It is not a little egotistical, but I should like to tell you (and I do not think I have) how I view my work. Nineteen years (!) ago it occurred to me that whilst otherwise employed on Natural History, I might perhaps do good if I noted any sort of facts bearing on the question of the origin of species, and this I have since been doing. Either species have been independently created, or they have descended from other species, like varieties from one species. I think it can be shown to be probable that man gets his most distinct varieties by preserving such as arise best worth keeping and destroying the others, but I should fill a quire if I were to go on. To be brief I assume that species arise like our domestic varieties with much extinction; and then test this

L L
I, 437

hypothesis by comparison with as many general and pretty well established propositions as I can find made out, - in geograph [ical] distribution, geological history, affinities, etc And it seems to me, that supposing that such hypothesis were to explain such general propositions, we ought, in accordance with the common way of following all sciences, to admit it, till some better hypothesis be found out. For to my mind to say that species were created so and so is no scientific explanation, only a reverent way of saying it is so and so. But it is nonsensical trying to show how I try to proceed in the compass of a note But as an honest man, I must tell you that I have come to the heterodox conclusion that there are no such things as independently created species - that species are only strongly defined varieties I know that this will make you despise me I do not much underrate the many hugh difficulties on this view, but yet it seems to me to explain too much, otherwise inexplicable, to be false. Just to allude to one point in your last note, viz. about species of the same genus generally having a common or continuous area; if they are actual lineal descendants of one species, this of course would be the case; and the sadly too many exceptions (for me) have to be explained by climatal and geological changes. . . . I have put a chapter together" on permanence of species, and [Sir Joseph] Hooker "kindly read it over I thought the exceptions and difficulties were so great that on the whole the balance weighed against my notions, but I was much pleased to find that it seemed to have considerable weight with Hooker, who said he had never been so much staggered about the permanence of species I must say one word more in justification (for I feel sure that your tendency will be to despise me and my crotchets) that all my notions about how species change are derived from long-continued study of the works of (and converse with) agriculturists and horticulturists; and I believe I see my way pretty clearly on the means used by nature to change her species and adapt them to the wondrous and exquisitely beautiful contingencies to which every living being is exposed." Thanks Gray for what he says about the possibility of crossing of grasses; has been astonished at what botanists say on fertilization in the bud; what Gray says on papilionacious flowers is very true; has no facts to show that varieties are crossed, yet he must believe that flowers are constructed partly in direct relation to visits of insects, and how insects can avoid bringing pollen from other individuals he cannot understand; "It is really pretty to watch the action of a Humble-Bee on the scarlet Kidney Bean, and in this genus . . . the honey is so placed that the Bee invariably alights on that one side of the flower towards which the spiral pistil is protruded (bringing out with it pollen) and by the depression of the wing-petal is forced against the Bee's side

1. Life and Letters, II, 434, calls this section of the letter "undated".

all dusted with pollen. In the Broom the pistil is rubbed on centre of back of Bee." Suspects there is something to be "made out" about the Leguminosae which will bring the case within "our" theory, though he has failed to do so; believes the theory will explain why, "in the vegetable and animal kingdoms, the act of fertilization even in hermaphrodite usually takes place sub-jove, though thus exposed to the great injury from damp and rain. In animals in which the semen cannot, like pollen, be occasionally carried by insects or wind, there is no case of land-animals being hermaphrodite without the concurrence of two individuals."

A L S 8 pp. 20 cm x 13 cm. No. 9-B

[4]

L L
II, 434

[1856]
Aug. 24

"Botany has been followed in so much more a philosophical spirit than Zoology, that I scarcely ever like to trust any general remark in Zoology, without I find that Botanists concur." Regarding intermediate varieties being rare, he "found it put . . . much too strongly . . . by a "very good naturalist", [Thomas Vernon] Wollaston, in regard to insects; "if it could be established as true it would . . . be a curious point. Your answer in regard to introduced plants not being particularly variable, agrees with an answer which H[ewett] C[ottrell] Watson has sent me in regard to British agrarian plants, or such . . . as are now found only in cultivated land. It seems to me very odd without any theoretical notions of any kind, that such plants should not be variable, but the evidence seems against it." Thanks Gray for his invitation to come to the United States; "There is nothing which I should enjoy more, but my health is not . . . strong enough, except for the quietest routine life in the country." Will be "glad of" the sheets of Gray's paper on geographical distribution; his [Darwin's] remark that he supposed there were but few plants common to Europe and the United States not ranging to the Arctic regions was founded on "vague grounds and partly on range of animals"; found from Watson's table that out of 499 plants believed to be common to the Old and New Worlds only 110 did not range on either side of the Atlantic up to the Arctic region; on writing Watson to ask whether he knew of any plants not ranging northward of Britain which were in common, he replies that he imagines there are very few, for with Mr. Syme's assistance he found 20 to 25 species thus circumstanced, but many of them, from one cause or other, he considered doubtful; hopes Gray will be "inclined to work out" for his next paper what number of his [Gray's] 321 in common do not range to the Arctic regions; "Such plants seem exposed to such much greater difficulties in diffusion." Asks Gray to send anything that should "occur" to him on variability of naturalized or agrarian plants.

A L S 4 pp. 25 cm x 20 cm. No. 36

[5]

M L
I, 430

[1856]
Oct. 12

Gray's "admirable" "Statistics [of the Flora of the Northern U S ", Silliman's Journal, XXII and XXIII] has arrived, and he knows of only one essay, Hooker's "New Zealand", on geographical distribution that approaches it in clearness; expresses thanks for information about "social" and "varying plants", and for giving some idea about the proportion of European plants which do not range to the extreme north; asks Gray to send the total number of genera and orders to which his 260 introduced plants belong; sees they include 113 genera non-indigenous; nothing has surprised him more than the greater generic and specific affinity with East Asia than with West America; asks if climate explains this greater affinity, or is it one of the "many utterly inexplicable problems" in botanical geography; inquires if East Asia is nearly as well known as West America; believes if the number of genera strictly or nearly strictly European were known, one could compare better with Asia and Southern America; is glad Gray intends to "work out" the north range of the 321 European species, and range of species in regard to number of species in genus; "I have been attempting to do this in a very few cases, but it is folly for any one but a Botanist to attempt it. I must think that [Alphonse Louis Pierre] de Candolle has fallen into error in attempting to do this for orders instead of for genera." [Points out a misprint, and gives suggestion for paging]; [Sir Joseph] Hooker has lately returned from a continental trip, and he is to see him Friday

A L S 8 pp 20 cm x 13 cm No 6

[6]

M L
I,433

[1857?]
Jan. 1

Has received the second part of Gray's paper ["Statistics of the Flora of the Northern United States", Silliman's Journal, 1857], and it "strikes" him as "quite exhausting the subject, and I now appreciate the character of your Flora. What a difference in regard to Europe your remarks in relation to the genera make" [Sir Charles] Lyell told me that Agassiz having a theory about when Saurians were first created, on hearing some careful observations opposed to this, said he did not believe it, 'for Nature never lied' - I am just in this predicament and repeat to you that 'Nature never lies', ergo, theorists are always right" One point is well worth "working out" - a comparison of the principal zone of habitation in the United States of the 320 European plants with the 130 representative native species, and then again with the classes of strictly congeneric and perhaps divergent congeneric species, would be astonished if Gray does not get a very curious and harmonic result on the great principle that nature never lies; is glad to see Gray's conclusion in regard to species of large genera widely ranging; considers it a great compliment to be mentioned in Gray's paper; Gray's conclusion that the line of connection of strictly Alpine plants is through Greenland makes him "groan", would like to see Gray's reasons published in detail, for it "riles me dreadfully" Concluded that trees would have a strong tendency toward flowers with dioecious, monoecious, or polygamous structure; took one little British Flora and found the result was in species, genera, and families as he anticipated; asked [Sir Joseph] Hooker to tabulate

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New Zealand Flora for this end; Hooker thought his [Darwin's] results sufficiently curious to do so; finds the accordance with Britain is very striking - and the more so because Hooker has made three classes of trees, bushes, and herbaceous plants; Hooker will work the Tasmanian Flora on the same principle; "Pray do not forget variability of naturalized plants." Has been comparing, as far as he can, Protean genera and has "left off in a maze of perplexity" Asks if such genera as Salix, Rubus, Rosa, Mentha, Saxifraga, Hieracium and Myosotis have equally Protean species in the United States, even if they have only one, but more especially if they have many; "I think you have no Rosa, and forget how it is with some of the other genera" It would be valuable to him if Gray would think over his half-dozen or dozen worst genera which have any European species, and then he [Darwin] could find out whether such are very troublesome in Europe "I think Hooker told me that in Himalaya, Rubus and Salix, though large genera, were not troublesome to make out. I think Protean genera of shells are troublesome at all geological times and in all places"

A L S 10 pp 20 cm x 13 cm No 7

[7]

L L
I,446

[1857]
Sep. 5

"I thought you would utterly despise me, when I told you what views I had arrived at, which I did because I thought I was bound as an honest man to do so. I should have been a strange mortal seeing how much I owe to your quite extraordinary kindness, if in saying this I had meant to attribute the least bad feeling to you. . . Before I had ever corresponded with you [Sir Joseph] Hooker had shown me several of your letters and these gave me the warmest feeling of respect for you, . . . But I did not feel in the least sure that when you knew whither I was tending, that you might not think me so wild and foolish in my views (God knows arrived at slowly enough, and I hope conscientiously) that you would think me worth no more notice or assistance. The last time I saw my dear old friend [Hugh] Falconer, he attacked me most vigorously but quite kindly, and told me 'You will do more harm than any ten naturalists will do good,' - 'I can see that you have already corrupted a half-spoiled Hooker.' (!! Now when I see such strong feeling in my oldest friend, you need not wonder that I always expect my views to be received with contempt. . . I go as far as almost anyone in seeing the grave difficulties against my doctrine. . . In animals, embryology leads me to an enormous and frightful range. The facts which kept me longest scientifically orthodox are those of adaptation - the pollen masses in Asclepias, - the mistletoe, with its pollen carried by insects, and seed by Birds - the woodpecker with its feet and tail beak and tongue to climb trees and secure insects. To talk of climate or Lamarckian habit producing such adaptations to other organic being is futile. The difficulty, I believe, I have surmounted. As you seem interested in the subject, and as it is an immense advantage to me to write to you, and to hear over so briefly, what you think, I will enclose the briefest abstract of my notions as to means by which nature makes her species. Why I think the species have really changed depends on general facts in the affinities, embryology, rudimentary

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organs, geological history, and geographical distribution of organic beings. You will, perhaps, think it paltry in me, when I ask you not to mention my doctrine; the reason is, if anyone, like the author of the Vestiges [of the Natural History of Creation, by Robert Chambers, 1845], were to hear of them, he might easily work them in and then I should have to quote from a work perhaps despised by naturalists and this would greatly injure any chance of my views being received by those whose opinion I value " Has been at work on a point lately which interests him much, namely, dividing the species of several Floras into 2 as nearly equal cohorts as possible, one with all those forming large genera and the other with the small genera; "Thus in your U. States Flora, I make . . . 1005 species in genera of 5 and upwards, and 917 in genera with 4 and downwards; and the large genera have 88/1000 varieties and the small genera only 50/1000. This rule seems to be general and Hooker is going to work out some Floras on same plan. But to my disgust your varieties marked by big type are only in proportion 40/1000 to 46/1000. Several things have made me confidently believe that 'close' species occurred most frequently in the larger genera, and you may remember that you made me the enclosed list. Now to my utter disgust, I find the case is somewhat the reverse of what I had so confidently expected, the close species hugging the smaller genera. Hence I have enclosed the list, and beg you kindly to run your eyes over it, and see whether, not understanding my motive, you could have attended more to the smaller than to the larger genera; but I can see that this is not probable. And do not think I want you to 'cook' the result for me. Are the close species very generally geographical representation species? This might make some difference." Has lately examined buds of the Kidney Bean with its pollen shed, but was led to believe that the pollen could hardly get on the stigma by the wind or otherwise, except by bees visiting and moving the petals; hence he placed small bunches of flowers in 2 bottles, in every way treated the same; the flowers in one he daily just momentarily moved as if by a bee; these set 3 fine pods, and the other not any; "Of course this little experiment must be tried again, and this year in England it is too late, as the flowers seem now seldom to set. If Bees are necessary to this flower's self-fertilization, Bees must almost cross them, as their dusted right side of head and right legs constantly touch the stigma " Has lately been observing daily *Lobelia fulgens*; the one in his garden is never visited by insects and never sets seeds "without pollen be put on its stigma", whereas the blue *Lobelia* is visited by bees and does set seed; "I mention this because there are such beautiful contrivances to prevent the stigma ever getting its own pollen; which seems only explicable on the doctrine of the advantage of crosses " Has received [Hewett Cottrell] Watson's papers and Gray's Lessons in Botany [First Lessons in Botany and Vegetable Physiology, 1857]; will get seeds of *Adlumia cirrhosa* and observe it next summer.

A L S 12 pp. 20 cm x 13 cm. No. 48

[E]

Enclosed is the abstract mentioned above, "Six Principles of Natural Selection", in another handwriting.

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II,455

[1857]

Every word of Gray's letter interested him, "When I said that your remarks on your Alpine plants 'riled' me, I did not mean to doubt them, except that they went against some theoretic notions of mine." Is glad to hear that Gray is thinking of discussing the relative ranges of the identical and allied United States and European species; "I presume [Sir Joseph] Hooker has been urging you to finish your great Flora before you do anything else. Now I would say it is your duty to generalize as far as you safely can from your as yet completed work. Undoubtedly careful discrimination of species is the foundation of all good work. . . . The observer can generalize his own observations incomparably better than anyone else. How many astronomers have labored their whole lives on observations and have not drawn a single conclusion. I think it is [John Frederick William] Herschel who has remarked how much better it would be if they had paused in their devoted work and seen what they could have deduced. So do pray look at this side of the question, and let us have another paper or two like the last admirable ones. . . . You ask about my doctrine which led me to expect that trees would tend to have separate sexes. I am inclined to believe that no organic being exists which perpetually self-fertilizes itself. This will appear very wild, but I can venture to say that if you were to read all my observations on this subject, you would agree it is not so wild as it will at first appear, . . . from flowers said to be always fertilized in bud, etc. It is a long subject which I have attended to for 18 years! Now it has occurred to me that in a large tree with hermaphrodite flowers, we will say it would be ten to one that it would be fertilized by pollen of its own flowers, and a thousand or ten-thousand to one that if crossed, it would be crossed only with pollen from another flower of the same tree, which would be opposed to my doctrine. Therefore on the great principle of 'nature not lying' I fully expected that trees would be apt to be dioecious or monoecious (which as pollen has to be carried from flower to flower every time, would favour a cross from another individual of the same species) and so it seems to be in Britain and New Zealand. Nor can this fact be explained by certain families having this structure and chancing to be trees, for the rule seems to hold both in genera and families as well as in species. I give you full permission to laugh your fill at this wild speculation; and I do not pretend but what it may be chance which, in this case, has led me apparently right. But I repeat that I feel sure that my doctrine has more probability, than at first it appears to have. . . . The Leguminosae are my greatest opposers; yet if I were to trust to observations on insects made during many years, I should fully expect crosses to take place in them, but I cannot find that our garden varieties ever cross each other." Asks Gray to inquire of "intelligent nurserymen" if they take any pains in raising the varieties of papilionaceous plants apart to prevent crossing; "The worst is that nurserymen are apt to attribute all variation to crossing. Finally I incline to believe that every living being requires an occasional cross with a distinct individual, and as trees from mere multitude of flowers

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offer obstacle to this, I suspect this obstacle is counteracted by tendency to have sexes separated. My maximum difficulty is trees having papilionaceous flowers. Some of them, I know have their keel-petals expanded when ready for fertilization, but [George] Bentham does not believe that this is general; nevertheless . . . I suspect that this will turn out so, or that they are eminently sought by bees dusted with pollen." Asks Gray to look at Robinias when in full flower and see whether stamens and pistils protrude and whether bees visit them; "A clever Australian gardener", Sir [William] Macarthur, told him "how odd it was that his Erythras in N S Wales would not set seed without he imitated the movements of the petals which Bees cause." Thanks Gray for information about Protean genera, as one of the greatest of his puzzles is to know or conjecture whether the great variability of such genera is due to their conditions of existence or whether it is apt to be innate in them at all times and places; is aware that this cannot be strictly predicated of any genus, for all have some fixed species; has sent the latter half of Gray's note, with list of such American genera, to [Hewett] [Ottrell] Watson, of whom he has the highest opinion, and is sending Watson's notes to Gray; calls attention to the fact that his question does not refer to genera having very close species, but to genera having very variable species; would be glad to have comments on Watson's papers.

A. L. S. 13 pp. 20 cm x 13 cm. No. 8 [9]

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[1858]
July 4

Explains not having answered Gray's letter because of death, illness and misery amongst his children; they are all going from home for some weeks. "It is really pretty to watch the Humble Bees sucking first on one or the other side of the several flowers [of Dicentra]; with their hind legs resting on the crest of the hood formed by the inner united petals, they push it to opposite side of flower, and the straight pistil is rubbed against their abdomens and inner side of thighs, which are white with pollen from the several flowers. It is impossible but what the individuals of Dicentra must be largely crossed. Your Adlumia has not flowered with me yet. In Fumaria and Corydalis we have another structure, viz - nectary on one side and here the pistil bends so that the two stigmas are presented in the gangway to the one nectary; and the hood slips off easiest in opposite direction, instead of equally easily to either side. Indeed in Corydalis lutea it almost springs off, and the pistil decidedly springs towards the nectary-bearing petal." Has observed only six Fumariaceae and wishes he knew whether the rule is general, for he must believe the structure of these flowers is related directly to the visits of the bees; suspects from his own few observations that the following rule may be general - that when honey is secreted on one point of circle of corolla, the pistil, if it bends, always bends so that the stigmas, when mature, lie in the "gangway" to nectary; "Thus in Columbine where there is a circle of nectaries the stigmas are straight; in Aquilegia grandiflora where there is one nectary the stigmas are rectangularly bent so that every Bee . . . hunches over them in extracting honey." Asks for date when sketch of notions of Natural Selection was sent, because [Alfred Russell] Wallace, who is

exploring New Guinea, has sent him an abstract of the same theory, "most curiously coincident even in expressions. And he could never have heard a word of my views. He directed me to forward it to [Sir Charles] Lyell. Lyell, who is acquainted with my notions, consulted with [Sir Joseph] Hooker (who read a dozen years ago a long sketch mine written in 1844) urged me with much kindness not to let myself be quite forestalled and to allow them to publish with Wallace's paper an abstract of mine; and as the only very brief thing which I had written out was a copy of my letters to you, I sent it and I believe it has just been read . . . before the Linnaean Society; and this is the reason, why I should be glad of the date . . . I am sure it was written in September, October or November of last year " In regard to bent pistils and nectaries, he largely judges of position of nectary by seeing where bees suck, and the rule holds in many cases

A L S 6 pp- 20 cm x 13 cm No. 20

[10]

[1858]
Aug 11

Isle of Wight

Has discussed in his long manuscript the later changes of climate and the effects on migration, and will now give an abstract of an abstract; cannot give facts, and must write "dogmatically", though he does not feel so on any point; has some foundation for his views, for [Sir Joseph] Hooker, who at first "demurred" to his main point, has since told him that "further reflection and new facts have made him a convert"; in the Pliocene Age the temperature was higher; of this there can be little doubt; the land on a large scale held much its present disposition, and the species, judging from shells, were mainly what they are now; at this period, when all animals and plants ranged 10 or 15 degrees nearer the poles, he believes the northern part of Siberia and of North America, being almost continuous, were peopled by a nearly uniform Fauna and Flora, just as the Arctic regions now are; the climate then grew gradually colder till it became what it now is, and then the temperate parts of Europe and America would be separated as far as migration is concerned, just as they now are; then came the Glacial Period, driving far south all living things, and Middle or even Southern Europe became peopled with Arctic productions; as the warmth returned the Arctic productions slowly crawled up the mountains as they became denuded of snow, and we now see on their summits the remnants of a once continuous Flora and Fauna; "This is [Edward] Forbes' theory, which I had written out 4 years before he published." Some facts have made him vaguely suspect that between the glacial and the present temperature there was a period of slightly greater warmth; according to his modification doctrines, he looks at many of the species of North America, which closely represent those of Europe, as having become modified since the Pliocene Age when, in the northern part of the world, there was nearly free communication between the Old and New worlds; but now comes a more important consideration; there is a considerable body of geological evidence that during the Glacial Epoch the whole world was colder; he inferred that, many years ago, from boulder phenomena carefully observed by him on both the east and west coasts of South America; now he is so "bold" as to believe that, at the height of the

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Glacial Epoch and when all tropical productions must have been considerably distressed, several temperate forms slowly travelled into the heart of the tropics and even reached the southern hemisphere, and some few southern forms penetrated in a reverse direction northward; wherever there was nearly continuous high land, this migration would have been immensely facilitated; hence the European character of the plants of Tierra del Fuego, summits of the Cordilleras, and Himalayas; as the temperature rose, all the temperate intruders would crawl up the mountains; hence European forms on Nilgherries, Ceylon, summit of Java, and Organ Mountains of Brazil; but these intruders, being surrounded with new forms, would be very liable to be improved or modified by natural selection to adapt them to the new forms with which they had to compete; hence most of the forms on the mountains of the tropics are not identical, but are representative forms of north temperate plants; there are similar classes of facts in marine productions; "All this will appear very rash to you, and rash it may be; but I am sure not so rash as it will at first appear to you, Hooker could not stomach it all at first, but has become largely a convert." From mammalia of shallow seas he believes Japan to have been joined to the mainland of China within no remote period, and then the migration north and south before, during, and after the Glacial Epoch would act on Japan, as on the corresponding latitude of China and the United States, "I should beyond anything like to know whether you have any Alpine collections from Japan, and what is their character."

A L 10 pp 20 cm x 13 cm Incomplete Last p or pp missing No. 42

[11]

L L
I, 491[1859]
Feb. 21

[Sir Joseph] Hooker suggests he ask Gray if he thinks that good botanists in drawing up a local Flora, whether small or large, or in making a Prodrromus like [Alphonse Louis Pierre] de Candolle's, would almost universally, but unintentionally and unconsciously, tend to record varieties in the large or in the small genera, or would the tendency be to record the varieties about equally in genera of all sizes; asks if Gray himself is conscious on reflection that he has "attended to", and recorded more carefully, the varieties in large or small or very small genera; knows what "fleeting and trifling things" varieties are, but says his "query" applies to such as have been thought worth marking and recording; asks if Gray knows whether any one has ever published any remarks on the geographical range of varieties of plants in comparison with the species to which they are supposed to belong

A L S 6 pp. 20 cm x 15 cm No 21

[12]

L L
I, 463

[1859]
Apr. 4

"Your kindness to me is really beyond thanks. Believe me that I feel it. By an odd chance yesterday morning, before I got your letter, I had just written down what I had to say on closely allied species in large genera; and I thought that you had forgotten all about your list, and knowing how hard you were worked, to my credit be it said, I firmly resolved that nothing should induce me to remind you. Therefore you may believe how delighted I was to get your list, which is now being tabulated. I am, also, particularly obliged for the answer to my question " [Sir Joseph] Hooker and several other botanists differ from Gray and think there would be a strong tendency to omit recording actually existing varieties in the smaller genera; "None of them pretend they even thought of this before I asked the question. From what little systematic work I have done myself, I cannot realize their views; and my tables of several local Floras in some respects contradict them in my opinion, and show that Botanists have worked rather more systematically and regularly in recording varieties than could have been anticipated " This is his view, but he does not know what Hooker will say when he reads, as he hopes he will, his discussion on this subject, taking books as his guide, he finds in local Floras and in the case of two entomological works which he has tried, that the rule is almost universal that the larger genera have more species with varieties, and a greater average number of varieties to the varying species, than the smaller genera, it will be the highest satisfaction to send Gray the printed sheets of his work [Origin of Species, 2^d Edition, London, 1860]¹ as they are finished, "I look at the request as a high compliment I shall not, you may depend, forget a request, which I look at as a favor. But (and it is a heavy 'but' to me) it will be long before I go to press. I can truly say I am never idle, indeed, I work too hard for my much weakened health, yet I can do only 3 hours of work daily, and I cannot at all see when I shall have finished " Has done 11 long chapters, but has some other very difficult ones to do, and has to correct and add largely to all those already done, finds that each chapter takes him on an average 3 months, "so slow I am. There is no end to the necessary digressions " Has just finished a chapter on Instinct, and "here I found grappling with such a subject as Bees' cells and comparing all my notes made during 20 years took up a despairing length of time " Has found Gray's letters very useful, has been lately looking them over and quoting from them, but assures him he will not quote anything he would dislike, for he tries to be very cautious on this head, hopes Gray may succeed in getting his "'incubus' of old work" off his hands and be in some degree a free man; Agassiz has most kindly sent him his Introduction to his First Part, but he confesses he is disappointed; "I cannot realize his rules on the value of the higher groups "

L L
I, 510

A L S 6 pp 20 cm x 13 cm. No 25

[13]

1 Months before the first edition of Origin of Species appeared, in November, 1859, Darwin had begun work on a second edition, to which this letter seems to refer

[1859*]
Aug. 4

Thanks Gray for the "great trouble which you have taken about the New Edit[ion] of The Origin [of Species]. From what you say it is evidently hopeless and I am sorry . . . for my own sake and for all your labor in vain. . . . Although the book is complete and bound, Murray for trade reasons will not sell it till November but he promised to send a copy to you." Hopes to begin printing his new book toward the close of the year, and will send sheets as printed in hopes that Gray will have the great kindness to spare for an American edition. Appended is a brief note from Asa Gray to Mr. Fribis [?] asking him to forward a line which can be sent to Darwin to encourage him to send advance sheets of his new book.

L. S. 3 pp. 20 cm x 13 cm. No. 85 [14]

[1859]
Nov. 11

Is ending abstract of The Origin of Species and will be infinitely gratified if Gray will read it and take time to send however short a note telling what he thinks are its weakest and best parts; "As you are not a geologist you will excuse my conceit in telling you that [Sir Charles] Lyell highly approves of the two geological chapters, and thinks that on the Imperfection of the Geological Record not exaggerated. He is nearly a convert to my views." Has been so hard worked and his health is so poor he has not yet read Gray's Japan Flora ["Diagnostic Characters of New Species of Phaenogamous Plants collected in Japan by Charles Wright. . . . with Observations upon the Relations of the Japanese Flora to that of North America and of other parts of the Northern Temperate Zone", Memoirs of American Academy of Arts and Science, Vol. VI, p. 377, 1857]; [Sir Joseph] Hooker has sent him a few pages in which Gray propounds a doctrine of migration into America like that which he [Darwin] sent Gray last summer; [James Dwight] Dana is, of course, far better authority than he, but Dana's arguments have by no means convinced him regarding the warm period subsequent to the Glacial Period; rather doubts whether Dana's and Gray's view will explain facts of distribution so well as his own view of migration during the certainly warmer period preceding the Glacial Period; he thought of Gray's doctrine several years ago and consulted Lyell, but rejected it as less safe than the warm period "antierioring" the Glacial Period; "There seemed to me some little confusion about your fossil elephants; the species in N. and S. States, I believe, are distinct according to [Hugh] Falconer. The northern one, anyhow, can hardly be adduced as evidence of warmer climate." Thinks Gray's Japan work must have been extremely interesting; asks if Gray has succeeded in getting any information on correlation of complexion in Europeans and tendency to yellow fever; fully admits there are many difficulties not satisfactorily explained by his theory of descent with modification, but cannot possibly believe that a false theory would explain so many classes of facts as he thinks it certainly does explain; "On these grounds I drop my anchor and believe that the difficulties will slowly disappear."

A. J. S. 7 pp. 18 cm x 11 cm. No. 17 [15]

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II, 13

L.L.
II, 13

* An asterisk denotes that the date of the year has been added in pencil, not in Darwin's handwriting, on the original manuscript.

[1859]
Nov. 29

Thanks Gray for his views; "Every criticism from a good man is of value to me; . . . you hint that my work will be grievously hypothetical, . . . my commonest error being probably induction from too few facts. I had not thought of your objection of my using the term 'Natural Selection' as an agent; . . . otherwise I should . . . have to expand it into . . . 'the tendency to the preservation (owing to the severe struggle for life to which all organic beings at some time or generation are exposed) of any to slightest variation in any part, which is of the slightest use or favorable to the life of the individual which has thus varied; together with the tendency to its inheritance'. Any variation, which was of no use whatever to the individual, would not be preserved by this process of 'natural selection'. . . Several varieties of sheep have been turned out together on the Cumberland Mountains, and one particular breed is found to succeed so much better . . . that it fairly starves the others to death. I should better say that natural selection picks out this breed, and would tend to improve it or aboriginally to have formed it." Thanks Gray for seed and specimen of Adlumia; from watching bees suck Fumaria he sees no difficulty whatever in their crossing the individuals; would venture to predict that it has a nectary on both sides, for the "sort of cap" of joined petals can be pushed with equal easiness both ways, but when there is only one nectary it can be pushed only one way; "Lecocq, I observe, brings forward Fumaria as a genus which should never be crossed by natural means, whereas I suspect its structure is formed in direct relation to former crossing!" Has sent Gardeners' Chronicle with notice on Kidney Beans; since writing it, he has received a "most curious" lot of Beans naturally crossed, and with the seed coats affected by the act of fertilization like Gartner's Pea-case; "I must tell you what I heard yesterday . . . on the subject of crossing of individuals. Barnacles are hermaphrodite and with their well shut-up shell offer as great a difficulty to crossing as can well be conceived. I found an individual with monstrous and imperforate penis, but yet with fertilized ova; but I did not know whether it might not be a case of parthogenesis [parthenogenesis] or a strange accident of some floating spermatozoa; well yesterday I had an account of a man watching some shells, saw one protrude its long [word illegible] formed penis, and insert it in the shell of an adjoining individual! So here is a load off my mind. You speak of species not having any material base to rest on; but is this any greater hardship than deciding what deserves to be called a variety and be designated by a Greek letter. . . . What a jump it is from a well-marked variety produced by natural cause, to a species produced by the separate act of the Hand of God. . . . [John] Phillips, the Palaeontologist . . . asked me 'How do you define a species?' I answered 'I cannot.' Whereupon he said 'At last I have found out the only true definition-'any form which has ever had a specific name!'" Thanks Gray for considering again a list of close species; "If you do it, will you please take, if in your power, large and small orders as they come, for possibly there may be some

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I, 126

difference in the rule in large natural and small broken families I intend to go into this with [Karl Friedrich von] Ledebour, as far as mere varieties are concerned. In all Ledebour and many other Floras, I find the rule universal of the large genera presenting most varieties. In the British Flora, by Mr. [Hewett Cottrell] Watson's aid, I have struck out the most trifling varieties and I find the rule holds good, as it also does with the forms which most British Botanists rank as species, but which some one Botanist has considered a variety. This rule, as I must consider it of the large genera varying most, I look at as most important for my work and I believe it to be the foundation of the manner in which all beings are grouped in classes together with what I rather vaguely call my principle of divergence, - the tending to the preservation from extinction of the most different members of each group " Wishes he knew what large moth or "Humble Bee" visits and fertilizes *Lobelia fulgens* in its native home; asks if Gray knows any southern botanist who would observe; suggests covering a plant with a very coarse gauze cap, and believes that not a pod would then set

A L S 12 pp. 20 cm x 13 cm. No 18 [16]

[1859] Has received Gray's kind, long and valuable letter, would be glad of an American Edition [of Origin of Species]; has made up his mind to be "well abused", but thinks it important his notions should be read by intelligent men, accustomed to scientific argument, though not naturalists, "It may seem absurd but I think such men will drag after them those naturalists, who have too firmly fixed in their heads that a species is an entity." First Edition of 1250 copies was sold the first day, and now his publisher is printing as rapidly as possible 3,000 more; would be obliged if Gray could aid an American Reprint and could make, "for my sake and publisher's", any arrangement for any profit; New Edition is only a Reprint, yet he has made a few important corrections; will send clean sheets in a few days of as many as are printed and remainder afterwards, and Gray may do anything he likes; would be glad for New Edition to be printed, and not the old.

Dec. 21 A L S 4 pp. 20 cm x 13 cm. No. 16 [17] L L II, 39

[1859] Has been at water cure for 10 weeks; expresses thanks for a copy of Gray's [Japan] memoir ["Diagnostic Characters of New Species of Phænogamous Plants collected in Japan by Charles Wright with Observations upon the Relations of the Japanese Flora to that of North America and of other parts of the Northern Temperate Zone", Memoirs of American Academy of Arts and Science, Vol. VI, p 377, 1857] which he found on his return through London; he has never for a moment put himself in competition with [James Dwight] Dana on the subject of climate, but "when one has thought on subject, one cannot avoid forming some opinion." [Sir Joseph] Hooker has asked him to write Gray on the subject, but he [Darwin] told Hooker he would not presume to express an opinion to Gray without careful deliberation; saw several years ago some speculation on a warmer period in the United States subsequent to the Glacial Period, consulted [Sir Charles] Lyell, whose judgment "is

Dec. 24 M L I, 455

really admirably cautious", and who seemed much to doubt; Gray's arguments in his paper and in his letter seem hardly sufficient, not that he [Darwin] would be at all sorry to admit this subsequent and intercalated warmer period; does not believe that introduction of Old World forms into the New World subsequent to the Glacial Period will do for the modified forms in the two worlds, as there has been too much change in comparison with the little change of the isolate Alpine forms; doubts whether meteorological knowledge is sufficient for the deduction that land was lower subsequent to the Glacial Period as evidenced by the whale; says it might be argued that a greater extent of water in the southern hemisphere made the temperature lower, and when much of the northern land was lower it would have been covered by the sea and intermigration between the Old and New World would have been checked; doubts whether any inference on the nature of climate can be deduced from extinct species of mammals; asks who would have ventured to surmise the excessively cold climate under which the musk ox and deer lived if they had been known only by fossil bones; refers to his Journal of Researches [into the Natural History and Geology of the countries visited during the Voyage of H. M. S. Beagle round the world, under the command of Capt. Fitz-Roy, R. N., 1845, Murray's Home and Colonial Library, Ch. V, p. 85] for food of large animals; it is inferred in England, from remains of elephants, that the climate at the period of their embedment was very severe; had formerly gathered from Lyell that the relative position of Megatherium and Mylodon with respect to glacial deposits had not been well made out, that it may have been solved recently, but that such are his reasons, which may be wrong and which he will not be sorry to have proved so, for not admitting a warmer period subsequent to the Glacial Epoch; will read Gray's essay with care, and thinks it very likely that some facts he could not formerly clearly understand will be clear enough; regrets Hooker's saying a word about his opinion; is interested in what Gray writes about "Creation" and the philosophy of the subject; "I rest on the fact that the theory of natural selection explains many lapses of facts, which, as far as we can see, repeated acts of Creation do not explain. On this latter view we can only say 'so it is' and not at all 'why it is so' Pray do not decide either way till you have read Ch. XIII [Origin of Species?] and the Recapitulation Ch. XIV which will, I think, aid you in balancing facts." Rejoices that Lyell, [Thomas Henry] Huxley, [William Benjamin] Carpenter, and H[ewett] C[ottrell] Watson are "converts", but still has many "bitter opponents"; he had written out the Forberian doctrine of Alpine plants 4 years before [Edward] Forbes published, as Hooker knew, but he does not believe Forbes ever heard of it

A L S 6 pp. 21 cm x 16 cm. No. 46

[18]

[1860]
Jan. 7

Has just read with great interest Gray's Japan memoir ["Diagnostic Characters of New Species of Phaenogamous Plants collected in Japan by Charles Wright . . . with Observations upon the Relations of the Japanese Flora to that of North America and of other parts of the Northern Temperate Zone", Memoirs of American Academy of Arts and Science, Vol. VI, p. 377, 1857] and it seems to him a most curious case of distribution; "How very well you argue and put the case for analogy on the high probability of single centers of creation. That great man Agassiz, when he comes to reason seems to me as great in taking a wrong view as he is great in observing and classifying. One of the points which has struck me as most remarkable and inexplicable in your memoir is the number of monotypic . . . genera amongst the representative forms of Japan and N. America. And how very singular the preponderance of identical and representative species in Eastern compared with Western America." He has no good map showing how wide the moderately low country is on the west side of the Rocky Mountains, nor does he know whether the whole of the low western territory has been botanized; it has occurred to him, looking at such maps as he has, that the eastern area must be larger than the western, which would account, to a small extent, for preponderance on the eastern side of the representative species; asks if there is any truth in this suspicion; Gray's memoir sets one marvelling and reflecting; confesses he is not able to understand Gray's geology on pages 447 and 448; was grieved to get a letter from [James Dwight] Dana at Florence giving a very poor account of his health; "What an admirable memoir on the distribution of Australian plants is that by [Sir Joseph] Hooker!"

A. L. S. 4 pp. 20 cm x 13 cm. No. 15

[19]

M L
I,459

[1860]
Jan. 28

[Sir Joseph] Hooker has forwarded to him Gray's letter; "I cannot express how deeply it has gratified me. To receive the approval of a man, whom one has long most sincerely respected, and whose judgment and knowledge are universally admitted, is the highest reward an author can possibly wish for." Has been absent from home for a few days so could not earlier answer Gray's letter; thanks him for taking so much trouble and interest about the [American] Edition [of The Origin of Species]; his publisher has made a mistake not thinking of sending the sheets; he himself had entirely forgotten Gray's offer of receiving them as printed off, but had he remembered he feels sure he would not have taken advantage of the offer, for he never dreamed of his book being so successful with general readers; "I believe I should have laughed at the idea of sending the sheets to America." On the strong advice of [Sir Charles] Lyell and others he has resolved to leave the present book as it is and to use all his strength, "which is but little", to bring out the first part of the 3 volumes which will make his bigger work; is therefore very unwilling to take up time making corrections for an American Edition; is enclosing a list of a few corrections in the second reprint, and could send 4 or 5 corrections or additions of equal brevity; also intends to write a short preface with a brief history of the subject which he will send Gray in a short time, "unless I hear that you have given up all idea

L L
II,63

of separate edition You will then be able to judge whether it is worth having new Edition with your Review prefixed. Whatever be the nature of your Review, I assure you I should feel it a great honour to have my Book thus preceded." Nothing would induce him to touch a penny from the American Edition; his terms with Murray are that he receives 2/3 of the profits and Murray 1/3; expects Gray will not consider a new edition worth thinking about, "though an answer to Agassiz would be a great advantage to the subject." Thanks Gray for telling him [Jeffries] Wyman's "magnificent compliment", and for sending the extract from Agassiz; "I cannot see the force of his argument; and if he wished to puff my book he could not have been more ingenious." Is delighted to hear that H[enry] D[arwin] Rogers, Professor at Glasgow and "so excellent a geologist", goes a very long way with his views; will value at any time Gray's criticism either in reviews or by letter. [No enclosure].

A L S 6 pp. 20 cm x 13 cm. No. 43

[20]

L L
II,63

[1860]
Feb. 18

Has received two sheets of Gray's review, [American Journal of Science and Arts, March, 1860 Reprinted in Darwiniana, 1876], read them and sent them to [Sir Joseph] Hooker; they are now returned, reread with care and sent to [Sir Charles] Lyell; it seems admirable - by far the best he had read; thanks Gray from the bottom of his heart for himself and for the subject's sake; "How curious your contrast between the views of Agassiz and such as mine." Hopes Gray will tell him if Agassiz writes anything on the subject; Gray's distinction between a hypothesis and a theory seems to him very ingenious, but he does not think it is ever followed; "Everyone now speaks of the undulatory theory of light; yet the ether is itself hypothetical and the undulations are inferred only from explaining the phenomenon of light. Even in the theory of gravitation, is the attractive power in any way known, except by explaining the fall of the apple and the movements of the Planets?" A review on his book in the last Annals and Magazine of Natural History is "rather bitter"; he feels sure it is by "my good friend the entomologist [Thomas Vernon] Wollaston. . . . Several clergymen go far with me. - Rev. L. Jenyns, a very good naturalist. [John Stevens] Henslow will go a very little way with me, and is not shocked at me. He has just been visiting me."

A L S 4 pp. 20 cm x 13 cm. No. 22

[21]

L L
II,80

[1860
Feb. ?]

Sends short Historical Preface and one page more of corrections; has "shamefully blundered" about Agassiz, but considers [Karl Ernst] von Baer fully as good an authority - "some would say better"; asks Gray to forward enclosure to Agassiz; is writing all the latter part of the volume [Origin of Species, new edition] from memory and feels it is a blessing more blunders have not yet been detected, but the state of his health left him no choice; if the manuscript is of no use no harm is done; has received Gray's letter of Jan. 23 which tells of "all the trouble" Gray has taken about an American Edition; fears there is no chance of Gray's review now appearing at the head, which would have greatly pleased him; asks

that any errors like "self fertilize itself" be corrected; thanks Gray heartily for all his generous kindness; "What you say about my Book gratifies me most deeply and I wish I could feel all was deserved by me. . . . A Review from a man who is not an entire convert, if fair and moderately favorable, is in all respects the best kind of Review. About weak points I agree. The eye to this day gives me a cold shudder, but when I think of the fine known gradations, my reason tells me I ought to conquer the cold shudder." Agrees about sterility, and especially about fertility of the strongest marked varieties, and in the manuscript already sent he has confessed more plainly the difficulty; "But a vast number of facts show how mysteriously and easily the reproductive system is affected" Asks Gray to tell Prof. [Jeffries] Wyman, for whose opinion he has the "highest respect", how very grateful he would be for any hints, information, or criticism; is sorry about [James Dwight] Dana's health; he puts down in his own mind Gray and 3 others as the judges whose opinions he values most of all; "If you keep the subject of Origin of Species before your mind, you will go further and further in your belief. It took me long years, and . . . I am astonished at the impression my Book has made on many minds. I fear 20 years ago I should not have been half as candid and open to conviction." [No enclosure].

A L S 2 pp 31 cm x 23 cm No. 11

[22]

[1860]
Apr. 3

He knows what a busy man Gray is, and asks him not to waste more time over him; "My book [Origin of Species], your Review, and letters etc. must have consumed an awful amount" Feels sure time spent on the review has not been wasted, for it will produce a great effect in leading people to think, and that is all he wants; [Sir Joseph] Hooker knows cases where Gray's articles have "greatly mollified opposition" to his book; "It is curious that I remember well time when the thought of the eye made me cold all over, but I have got over this stage of the complaint, and now small trifling particulars of structure often make me very uncomfortable. The sight of a feather in a peacock's tail, whenever I gaze at it, makes me sick! Under this point of view your story of the Black Pigs in the Everglades delights me, and supports other cases, which though founded on very good evidence I could hardly digest" Asks Gray to "keep Prof. [Jeffries] Wyman up to the mark" about writing him, would consider it a great favor if Gray could find out positively the name of the red nuts; is very curious to see Agassiz's remarks; "I met a few days ago Prof. [Josiah Parsons] Cooke of your Cambridge and he brought me direct from Agassiz all sorts of very civil speeches. I hope to God A[agassiz] is a sincere man; I had always fancied that he was so." [Adam] Sedgwick has reviewed his [Darwin's] book "savagely and unfairly" in the Spectator; "The notice includes much abuse and is hardly fair in several respects. He would actually lead anyone, who was ignorant of geology, to suppose that I had invented the great gaps between successive geological formations; instead of its

[1860]
Apr 3

being an almost universally admitted dogma. But my dear old friend Sedgwick with his noble heart is old and is rabid with indignation. It is hard to please everyone; you may remember that in my last letter I asked you to leave out about the Weald denudation; I told [Joseph Beete] Jukes this (who is head man of the Irish geological survey) and he blamed me much for he believed every word of it, and thought it not at all exaggerated! In fact geologists have no means of gauging the infinitude of past time." Tells of a "prodigy of a Review", in opposition, by [Francois Jules] Pictet, the palaeontologist, in the Bibliothèque Universelle of Geneva, which he calls perfectly fair, "our only difference being that he attaches less weight to arguments in favor and more to arguments opposed, than I do. Of all the opposed reviews I think this the only quite fair one, and I never expected to see one." Does not class Gray's review as opposed, though Gray himself thinks so; "It has done me much too good service ever to appear in that rank in my eyes. I should rather think there was a good chance of my becoming the most egotistical man in all Europe! What a proud preeminence! Well, you have helped to make me so, and therefore you must forgive me if you can."

A. L. S. 8 pp. 20 cm x 13 cm. No. 47

[23]

L L
II, 90

[1860]
Apr. 25

Thanks Gray for copy of "Review of the Origin" [The Origin of Species] in North American Review; "It seems to me clever, and I do not doubt will damage my Book. I had meant to have made some remarks on it; but [Sir Charles] Lyell wished to keep it. . . . The Reviewer is wrong about Bees' cells, i.e. about the distance; any lesser distance would do, or even greater distance, but then some of the planes would lie outside the generative spheres, but this would not add much difficulty to work. The Reviewer takes a strange view of instinct; he seems to regard intelligence as developed instinct; which I believe is wholly false. I suspect he has never much attended to instinct and minds of animals, except perhaps by reading." Requests Gray to procure for him a copy of New York Times for Wednesday, Mar. 28, as it contains a very striking review of his book - one "not really useful, but . . . impressive." Asks if Gray has seen how [Sir Richard] Owen "thrashed" him in the last Edinburgh Review [April, 1860]; "He misquotes and misrepresents me badly, and how he lauds himself. But the manner in which he sneers at [Sir Joseph] Hooker is scandalous. . . . When Hooker's Essay appeared, Owen wrote a note, which I have seen, full of strongest praise! . . . All say his malignity is merely envy because my Book has made a little noise. How strange it is that he can be envious about a naturalist like myself, immeasurably his inferior! But it has amazed me a good deal to be treated thus by a friend of 25 years' duration. He might have been just as severe without being so spiteful. Owen consoles himself by saying that the whole subject will be forgotten in ten years." Hooker is planning to experiment on various subjects at Kew, including the attempt to degenerate culinary vegetables; hopes he will not get too much immersed in his and Bentham's Genera Plantarum, [1862], so as not to spare some time for Geographical Distribution and other such questions; "I have begun to work steadily, but very slowly as usual, at details on Variation under Domestication [The Variation of Animals and Plants under Domestication]."

A L S 6 pp. 20 c 13 cm. No. 13

[24]

L L
II, 98

L L
II, 98

[1860]
May 22

Thanks Gray for sending L22; is astonished at all the "kind trouble" Gray has taken for him; returns Appleton's account; sends a formal acknowledgment, in case Gray wishes it; asks Gray to thank Appleton for his generosity, "for it is generosity in my opinion" Is not at all surprised at the sale [of The Origin of Species] diminishing; "my extreme surprise is at the greatness of sale No doubt the public has been shamefully imposed on! for they bought the book, thinking that it would be nice easy reading I expect the sale to stop soon in England; yet [Sir Charles] Lyell wrote to me the other day that calling at Murray's he heard that fifty copies had gone in previous 48 hours." Is glad Gray will notice in Silliman ['s Journal of Science] additions in The Origin [of Species]; "Judging from letters (and I have just seen one from [George Henry Kendrick] Thwaites to [Sir Joseph] Hooker), and from remarks, the most serious omission in my book was not explaining how it is, as I believe, that all forms do not necessarily advance, - how there can be simple organisms still existing" Mentions reviews by [William Benjamin] Carpenter and [Francois Jules] Pictet; "[Adam] Sedgwick has been firing broadsides at me. . . Prof. [James Henry?] Clarke of Cambridge says publicly that the chief characteristic of such books as mine is their 'consummate impudence'" Mentions other reviews; would be glad to see any good American reviews; "[Thomas Henry] Huxley told me some time ago that after a time he would write review on all the Reviews, whether he will I know not" [Sir Richard] Owen's review in Saturday Review [of Politics, Literature, Science and Art], "one of our cleverest periodicals", defends Huxley but not Hooker, whom Owen, he thinks, treats most ungenerously; "With respect to the theological view of the question this is always painful to me I am bewildered I had no intention to write atheistically But I own that I cannot see, as plainly as others do, and as I should wish to do, evidence of design and beneficence on all sides of us There seems to me too much misery in the world. I cannot persuade myself that a beneficent and omnipotent God would have designedly created the Ichneumonidae with the express intention of their feeding within the living bodies of Caterpillars, or that a cat should play with mice Not believing this, I see no necessity in the belief that the eye was expressly designed. On the other hand I cannot anyhow be contented to view this wonderful universe and especially the nature of man, and to conclude that everything is the result of brute force I am inclined to look at everything as resulting from designed laws, with the details, whether good or bad, left to the working out of what we may call chance. Not that this notion at all satisfies me. I feel most deeply that the whole subject is too profound for the human intellect. A dog might as well speculate on the mind of [Sir Isaac] Newton. Let each man hope and believe what he can. Certainly I agree with you that my views are not at all necessarily atheistical. The lightning kills a man, whether a good one or bad one, owing to the excessively complex action of natural laws, a child (who may turn out an idiot) is born by action of even more complex laws, - and I can see no reason, why a man, or other animal, may not have been oboriginally produced by other laws; and that all these laws may have been expressly designed by an omniscient Creator, who foresaw every future event and consequence. But the more I think the more bewildered I become; as indeed I probably have shown by this letter"

L L
II,104

L L
II,104

L L
II,104

[1860]
June 8

Has anticipated Gray's request by making a few remarks on [Sir M L Richard] Owen's review [of Origin of Species]; "I have lately had I,152 many 'more kicks than half-pence'. A review in the last Dublin Natural History Mag[azine] is . . . one mass of misrepresentation. It is evidently by [Samuel] Houghton, the geologist, chemist and mathematician. It shows immeasurable conceit and contempt of all, who are not mathematicians. . . . The article is a curiosity of unfairness and arrogance. . . . It is clear he cannot reason. He is a friend of [William Henry] Harvey with whom I have had some correspondence. Your article has clearly as he admits, influenced him. He admits to certain extent Natural Selection, yet I am sure he does not understand me. It is strange that very few do, and I am become quite convinced that I must be an extremely bad explainer. . . . Owen . . . grossly misrepresents and is very unfair to [Thomas Henry] Huxley. You say that you think the article must be by a pupil of Owen; but no one fact tells so strongly against Owen, considering his former position at College of Surgeons, as that he has never reared one pupil or follower. . . . In . . . Fraser's Magazine, there is an article or Review on [Jean de] Lamarck and me by W[illiam] Hopkins, the mathematician; who like Houghton despises the reasoning powers of all Naturalists. Personally he is extremely kind toward me; but he evidently in the following number means to blow me into atoms. He does not in least appreciate the difference in my views and Lamarck's, as explaining adaptation the principal of divergence, the increase of dominant groups, and the almost necessary extinction of the less dominant and smaller groups." Has lately been looking at the common L L Orchids, and though the facts may be "as old and well-known as the II,438 hills" he has been so "struck with admiration at the contrivances" that he has sent notices to the Gardeners' Chronicle; *Ophrys apifera* offers, as Gray will see, a curious contradiction in structure; gets on very slowly with his larger work on account of his daughter's illness; has been making observations with Primroses and Cowslips which L L have interested and perplexed him; finds about an equal number of II,472 male and female plants; expected to find the male plants barren, but, judging from the feel of the capsules, this is not the case, and he is very much surprised at the difference in the pollen, which he measured many times and always found 2 to 3/6000 of an inch less in diameter in the female than in the male plants; "If it should prove that the so-called male plants produce less seed than the so-called female, what a beautiful case of gradation from hermaphrodite to unisexual condition it would be! If they produce about an equal number of seed, how perplexing it will be."

A. L. S. 7 pp. 20 cm x 13 cm. No. 40

[26]

At water cure.

[1860]
July 3

Received the mathematical papers, but has not yet had time to try to understand them; has had an unhappy household of late; his eldest daughter has been in bed for 9 weeks with fever, but at last it was possible to move her; it will soon be necessary to take her to the seaside; his own health "quite broke down" from anxiety, and he needs a "considerable" change; has done almost nothing for 6 weeks; is very sorry to hear how extremely hard Gray is pressed with work; "It is a pity that you should spend more time over Reviews of my Book, [Origin of Species]; you have . . . been of incalculable service. I feel very grateful; though I know well that this is not a personal affair; you wish that the subject should be fairly treated and discussed. Nevertheless I cannot help feeling deeply obliged to you I can now very plainly see from many late Reviews, that I should have been fairly annihilated, had it not been for 4 or 5 men, including yourself." A letter from [Sir Joseph] Hooker, at B[ritish] Assoc[iation] at Oxford, tells him there was one day a "savage fight" on his book between [Sir Richard] Owen and [Thomas Henry] Huxley, and subsequently a discussion of "utmost warmth", of 4 hours' duration, on a paper by [John William] Draper, of the United States, in which his [Darwin's] book became the subject; "Bishop [Samuel Wilberforce] of Oxford, one of the most eloquent men in England, ridiculed me at great length and with much spirit; and Hooker answered him, I imagine, with wonderful spirit and success. Owen will not prove right, when he said that the whole subject would be forgotten in 10 years. My book has stirred up the mud with a vengeance; and it will be a blessing to me if all my friends do not get to hate me. But I look at it as certain, if I had not stirred up the mud some one else would very soon; so that the sooner the battle is fought the sooner it will be settled; not that the subject will be settled in our lives' time. It will be an immense gain, if the question becomes a fairly open one; so that each man may try his new facts on it pro and contra." Thanks Gray about the New York Times; "I daresay you will be disappointed with the article; and I cannot for the life of me tell what it is that struck [Sir Charles] Lyell and me in it - I hope I may find it at home when I return there in 3 or 4 days." Will order the 2 numbers of Atlantic [Monthly] when he knows which months contain Gray's articles, as he will be very anxious to see them; has just reread Gray's letter; "in truth I am myself quite conscious that my mind is in a muddle about 'designed laws' and 'undesigned consequences' Does not [Immanuel] Kant say that there are several subjects on which directly opposite conclusions can be proved true?" Refers to a "strong" article by "our great man", W[illiam] Hopkins, in Fraser's Magazine for July; "It is written with very fair spirit and without more of the arrogance of a mathematician, than might have been expected. I have remonstrated with him for so coolly saying that I base my views on what I rank as great difficulties. Anyone by taking these difficulties alone can make a most strong case against me. I could myself write a more daring Review than has as yet appeared! On question of Hybridity he passes over the . . . fine graduations

from utter sterility to complete fertility - the fertility of some hybrids - and the sterility of Verbascum, and of Tobacco, which latter facts you, by the way, never notice " Gray's letter gave him much pleasure, but he begs him not to write while he is so over-worked; has "this minute" received a letter from Lyell who is just starting for the continent; on his return he [Lyell] is going to investigate reported cases of the Hippopotamus subsequent to the Glacial Epoch; he [Lyell] finds others now believe in this remarkable fact; if so, there has probably been in Europe a warmer period subsequent to the Glacial Epoch, "Do you remember my saying that I hoped I should be proved wrong to punish me for disbelieving in you, and it seems that my punishment is at hand!" Not being able lately to work, he has "amused" himself about Orchids, has been "struck with amazement" at the beauty of the contrivances with respect to fertilization by insects; the insect led him to find that the 2 horns in *Gymnadenia conopsea* are stigmas; asks if Gray knows Hooker's paper on *Listeria*; "he misapprehended meaning of his facts " Finds the rostellum so delicate that the explosion takes place by touch of human hair, and the fluid sets hard in under 2 seconds; "It was really beautiful to see a little insect licking the labellum, and as soon as its head touched the rostellum the explosion took place, and the insect crawled out with the 2 pollen-masses stuck to its forehead, ready to impregnate next flower into which it crawled One word more on 'designed laws' and 'undesigned results' I see a bird which I want for food, take my gun and kill it. I do this designedly. An innocent and good man stands under tree and is killed by flash of lightning Do you believe (and I really should like to hear) that God designedly killed this man? Many or most persons do believe this; I can't and don't If you believe so, do you believe that when a swallow snaps up a gnat that God designed that that particular swallow should snap up that particular gnat at that particular instant? I believe that the man and the gnat are in the same predicament If the death of neither man or gnat are designed, I see no good reason to believe that their first birth or production should be necessarily designed Yet I cannot persuade myself that electricity acts, that the tree grows, that man aspires to loftiest conceptions all from blind, brute force "

A L S 10 pp 20 cm x 13 cm No 41 [27]

L L
I, 284[1860]
July 22

[Hartfield, Sussex]

Due to absence from home at water cure and his daughter's illness he has only lately read the discussion in the American Acad- [my]; expresses his sincere admiration for Gray's clear powers of reasoning, quotes [Sir Joseph] Hooker as saying that Gray, more than any one else, is the thorough master of the subject; "I declare that you know my book [Origin of Species] as well as I do myself, and bring to the question new lines of illustration and argument, in a manner which excites my astonishment and almost my envy' I admire these discussions, I think, almost more than your article in *Silliman's Journal* [of Science] Every single word seems weighed carefully, and tells like a 32-pound shot " Wishes Gray had time to write more in detail and give facts on the variability of American

L L
II, 119

wild fruits; has sent his copy to the Athenaeum, which has the largest circulation, with a request that the editor republish the first discussion; fears he will not, "as he reviewed the subject in so hostile a spirit, and is not a liberal man" Will order the August Atlantic Monthly as soon as he knows it contains Gray's "Review of Reviews". "My conclusion is that you have made a mistake in being a Botanist. you ought to have been a Lawyer, and you would have rolled in wealth." A review in the Quarterly [Journal of Science] by Bishop [Samuel] Wilberforce of Oxford, aided by [Sir Richard] Owen, "is uncommonly clever, not worth anything scientifically, but quizzes me in splendid style. I chuckled with laughter at myself From all I gather, the B[ritish] Assoc[iation] at Oxford advanced the subject greatly, owing to the effort of Hooker, [Thomas Henry] Huxley and [Sir John William] Lubbock, notwithstanding incessant attacks. [John Stevens] Henslow and [Charles Giles Bridle] Daubeny are shaken I hear from Hooker that he hears from [Baron Ferdinand Christian von] Hochstetter that my views are making very considerable progress in Germany." [H G] Bronn, at the end of his translation, has a chapter of criticism, but the German is so difficult he has not yet read it; [William] Hopkins' review in Fraser's Magazine is thought "the best which has appeared against us. I believe that Hopkins is so much opposed because his course of study has never led him to reflect much on such subjects as Geograph[ical] Distribution, Classification, Homologies, etc so that he does not feel it a relief to have some kind of explanation" Sees most clearly that his book would have been a "dead failure had it not been for all the generous labor bestowed on it . . . by yourself, Hooker, Huxley, and [William Benjamin] Carpenter; and to these names I hope soon [Sir Charles] Lyell's may be added. But I know the Quarterly will cause Lyell to shake in his shoes. Considering his age, his former views and position in the Society, I think his conduct has been heroic on this subject."

A L S 3 pp 26 cm x 21 cm No. 30

[28]

L L
II, 119

L L
II, 119

[1860]
Aug. 11

On his return from Sussex about a week ago he found several articles sent by Gray, is very glad to possess the one from the Atlantic Monthly; the editor of the Athenaeum has inserted Gray's answers to "Agassiz, Bowen, & Co.", and when he read them there he admired them even more than at first; "They really seem to me admirable in their condensation; force, clearness and novelty I am surprised that Agassiz did not succeed in writing something better How absurd that logical quibble - 'if species do not exist, how can they vary?' As if anyone doubted their temporary existence. How coolly he assumes that there is some clearly defined distinction between individual differences and varieties It is no wonder that a man who calls identical forms when found in two countries distinct species, cannot find variation in nature Again how unreasonable to suppose that domestic varieties selected by man for his own fancy (p 147) should resemble natural varieties or species. The whole article seems to me poor; it seems to me hardly worth a detailed answer (even if I could do it, and I much doubt whether I possess your skill in picking out salient points and driving a nail into them) and indeed you have already

L L
II, 125

answered several points. Agassiz's name, no doubt, is a heavy weight L.L
 against us, but yesterday I heard that a man, whom I believe to be II,125
 greater than Agassiz, viz [Karl Ernst] von Baer goes a long
 way . . . way with me, and has spoken out publicly and
 will probably publish. R. Wagner has published, also, in Germany an
 abstract of Agassiz's Essay on Classification, and says he believes
 the truth lies between us two; and this will make A[gassiz] very L.I.
 savage, I should think." Asks Gray to thank Prof. [Theophilus II,125
 Parsons for the extremely liberal and fair spirit in which his essay
 is written, and to tell him that he [Darwin] reflected much on the
 chance of favorable monstrosities - great and sudden variations -
 arising; he has no objection to them, as they would be a great aid;
 but he did not allude to the subject for, after much labor, he could
 find nothing which satisfied him of the probability of such occur-
 rences; "There seems to me in almost every case too much, too complex,
 and too beautiful adaptation in every structure to believe in its sud-
 den production. I have alluded under the head of . . . seeds to
 such possibility. Monsters are apt to be sterile, or not to transmit
 monstrous peculiarities... Look at the fineness of gradation in the
 shells of successive sub-stages of same great formation. I could give
 many other considerations, which made me doubt such view. It holds
 to certain extent with domestic productions no doubt, where man pre-
 serves some abrupt change in structure. It amused me to see Sir
 R[oderick Impey] Murchison quoted as a judge of affinities of animals;
 and it gave me a 'cold shudder' to hear of any one speculating about
 a true Crustacean giving birth to a true Fish!" Gray gives him valu-
 able hints about dioeciodimorphous flowers; is "all at sea" about the
 difference in fertility of Cowslip because he was forced to gather
 his seed too soon, but will try to work the case out next summer; his
 daughter is decidedly better, though still very ill and weak.

A. L. S. 7 pp. 20 cm x 13 cm. No. 35

[29]

[1860]
 Sep. 10

On receipt of Gray's letter, through [Sir Joseph] Hooker, he
 ordered his book seller to send Gray [William] Hopkins' second article
 in Fraser's Magazine; considers himself stupid not to have sent the
Quarterly [Journal of Science], but presumes it has long ago arrived
 in the United States; has ordered Murray to send The Origin [of
 Species] to the American Academy of Science, in Gray's care, as he
 did not know the proper address; thanks Gray for sheets replacing
 those sent to the Athenaeum; sent the copy of this Athenaeum to
 [Sir Charles] Lyell, in Germany, who writes that he thinks Gray's
 argument quite admirable; has not yet received the papers mentioned
 in Gray's letter: "You will have heard of Hooker's astonishing ex-
 pedition to Syria; if he ascends Lebanon, it may answer scientific-
 ally. . . . His absence is so great a loss to me, that I am hardly
 fair judge of the wisdom of the journey." Has the second article in
Atlantic Monthly, for a copy was sent his brother-in-law, Hensleigh
 Wedgwood, on account of a review of his Dictionary; asks who the author
 is, as his brother would like to know; has ordered another copy of
 this August number [of Atlantic Monthly], as he would like to send a
 copy to [Francois Jules] Pictet; has been thinking, and will consult
 [Thomas Henry] Huxley, whether he will not get it reprinted in some

English journal; has ordered, by anticipation, 2 copies of the October number, so Gray need not trouble to send it; is thinking of taking a very great liberty, but, after much consideration, does not think Gray can object; "You said that it was known that you were the author of the 1st article; and as the best chance of getting it reprinted in England in a scientific Journal would be to affix your name, I think of doing this and I hope to Heaven that you will not think this an unwarrantable liberty I think most highly of this article [by Gray, in Atlantic Monthly, July, 1860] and I cannot bear to think it should not be known in England. You will be weary of my praise, but it does strike me as quite admirably argued; and so well and pleasantly written Your many metaphors and inimitably good I said in a former letter that you were a Lawyer; but I made a gross mistake. I am sure that you are a poet No by Jove I will tell you what you are, a hybrid, a complex cross of Lawyer, Poet, Naturalist, and Theologian' Was there ever such a monster seen before?" Has just looked through the passages which he has marked as appearing to him extra good, but sees they are too numerous to specify, "and this is no exaggeration My eye just alights on the happy comparison of colors of prism and our artificial groups I see one little error of fossil cattle in S America. It is curious how each one, I suppose weighs arguments in a different balance; embryology is to me by far strongest single class of facts in favour of change of forms, and not one, I think, of my reviewers has alluded to this Variation not coming on at a very early age, and being inherited at not very early corresponding period, explains, as it seems to me, the grandest of all facts in Nat[ural] History, or rather, in Zoology, viz the resemblance of embryos. Hensleigh Wedgwood is a very strong Theist, and I put it to him, whether he thought that each time a fly was snapped up by a swallow, its death was designed; and he admitted he did not believe so, only that God ordered general laws and left the result to what has been so far called chance, that there was no design in the death of each individual fly "

A L S 8 pp. 20 cm x 13 cm. No. 34

[30]

[1860]
Sep. 26 Statement about guinea pig, by [Karl Ernst] von Baer, can hardly be trusted unless he has brought forward some quite new evidence with respect to unknown wild types; has seen hybrids of rabbit and hare, believes case is to be trusted, but does not know that the exact half-bloods are perfectly fertile; "It is a particularly curious case, because many have perseveringly tried and utterly failed even to unite these two species " Gray's arguments about Design seem excellent; has a feeling that the existence of the multitude of stars and the motion of the planetary system are equally good with living beings to prove a first cause, yet he believes if there were no living beings there could hardly be Design; knows well he is "muddled" on this subject, however; Saturday Review [of Politics, Literature, Science and Art] has lately been discussing Design, so he will send the "Dialogue" to it, of course without Gray's name; whether they will print, he doubts; L L a letter from [William Henry] Harvey against his book [Origin of Species] has some "ingenious and new remarks", but it is an "extraordinary fact" that Harvey does not understand at all what he [Darwin]

L L
II,130

L L
II,137

means by Natural Selection; he has begged Harvey to read the "Dialogue" in the next Silliman ['s Journal of Science], as Gray never touches the subject without making it clearer; [Sir Charles] Lyell, [Sir Joseph] Hooker, and others who perfectly understand his book sometimes use expressions to which he objects, as he had to write his book again he would use "Natural Preservation" and drop "Selection"; Dr. [John Edward] Gray, of British Museum, says, "It is, you know, obviously impossible that there can be any Selection in case of Plants"; is convinced that had it not been for Gray, Hooker, [Thomas Henry] Huxley, and Lyell, his book "would scientifically have been a complete failure"; hopes and almost believes the time will come when Gray will go further in believing a very large amount of modification of species than he did at first or does now; from his immense correspondence with Lyell and Hooker he can perceive that where they objected to much at first, they have, perhaps unconsciously, converted themselves during the last 6 months; finds that the movements of Drosera are really curious and the manner in which the leaves detect certain nitrogenous compounds is marvelous, imagines Gray will laugh, but at present he believes they detect the 1/2880 part of a single grain of nitrate of ammonia, but muriate and sulphate bother their chemical skill and they cannot make anything of the nitrogen in these salts; he began work on Drosera in relation to gradation as throwing light on Dionaea

L L
II,137L L
II,137L L
II,490

A. L. S. 5 pp. 26 cm x 21 cm. No. 28

[31]

Eastbourne

Is in much distress as his daughter has had a relapse, but she is rallying again; is writing hurriedly to say that [Sir Charles] Lyell, "like a good and kind man", has been consulting with Murray about publishing Gray's review [of Origin of Species] in pamphlet, and that although it is against their will to publish pamphlets, they would "break it through this time", but do not advise it, for it would be necessary to spend more in advertisement than the cost of publication, and they are well convinced that it is impossible to get a pamphlet circulated; under these circumstances he feels it would be of no use to attempt it, and he is "much vexed", Lyell has the highest opinion of the "talent and science" shown in the 3 reviews and feels it would be well worth while if a little book could be compiled by Gray; hopes Annals [and Magazine] of Natural History will take the second part, and, if they do, he will try to place the third part with them; offers to pay whole expense of paper and printing "if they will condescend to accept it. I cannot bear that such admirable essays should not be printed in this country." Has no idea whether Saturday Review [of Politics, Literature, Science and Art] will insert dialogue.

A. L. S. 4 pp. 20 cm x 13 cm. No. 33

[32]

[1860*]
Oct. 24

1860*]
ct. 31

15 Marion Parade, Eastbourne.

His daughter is seriously ill; thanks Gray for October Atlantic Monthly; has ordered 2 copies besides; wishes Gray had time to write on affinities in relation to descent with modifications; Gray has done more than he promised in getting his [Darwin's] views a fair hearing; has been reflecting about getting, as Gray suggests, if it can be done, 200 or 250 copies of the 3 articles [review of Origin of Species] of the Atlantic reprinted from the plates in America and sent to England; he would gladly pay £4 or £5, wishes the title page to bear Gray's name and titles; offers to post copies to all the scientific men whose addresses he can get from the Society; feels sure that unless Gray's name is appended the articles would not be received in England, encloses diagram regarding Spiranthera, wishes Gray to observe his own species; if it is a very distinct species, the contrivance will probably differ, as contrivances are endlessly diversified; intends to publish on Drosera; since Gray has spoken of determinate movements for an end in plants, he gives a case in detail, with diagrams, of Orchis pyramidalis. [No enclosure]

A L S 4 pp 20 cm x 16 cm. No. 45

[33]

[1860]
Nov. 26

Thanks Gray for letter with corrections, written before he had received his [Darwin's] letter asking for an American reprint [of Origin of Species] and saying it was hopeless to print Gray's reviews as a pamphlet, owing to the impossibility of getting pamphlets known; is glad to say that the August or second Atlantic Monthly article has been reprinted in the Annals and Magazine of Natural History; read over with care yesterday the third article which seems, as before, admirable; "But I grieve to say that I cannot honestly go as far as you do about Design. I am conscious that I am in an utterly hopeless muddle. I cannot think that the world, as we see it, is the result of chance; and yet I cannot look at each separate thing as the result of Design. To take a crucial example, you lead me to infer (p. 414) that you believe 'that variation has been led along certain beneficial lines' I cannot believe this; and I think you would have to believe, that the tail of the Fan-tail was led to vary in the number and direction of its feathers in order to gratify the caprice of a few men. Yet if the Fan-tail had been a wild bird and had used its abnormal tail for some special end, as to sail before the wind, unlike other birds, every one would have said what beautiful and designed adaptation. Again I say I am, and shall ever remain, in a hopeless muddle." Thanks Gray for [Francis] Bowen's fourth review [Memoirs of American Academy of Arts and Sciences, vol. VIII]; "The coolness with which he makes all animals to be destitute of reason is simply absurd. It is monstrous at p. 103, that he should argue against the possibility of accumulative variation and actually leave out entirely Selection! The chance that an improved Short-Horn, or improved Pouter-pigeon should be produced by accumulative variation, without man's selection is as almost infinity to nothing; so with natural species without natural selection. How capitally in the Atlantic, you show that Geology and Astronomy are according to Bowen Metaphysics; but he leaves out this

L L
II, 145

rubbish in the 4th Memoir." Has just heard that [Emile Heinrich] Du Bois Reymond agrees with him; the sale of his book goes on well and has not been stopped by the multitude of reviews; Murray sold 700 copies a few days ago, so he must begin at once on a new corrected edition; "I will send you a copy; for the chance of your ever re-reading; but good Heavens how sick you must be of it." [Sir Joseph] Hooker has returned, and says he found traces of glacial action on Lebanon; has gone on working on Drosera, but will not publish till next summer as he is "frightened" at his results and must retest them; has been rereading, in consequence, some parts of Gray's Lessons in Botany [First Lessons in Botany and Vegetable Physiology, 1857]; and has been pleased with the extremely clear way he puts things, "but you may rely on the truth of the fact that the prolonged weight of an . . . atom, placed with all care on one of the glands, though it weighed only 1/78,000 of one grain caused conspicuous movement. I got the weight by weighing a length of fine hair and cutting off atoms and measuring them with a micrometer. . . . This suffices to start the movement. Moreover it produces such changes within the cells of the glandular hairs; that an hour after weight had been put on, I could distinguish which hair had carried this fairy weight for all the other 100 and more hairs on the leaf." Asks Gray to observe whether the flowers of Apocynum androsaemifolium catch numbers of flies by their proboscides as in England, and if bees visit the flowers; means to get this plant, if he can, and observe it; was surprised, as a boy, at the number of flies captured; asks Gray to make a memorandum about this plant and the Spiranthus; his daughter is slowly improving.

A L S. 3 pp. 26 cm x 21 cm. No. 27

[34]

[1861] Apr. 11 Expresses pleasure at having received Gray's photograph; is expecting his own which he will send off as soon as it comes; "It is an ugly affair, and I fear the fault does not lie with the photographer." [Chauncy] Wright's review has come, [Thomas Henry] Huxley has taken it away, but they both fear it is too general, although it is very clever; asks what he shall do with it if Huxley does not take it [for his Natural History Review], as he knows no other Review; has received several letters full of the highest commendation of Gray's essay; all agree that it is by far the best thing written, and he feels it has done The Origin [of Species] much good; has not yet heard how it has sold; calls Gray's attention to the review in G[ardeners'] Chronicle; has received the letter of credit returned; is much pleased and surprised at the profit from the American Edition; Gray is to be at no expense about his [Gray's] essay; presumes nothing literary now sells in the "troubled U. S."; "Poor dear [John Stevens] Henslow, to whom I owe much, is dying; and [Sir Joseph] Hooker is with him." Thanks Gray for 2 sets of sheets of his Proceedings; cannot understand what Agassiz is driving at; "You once spoke, I think, of Prof [Francis] Bowen, as a very clever man. I should have thought him a singularly unobservant and weak man from his writings. If ever he agrees with me on any one point, I shall conclude that I must be in error on that. He never can have seen much of animals or he would have seen the difference of old and wise dogs and young ones. His paper about

hereditariness beats everything. Tell a breeder that he might pick out his worst individual animals and breed from them and hope to win a prize; and he would think you not a fool, but insane. I believe Bowen is a metaphysician and that I presume accounts for an entire want of common sense." Reminds Gray that if he inserts grass into *Spiranthes* he must bend or bow it toward the rostellum before he withdraws it; asks him to observe whether wild *Apocynum* catches flies as it does in England; encloses his photograph and one of his eldest son. [No enclosures].

A. L. S. 2 pp. 26 cm x 21 cm. No. 53

[35]

L. L.
II, 164[1861]
May 11

Thanks Gray for 2 or 3 little notes; was glad to receive the one on [Sir Charles] Lyell and will tell him what Gray says on species; is pleased at it, but cannot quite agree; "You speak of Lyell as a Judge; now what I complain of is that he declines to be Judge. It puts me into despair, when I see such men as Lyell and you incapable (as you think) of deciding. I have sometimes almost wished that Lyell had pronounced against me. When I say 'me', I mean my change of species by descent. That seems to me the turning point. Personally, of course, I care much about Natural Selection; but that seems to me utterly unimportant compared to the question of Creation or Modification." Considers Gray's remark about Language and Design "clever and original and candid", "Your little discussion on Angles of Divergence of leaves in a spire has almost driven me mad. My boy George . . . said they formed a converging series . . . I have been drawing all the real and unreal angles . . . and I see the angles which do not occur in nature, are just as symmetrical in position as the real angles. If you wish to save me from a miserable death, do tell me why the angles 1/2, 1/3, 2/5, 3/8, etc series occur, and no other angles. It is enough to drive the quietest man mad. Did you and some mathematician publish some paper on the subject? [Sir Joseph] Hooker says you did; where is it?" Has been visiting relatives to try to get a little health for his youngest boy "the Natural Selection Hero", and for himself, "with very poor success. This has led me to muddle my brain over the angles of leaves. Do you know of any plant in which angle is fluctuating or variable? I often bless science; for when observing I forget my discomfort and at no other time am I comfortable for two successive hours" Has been looking at *Plantago lanceolata*, and finds it is a female dichogamous, which is rather rare "i. e. pistil matures . . . long before anthers of same flower mature; fertilized by the wind; a few plants have imperfect anthers, containing little pollen and a part of this imperfect" Finds *Euphorbia amygdaloides* is also a female dichogamous monoecious plant, and is dioecious in function at any one period.

A. L. S. 4 pp. 20 cm x 16 cm No. 59

[36]

L. I.
II, 235

[1861]
June 5

Hopes Gray has received the Third Edition of The Origin [of Species]; Andrew Murray, an entomologist and horticulturist and now Secretary to the Horticultural Society of London, read a "long and hostile and rather weak Review of the Origin at the Royal Society of Edinburgh." Has heard nothing from Trömer about the sale of Gray's essay so fears it has not been great; sent a copy to Sir [ohn Frederick William] Herschel, and "in his new Edit[ion] of his Physical Geography he has note on the Origin of Species, and agrees to certain limited extent; but puts in a caution on design, so much like yours that I suspect it is borrowed. - I have been led to think more on this subject of late, and grieve to say that I come to differ more from you. It is not that designed variation makes, as it seems to me, my Deity 'Natural Selection' superfluous; but rather from studying lately domestic variation and seeing what an enormous field of undesigned variability there is ready for natural selection to appropriate for any purpose useful to each creature." Thanks Gray for sending his review of [John] Phillips; "I remember once telling you a lot of trades which you ought to have followed; but now I am convinced that you are a born Reviewer. By Jove how well and often you hit the nail on the head." Believes Gray ranks Phillips' book [Life on the Earth, 1860] higher than he does, or than [Sir Charles] Lyell does, "who thinks it fearfully retrograde. I amused myself by parodying Phillips' argument as applied to domestic variation; and you might thus prove that the Duck or Pigeon has not varied because the Goose has not, though more anciently domesticated, and no good reason can be assigned why it has not produced many varieties." Believes small area, compared with sea or land, comes into play with respect to fresh water; the rate of change and of extinction in fresh water having been much slower, hence Ganoid fishes are all fresh water; has been idling and working at Primula and thinks his experiments will explain their dimorphism; knows there are many cases of dimorphic plants, but asks if the two forms are not always borne on the same plant; asks, also, if there are other cases of two forms living mingled in nearly equal numbers; has been working on insect fertilization of Orchids - "beautiful facts"; wants information on Cypripedium; asks if Gray could cover up a plant with net and leave one uncovered, if it be one which sets seeds, and see whether the protected one sets seeds, and whether the pollen of the two after an interval of time is in the same state; asks Gray not to forget to look at flowers of *Spiranthes* just opening, for he wishes to know whether they have the same curious structure as do the English *Spiranthes*; "I never knew the newspapers so profoundly interesting. America does not do England justice; I have not seen or heard of a soul who is not with the North. Some few, and I am one, even wish to God, though at the loss of millions of lives, that the North would proclaim a crusade against slavery. In the long run, a million horrid deaths would be amply repaid in the cause of humanity. What wonderful times we live in. Massachusetts seems to show noble enthusiasm. Great God how I should like to see that greatest curse on earth slavery abolished."

L L
II,165

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II,165

[1861?
Sep. 17

"Thank you sincerely for your very long and interesting letters, L L
political and scientific. . . I agree with much of what you say and II,169
I hope to God we English are utterly wrong in doubting (1) whether
the N[orth] can conquer the S[outh], (2) whether the N[orth] has
many friends in the South, and (3) whether you noble men of Massachu-
setts are right in transferring your own good feelings to the men of
Washington. Again I say I hope to God we are wrong in doubting on
these points. It is number (5) which alone causes England not to be
enthusiastic with you. What it may be in Lancashire I know not, but in
S. England cotton has nothing whatever to do with our doubts. If
abolition does follow with your victory the whole world will look
brighter in my eyes and in many eyes. It would be a great gain even
to stop the spread of Slavery into the Territories, - if that be
possible without abolition, which I should have doubted. You ought
not to wonder so much at England's coldness, when you recollect at
the commencement of the war how many propositions were made to get
things back to the old state with the old line of Latitude.
All I can say is that Massachusetts and the adjoining States have the
full sympathy of every good man whom I see; and this sympathy would
be extended the whole Federal States if we could be persuaded that
your feelings were at all common to them. But enough of this. It is
out of my line, though I read every word of news and formerly well
studied [Frederick Law] Olmsted." His other enclosed letter was un-
fortunately written last night; he sends it because he is not yet
sure Gray understands what he wants; has just looked at Gray's
Manual [of the Botany of the Northern U S] and now sees that the
case of the Rubiaceae is exactly the same as in Primula and Linum;
asks if Gray knows that in any case the pollen of the one form is
not fitted to fully fertilize its own stigma - that sterility ensued
at about the same degree as when allied species are crossed; this
fact seems to him to make the case very interesting; would be grate-
ful for any other cases in other Orders; "I have found an old note
of yours in which you say cases abound in 'Rubiaceae, Borragineae,
Lobirota, etc.'" Asks for seed of any Rubiaceae; though these would
be "bad" plants to experiment on, he could to a certain extent; was
working at Galium cruciatum this summer and found many flowers ex-
clusively male, but did not think of looking at the pollen of the
hermaphrodite flowers; fears he will weary Gray, but must write a
little about Spiranthus; at Torquay he was able to examine growing
flowers before he had examined any cut flowers; "In my note to you
I blundered somewhat. I probably spoke of the 'canoe' as being em-
bedded within the rostellum; so it is in early bud, in so far as
that the back of the 'canoe' is covered with large cells with viscid
matter, which burst and thus attach to the pollinia; a process which
I could never before understand. Just as you say, when flower first
opens only a hair or needle can be passed down and this though
straight, surely removes the pollinia; in this early condition of the
flowers you will never find pollen on stigma; but after 24 - 48 hours,
the Labellum moves a little away and then position of the nectar and
oblique projecting stigma, allow the tips of pollen-masses beautifully
to strike the stigma. If pollen-masses have not been removed in early

period, the burying of the proboscis is necessary for their removal. The Bees which I saw at work (one had 5 canoes, one over the other, stuck to it), alight at the base of spike and go spirally up it; when they get to the upper flowers the pollinia are attached to proboscis; they then fly to another plant and always alighting at base, they insert the pollinia in the lower and more expanded flowers and leave masses of pollen on the stigma as I said. You do not seem to have noticed the rupturing of the front of rostellum, with the most delicate touch, which seemed to me a vital action for the requisite touch was too delicate for mechanical action." As-sures Gray that he tried D[rosera] rotundifolia so often and showed the leaves to so many persons that there can be no mistake in his observations; "For some time . . . after catching a fly the leaf cannot act. This is incidentally of use to plant for whilst the greater number of glands are dry, any useless object like bit of moss or cinder which has been clasped gets easily jerked or blown off. I long to complete my work on Drosera; but must out of virtue defer it till next year; otherwise I shall never, with my small power of work, get even one volume of my larger work finished." Wishes Gray would observe D[rosera] filiformis; can hardly believe that any Drosera does not digest animal food; has found the best plan is to try leaves which have opened after plants have been planted in a soup plate; "Heaven forgive me, if you cannot, for scribbling at such length. Your question what would convince me of Design is a poser. If I saw an angel come down to teach us good, and I was convinced, from others seeing him, that I was not mad, I should believe in Design. If I could be convinced thoroughly that life and mind was in an unknown way a function of other imperderable forces, I should be convinced. If man was made of brass or iron and no way connected with any other organism which had ever lived, I should perhaps be convinced. . . . I have lately been corresponding with [Sir Charles] Lyell, who, I think, adopts your idea of the stream of variation having been led or designed. I have asked him . . . whether he believes that the shape of my nose was designed. If he does, I have nothing more to say. If not, seeing what Fanciers have done by selecting individual differences in the nasal bones of Pigeons, I must think that it is illogical to suppose that the variations, which Nat[ural] Selection, preserves for the good of any being, have been designed. But I know that I am in the same sort of muddle . . . as all the world seems to be in, with respect to free will, and yet with every [word omitted] supposed to have been preserved or pre-ordained." English Holly, and all the cultivated varieties, are absolutely dioecious; has just been looking at Gray's Manual; "Could I anywhere find out a fuller account of the state of some of the species of your Ilex, for instance whether the female flowers have any pollen (ours has anthers but no pollen), and whether the male flowers ever set seeds. Our male trees never bear a berry This would make a good case of gradation. [William Henry] Harvey observed our Holly years ago. I have always felt curious about the steps by which it became dioecious." [No enclosure].

L.L.
II, 169

[1861]
Dec. 11

Sends cordial thanks for 2 most valuable notes from Gray; "What a thing it is, that when you receive this we may be at war, and we may be bound, as good patriots, to hate each other, though I shall find this hating you very hard work. How curious it is to see two countries, just like two angry and silly men, taking so opposite a view of the same transaction. So far as I can see we rest entirely on [Capt. Charles] Wilkes' acting as Judge. I fear there is no shadow of doubt we shall fight, if the two Southern rogues [James M. Masc and John Slidell] are not given up. And what a wretched thing it will be, if we fight on the side of slavery. No doubt it will be said that we fight to get cotton; but I fully believe that this has not entered into the motive in the least. Well, thank [word omitted] we private individuals, have nothing to do with so awful a responsibility. Again how curious it is that you seem to think that you can conquer the South; and I never meet a soul, even those who would most wish it, who thinks it possible, - that is to conquer and retain it. I do not suppose the mass of people in your country will believe it, but I feel sure if we do go to war, it will be with the utmost reluctance by all classes, ministers of government and all. Time will show, and it is no use writing or thinking about it. I called the other day on Dr. [Francis] Boott and was pleased to find him pretty well and cheerful. I see . . . he takes quite an English opinion of American affairs, though an American in heart. [Henry Thomas?] Buckle might write a chapter on opinion being entirely dependent on Longitude!" Thanks Gray for facts on dimorphism in Borragineae; "What a riddle is the Mertensia." Presumes it would be impossible to get seeds; considers it very kind of Gray to try to send him a living plant of Houstonia; will send a copy of his Primula paper which he believes will not be printed until April, after his Orchid book [The Various Contrivances by which Orchids are Fertilized by Insects]; cares more for Gray's and [Sir Joseph] Hooker's opinions than for that of "all the rest of the world", and for [Sir Charles] Lyell's on geological points; [George] Bentham and Hooker thought well of his paper when read, "but no one can judge of evidence by merely hearing a paper"; the only thing which has interested him of late is making out that *Catasetum tridentatum* is male, *Monacanthus viridis* female, and *Myanthus barbatus* the hermaphrodite, of the same species; this accounts for all 3 forms appearing sometimes on the same plant; "they differ as much as, or more than, a peacock and pea-hen"; Bentham has given him a list of species of *Oxalis*, dimorphous like *Primula*, "and some *Menthas* are so he says - but I am not sure that he distinguishes such cases as *Thymus*." Has not yet had time to examine the dried specimen of *Amsinckia* which Gray sent; asks how [James Dwight] Dana is; Lyell is going to publish an interesting book on the Geological History of Man [The Geological Evidences of the Antiquity of Man, London, 1863]; believes he will touch on permanence of species; "With respect to Design, I feel more inclined to show a white flag than to fire my usual long-range shot. I like to try and ask you a puzzling question, but when you return the compliment I have great doubts whether it is a fair way of arguing. If anything is designed, certainly man

L L
II, 173

L L
II, 173

must be; one's 'inner consciousness' (though a false guide) tells one so; yet I cannot admit that man's rudimentary mammae bladder drained as if he went on all four legs; and pug-nose were designed. If I was to say I believed this, I should believe it in the same incredible manner as the orthodox believe the Trinity in Unity. You say that you are in a haze; I am in thick mud; the orthodox would say in fetid abominable mud. I believe I am in much the same frame of mind as an old gorilla would be in if set to learn the first book of Euclid. The old gorilla would say it was of no manner of use; and I am much of the same mind; yet I cannot keep out of the question." [39]

A. L. S. 5 pp. 20 cm x 16 cm. No. 62

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L L
II,175

L L
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[1862?]
Feb. 16

Has been trying a few experiments on Melastomatads and says they seem to indicate that the pollen of the 2 curious sets of anthers have very different powers; thinks he can now understand the structure of flowers and means of fertilization if there are 2 forms - one with pistil bent rectangularly out of the flower, and the other with it nearly straight; "Study the enclosed magnificent diagram!" His hothouse and greenhouse plants have probably all descended by cuttings from a single plant of each species, so he can "make out nothing from them"; has applied in vain to [George] Bentham and [Sir Joseph] Hooker, "but [W V] Oliver picked out some sentences from [Charles] Naudin, which seem to indicate differences in the position of the pistil." Sees that Rhexia grows in Massachusetts, and supposes it has two different sets of stamens; asks Gray to observe the position of the pistils in different plants in lately-opened flowers of the same age; specifies this because in Monochaetum he finds great changes of position in pistil and stamens as the flower gets old; "Supposing that my prophesy should turn out right, please observe whether in both forms the passage into the flower is not on the upper side of pistil, owing to the basal part of the pistil lying close to the ring of filaments on the under side of the flower " Would like to know, also, the color of the two sets of anthers; this would take only a few minutes and is the only way he can find out whether the plants are dimorphic in this peculiar way - only in the position of the pistil and in its relation to the two kinds of pollen; is anxious about this because if it should prove so, it will show that all plants with longer and shorter or otherwise different anthers will have to be examined for dimorphism; asks Gray to keep this letter as a memorandum.

M L
II,294

A. L. S. 4 pp. 20 cm x 13 cm. No. 63
Enclosed is diagram of Melastomatad.

[40]

[1862]
Mar. 15

"It is really almost a pleasure to receive stabs from so smooth, polished and sharp a dagger as your pen I heartily wish I could sympathize more fully with you, instead of merely hating the South We cannot enter into your feelings; if Scotland were to rebel, I presume we should be very wrath, but I do not think we should care a penny what other nations thought " Believes Gray's pamphlet has done his [Darwin's] book "great good", and natural science a "good turn", for Natural Selection seems to be making a little progress in England and on the continent; a new German Edition is called for, a French Edition has just appeared, and there has even been a Dutch Edition; "One of the best men, though at present unknown, who has taken up these views, is Mr. [Henry Walter, Bates; pray read his Travels in Amazonia when it appears." Since writing about Rhoxia he has been experimenting on Monochaetum, finds that the pistil is first bent rectangularly, in a few days becomes straight, and that the stamens also move; asks Gray if there are not two forms of Rhoxia, and to compare the position of the parts in young and old flowers, for he has a suspicion that one set of anthers is adapted to the pistil in the early state and the other in its later state; also asks, if bees visit Rhoxia, exactly how the anther and stigma strike them in both old and young flowers; he would like a sketch of this; has many seeds planted for experiment this summer, including Amsinckia spectabilis.

A. L. S. 5 pp. 20 cm x 13 cm. No. 64 [41]

[1862]
June 10

"Your generous sympathy makes you overestimate what you have read of my Orchid book [The Various Contrivances by which Orchids are Fertilized by Insects]. But your letter of May 18th and 26th has given me an almost foolish amount of satisfaction. The subject interested me, I knew, beyond its real value, but I had lately got to think that I had made myself a complete fool by publishing in a semi-popular form. Now I shall confidently defy the world " Has heard that [George] Bentham and [W. V.] Oliver approve of it, but has heard comments from no one else "whose opinion is worth a farthing. What strange creatures these Orchids are; for instance Mormodes, of which I have this morning examined another species, and which supports all that I have said, but which has completely puzzled me." Thanks Gray for notes on several American species, "I am not surprised as no true Orchid grows near you, that the pollinia of [Orchis] spectabilis were not secured I should expect that it would take probably a long time before new insects would leave the [word illegible]. You probably pushed too hard and crumpled the membrane, which, I know, interferes with the proper movement " Will write to Murray about costs of 3 first woodcuts, but doubts whether he will and the costs, "I will do my best, but by Jove you shall not pay for them. If there be an American Edit[ion] Murray will expect a little more than simple cost " Thinks he has written "enough and too much about my Orchids which are now again become beloved in my eyes, and which were quite lately accursed." Expresses thanks about copies of Gray's pamphlet,

tells him not to "trouble" about Hollies, thought they grew near, and does not consider the case important; fears nothing will be made out about Rhexia unless a plant could be protected from insects; has now a Rhexia glandulosa under trial, but finds there is little difference in stamens; is working at several Melastomas, and is certain there is something very remarkable, for pollen of the set of anthers produce less seed and, to his amazement, their seedlings are dwarfs compared to the other set, all produced from the same plant; Mr. Mechem has sent him his paper on parallel differences in trees of North America and Europe; asks if this can be approximately true, for it interests him much as the best case he has seen of apparently direct action of condition of life; "Forgive me for one bit more trouble I have a Boy with the collecting mania and it has taken the poor form of collecting Postage stamps; he is terribly eager for 'Wells, Fargo & Co. Pony Express' 2 and 4 stamps and 'Blood's 1 Penny Envelope 1, 3, and 10' cents'. If you will make him this present you will give my dear little man as much pleasure as a new and curious genus gives us old souls. Since this was written the little man has been struck down with scarlet fever; but thank God this morning the case has taken a mild form." Has just received Gray's long, "profoundly interesting" notes on *Cypripedium*; asks if he is not going to publish them; "Your notes are more interesting than you will suppose, for since publishing I saw at Flower show *Cypripedium hirsutissimum*, but could not touch it, but it seemed to me that the sterile anther entirely covered the passages of the anthers. I was amazed and I saw clearly that there must be some quite distinct manner of fertilization. But I did not think of insects crawling into flower; still less of different kind of pollen and a somewhat . . . viscid stigma. . . . You have hit on the same very idea which latterly has overpowered me, viz the exuberance of contrivances for same object. You will find this point discussed and attempted to be partly explained in the last chapter. No doubt my volume contains much error: how curiously difficult it is, to be accurate, though I try my utmost. Your notes have interested me beyond measure. I can now afford to d - my critics with ineffable complacency of mind. Cordial thanks for this benefit. It is surprising to me that you should have strength of mind to care for science, amidst the awful events daily occurring in your country. I daily look at the Times with almost as much interest as an American could do. When will peace come it is dreadful to think of the desolation of large parts of your magnificent country, and all the speechless misery suffered by many. I hope and think it not unlikely that we English are wrong in concluding that it will take a long time for prosperity to return to you. It is an awful subject to reflect on." Has never received a dull letter from Gray, seldom sees or hears from a soul on science; most of his scientific friends are so busy he does not write to them, Arethusa is very pretty; "How well you are attending to *Cypripedium*" Can return Gray's notes on this genus, or any other notes, at any time; will make a copy for himself; "How very very kind it is in you, overworked as you are, to send me so many notes." Expresses thanks about *Houstonia*; is working hard at that subject, for it interests him much; "Did you ever look at the little (so-called imperfect) flowers of *Viola* and *Oxalis*; they are very curious, the

I. J.
II, 445

pollen-grains emit their tubes whilst within the anthers; it is curious to see these tubes traveling up in straight lines from the lower anthers in Oxalis, right to the stigmas; it is like spermatozoa finding their way to ovules " Has just received a French translation of The Origin [of Species] by a Mlle [Clemence] Royer, "who must be one of the cleverest and oddest women in Europe, is an ardent Deist and hates Christianity, and declares that natural selection and the struggle for life will explain all morality, natures of man, politics, etc.!!" She makes some very curious and good hits, and says she shall publish a book on another subject, and a strange production it will be." Has had another look at Gray's Arethusa, and finds that the structure seems very like Vanilla and unlike that of other Orchids; asks Gray to look at Specularia and tell him whether the pollen-grains emit tubes direct from the anthers or are grains collected on collecting hairs; has just had a letter from [Alphonse Louis Pierre] de Candolle containing facts about Primula, and "his queries show he appreciates the case, and about Natural Selection He says he goes as far as you about change of species . . . I think from his letter you go further. He says he wants direct proof of Nat[ural] Selection and he will have to wait a long time for that "

A L S 10 pp. 20 cm x 16 cm No 66

[42]

L L
II, 179

[1862]
July 23

"Our poor Boy" is very sick "I despaired of his life, but . . . think [he] has passed the crisis . . . I hope . . . to get my brains in order and then I will pick out all your Orchid letters . . . and return them in hopes of your making use of them " Is not sure he understands the passages by which insects crawl in and out; asks Gray to draw a diagram, is so in arrears on letters and so many experiments have been "going to the dogs" he has not had time to make an abstract of Gray's letters; asks him to return such as he does not use, but hopes he will use all some time or another, will be glad to hear of [J. Trimble] Rothrock's observations on Houstonia, Gray only alluded to them, although he did formerly write about Specularia; the case in Viola and Oxalis seems "much too remarkable to be called 'precocious flowering'." Hopes Rothrock will publish notes; hears that the French say that his [Darwin's] paper on Primula is all pure imagination, but cannot find this is grounded on any observations; "No one else [but you] has perceived that my chief interest in my orchid book, [The Various Contrivances by which Orchids are Fertilized by Insects] has been that it was a 'flank movement' on the enemy " Lives in such solitude he has no idea to what Gray alludes about [George] Bentham and the Orchids and species, "One of my chief enemies [Sir Richard] Owen I hear has been lecturing on Birds, and admits that all have descended from one and advances as his own idea that the oceanic wingless Birds have lost their wings by gradual disuse He never alludes to me or only with bitter sneers . . . It has been an amusement . . . scribbling egotistically about myself and my doings." Has managed to "skim" the newspaper, but "had not heart to read all the bloody details Good God what will the end be; perhaps we are too despondent here, but I must think you are too hopeful on your side of the water. I never believed the 'canard' of the

M L
I, 202

M L
I, 202

army of the Potomac having capitulated My good dear wife and self are come to wish for Peace at any price " Wishes to hear what Gray thinks about what he says in the last chapter of the Orchid Book on the meaning and cause of the endless diversity of means for the same general purpose, "It bears on design - that endless question " After writing "the above" he read the great bundle of notes, "What admirable observations You have distanced me on my own hobby-horse" Plat[anthera] hyperborea is a curious and most interesting case to him, "How like the *Fee oparys* " Asks if it lives in Arctic regions where insects may be scarce; thinks it would be valuable to ascertain whether there is occasional crossing of pollinia in this species; "How curious about the nectary " Finds *Aceras* leads closely into *O[rochis] hircina*, "How organic beings are connected" Gray has worked *Cypripedium* spectabile excellently; admits he [Darwin] may be wrong, and fertilization may always be by small insects bodily crawling in; "What diversity in *Platanthera* Your observations seem to me much too good to be sunk in any review of my Book "

A L S 5 pp. 20 cm x 21 cm. No 76

[43]

M L
I,202

[1862]
July 28

Thanks Gray for stamps sent to his son who is very ill, and for "a capital notice you have published on the orchids! It could not have been better; but I fear that you overrate it I am very sure that I had not the least idea that you or anyone would approve of it so much." Returns Gray's last note in case he wishes to publish a notice on the subject; "but after all perhaps you may not think it worth while; yet in my judgment several of your facts, especially *P[latanthera] hyperborea*, are much too good to be merged in a Review. But I have always noticed that you are prodigal in originality in your Reviews " Wishes he could understand use of the common labellum in foreign Orchids; insects would have to be watched at work, mentions Gray's having spoken of 2 *Egones* always growing together, has sent a copy of Gray's reviews to [Sir Joseph] Hooker, Gray exactly expresses his conclusion about Greenland, while Hooker underrated occasional means of transport, "Till I proved the contrary, he used to maintain vehemently without a fact that the sea would kill all seeds." Has not had time to look at *Mitchella*, asks if Gray can't persuade his pupil to protect under fine net and experiment on some plants, thinks *Houstonia* may be visited by moths, has reason to suspect that many *Galiaceae* are so visited; has been looking at *Lythrum Salicaria*, finds it "beautifully dimorphic like *Primula*", but with additions in both forms of 6 short stamens, is curious to make out the use of them, believes there is a third form, "But I must hold hard, otherwise I shall spend my life over dimorphism." Has confirmed by experiment what he said about pollens and stigmas of *Linum grandiflorum*; has written to Trübner about copies of Orchid Book [The Various Contrivances by which Orchids are Fertilized by insects].

A L S 2 pp 26 cm x 21 cm. No. 75

[44]

L L
II,447

[1862]
Aug 9

Thanks Gray for sending stamps "of superlative value" to his son who has "gloated" over them; is hoping to get his son to the sea in a bed-carriage; has settled with Trubner who probably spoke to Murray, for he has charged him [Darwin] a reduced price, requests Gray to present a copy from him [Darwin] to his [Gray's] pupil, the Mitchellia is very good, but the pollen is equal sized, has just examined Houstonia and found a grand difference in pollen, calls Echium vulgare a "humbug, merely case like Thymus." Is almost "stark staring mad over Lythrum"; if he can prove what he fully believes, it is a "grand case" of trimorphism with 3 different pollens and 5 stigmas; has castrated and fertilized above 90 flowers trying all 18 distinct crosses which are possible within limits of this one species; feels sure Gray would think it a "grand case", wonders if Gray might have Lythrum in North America; asks him to look at it, and, if possible, get him some seeds; wishes to try species with few stamens, if they are dimorphic, would expect Nesaea verticillata to be trimorphic, "There is reason in my madness for I can see that to those who al ready believe in change of species these facts will modify to certain extent whole view of Hybridity " Homomorphic "grandchildren" of Primula have become more sterile

L L
II, 475

A L S 4 pp 20 cm x 33 cm. No 71

[45]

[1862]
Aug 21

Southampton

"We are a wretched family and ought to be exterminated We slept here to rest our poor Boy on his journey to Bournemouth, and my poor dear wife sickened with Scarlet fever and has had it pretty sharply, but is recovering well Our Boy suffered sadly from the journey, though we took it on the advice of two Doctors I fear he will be an invalid for months, if not years There is no end of trouble in this weary world I shall not feel safe till we are all at home together, and when that will be I know not But it is foolish complaining " He received Gray's letter with all the interesting details on Houstonia, "It seems a grand case " Hopes [J Trimble] Rothrock will surely publish them; feels that the simple fact of 2 pollens in the same species and the reciprocal action of 2 hermaphrodities is well worth establishing, and that until any first account is confirmed, nothing can be considered as established; has no sort of doubt after repeating his experiment on Primula, but will probably not publish till winter, if then, so Rothrock could first establish the case; thanks Gray for sending stamps to his son, "I wrote you a mad letter the other day about Lythrum, but the case is worth some madness " Appreciates Gray's remarks about Rhexia, and what he writes about pollen agrees with what he has seen, "My Rhexia glandulosa seems very different Heterocentron will, I suspect, turn out, as I prophesied something marvelous like Lythrum I know almost as well as you, that systematic work is the foundation of everything, yet in your case and [Sir Joseph] Hooker's case, I perpetually feel inclined to do no systematic work " Had a note from Hooker giving a fair account of Mrs Hooker, but it seems almost as if her heart is slightly affected; Hooker has got 2 wonderfully different flowers on the same spike of Vanda, and [Thomas Henry] Huxley is going to bring

L L
II, 175

L L
II, 175

out a very curious book on Man and Monkey; can see no honey on Melastoma; secretion of honey depends on the most delicate contribution of circumstances, "The common Polygala will go on for many days and secrete none, and then will suddenly all commence . . . Affairs seem to be getting with you more and more terrible. What will the end be . . . It seems to us here far more fearful, than it apparently does to you " Would like to hear what Gray thinks of the last chapter in his Orchid book [The Various Contrivances by which Orchids are Fertilized by Insects]

A L S 6 pp 20 cm x 13 cm No 67

[46]

[1862]
Nov 6

Agrees with Gray the Max Müller's book [Lectures on the Science of Language, 1861] is extremely interesting but considers the latter part, about the first origin of language, much the least satisfactory; "It is a marvelous problem." Has heard, and the book gives him the same impression, that Müller is very much afraid of not being thought strictly orthodox; "He even hints at truth of Tower of Babel! I thus accounted for covert sneers at me, which he seems to get the better of towards the close of the book. I cannot quite see how it will forward 'my cause' as you call it; but I can see how anyone with literary talent . . . could make great use of the subject, in illustration . . . What pretty metaphors you would make from it! I wish some one would keep a lot of the most noisy monkeys, half free, and study their means of communication!" A book by Bishop [John William] Colenso has just appeared in England which will "make a noise", and which, judging from extracts, "smashes most of the Old Testament " Is reading Miss Cooper's Journal of a Naturalist which pleases him though it is "very innocent food"; asks who she is; she seems a very clever woman, and gives a "capital account of the battle between our and your weeds. Does it not hurt your Yankee pride that we thrash you so confoundedly I am sure Mrs Gray will stick up for your own weeds. Ask her whether they are not more honest downright good sort of weeds " The book gives an extremely pretty picture of an American village, "but I see your autumn, though so much more gorgeous than ours, comes on sooner, and that is one comfort." Is glad Gray has sent off his account of Orchids to New Haven; asks for a copy, as he sees no periodicals; wishes Gray had an active pupil in the country who could black up with cotton the holes on each side of the sterile anther in Cypripedium, and then if pollen were at all disturbed it would show that little insects had entered by the toe, would be very glad of Mitchella and of seed of Nesaea; is more than ever interested in Lythrum; the seed of his 88 crossed flowers prove the truth of the diagram he sent Gray, but finds that the mid-styled is in addition moderately fertile with half its own stamens, and he must make many more crosses, and will not publish this year; thinks the case is worth any labor; asks for reference to Gray's notice of Gourds affecting each other's fruit, and on movements of the tendrils; would like information on any tendrils descriptive of all American varieties of maize; asks for a few grains of the most marked varieties of Maize; "I am crawling steadily on", compiling data on peaches and nectarines; finds it a curious case; would like to try a few experiments on Gray's tendrils; wonders what would be

L L
II,182
L L
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a good easy plant to raise in a pot; "God help your poor country, though perhaps you scorn our pity "

A L S 6 pp. 20 cm x 13 cm No 78

[47]

[1862]
Nov 26

Gray's letter and the review in Silliman ['s Journal of Science] L L
have reached him; "We were all very much interested by the political II, 477
part of your letter; and in some odd way one never feels that in
formation and opinions printed in a newspaper come from a living
source; they seem dead, whereas all that you write is full of life "
Thanks Gray for information about Maize; "if the husked form had
been the aboriginal, it would surely have not varied so readily;
there must be some mistake in the statement of Julian, quoted by
Aug[uste Prouvencale de] St. Hilaire." Was "profoundly interested" L L
in the reviews; "you rashly ask for my opinion, and you must con- II, 477
sequently endure a long letter." Regarding dimorphism, he does not
at present like the term "dioeciodimorphism", for he thinks it gives
quite a false notion that the phenomena are connected with a separa-
tion of the sexes; "Certainly in *Primula* there is unequal fertility
in the two forms, and I suspect this is case with *Linum*; and there-
fore I felt bound in *Primula* paper to state that it might be a step
toward dioecious condition; though I believe there are no dioecious
forms in *Primulaceae* or *Linaceae*. But the three forms in *Lythrum*
convince me that the phenomenon is in no way necessarily connected
with any tendency to separation of sexes. The case seems to me in
result or function to be almost identical with what old C K
[Christian Conrad ?] Sprengel called 'dichogamy', and which is so
frequent in truly hermaphrodite groups; namely the pollen and stigma
of each flower being mature at different periods. If I am right it
is very advisable not to use term 'dioecious', as this at once brings
notion of separation of sexes." Hopes Gray will be able to attend a
little to *Plantago*; asks in what form the stigma projects in the bud,
"(this occurs in long-styled *Lythrum*, but is not then fertilized)?"
Asks if short-styled, "your long-stamened", is really sterile, does L L
not like Gray's term "preccious fertilization" for his second class II, 482
of dimorphism; "If I can trust my memory, the state of corolla, of
stigma and pollen-grains is different from state of parts in bud;
that they are in a condition of special modification. The
temporary theory which I have formed on this class of Dimorphism,
just to guide experiment, is that the perfect flowers can only be
perfectly fertilized by insects and are in this case abundantly
crossed; but that these flowers are not always, especially in early
spring, visited enough by insects, and therefore the little im-
perfect self-fertilizing flowers are developed to ensure a suffi-
ciency of seed for present generations. *Viola canina* is sterile,
when not visited by insects, but when so visited forms plenty of
seed. I infer from the structure of 3 or 4 forms of *Balsaminae*
that they require insects; at least there is almost as plain adapta-
tion to insects as in *Orchids*. I have *Oxalis acetosella* ready in
pots for experiment next spring; and I fear this will upset my little
theory; . . . *Campanula carpatica* . . . is absolutely sterile if
insects are excluded. *Specularia specularia* is fairly fertile when
enclosed; and this seemed to me to be partially effected by the

frequent closing of the flower, the inward angular folds of corolla corresponding with the clefts of the open stigma, and in this action pushing pollen from outside of stigma onto its surface Does *Sp[ecularia]* *perfoliata* close its flower like *S[pecularia]* *speculum* with angular inward folds; if so I am smashed without some fearful 'wriggling'. Are the imperfect flowers of your *Specularia* the early or the late ones? Very early or very late?" Entirely agrees with Gray on the part which crossing plays; was much perplexed by [W V] Oliver's remarks in the Natural History Review on the lower plants of the *Primula* having sexes more often separated than in the higher plants, so exactly the reverse of what takes place in animals; [Sir Joseph] Hooker repeats this remark in his "Review of Orchids"; "There seems to me much truth in what you say, and it did not occur to me, about no improbability of specialization in certain lines in lowly organized beings. I could hardly doubt that the hermaphrodite state is the aboriginal one. But how is it in the Conjugation of Confervae - is not one of the two individuals here in fact male and the other female??" Has been much puzzled by this contrast in sexual arrangements between plants and animals; "Can there be anything in following consideration. By roughest calculation about 1/3 of the British genera of aquatic plants belong to the Linnean classes of Mono and Dioecia; whilst of terrestrial plants (the aquatic genera being subtracted) only 1/13 of genera belong to these two classes. . . . Does not [Alphonse Louis Pierre] de Candolle say that aquatic plants taken as a whole are lowly organized compared with terrestrial; and may not Oliver's remark on separation of sexes in lowly organized plants stand in some relation to their being frequently aquatic? Or is this all rubbish?" Admires the cleverness and ingenuity with which Gray explains and describes all forms of Orchids; asks if Gray's *Platantheras* smell sweetest at night; if so, he suspects moths are the fertilizers; has been especially interested in the case of *P[latanthera]* *psycodes*, more especially since the Duke of Argyll's "contemptuous remarks on my case of *Angraecum* which in action seems analogous to your case. But by far the most wonderful is the case of *Glymnaedia* *tridentata*; . . . If I understand rightly the rostellum alone is penetrated. . . . I daresay you are quite right about self-fertilization being much commoner than I thought with orchids. . . . I have found in *Neottia nidus avis* that this ensues, if in course of few days the flowers are not visited by insects?" Gray's observations on *Cypripedium* seem "excellent" and probably he [Darwin] is wholly wrong; it seems to him now more likely that small insects should lick the juice off the hairs with jaws or short proboscis than with long proboscis; "How curious about the little bristles on the stigma! . . . You and Hooker seemed determined to turn my head with conceit and vanity (if not already turned) and make me an unbearable wretch." Asks if Gray could induce any of his zoological coeditors to notice [Henry Walter] Bates' paper; "It would be a good and charitable deed, for it would encourage and please a man, that wants and deserves encouragement." Asks if *Fragaria vesca* and *virginiana* differ much botanically, for he cannot make out that any one has succeeded in crossing them; has just had a long letter from

L L
II, 48L L
II, 47L L
II, 47

Hooker on the part crossing plays in nature; must consider it well and see if it alters his notions.

A. L. S. 6 pp. 26 cm x 21 cm. No. 50

[49]

[1863]
Jan. 19

Is glad to hear of *Platanthera* and the Butterfly and thanks Gray for the Indian Corn; "What little grains' I know nothing about 'glucose partly replacing starch'." Was "muddled" about the first part of Gray's review of *Orchids*; has been thinking how interesting it would be to experiment on the three kinds of flowers of *Linium Lewisii*, but fears it would be impossible to get seed; "I have been at those confounded *Melastomes* again; throwing good money (i.e. time) after bad." Asks if Gray remembers telling him he could see no nectar in his *Rhexia*; he can find none in *Monochaetum*, and [Henry Walter] Bates tells him the flowers are in the most marked manner "neglected" by bees and *Lepidoptera* in Amazonia; the curious projections or horns to the stamens of *Monochaetum* are full of fluid, and he suspects that diptera or small *hymenoptera* may puncture these horns as they do the dry nectaries of true *Orchis*; wishes Gray would watch *Rhexia* next summer on a warm day, see whether they are visited by small insects, and what they do; "Your President [Abraham Lincoln] has issued his fiat against Slavery. God grant it may have some effect. The present generation here never heard much about Slavery. I sometimes cannot help taking most gloomy view about your future. I hope that you may prove right and good come out of it."

M L
II, 297

A. L. S. 6 pp. 20 cm x 16 cm. No. 57

[49]

[1863]
Feb. 23

"The Maize seed has proved a treasure; for besides seeing the kinds, a young man at Edinburgh will experiment on the mutual fertility of some of the varieties." Asks Gray to thank Dr [Horace] Scudder about pollinia; "that was very good remark about attachment possible only to eye or proboscis; and these are only two parts where I have seen attachment." Appreciates, also, information about the highness and lowness of the oak tree; [Sir Joseph] Hooker was pleased about the Commonwealth of Plants; will send his *Linum* paper and then Gray will see about [Linum] *Lewisii*; thinks Gray will be interested by parts of [Sir Charles] Lyell's book on Man [The Geological Evidences of the Antiquity of Man], but fears the best part, about the Glacial Period, may be too geological for any one except a regular geologist; "He quotes you at end with gusto. By the way he told me the other day how pleased some had been by hearing that they could purchase your pamphlet. The 'Parthenon' also speaks of it as the ablest contribution to the literature of the subject. It delights me when I see your work appreciated. The Lyells come here this day week, and I shall grumble at his excessive caution. . . . The public may well say, if such a man dare not or will not speak out his mind, how can we who are ignorant, form even a guess on subject. Lyell was pleased, when I told him lately that you thought that language might be used as excellent illustration of derivation of species; you will see that he has an admirable chapter on this." Has received the correspondence between [Charles G.] Loring and Field and thinks it curious that two able and honest men should differ so enormously; "Of course I side chiefly with the Englishman; but I never so well understood your horror of Disunion. It is very natural that you should dread becoming split up like

L L
II, 195

Germany; but to us it does not seem quite so horrible I think both correspondents underrate the very general belief entertained for many years in England, that your Government delighted in making us eat dirt . . . about Boundary Line, Right of Search, Vancouver, etc. I believe that this has greatly checked all sympathy with you; and made the whole country fire up, when, as we thought, you had pressed our swallowing powers in the Trent affair. . . . I read [Prof. J E.] Cairns's excellent Lecture ["The Slave Power", 1862] which shows so well how your quarrel arose from Slavery. It made me for a time wish honestly for the north; but I could never help, though I tried, all the time thinking how we should be bullied and forced into a war by you, when you were triumphant. But I do, most truly think it dreadful that the South, with its accursed Slavery, should triumph, and spread the evil. I think if I had power, which thank God I have not, I would let you conquer the border states, and all west of Mississippi and then force you to acknowledge the Cotton States. For do you not now begin to doubt whether you can conquer and hold them? . . . The Times is getting more detestable . . . than ever My good wife wishes to give it up; but I tell her that is a pitch of heroism, to which only a woman is equal to, to give up the 'Bloody old Times' as Cobbett used to call it, would be to give up meat drink and air."

L L
II,195

A L S 5 pp. 20 cm x 16 cm. No 55

[50]

[1863]
Mar. 20

[Writes at length of his son's stamp collection]; has tried in vain at Kew and elsewhere to get seed of *Campanula perfoliata*; is glad Gray likes [Henry Walter] Bates' paper and expects his "Amazonian Travels" will be good; asks for Gray's opinion on [Sir Charles] Lyell's book; "[Sir Joseph] Hooker and I have told him that we regret much that he did not speak more boldly out about Species. He answers that his belief in change fluctuates " Lyell's book has made him reread Gray's essay and he admires it as much as ever; explains that by "dichogamy" [Christian Conrad ?] Sprengel means a plant in which each flower matures and sheds its pollen and then has its stigma mature, and much more rarely matures its stigma first, so that these plants are in function monoecious; is sure his observations are to a large extent correct, and the case is very common; in the *Primula*-like cases the plants are in function dioecious; has an interesting letter from Dr. Crüger, of the botanical gardens of Trinidad, who tells of odd facts about a native species of *Cattleya* which never open their flowers and yet set seed capsules; "Happy man, he has actually seen crowds of Bees flying round *Catasetum* with the pollinia sticking to their backs!" He wrote Crüger asking him to observe what the insects did in flowers of *Melastomaceae*, he replied it was not yet the proper season, but that on one species a small bee seemed busy about the horn-like appendages to the anthers; thinks it would be "too good luck" if his study of the flowers in the greenhouse has led him to the right interpretation of these queer appendages; has just built a hothouse and got some Orchids, and it "amuses" him much; "Some plants of *Amsinckia spectabilis*, at least the seed was so named

L L
II,439
M L
II,299

(small dark orange flowers, elongated hairy leaves) have just begun to flower, and I find in two plants the stigma stands on exact level with anthers; hence I fear they cannot be dimorphic " Gray's Mitchellas look healthy, but he hopes they will not flower very soon, for his health is so bad the family is going about the middle of April to water cure for 6 or 8 weeks; "I shall never get my present Book on Variation under Domestication [Variation of Animals and Plants under Domestication] finished; yet it interests me much and I am now in the middle of long chapter on Inheritance Reversion etc, giving results of my own and other Breeders' experiments."

A L S 7 pp 18 cm x 13 cm No. 58

[51]

[1863?]
May 31

Gray's review of [Alphonse Louis Pierre] de Candolle [on the Oaks, Asa Gray's Scientific Papers, I, p 130] seems excellent, Gray speaks out more plainly in favor of derivation of species, though doubtful about natural selection; asks if Gray does not consider such cases as all Orchids next thing to a demonstration against [Oswald?] Heer's view of species arising suddenly by monstrosities; "It is impossible to imagine so many coadaptations being formed all by a chance blow Of course Creationists would cut the enigma." Has written twice lately and sent 2 copies of his Linum paper which probably were lost; remembers writing how right Gray was about the fertilization of Cyripedium; of the species sent by Gray, C[yripedium] acaule alone has flowered and has puzzled him, Mitchella also does not look very healthy; asks Gray to thank [Horace] Scudder for his interesting paper on Pogonia, was glad to see in Gray's review his remarks in answer to [Hugh] Falconer on Phyllotaxy; infers Gray cannot explain why there are not intermediate angles; has been looking at [Karl Wilhelm von] Nägeli's work on this subject and is astonished to see that the angle is not always the same in young shoots when leaf buds are first distinguishable, as in the full grown branch; "This shows there must be some potent cause for those angles which do occur; I daresay there is some explanation as simple as that for the angles of the Bee-cells." De Candolle sent him a copy of part of a letter from [Marquis G. de?] Saporta in which he expressed the strong belief that natural selection would ultimately be triumphant in France though now quite ignored; is working on his "big book" and is now at all causes of sterility under domestication and cultivation, has such an "immense collection" of facts that the work, though laborious and slow, interests him, as he can generally come to some sort of conclusion; "There never will be a man who will read my big book; it will be a sort of encyclopedia on special cases " Has been looking again at the imperfect flowers of Oxalis and Viola and finds he was wrong in supposing that in Oxalis the perfect flowers required insect-aid for fertilization, "so this view is knocked on the head " Finds Viola does require insects; must stick to his opinion that the imperfect flowers of Viola, at least, deserve to be ranked as something more than mere precocious flowers; "In V[iola] canina only 2 anthers are developed; the pollen-grains are swollen - the pistil widely different in shape; no nectar appendages to the two fertile stamens and no spur." Asks for seed of Campanula perfoliate; has been observing common Broom, "hardly any Orchid shows prettier adaptation to insects which are necessary

] M L
I, 242

] L L
II, 236

for its fertilization. The upper and lower surface of thorax of Bees get dusted with pollen, and first the stigma rubs the upper side of thorax and afterwards is rubbed by the lower side of thorax.

A. L. S. 6 pp. 20 cm x 16 cm. No. 84

[52]

[1863]
Aug. 4

Is glad to hear Gray is refreshed by his short holiday, and the news about his legacy is "capital"; "How good natured you are to my little man about stamps." Thanks Gray for Spicularia; fancied it was a specimen to show the flowers, put it in warm water, and then discovered his mistake; hopes he has not killed the seeds; has had a "kind" note from Mr. [Charles Loring] Brace; will not answer, as he had previously written thanking him for his present of his book on Hungary; "After I had written to you [Oswald?] Heer's doctrine of sudden changes, I suspected what you would say; what, I think, ought to give you the severest 'cold chill' is the case of pouter, Fantail-pigeons, etc.; were not the variations accidental as far as the purpose man has put them to? [Sir Charles] Lyell said he would grapple with this, but I suspect he found it would be most prudent to shirk the question." In his present book he has been comparing variation to the shapes of stones fallen from a cliff, and natural or artificial selection to the architect, "but I cannot at all work a metaphor like you do." The case of the Orchid with the prominence on the labelum seems a very "pretty" one; has "lots of Hobby-horses" at present - fertility of peloric flowers, and especially of "Homorphic" seedlings, which he suspects will throw much theoretical light on hybrids; has worked on Lythrum "like a Trojan", and has finished 134 crosses, but the case is worth any labor, for it seems about the oddest case of reproduction he has ever noticed - "a triple marriage between these hermaphrodites." His present "Hobby-horse" - tendrils - he owes to Gray; "About the spontaneous movements (independent of touch) of the tendrils and upper internodes I am rather taken aback by your saying 'is it not well known?' I can find nothing in any book which I have; neither [Sir Joseph] Hooker nor [W. V.] Oliver knew anything of their movements. The spontaneous movement of the tendrils is independent of the movement of the upper internodes, but both work harmoniously together in sweeping a circle for the tendrils to grasp a stick. So with all climbing plants (without tendrils) as yet examined, the upper internodes go on night and day sweeping a circle in one fixed direction. It is surprising to watch the Apocynae with shoots 18 [inches] long, beyond the supporting stick, steadily searching for something to climb up. When the shoot meets a stick, the motion at that point is arrested, but in the upper part is continued, so that the climbing of all plants yet examined is the simple result of the spontaneous circulatory movement of the upper internodes. . . . Has anything been published on this subject?" Is glad Gray is going to review [Henry Walter] Bates' paper; "I enjoy anything riles [Louis J R?] Agassiz. He seems to grow bigoted with increasing years. I once saw him years ago, and was charmed with him. . . . You are unjust on the merits of Drosera; it is a wonderful plant, or rather a most sagacious animal. I will stick up for Drosera to the day of my death. Heaven knows whether I shall ever publish my pile of experiments on it."

L L
II,486

L L
II,486

Asks Gray not to hate "poor old England too much. Anyhow she is the Mother of five children all over the world . . . No man could have tried to wish more sincerely for the North than I have done. My reason tells me that perhaps it would be best . . . if it would end Slavery, but I cannot pump up enthusiasm. The boasting of your newspapers and of your little men, and the abuse of England, and the treatment of the free colored population, and the not freeing Maryland slaves stops all my enthusiasm. If all the States were like New England the case would be different. . . . What devils the low Irish have proved themselves in New York. If you conquer the South you will have an Ireland fastened to your tail."

A. L. S. 6 pp. 26 cm x 17 cm. No. 83

[53]

[1864]
Feb. 25

"You have been so kind and good a friend to me, that I think you will like . . . to hear that I am better." The vomiting is not now daily, and on his good days he is much stronger; his head hardly even troubles him, except the ringing in the ears; has not done a "stroke of work" for over 6 months, but begins to hope he may be able to work again in a few more months; is able to get to his hothouse most days now; is "amusing" himself a little by looking at climbing plants; the first job which he will do will be to draw up results on *Lythrum* crosses and on movements of climbing plants; has seen and heard from no one except "good dear [Sir Joseph] Hooker", who, though so overworked, like a good and true friend, often writes him; has had one letter which has interested him greatly, with a paper by Dr. [Hermann] Crüger, of Trinidad, which will appear in the Linnean Journal, and which shows he [Darwin] is all right about *Catasetum*, even to the spot where the pollinia adhere to bees which visit the flower, as he had said, to gnaw the labellum; "Crüger's account of *Coryanthes* and the use of the bucket-like labellum full of water beats everything." Suspects that the bees, "being much wetted", flatten the hairs and allow the viscid disc to adhere; has given up hearing newspapers read aloud, as books are more amusing and less tiring; "Good Heavens, the lot of trashy words which I have heard is astonishing." Has heard little about America; "Sometime let me hear what you are doing and what you expect for your Country."

A L S 6 pp. 20 cm x 13 cm. No. 80

[54]

L.L.
II, 458

[1864]¹
Apr. 19

Has nothing in particular to say, but "the grand news of Richmond has stirred me up to write. I congratulate you, and I can do this honestly, as my reason has always urged and ordered me to be a hearty good wisher for the north, though I could not do so enthusiastically, as I felt we were so hated you. Well, I suppose we shall all be proved utterly wrong who thought that you could not

1. Life and Letters, II, 488, dates one section of the letter Apr. 9, 1865.

entirely subdue the South. I have always thought that the destruction of Slavery would be well worth a dozen years war. Two days ago a very charming man, enthusiastic for the North, called here, . . . and he does not believe that you will attack us and Canada. I fear it will take many years before your country will shake down to its old routine." He received a paper with a good account of Gray's Herbarium and Library, also, a long time previously Gray's excellent review of [John] Scott's Primulaceae ["On the functions and structure of the reproductive organs in the Primulaceae"] which he forwarded to Scott in India, as it would much please him; was glad to see in it a new case of dimorphism, and will be grateful to hear of any other cases, as he still feels an interest in the subject; would be very glad to get some seed of Gray's dimorphic Plantagos, for he cannot banish the suspicion that they must belong to a very different class like that of the common Thyme; asks how the wind, which is the agent of fertilization, could, with Plantago, fertilize "reciprocally dimorphic" flowers like Primula; "Theory says this cannot be, and in such cases of one's own theories I follow Agassiz and declare 'that nature never lies'." Would even be very glad to examine the 2 dried forms of Plantago or any other dimorphic plants; asks if his Lythrum paper interested Gray; "I crawl on at rate of 2 hours per diem with Variation under Domestication [The Variation of Animals and Plants under Domestication]; and I have begun correcting proof of my paper on 'Climbing Plants'. I suppose I shall be able to send you a copy in 4 or 5 weeks. I think it contains a good deal new and some curious points, but it is so fearfully long, that no one will ever read it. If however, you do not skim through it, you will be an unnatural parent, for it is your child."

A. L. S. 8 pp. 20 cm x 13 cm. No. 77

L. L.
II, 479

L. L.
II, 488

[55]

[1864]
May 28

"Your kindness will make you glad to hear that I am nearly as well as I have been of late years, though a good deal weaker." Has been slowly writing a paper on Lythrum and this has disinclined him for the exertion of writing letters; finds it very pleasant doing a little work after 8 months of inaction; Gray's Nesaeas are looking very healthy, and Mitchella moderately so; has received Dr. [Chauncey] Wright's letter about Orchids, and asks Gray to beg him to note what attracts insects to Begonias and whether they gnaw or penetrate the petals; also, but he cares less, what attracts them to Melastomas; "Poor Dr. Cruger of Trinidad, who promised to observe, is dead." Will send a copy of his Lythrum paper when it is printed, and will like to hear whether Gray thinks it as curious a case as he does; he has another new sub-class of dimorphic plants; asks if Gray has ever traveled south, and, if so, can he tell him whether the trees which Bignonia capreolata climbs are covered with moss or filamentous lichen or Tillandsia, because its tendrils "abhor a simple stick, do not much relish rough bark, but delight in wool or moss." [Describes how they adhere]; encloses some specimens and asks Gray, if he thinks it worth while, to put them under the microscope; thinks it remarkable how specially adapted some tendrils are;

L. L.
II, 487

"Eccremocarpus scaber do not like a stick, will have nothing to say to wool; but give them . . . grass or . . . bristles and they seize them well." Has been reading von Melah on imperfect self-fertile flowers, and says he quotes Gray that perfect flowers of *Voandzeia* are quite sterile; asks how this is known; presumes Gray knows that wild plants of *Amphicarpaea* are generally sterile, and wishes he might have seed to ascertain whether this plant is sterile when fertilized; "What a curious . . . case is that of *Leersia*. I have just got plants of this group." Asks for seeds of *Campanula perfoliata*, if Gray ever comes across it; asks whether American *Hollies* are some hermaphrodite and some female; he has a dimorphic case; suggests Gray read [Alfred Russell] Wallace's article in Anthropological Review on "Natural Selection of Man"; asks if the museum for Gray's Herbarium is settled; "I am much in arrears" on public news, "for I gave up for months hearing the newspaper, as I found it more fatiguing than words. . . . What dreadful carnage you have just recently suffered. What will the end be?" Asks if [William Henry] Harvey will publish; is sending a photograph of himself with his beard; "Do I not look venerable?" [No enclosures].

L L
II, 487

A. L. S. 6 pp. 20 cm x 16 cm. No. 79

[56]

[1864]
Sep. 13

Has delayed answering Gray's note, not because he was ungrateful, but because he has less strength; after his 2 hours' work he is glad to be quite idle; is still gaining some, and now, at last, is living down stairs; "My soul has been absorbed with Climbing plants, now finished and tomorrow I begin again, after 13 months interruption on 'Variation under Domestication' [Variation of Animals and Plants under Domestication]." He received lately a review on [Herbert] Spencer with the address in Gray's handwriting; likes much all the latter part, but cannot believe Gray wrote it; "If you did you had muddled your brains (in the first part) by reading metaphysics, and all elasticity had gone out of your style." He also received an interesting review on [James Dwight] Dana, in Gray's handwriting; is sending a paper by a gardener, John Scott, which has interested him greatly; has just thought that, for chance of Gray's noticing it in the [American Journal of Science], he will point out the new and very remarkable facts; he has paid the "poor fellow's passage out to India", where he [Darwin] hopes he will succeed; "We are profoundly interested in your politics; and do not in the least know whether the 'old Bloody Times' is to be trusted that there will be peace and that the middle States will join with the South on Slavery and eject the Northern States. In the latter case, I hope you will marry Canada and divorce England and make a grand country, counterbalancing the devilish South." [Enclosed is an abstract on Scott's paper on red Cowslip].

M. J.
II, 327

A. L. S. 3 pp. 20 cm x 13 cm. No. 89

[57]

[1864]
Oct. 29

"I have little or nothing to say and it is no wonder, as I live so uniform a life." Is writing chiefly to ask Gray to get from any ornithologist or oologist answers to enclosed questions; knows there is some good man at Cambridge or Boston whose name he has forgotten; tells Gray to read [Thomas Henry] Huxley in the last Natural History Review; "you, yourself could not have done it better " Had a letter from B[enjamin Dann] Walsh of Illinois, "a good believer in change of species . . . There are good philosophical remarks in his paper and for some odd cause, philosophy is rarely found in entomological works " Is able now to work on his good days for about 2 hours; thinks phosphate of iron has done him good; Lady Lyell has given him wonderful accounts of the benefits to be derived from a Philadelphia medicine which is imported into England and is called Syrup of Phosphates; asks if Gray has ever heard of it; is tempted to try it if he knew of what it was composed; is "plodding on with little success" on Laws of Variation, and has succeeded only in making a "disjointed skeleton on which to hang a multitude of queer facts." Has not been able to resist doing a little more at "your God-child, my Climbing paper", which he will have copied out by June, "else I shall never stop. This has been new sort of work for me and I have been pleased to find what a capital guide for observation, a full conviction of the change of species is." His family always like to hear Gray's opinion on public news; "My wife in indignation has changed the Times for the Daily News, which I find rather dull, but it does not much concern me, for I read but little, and live on endless foolish novels which are read aloud to me by my dear women-kind " [No enclosure]

L L
II, 488

A L S 4 pp 20 cm x 15 cm No. 89

[58]

[1866]¹
Sep. 10

Quotes his son's thanks for stamps; "Your note about Agassiz has interested and amused me much for the day before I had been reading the Atlantic Monthly and the copy of a letter from Mme. Agassiz to [Sir Charles] Lyell and one from him all about the Amazonian Glacier. We were both lost in astonishment at the nonsense which Agassiz writes, and I could not resist sending to Lyell a copy of . . . your note, for his pre-determined wish partly explains what he fancies he observed. The evidence advanced by him is so weak that I do not think it would be admitted for the former existence of glaciers even in a temperate region. . . . You speak of reading the sheets of the [4th edition, June, 1866, of] Origin [of Species], but Murray promised me to send you a bound copy. . . . As there is no chance of a new edition perhaps it would be as well to let the Appletons have the sheets if they would make any use of them." Doubts the success of the next book on domestic animals [The Variation of Animals and Plants under Domestication ?], as

1. Life and Letters, II, 480, dates one section of this letter 1867.

some chapters are "curious", but many others have too minute details for general reading; is grateful to Gray for making a bargain with Ticknor about publishing it; thanks Gray for specimens of *Rhamnus*; both he and his son have looked at the pollen of both forms, but can find no difference; the difference seems confined to the pistils and the peduncles; they cannot conjecture whether this species is reciprocally dimorphic like *Primula* or is merely tending to become dioecious, which is a great disappointment to them; the nature of the 2 forms could only be made out by experiment or by observing their seed-production in their natural state; wonders if this species could not be purchased in American nursery gardens; asked in his last letter if Gray knew of any striking cases of endemic or naturalized plants which never flowered or which never seeded; if Gray does not answer he will understand that he knows of no cases like the Acorns or Horseradish in Europe; the only point he has made out this summer which could possibly interest Gray is that the common Oxlip found everywhere in England is certainly a hybrid between the Primrose and Cowslip, while the *P[rimula] elatior*, found only in the Eastern counties, is a perfectly distinct and good species hardly distinguishable from the common Oxlip except by the length of the seed-capsule relatively to the calyx; "This seems to me rather a horrid fact for all systematic botanists." Has just begun a large course of experiments on the germination of the seed and on the growth of the young plants when raised from a pistil fertilized by pollen from the same flowers, and from pollen from a distinct plant of the same or of some other variety; has not made sufficient experiments to judge certainly, but in some cases the difference in the growth of the young plants is highly remarkable; "I have taken every precaution in getting seed from the same plant, in germinating the seed on my own chimney-piece, in planting the seedlings in the same flower pot, and under this similar treatment, I have seen the young seedlings from the crossed seed exactly twice as tall as seedlings from the self-fertilized seed, both seeds having germinated on same day. If I can establish this fact . . . in some 50 cases, with plants of different orders, I think it will be very important, for then we shall positively know why the structure of every flower permits, or favours, or necessitates an occasional cross with a distinct individual."

L. S. 9 pp. 20 cm x 13 cm. No. 92

[59]

[1967]
Oct. 16

Is sending by this post clean sheets of Volume I [The Variation of Animals and Plants under Domestication, 1868] up to page 336, and there are only 411 pages in this volume; is very glad that Gray is going to review his book, "but if the Nation is a newspaper, I wish it were at the bottom of the sea, for I fear that you will thus be stopped reviewing me in a scientific journal." The first volume is all details and Gray will not be able to read it; the chapters on plants are written for naturalists who are not botanists; the last chapter, on bud variation, is a curious compilation of facts; however, some of the chapters in Volume II are more interesting, and he will be very curious to hear Gray's verdict on the chapter on close inter-breeding; "Chapter on what I call Pangenesis will be

L L
II, 256

called a mad dream, and I shall be pretty well satisfied if you think it a dream worth publishing; . I think it contains a great truth. I finish my book with the semi-theological paragraph, in which I quote and differ from you." A man in Natal sent him a packet of the dung of locusts with the statement that it was believed that locusts brought new plants to the districts which they visited; 6 grasses belonging to 2 species have germinated out of the dung and the seeds were fairly enclosed in the little pellets as he ascertained by dissection; "This verifies what I said in the Origin [of Species] that many new methods of transport would be discovered; for locusts are often blown many 100 miles out to sea." The rest of the sheets which have all been corrected will be printed by the middle of November and sent to Gray

L S 4 pp. 20 cm x 13 cm No. 95

[60]

L L
II,256

[1868]
May 8

Apologizes for having seemed ungrateful not to have thanked Gray for sending copies of the Nation and for his aid regarding the American Edition "with your nice preface" which he received this morning; Gray's article in the Nation seems very good and gives an excellent idea of Pangenesis, "an infant cherished by few as yet, except his tender parent, but which will live a long life There is parental presumption for you' You give a good slap at my concluding metaphor; undoubtedly I ought to have brought in and contrasted natural and artificial selection; but it seemed so obvious to me that natural selection depended on contingencies even more complex than those which must have determined the shape of each fragment at the base of my precipice What I wanted to show was that in reference to preordainment whatever holds good in the function of a pouter pidgeon holds good in the formation of a natural species of Pidgeon. I cannot see that this is false. If the right variations occurred and no others natural selection would be superfluous." Quotes a reviewer in an Edinburgh paper, "who treats me with profound contempt", as saying on this subject that Prof. Asa Gray could with the greatest ease smash him [Darwin] into little pieces.

A L S 4 pp. 20 cm x 13 cm. No. 94

[61]

L L
II,266

[1868*]
Aug. 15

Freshwater, Isle of Wight
[Sir Joseph] Hooker, who left yesterday for his presidency of B[ritish?] Assoc[iation], told him the "wonderful and good news" that Dr. and Mrs. Gray are coming to Europe for a year; invites them to visit at Down and "see our solitary and very quiet life" Has been spending a month at Freshwater, and returns home in a week; was in bed for a month before starting, unable to do anything, and to his great vexation this outing has done hardly anything for him; doubts whether he can resume his small modicum of work; hears that Gray is coming to Europe to begin a great work, and wishes he could have attended the B[ritish] Assoc[iation] under the presidency of "our good friend". Is greatly disappointed that he himself had not strength enough for the undertaking.

A L S 3 pp 20 cm x 13 cm. No. 98

[62]

[1869*]
June 1

Down, Beckenham, Kent.

Expresses many thanks to Gray and Mrs. Gray for their letters on Expression; one of Mrs. Gray's answers about the negro is of special value, as he had failed on this head with all the more distinct types of man and had begun even to doubt whether it would be general; also of special use will be the case of the shrugging of the shoulders, and that about the head not being shaken laterally for negation is very disagreeable; appreciates Gray's kind expressions about his accident; his horse fell and partly rolled over on him; he thought he would require 3 months to move, but was nearly well in 3 weeks, although he has had more pain lately and has not been very brisk; "My good and dear wife, in consequence, is going to take me, . . . on the 10th, to a house, which we have heard of in N. Wales for 6 or 7 weeks " They all wish Gray could have given a better account of Mrs. Gray's thoughts; feels it is very disheartening that the voyage should have driven away so much of the good effects of the Grays' trip up the Nile; has been preparing a New Edition of the "ever-lasting old" Origin [of Species] which has consumed 8 weeks, and notes for a French Edition of his Orchid book [The Various Circumstances by which Orchids are Fertilized by Insects], so that his regular work has been much interrupted.

A L S 4 pp. 20 cm x 13 cm. No no.

[63]

[1870]
Dec. 7

Down, Beckenham, Kent.

"I have been half worked to death in correcting my uncouth English for my new book." Has been glad to hear of Gray's cases appearing like incipient dimorphism; believes they are due to mere variability and have no significance; he found good instances in *Nolana prostrata* and *Amsinckia* and experimented on them, but the forms did not differ in fertility; has long thought that such variations afforded the basis for the development of dimorphism; was not aware of such cases in *Phlox*, but has admired arrangement of anthers, causing them to be all raked by an inserted proboscis; is glad to learn of Gray's curious case of variability in ovules; Gray's *Drosera* at last made a shoot which he could observe and the case is rather interesting; "The filament of *Dionaea* is not sensitive to very light prolonged pressure or to nitrogenous matter, but is exquisitely sensitive to the slightest touch. In our *Drosera* the filaments are not sensitive to a slight touch, but are sensitive to prolonged pressure from the smallest object of any nature; they are also sensitive to solid or fluid nitrogenous matter. In your *Drosera* the filaments are not sensitive to a rough touch or to any pressure from non-nitrogenous matter, but are sensitive to solid or fluid nitrogenous matter. Is it not curious that there should be such diversified sensitiveness in allied plants?"

L S 4 pp. 20 cm x 13 cm. No. 90

[64]

M.L
II, 324

[18]72
Jan. 15

Down, Beckenham, Kent.

Has taken up an old subject which formerly interested him - namely, the amount of earth brought to the surface by worms; wants to know whether there are in the United States the little vermiform piles of earth which are so common on English lawns, fields, woods, and waste lands; asks if they are as numerous in the United States as they are in England; would have assumed this would naturally be the case had it not occurred to him that the severe winters might make a difference; a very few lines in answer would suffice; suggests, if Gray has any correspondent in northern Canada, he send this letter to him for additional information

L. S. 4 pp. 20 cm x 13 cm. No 99

[65]

[1872]
Oct. 22

[Sevenoaks], Down, Beckenham, Kent

Has received Gray's "Dubuque Address" and has seen a short extract from it on Sequoia; would gladly accept Gray's view to account for the spiral winding of a tendril which has clasped nothing, had it not been for the fact of the same tendril, when it has clasped an object, extending in opposite directions; the concave side of the lower part, in this latter case, can hardly have contracted; thinks he has explained the approximate cause of the reversed spiral curvature, but cannot understand the more remote cause, worked hard for 4 or 5 weeks on Drosera, and then "broke down" and went away for a complete rest; has "very little power of working now and must put off the rest of the work on Drosera till next spring", as his plants are dying; "It is an endless subject, and I must cut it short, and for this reason shall not do much on Dionaea." The point which interests him most is tracing the nerves which follow the vascular bundles; "By a prick with a sharp lancet at a certain point, I can paralyze 1/2 the leaf, so that a stimulus to the other half causes no movement. It is just like dividing the spinal marrow of a Frog - no stimulus can be sent from the Brain or exterior part of spine to the hind legs, but if the latter are stimulated, they move by reflex action." Finds his old results about the astonishing sensitiveness of the nervous system of Drosera to various stimulants fully confirmed and extended; asks Gray to make for him next spring two observations on D[rosera] filiformis when growing vigorously and on a warm day, he had the Kew specimens to experiment upon, but is afraid of trusting to his results; includes directions for experiments, and asks Gray to keep the paper, till next spring.

L L
II, 495

A L S 6 pp. 20 cm x 13 cm. No 100

[66]

[18]73
Jan. 8

Down, Beckenham, Kent.

Has received a letter about *Dionaea*, sent by Gray, dated Wilmington, July 9, [18]67; "It has no signature but you refer to it as written by Mr. Canlay or Canbay or Cawley. Will you be so kind as to write the name for me distinctly, for some people are so foolish as to say that your handwriting, like mine, is not very legible." The letter has interested him much in some respects and the gentleman seems very kind and willing to oblige; requests Gray to ask him to observe whether the *Dionaea* catches large or small insects; suggests he gather a dozen or score of leaves which are quite closely shut, bring them home, and open them; "As 'large' and 'small' are such vague terms it would be very advisable, if not causing too much trouble, to measure the breadth of the broadest part and length (from end of head to end of abdomen) of an average sized captured insect; and then state how many exceeded or were less than this mean. But it should be particularly stated whether any of the captured insects were quite minute. Does your friend still abide by his conclusion that a leaf after catching an insect never acts again? This agrees with my small experience with cultivated plants." Has received a letter from Mrs. Treat of Vineland saying she would observe *D[rosera] filiformis* next summer; "You will see by this letter that I am obeying your orders and working on *Drosera* & Co."

L. S. 4 pp. 20 cm x 13 cm. No. 102

[67]

[1873]
Mar. 11

Down, Kent.

Is astonished that Agassiz should use such an argument as that of the trees; has sent the memorandum to Nature, but believes they receive so many articles they can find room for only a small portion of them; he worked hard last summer on *Drosera*, could not finish until he got fresh plants, consequently took up the effects of crossing and self-fertilizing plants, and got so interested that "*Drosera* must go to the dogs till I finish with this and get it published; but then I will resume my beloved *Drosera* and I heartily apologize for having sent the precious little things even for a moment to the dogs." Mrs. Darwin is taking him on Friday, "as an abject prisoner" to London for a month, and he hates having to stop work; thanks Gray for the "Dubuque Address" which he read with the greatest interest.

A L S 3 pp. 20 cm x 13 cm. No 106

[68]

L. L.
II, 465

[1874]
June 3

Down, Beckenham, Kent

Was astonished to see about a week ago that Gray was going to write in Nature an article on him [Darwin], and this morning he received an advance copy; "It is the grandest thing ever written about me, especially as coming from a man like yourself. It has deeply pleased me, particularly some of your side remarks. It is a wonderful thing to me to live to see my name coupled in any fashion with that of Robert Brown. But you are a bold man, for I am sure that you will be sneered at by not a few botanists. I have never been so honoured before, and I hope it will do me good and make me try to be as careful as possible; and good Heavens how

L. L.
II, 457

difficult accuracy is." Is now hard at work getting his book on Drosera ready for the printers, but it will take some time for he is always finding out new points to observe; thinks Gray will be interested in his observations on the digestive process in Drosera - the secretion contains an acid of the acetic series and some ferment closely analogous to, but not identical with, pepsin; has been making a long series of comparative trials; "No human being will believe what I shall publish about the smallness of the doses of phosphate of ammonia which act." Found out day before yesterday that Pinguicula digests and then absorbs animal matter; knows this holds good for albumen, gelatine, and insects, but is now in the midst of his observations; began to read the Madagascan "squib" quite gravely; when he found it stated that Felis and Bos inhabited Madagascar he thought it was a false story, and did not perceive it was a hoax till he came to the woman; asks Gray to thank Dr. Rodd for the sketch of the ears; has been glad to see the account, but it is too late for use, as he has finished correcting the early sheets of a New Edition of The Descent [of Man]; has been forced to say he does not feel so confident about the Darwinian theories as he did before; he and Mrs. Darwin have their game of backgammon every evening and he often thinks of the scene between Gray and Mrs. Gray.

A. L., S. 7 pp. 20 cm x 13 cm. No. 103

[69]

[1874]
June 5

Down, Beckenham, Kent.

Has read Gray's article ["Charles Darwin", June 4, 1874] in Nature, and now cannot be easy without expressing his profound gratification; "Everyone, I suppose occasionally thinks that he has worked in vain, and when one of these fits overtakes me, I will think of your article, and if that does not expel the evil spirit, I shall know that I am at the time a little bit insane, as we all are occasionally." Does not think any one ever noticed the point Gray makes about teleology; "I have always said you were the man to hit the nail on the head."

A. L. S. 2 pp. 20 cm x 13 cm. No. 104

[70]

[18]74
Dec. 25

Down, Beckenham, Kent.

Has read with great interest Gray's article on the longevity or duration of varieties; has long felt interest in this subject but has never before connected it with inter-crossings; thinks Gray has put the case very clearly; has heard from Mrs. Treat about Utricularia, but she does not go very deep into any subject; has difficulty in believing some of her statements; the whole of his book is in manuscript form, but does not know how long it will take to get it ready for the printers; hopes it will be out late in the spring; will send Gray a copy; "The death of Mrs. Hooker has indeed been a terrible blow. Poor [Sir Joseph] Hooker came here directly after the funeral and bore up manfully. I know I would much sooner die than suffer such a loss."

L. S. 3 pp. 20 cm x 13 cm. No. 110

[71]

L L
II, 497

L L
II, 497

L L
II, 367

[18]76
Jan. 28

Down, Beckenham, Kent.

Expresses thanks for many things, especially reviews in the Nation and American Journal of Science; the articles are instructive and gratifying and give the clearest possible account of his [Darwin's] work; he was interested in Gray's essay on diversified means of dispersal of seeds, and [F] Hildebrand's capital essay gives many analogous cases; thanks Gray for facts about Mauraania; is resolved not to correct his books more than once, but to "use the small quantity of work left in me" for new matter; is preparing a book on advantages of crossing, which will be a sort of complement to his Orchid book [The Various Contrivances by which Orchids are Fertilized by Insects]; [Sir Joseph] Hooker is absorbed in routine work; [postscript in Darwin's handwriting.] Gray is to tell Mrs. Gray that in a backgammon tournament with his wife she won 2490 games while he won 2795.

L S 4 pp. 20 cm x 13 cm. No. 111 [72]

[18]76
June 25

Down, Beckenham, Kent.

Has been at work on Pinguicula and finds it proves an "excellent digester of fibrin, albumin, and meat"; has been interested to find that if a row of flies be placed near the margin, the edge of the leaf in 2 or 3 hours turns over so as to bring the secreting and absorbing glands into contact with the upper as well as the lower surface of the flies; the point which interested him most is that the leaves certainly absorb nutritious matter from little leaves and seeds which are blown onto them, hence the plant is not only insectivorous, but graminivorous and granivorous; has had several leaves sent him from "N Wales", and it is extremely rare to find a single leaf without more than 1 captured insect; found also that each had more than 1 leaf, on an average, of some other plant adhering to it, and 2 seed capsules; asks if Pinguicula grows near Gray, and, if it does, wishes Gray would look at a number of leaves and tell him whether he finds any seeds or leaves of other plants adhering to them; "This would be valuable. It will amuse me much to make a good case about the omnivorous habits of this plant."

L S 3 pp. 20 cm x 13 cm. No. 108 [73]

[18]76
Aug. 9

Down, Beckenham, Kent.

Has just received Darwiniana, [1876, by Asa Gray], and is much obliged for it; is "uncommonly glad" Gray has been urged to compile his writings not only on his [Darwin's] account but for the public good, for every one of the articles seem to him excellent; he will read the whole soon but will not be able to resist reading the 2 new articles first; sees by the Table of Contents that Gray has discussed one subject - the meaning of sex - on which he himself has entered in a new book now gone to press, [The Effects of Cross and Self Fertilization in the Vegetable Kingdom]; this will complete all he will ever do on the subject; is, however, preparing a New Edition of his Orchid book [The Various Contrivances by which Orchids are Fertilized by Insects] and this has led him to review several of 4 short notices on this subject; so much has been published he has been able to give only the briefest abstract of what has been done;

as it is, he has had to cut up the book immensely; both books will be sent Gray when they are published; asks if Gray had time to glance at his son Frank's [Francis'] paper on Stipa; he [Francis] makes out nicely that the twisting depends on the twisting of each separate cell. "He has now made a fine discovery, but it is too long a story."

A L S 4 pp. 20 cm x 13 cm No 112 [74]

[18]76
Oct. 28

Down, Beckenham, Kent.

Is sending all the clean sheets [of The Effects of Cross and Self Fertilization in the Vegetable Kingdom] as yet printed and others within a fortnight; "Please observe that the 6 first chapters are not readable, and the 6 last very dull Still I believe the results are valuable." He will be curious to see what Gray thinks of the book, as he cares more for Gray's judgment than for that of almost any one else; "You will speak the truth whether you approve or disapprove. Very few will take the trouble to read the book, and I do not expect you to read the whole, but I hope you will read the latter chapters." Appleton will publish this and his Orchid book [The Various Contrivances by which Orchids are Fertilized by Insects] in America; "I am so sick of correcting proof and licking my horrid bad style into intelligible English."

L.L.
II,467

L L
II,467

A. L. S. 3 pp. 20 cm x 13 cm No. 113 [75]

[18]76
Nov. 27

Down, Beckenham, Kent.

Thanks Gray for correcting a "stupid blunder"; hopes he has received by this time a nearly complete set of sheets [of The Effects of Cross and Self Fertilization in the Vegetable Kingdom], but, in case they should have failed, is sending another set with the exception of a few last pages and the title page which will be sent very soon; will be delighted to send sheets in advance of the Orchid book [The Various Contrivances by which Orchids are Fertilized by Insects] which he has written for today; only yesterday he discovered he had overlooked one of Gray's papers on Platanthera, from which he could have extracted 2 or 3 good facts, and this has "vexed" him; finds it very difficult to keep references distinct for various subjects.

L S 3 pp. 20 cm x 13 cm. No. 114 [76]

1876
Dec. 4

Down, Beckenham, Kent

Is going to republish his "Dimorphic Papers", with additions; has become convinced that plants of this class cannot be recognized merely by the varying lengths of pistils and stamens in a few specimens; it is necessary to compare size of pollen grains and state of stigma; therefore he wants Gray to send him one or two flowers of both forms of Leucosmia and Drymisperrum (mentioned in American Journal of Science, Vol. 39, page 104) if not very rare and precious; asks for rather young flowers, otherwise the pollen will have been shed or lost; his object is to see plants in as many natural families as possible, and he would be very much obliged if Gray could spare him flowers of any dimorphic plant not included in the Primulaceae,

Lincae, Oxalidae, Gentianeae, Verbenaceae, Borragineae, Rubiaceae, and Lythraceae; asks if any of the dimorphic plants known to Gray inhabit water or marshes.

L. S. 3 pp. 20 cm x 13 cm. No. 115

[77]

1876

Dec. 20

Down, Beckenham, Kent.

Thanks Gray for information about *Hottonia*; refers to Gray's having mentioned *Forsythia* in the American Naturalist; he has just examined dried flowers from Kew, finds that [*Forsythia*] *suspensa* is beautifully dimorphic, and so he has a new family; has been thinking about Gray's proposed new terms, he cannot "for very shame" change again; has used this term in 2 or 3 printed articles, and it is used by several German and Haitian writers; "Kahn objected to the term on the same ground as you do; but no one objects to *Vertebrata*, because it includes an animal without vertebrae" Moreover, "heterostyled" seems to him more definite than "heterogone", as the latter would apply to di- and monoecious and to polygamous plants; he is, of course, not able to appreciate and difficulty of working in the term in systematic works, but says [George Henry Kendrick] Thwaites speaks of "forma *stylosa*", and asks why a species may not be called "heterostylosa"; "However this may be it really would be too ridiculous for me to change again."

L. S. 3 pp. 20 cm x 13 cm. No. 116

[78]

1877

Jan. 3

Down, Beckenham, Kent.

Appreciates Gray's willingness to send such rare specimens, but could not think of accepting the offer on the mere chance of making out whether the plants are heterostyled; moreover, feels the chance would be very small without better specimens, as with dead plants no evidence is sufficient except difference in size of pollen-grains; has given Kew so much trouble lately he has vowed he will give them no more for some time, but will find out later whether they can spare any of the plants in question; is very glad of the notice about the black pigs; "My faith in [Jeffries] Wyman is so great that I have not been shaken by [Alfred Russell] Wallace who founds his speculation on a very feeble basis." Asks whether *Primula mistassinica*, *Linum virginianum*, and *Boottii* [*Boottia*?] are dimorphic.

L. S. 3 pp. 20 cm x 13 cm. No. 118

[79]

[18]77

Jan. 23

Down, Beckenham, Kent.

Thanks Gray for his card about *Pontederia*; refers to Gray's letter of Nov. 21, 1870, which says that *Phlox subulata* presents 2 forms which have been named as species but which Gray is inclined to think is a case of di- or tri-morphism; Gray has spoken of this as a common species; asks, if that is so, that he send 2 or 3 dried flowers of the different forms for comparison of pollen grains and stigmas; Gray also has mentioned *Gilia aggregata* (*pulchella*) with stamens and pistils varying much in length; asks, if this is not a rare plant, that specimens of the 2 forms be sent for examination

L. S. 3 pp. 20 cm x 13 cm. No. 120

[80]

[1877*]
Feb. 12

Down, Beckenham, Kent.

"Your abstract of my book [The Effects of Cross and Self Fertilization in the Vegetable Kingdom] is inimitably good. You have given everything. . . . By Jove I ought to owe you a grudge! In earnest it could not in my opinion be improved." Thanks Gray for the specimens and for not "hating" him for "bothering" him so much; will examine them with greatest interest in about a week's time; is sending his son, Frank [Francis], to Kew to look at specimens of *Leucosmia* and *Polemoniaceae*; longs to get this old work off his hands and so will publish too soon to profit by sowing seeds of *Gilia*; has sent title of Orchid book [The Various Contrivances by which Orchids are Fertilized by Insects] and has written Murray to send a complete copy; some of Gray's criticisms and suggestions in his 2 reviews are very good; [gives directions for correction of manuscript].

A. L. S. 4 pp. 20 cm x 13 cm. No. 122

[81]

[18]77
Mar. 8

Down, Beckenham, Kent.

Judges from length and positions of stamens and pistils, but more especially from stigmas of the two forms, that *Leucosmia Burnettiana* is in all probability dimorphic; the pollen-grains do not differ in size, which is the best evidence; two forms of *Gilia pulchella* differ in their stigmas but not in their pollen-grains, and the case was left quite doubtful had not *Gilia micrantha* expanded in exactly the same measure in the stigma and moreover in the diameter of the pollen-grains; therefore he does not doubt that both *Gilias* and the others to which Gray alludes are truly hetero-styled; "Phlox subulata is a devil incarnate and as bad as *Rhamnus*; perhaps it was once heterostyled, with the short-styled forms since rendered more feminine in nature." Knows altogether of 39 genera, in 14 families, which include heterostyled species; this pleases him; finds it "doubtful work making out anything about dried flowers; I never look at one without feeling profound pity for all botanists, but I suppose you are used to it like ells to be skinned alive."

A. L. S. 4 pp. 20 cm x 13 cm. No. 117

[92]

[1877]
June 4

Down, Beckenham, Kent.

Prof. [Charles Edwin?] Bessey's case has come too late, as the sheets on this subject are printed; had it come earlier he would not have known what to do with it; "The pollen-grains and stigmas ought to be compared. The case seems to be well worth careful investigation and I would have given my eyes for seeds formerly; but now I have done with the subject." Suggests, if Bessey likes experimental work, he might raise seedlings and fertilize short and long pistils with pollen from long and short stamens from distinct plants and on the same plant, counting the proportion of flowers which set fruit when fertilized in the various ways, and the number of seeds per fruit; Bessey's diagram shows the nature and difference between the flowers excellently; will send him his book when published in 4 or 5 weeks, which will show him how to experiment on the plants; thinks the case may be one merely of great variability or it may be one of incipient heterostylism, and under this point of view he would

formerly have investigated it most carefully; "When you receive my little book, you will see that I have done an audacious deed with respect to you." Is now trying to "make out" the use or function of "bloom" or the waxy secretion on leaves and fruit of plants, but is very doubtful whether he will succeed; asks if Gray can give him any light as to whether such plants are commoner in warm than in colder climates; he asks because he often walks out in heavy rain and sees leaves of a very few wild dicotyledons with drops of water "rolling off them like quicksilver", whereas in his garden, greenhouse, and hothouses there are several; asks if bloom-protected plants are common on the western plains of the United States; Sir Joseph Hooker thinks they are common at Cape of Good Hope, but it would be a "puzzle" to him if they are common in very dry climates; finds bloom very common on Acacias and Eucalypti of Australia; some of the Eucalypti which do not appear to be covered with bloom have the epidermis protected by a layer of some substance which dissolves in boiling alcohol; asks if there are any bloom-protected leaves or fruit in the Arctic regions.

L L
11,512

A. L. S. 6 pp. 20 cm x 13 cm. No. 119

[83]

London.

Has received Gray's reviews on his [The Different] Forms of Flowers [on Plants of the Same Species] which pleased and instructed him; was especially pleased that Gray approved of the suggestion of giving names to subdivisions of polygamous plants; expresses thanks for the review of Mr. Cook, which he supposes Gray wrote; [Thomas?] Carlyle's letter about him [Darwin] was a forgery or "an infernal lie". Suggests Gray get some young man to experiment on grades of fertility of Epigaea and Rhamnus; Herman Müller describes Valeriana dioica as consisting of 4 analogous forms, and attributes these cases primarily to the existence of 2 forms - one with longer and one with shorter corolla, such as he has shown exist in other cases; but Müller's German was too "obscure" for him to follow; is not sure of the uses of bloom or waxy secretion on leaves and stems of plants; asks whether glaucous plants are more or less common in arid countries to the west or in humid districts on the Atlantic; expresses delight at his son's having married an American lady.

A L. S. 4 pp. 20 cm x 13 cm. No. 123

[84]

Down, Beckenham, Kent.

Asks Gray to send seeds of Echinocystis lobata to Dr. Hugo De Vries, Professor of Botany at Amsterdam, who has done such excellent work on climbing plants and who wishes, at his [Darwin's] suggestion, to make some observations on its tendrils; "I see we are both elected Corr[esponding] Members of the Institute [of France?]. It is rather a good joke that I should be elected in the Botanical section, as the extent of my knowledge is little more than that a daisy is a Compositous plant and a pea a Leguminous one." [Letter contains 4 pages on the construction and fertilization of Spiranthes autumnalis, and 2 diagrams].

L.L.
II,400

A L. S. 7 pp. 20 cm x 13 cm. No. 124

[85]

[18]78
Jan. 21

[18]78
Aug. 15

[18]79
Oct. 24

Down, Beckenham, Kent.

Has procured and read the New Edition of Gray's Text-Book of Botany [6th edition of Botanical Text-Book, 1879, entitled Structural Botany or Organography on the basis of Morphology] which has been greatly developed since old times, and finds at pages 21 and 22 a curious account of some seedlings; asks if it is possible for Gray to send him a few seeds of *Ipomoea leptophylla* and *Megarrhiza californica*; has procured *Delphinium nudicaule* from a nurseryman; has attended somewhat to the manner in which seedlings break through the ground and it is for this object he wants the seeds; has written a rather big book - "more is the pity" - on the Movements of Plants [The Power of Movement in Plants], and is now just beginning to go over the manuscript the second time, "which is a horrid bore".

A. L. S. 3 pp. 20 cm x 13 cm. No. 125

[36]

1879
Dec. 16

Down, Beckenham, Kent.

Thanks Gray for having taken "so much trouble about the seeds" but feels "rather guilty", for though he was very "curious" to see them germinate yet the points in question were not of much importance; *Ipomoea leptophylla* has not yet germinated, but he has a good many seeds to sow again in early spring if these already sown do not germinate; has often suspected that some kinds of seeds have "an obstinate habit" of dying if sown in the winter; has just put 5 *Megarrhiza* seeds to soak, only 1 sinks, and that "alarms" him; wishes very much to see whether the curious heel-like projection at the base of the hypophyllum stem which splits the seed-coats so beautifully in other cucurbitas is here absent, as he hopes, and as ought to be the case, as the cotyledons are not withdrawn from the seed-coats; asks for a few more seeds of the common cotton which he cannot get in England; the cotyledons "behave oddly at night, for when old and only when old, they sink downwards" and he did not investigate the point sufficiently when he had seedlings.

A. L. S. 4 pp. 20 cm x 13 cm. No no.

[87]

[18]80
Jan. 19

Down, Beckenham, Kent.

Has been greatly interested with the *Megarrhiza* seeds Gray sent; Gray has been misinformed about their germination and thinks he cannot have watched the whole process; he placed some seeds on the surface, others half an inch beneath, and others deeper, but none of the cotyledons were lifted up; one seed on the surface was a little tilted by the root not penetrating the ground, but this often occurs with all kinds of seeds; "The petioles of my specimens were not stiff enough to bear the weight of the seed. What takes place is that the radicle bends down and penetrates the ground, but grows only to a length of about half an inch or less (length rather doubtful as I did not wish to kill specimens by making sections). When of this length its growth is arrested, and the lower ends of the tubular petioles grow quickly and penetrate the ground just like a root to a depth of nearly 2 1/2 inches; then their growth ceases, and now the radicle takes up the game and grows very quickly. In every case

the base of the radicle lay 2 1/2 inches beneath the surface. You probably know that if ordinary seedlings are placed in solution of permanganate of potassium, the radicle is coloured brown whilst the hypocotyl and cotyledons are left uncoloured. Now when a seedling *Megarrhiza* with the plumule just reaching the surface was thus treated, the whole radicle (and hypocotyl) and the whole of the tubular petioles (densely covered with root hairs) became brown whilst the plumule was quite uncoloured. Therefore I think it certain that the tubular petioles act functionally like a root and that the cotyledons are hypogaeal. The sole use of this wonderful manner of growth which occurs to me is to hide the enlarged root, at least first, beneath 2 1/2 inches of soil as a protection against enemies. When my plants are two or three weeks old I will cut a slice from the root, and taste it and test it for starch." Asks whether the plant is an annual or perennial, if the root comes to the surface when it has become large, and if it is then hard and bitter; wonders whether it is attacked by beasts, birds, insects, or slugs in California; it has been a "great grief" that not one of the seeds of *Ipomoea leptophylla* has germinated; his gardener opened some and found them rotten.

L. S. 6 pp. 20 cm x 13 cm. No 126

[88]

[1880*]
Jan. 20
& 21

Down, Beckenham, Kent.

Drawings on page 21 of Gray's textbook [6th edition of Botanical Text-Book, 1879, entitled Structural Botany or Organography on the basis of Morphology], showing that the seed had been down at 1/2 inch depth, represent perfectly all that he has seen; thinks Gray may like to hear that the first true leaves break out through a split at the base of the confluent petioles of *Delphinium nudicaule* precisely as in *Megarrhiza* where the seedling of the plumules bursts the tube, and then the bowing downward of the tip of the plumule forces it laterally out of the tube; the bowing down of the tip, which is at first straight, is a common movement with seedlings, but here it plays a new part; quotes [Prof. Thomas?] Meehan, in a paper lately read before the "Philadelphia Society", as saying that a single plant of *Linum perenne*, brought from Colorado to him, was quite fertile with him, whereas he [Darwin] thought it was absolutely sterile with its own pollen; Meehan does not state whether his plant was long-styled or short, but, as it came from Colorado, he imagines it was endemic; asks if *Linum perenne* grows in Colorado; Dr. Alefeld says none of the true American species are heterostyled; thinks, if Meehan has mistaken the species, it would be "too bad" to doubt on another man's accuracy without taking the smallest pains to be accurate himself; was tempted to write to the Philadelphia Society to inquire how the case really stands, but has decided not to, as [F] Hildebrand has fully confirmed Meehan's statement; Meehan in accuracy seems to him to be "impervious to Science". Has just spent a delightful 2 hours at Kew and heard prodigies of Gray's strength and activity that "[word illegible] me up a mountain like a cat."

A. L. S. 7 pp. 20 cm x 13 cm. No. 127

[89]

[18]80
Feb. 17

Down, Beckenham, Kent.

"If my letter opened your eyes, yours has opened mine much wider. It is very strange that plants, if they belong to the same species, should behave so differently." His seeds were laid on the surface or buried in a mixture of peat, sand, and common soil, and this may have yielded more easily than Gray's soil; believes, from the extraordinary intermission in the growth of the true radicle, from the root-hairs, and from the petiole staining brown with permanganate of potash, that the normal function is to bury itself; his plants are growing very vigorously and should they flower he will send some dried, with leaves, for the chance of Gray's being able to name them; "I suppose when the petioles grow in the air they are stiffer than when hypogaeal, for mine could not support the weight of the cotyledons. One seed germinated abnormally; one alone of the 2 cotyledons emitted its petiole which was a hollow 1/2 cylinder as in sketch." Some of the seeds received last were a little flattened and evidently different; they were sown separately, but not one germinated; [2 pp. in another handwriting].

A. L. S. 4 pp. 20 cm x 13 cm. No. 128

[90]

Encloses small sketch of petiole.

1881
Jan. 29

Down, Beckenham, Kent.

Expresses thanks for Gray's reviews in the [American] Journal of Science, "C. Darwin and F. Darwin, 'Power of Movement in Plants'", III, XXI, 245] and the Nation; they pleased him greatly because there is hardly any one in the world whose approbation he values more highly than he does Gray's; "That was a stupid blunder about Opium, but you cannot put yourself in my frame of mind. . . . Nothing in your Review pleased me more than your opening sentence about Frank [Francis Darwin]. If you knew him well, you would know that such an idea as being offended with you never could cross his mind. In fact I wish I could infuse a few [word illegible] of vanity and self-conceit into his veins, for he never will value in the least what he does. Therefore I am certain that the notion or wish that you would speak in his praise would never have occurred to him." Hopes when Gray and Mrs. Gray come back to the continent they may find time to pay a little visit at Down.

A L S. 4 pp. 20 cm x 13 cm. No no.

[91]

Jan. 2

Thanks Gray for two letters and for box with plants; *Mitchella* looked as fresh as if dug up the day before; "What a pretty little creeper it is with its scarlet berries." Hopes the *Cypripedium* will flower; has decided to put live insects in by stopping up the end of the stopper and catching them as they come out of the lateral orifices; if they are smeared with pollen he will put them in again, and so make them go to work and then examine the stigma; has just finished a paper for the Linum Society on dimorphism of *Linum*- "a much better case than *Primula*". Tenchon finds the *L[inum] Lewisii* bears on the same plant flowers with pistils longer and shorter than, and equal to, the anthers; wishes he could get seed of the Arctic plant for he would like to see this new case; has *Amsinckia* and

Mitchella growing well in his garden; will send his Linum paper when it is published; was glad to receive a note from [James Dwight] Dana giving a moderately good account of himself, asks Gray to weigh in grains one of his wild *Fragaria virginiana*; would be glad to add to his large and valuable collection of facts on variability any cases Gray might send on bud variations among garden plants; [Sir Joseph] Hooker has finished *Welwitschia* ["On *Welwitschia*", Linnean Society Transactions, 1863] and has gone to Paris; hears that *Cinchona* is dimorphic and has written [George Henry Kendrick] Twaites in Ceylon to cry the pollen; "I wish to Heaven the north did not hate us so. . . . I doubt the war being justifiable." Asks, if flowers of an oak or beech tree had five grand well-colored corollae and calyx, would they be still classed as low in the vegetable kingdom.

A. L. S. 8 pp. 20 cm x 13 cm. No. 56 [92]

Jan. 22

Expresses interest in conditions in the United States and thanks Gray for the new cases of dimorphism; new cases are "tumbling in" almost daily, but he has no time to work a quarter of them; has sent his *Primula* paper; influenza has delayed his Orchid book [The Various Contrivances by which Orchids are Fertilized by Insects] for 3 weeks; asks Gray's reactions on the *Primula* paper; the results may be of no general use but are worth while to himself; is glad of the news about [James Dwight] Dana whom he respects; has forwarded Gray's letters to [Francis] Boott and [Sir Joseph] Hooker; Hooker jokingly writes that his [Darwin's] book on Natural Selection [Origin of Species?] has made him an aristocrat and that he thinks the high breeding of the aristocracy of highest importance; regrets their different views on political matters and deploras recent speeches and actions of America's leading man - [Capt. Charles] Wilkes' having been made a hero for boarding an unarmed vessel, the judge's advice to him, and Massachusetts' governor triumphing at a shot being fired, right or wrong, across the bow of a British vessel; suggests it would be well for the peace of the world for the United States to split up into 2 or 3 nations, but, on the other hand, he cannot bear slavery to triumph and thinks a division of North and South, with armies and fortifications and custom houses without end, fearful; feels he has "done for" himself in Gray's and Mrs. Gray's eyes; charges difference of opinion to "that confounded Longitude"; expresses affection for the Grays; has finished looking through "9 big volumes" of Lecoq's Botanical Geography - "a horrid, dull book", but has "stumbled" on a few good facts and on several cases of dimorphism in *Borraginea* and *Lobirota*; *Lythrum* seems to him a very curious case, as 2 or 3 kinds of flowers occur on the same plant; is experimenting with *Melastomas* and suspects that the 2 sets of anthers have different functions; says *Hottonia* is dimorphic, like *Primula*.

A. L. S. 6 pp. 20 cm x 16 cm. No. 74 [93]

Jan. 26

Thanks Gray for 2 pleasant letters, the former about reviews in the Athenaeum and many other points, and the second with answers to several questions; "In the latter you seem cruelly overworked. Although it is one of my pleasures to write to you and a very great pleasure to receive a letter from you; I earnestly beg you never to write to me when so busy; if I did not hear for six months or twelve months, I should understand the cause. Remember what a number of valuable and most interesting letters I have received from you. So pray do not write unless you have a little leisure, which seems rare with you." Has "little or nothing" to say, for he sees no one and hears from no one except "dear" [Sir Joseph] Hooker; "How curious the lie about Ohio marriages! I find it a dreadful evil in my compiling work, not knowing what to trust." Thanks Gray for references about Phyllotaxis; has been "half-mad" over it, but is having a lull; has made no end of diagrams, but all his attempts have "signally failed", as might have been expected; has received Gray's review on [Alphonse Louis Pierre] de Condolle; hopes Gray has received [George] Bentham's address "which has pleased me much, more than I understand why; it will do a world of good for our side." What Gray says about P[hlox] sounds "very suspicious"; has been looking at his P[hlox] Drummondii which is not dimorphic; Euonymus is dimorphic like Thyme, with hermaphrodite and female plants; Mitchellia has only 2 flower buds, but he has just found out it is unhealthy because he has given the plants too much water; seeds of Sicyos did not germinate, and only 1 plant of Echinocystis has come up; has been looking at its tendrils and has seen "with great interest their irritability; it is a very pretty little discovery of yours." Is observing the plant in another respect - "the incessant rotary movement of the leading shoots, which bring the tendrils into contact with any body within a circle of a foot or 20 inches in diameter. If I can make out anything clear about this movement, and do not find that it is known, I will perhaps write a letter to you for the chance of its being worth inserting in Silliman['s Journal of Science] or elsewhere."

A. L. S. 4 pp. 20 cm x 16 cm. No. 82 [94]

Feb. 1

Is glad there is to be a Reprint [of Origin of Species], but especially hopes Gray's review will appear in it; suggests the edition be entitled "Reprinted from corrected Second Edition [1860] with additional corrections"; is sending additions today, and in a fortnight will send Preface giving a short history of opinion on The Origin of Species; has a letter from Murray, the publisher, saying that whatever benefit may arise from the Reprint in the United States Gray is entitled to, and Murray will claim nothing; "Perhaps you would like me to stand in Murray's position and take 1/3 of the profits whatever they may be. Nothing would pain me so much as to take all the profit, as I wholly and absolutely shall owe all to you; and if you will print your Review it will be a joint publication."

A L. S. 2 pp. 20 cm x 20 cm. No. 44 [95]

Feb. 5

Down, Beckenham, Kent.

Encloses a question requesting that it be forwarded for an answer to the person in charge of Laura Bridgman; [Dr. Francis] Lieber's paper states that when Miss Bridgman is astonished she raises both hands with fingers apart and open palms directed toward the person causing the astonishment; this has led him to inquire about gestures of importance, such as the shrugging of shoulders; has finished his book on Descent of Man, and its publication is delayed only by the Index; will send Gray a copy, although he does not know that he will "care about it", as parts of it, like that on the moral sense, will probably "aggravate" him; "If I hear from you, I shall probably receive a few stabs from your polished stiletto of a pen." Is hard at work on his essay on Expression. [No enclosure].

A. L. S. 3 pp. 20 cm x 13 cm. No. 86 [96]

Feb. 17

Is sincerely sorry to hear of the accident to Gray's thumb; will send his note to [Sir Joseph] Hooker soon, that he may hear of it; is glad to hear of the 250 copies [of Gray's pamphlet] now at Trübner's; wrote Trübner today about a few advertisements and copies for distribution; hopes that Murray and [Sir Charles] Lyell will not prove entirely true; that it is impossible to circulate a pamphlet in England; fears that conditions in the United States must stop all interest in everything not political; will inquire of his bankers how he can repay Gray the £ 7, and wishes he would let him pay the whole £ 15; has asked Trübner to forward C[haucey] Wright's article; printers have been very slow with his New Edition of The Origin [of Species], so that he has been able to insert notice of Gray's pamphlet with title in full, for which he is especially glad; "The other day a very clever lady was staying here and read your Reviews; and remarked, 'how extraordinary if such a writer has written only on Science.'" Gray was right with respect to Pumilo; "I knew nothing whatever on the subject, but I looked at the seeds to amuse myself, and asked Hooker, . . . and he thought [John] Lindley would like an account for the G[ardeners'] Chronicle. It was foolish of Lindley to put it so conspicuous; in my note to him I said that he might like to use it some time just to fill up space. It strikes me, however, as pretty case of adaptation for the ignorant, like myself." His daughter's illness, though very severe, lasted only 4 days; suggests that Gray put the minutest atom, under a lens, on any one single extreme marginal gland of a Drosera leaf, which has all the hairs equally expanded, and watch it or look again in 10 minutes; or put a fragment of a hair of his head and look in an hour's time; intends to try many more experiments this summer and then publish; is doubtful on many points; "But the worst is that my health is failing much. I literally cannot listen to a novel for 1/2 hour without fatigue. My good dear wife declares I must go with our whole family (if my girl can be moved) for 2 months to Water Cure; and I fear I must, but it will be quits to all my experiments." Remembers having read Gray's extremely curious observations on tendrils, but thanks him for writing about them; "With respect to Design etc., you say that you suppose that I have 'not brought forward my real objections against your views'. I have no

real objections, nor any real foundation, nor any clear view. As I before said, I flounder hopelessly in the mud." Is amused by Gray's account of Agassiz denying the community of descent of allied languages, and of [Francis] Bowen denying heredity; "I cannot believe that Bowen is a strong man. What an odd and foolish fancy he must think it that all breeders of Race-Horses, Cattle and Pigs etc. should keep pedigrees, and would certainly prefer breeding from a poor animal of good pedigree than from the finest of bad pedigree. These men in fact work on my (I wish I could say our) side."

A. L. S. 8 pp. 20 cm x 13 cm. No. 54

[97]

Feb. 17

Down, Beckenham, Kent.

Thanks Gray for his pleasant letter of Feb. 8; he told [Sir Joseph] Hooker about *Linum perenne*; Hooker was interested, looked to his Colorado specimens, finds the American form is less strongly heterostyled than the European, that the stamens and styles are even equal in some specimens, but also finds variability in length in the European specimens; "If I was forced to wager I would bet that the American form would prove at least functionally a distinct species. - If you could get and send me seed of the Colorado form, I would grow both forms and see if they could be intercrossed artificially, and I would try whether the homostyled individuals were self-fertile."

A. L. S. 4 pp. 20 cm x 13 cm. No. 129

[98]

Feb. 24

Remaining sheets of Gray's review which have been forwarded to [Sir Joseph] Hooker, to go to Dr. [Francis] Boott and then to [Sir Charles] Lyell, confirm his [Darwin's] opinion that it is by far the "most able" which has appeared and will do the subject "infinite service"; Lyell agrees with him; has sent Gray's letter to Hooker, as it contains "such curious botanical facts"; hopes, as a "punishment" to himself, that he may be proved "egregiously wrong about the warmer period subsequent to the Glacial Epoch"; very much more has to be "made out" about the latest geological periods; is glad the greater resemblance of Northeast America than that of Northwest America to Asia in its plants is not quite so great an anomaly as at first appears; is interested in Gray's theological remarks in the review, but must reconsider them; "It has always seemed to me that for an Omnipotent and Omniscient Creator to foresee is the same as to pre-ordain. . . . I get into an uncomfortable puzzle something analogous with 'Necessity and Free-will' or the 'origin of evil', or other such subject quite beyond the scope of the human intellect." Has been reading a book on [Sir Isaac] Newton and gravity; [Baron Gottfried Wilhelm von] Leibnitz "attacked" the Law of Gravity, and "attacked" Newton for having used gravity, "an occult quality", to explain motions of the planets; Newton "answered" that it is philosophy to explain movements of wheels of a clock though the cause of descent of the weight could not be explained; feels this bears on what Gray says about natural selection not being proved as "vera causa"; thinks Gray lays "rather too much stress on new organs appearing in the animal kingdom; at best, it is most difficult to show any number of new organs; Edwards has tried with very little

success. "One should never forget such transitions or at least such gradations, as a swim-bladder into a Lung. - So even with the Eye, as numerous fine gradations can be shown to exist, the perfecting this wondrous organ by Nat[ural] Selection I must look at as a difficulty to our imagination and not to our reason." Had he known there would be a second American Edition he would have sent a few additions and corrections.

A. L. S. 7 pp. 20 cm x 13 cm. No. 25 [99]

Feb. 26

According to the bankers, this is "the only way" to transmit the money; hopes it will not be "troublesome" to Gray; Messrs. Trübner have been most liberal and kind and say they will make no charge for all their trouble; has settled about a few advertisements, and Trübner will "gratuitously" insert one in his own periodical; suggests Gray write and thank him; has agreed to the price Trübner recommended; has distributed 100 copies to men of science, reviewers, and libraries; believes Gray's pamphlet will do Natural Selection "right good service"; thinks the title very good; wishes the type had been rather larger, yet this would have cost more; has told Trübner he will be responsible for cost of freight box and the advertisement, but hopes the sale will cover this; every one tells him that the pamphlet will not sell, but, if it should, he will inform Gray on the chance of more copies being sent over; "I wish heartily it would sell for your sake and that some profit might be sent to you."

A L. S. 2 pp. 25 cm x 20 cm. No. 57-A [100]

Mar. 2

Has had a slight touch of pleurisy; hopes to send within a week a few more rather important additions and expansions for The Origin [of Species]; will be glad if Gray can manage to get them inserted; a few of the most "objectionable parts" are expanded a little.

A L. S. 2 pp. 25 cm x 20 cm. No. 57-A [101]

Mar. 8

Is sending some more manuscripts and hopes they are not too late for the [American] Edition [of The Origin of Species]; "If the long addition for p. 126 is too late, that at p. 336 cannot be inserted, as they hang together - unless indeed the m.s. for p. 126 were given as a supplement. These additions seem to me, judging from number of letters which I have received on the subject of some little importance. - I will for future keep to my resolution of not wasting more time on the Origin. So that you need not fear my giving more trouble." Thanks Gray for his "generous kindness and interest" about the book; "If there be any profit I am not so foolish as to despise it, but you are perfectly right that I care more for a good and corrected American Edition; and it is the simple truth that I should be infinitely pleased to share profits with you; and this would be fair in many ways." Hopes the publishers will agree that Gray's "truly admirable" review would make a "most useful" prefix; has not yet been able to read "coolly a second time" Gray's review, he has had to lend it to so many persons; has had a long letter from Dr. [Francis] Boott, "full of the most noble love of

truth and candour. He goes far with me but cannot swallow all. No one could until he had enlarged his gullet by years of practice, as in my own case." Replies to Gray's question about Zoonomia Darwin [Zoonomia, 1794-96, by Erasmus Darwin, grandfather of Charles Darwin] that he [Charles Darwin] is his [Erasmus Darwin's] grandson; "How extraordinary is the state of mind of Agassiz; it is, as you say a case of 'science run mad'. [Sir Richard] Owen after much shuffling and secrecy, with bitter sneers to some and modified very slight praise to others, has just spoken out that he rejects my views on the ground of the imperfection of the geological record. Now this is just the subject on which he knows nothing; for in his life he has never examined a single stratum. I can count in England 4 geological converts and 8 or 9 other naturalists." Hears that [George Henry Kendrick] Thwaites, of Ceylon, is one; asks if Gray saw [William Henry] Harvey's article versus The Origin; considers the article "rather weak", and [Sir Joseph] Hooker's answer "admirably good"; since writing "the above", has received Appleton's letter to Gray; fears the additions herewith sent will be too late; asks Gray to tell the Appletons he means to leave all future editions of The Origin "in its present state with the additions now sent."

A. L. S. 2 pp. 26 cm x 21 cm. No. 31 [102]

Mar. 12

Has received [Chauncey] Wright's article "this morning", but has hardly glanced at it; will give it to [Thomas Henry] Huxley who is coming in a few days and who will decide about it [for his Natural History Review]; "If it does not suit him, what on earth shall I do with it?" Thanks Gray for sending the 4 volumes of Journal of American Academy; appreciates Gray's allowing him to bear his "small risk of publication. I am surprised and pleased at sale of new Edition] of Origin [of Species] in America." Gray's pamphlet "will do the subject very great good, whether or not it sells." Quotes from many complimentary letters he has received about the pamphlet; has sent copies to [Henshaw] Ward, [James?] Martineau, [Dean?] Mansell, [Sir Joseph] Hooker, [George] Bentham, Huxley, and [Isaac Bayley] Balfour; asks if any species of Cyripodium is common with Gray, and if the pollen-masses are removed by insects; would like Gray to procure for him a pamphlet published in Charleston entitled Letter to J. Bachman on the Question of Hybridity in Animals, by G. D. Morton; "I know his paper in Silliman['s Journal of Science] (and poor it is)."

A. L. S. 4 pp. 20 cm x 13 cm. No. 52 [103]

Mar. 15

Down, Beckenham, Kent.

Thanks Gray and Mrs. Gray for their letters and enclosures; feels sure if Mrs. Gray ever learns anything authentic about the nursing of her dog she will inform him; Mrs. Darwin read him a passage from Miss Mistford's life, minutely describing a dog which had been nursed by a cat, and which "licked its paws! But as this is a second-hand account, it will not do to quote; and the description of the cat-like habits of this dog was too much even for my capacious gullet." Asks Gray to thank Agassiz for the kind message and information which he feared would prove negative; wishes he could feel he

deserves what Agassiz says of him; has just returned from a week in London where the family went for a rest, as he was "pretty well worn out." Has just moved Gray's Droseras into the greenhouse, but does not know whether they are dead or alive.

A. L. S. 4 pp. 20 cm x 13 cm. No. 91

[104]

Mar. 24

Down, Beckenham, Kent.

Thanks Gray for the 2 seeds of Megarrhiza and hopes they may germinate, for he would very much like to see a longitudinal section of the proportion of the parts when the root is only one half or one third of an inch out of the seed-coats; "You must not suppose that what is obvious to you is so to me; for as the confluent petioles of the cotyledon[s] of the Delphinium are not tubular at first, I was astonished to see the young leaves coming out of a hole or slit at their base." Thanks Gray for all the information about the Megarrhiza, the germination of which has interested him greatly; "I was much amused by your little article on the Philadelphia lawyer. You are a first-rate hand in touching up a fact. The lawyer is a cool man in trying to make me out a rogue; but this seems the fashion and according to Mr. [Samuel] Butler in the Athenaeum I am a rogue of the deepest dye, because I forgot to state that Dr. [Ernst] Krause had altered his article on Erasmus Darwin before sending it to England for translation."

A. L. S. 4 pp. 20 cm x 13 cm. No. 130

[105]

Apr. 15

Thanks Gray for taking "so much trouble" about the Expression "queries"; wishes he had thought earlier of having them printed, for he might have sent a dozen to each of his few correspondents; as it is, he can think of no one to whom to send them, so does not want any more; will send a couple to [George Henry Kendrick] Thwaites, in Ceylon, however; has been getting out and looking over his old notes on Expression, and fears he will not make so much of his "hobby-horse" as he thought he could; nevertheless, it seems to him a "curious subject, which has been strangely neglected"; has seen no one for months and has no news, but rejoices to say that [Sir Joseph] Hooker will be there next Saturday; is "plodding on heavily correcting, and trying to make an atrociously bad style a little better", in his book on The Variation of Animals and Plants under Domestication; would offer to send clean sheets, but does not think Gray would care to receive them, as there is not much about plants, and what there is is "almost all mere compilation it will be a fearfully big book in two volume[s] and I shall be the next 5 or 6 months merely correcting the press; it is enough to make one curse one's fate in being an author." Manages to "get a little amusement" by some of his experiments; has found that the trimorphic species of Oxalis behave in exactly the same "complicated manner" regarding their fertilization as Lythrum; is going on with his trials of the growth of plants raised from self-fertilized and crossed seeds, and begins now to suspect that the wonderful difference in growth and constitutional vigour occurs only with exotic plants which have been raised by seed during many generations in England but which are not properly

visited by insects, and so have been rarely crossed; has just heard of a case which has interested him "hugely" and which he is inclined to believe is true - that by cutting the tubers of differently colored potatoes through the eye, and joining them, you can make a hybrid or "mongrel"; is repeating this experiment on a large scale, for it seems to him, if true, a wonderful physiological fact.

A L S. 5 pp. 20 cm x 13 cm. No. 97 [106]

Apr. 16

Thanks Gray for second article on climbing plants; has been ill with influenza for a week; [Sir Joseph] Hooker visited him for a day, was well and in good spirits, and was going soon to write a paper on St. Helena Flora from Burchell's collections; received a letter from Fritz Muller, in Brazil, full of curious observations; one case which Müller is to publish in Germany concerns a plant with a long tubular corolla and with its stigma in the middle, and when an insect or any object touches the filaments of the stamens they suddenly and violently bend and eject pollen, which has been collected into a ball between the anthers, against the intruding object; the same movement closes the tube of the corolla so that the insect cannot fertilize the flower; in about 3 hours the tube opens and then the insect, dusted with ejected pollen from a distinct flower, can do the work of fertilization; was well at work on his new book when Murray required a new edition of The Origin [of Species]; has been "correcting and adding matter of some importance ever since"; feels the amount of time spent is compensated for by the fact that the work will serve for a new German Edition which is wanted; regrets that the American Edition was stereotyped, as alteration in stereotype is impossible; presumes the sale has stopped, and even if it has not, it would be useless to ask publishers to bring out an amended edition; will send Gray a copy; asks if Gray has any new facts on direct influence of pollen on fruit borne by mother-plant, and, also, if he knows of any cases of bud with blended character produced at junction of stock and graft; has been reading a paper by Carfery on the subject; hopes nothing will cause "more trouble and hatred" between America and England; is glad that slavery in America is abolished; he works 2 to 3 hours and walks 3 to 4 miles daily, yet never escapes physical discomfort.

A. L. S. 8 pp. 20 cm x 15 cm. No. 96 [107]

Apr. 21

He would greatly prefer Gray's not returning any of Trübner's remittance, but insists he must not return more than half, as otherwise he [Darwin] would have gained an "immense advantage" in having given away many copies of Gray's pamphlet; "So add to all your kindness by letting matters remain as they are." Would like a few copies to give away; was asked for one yesterday; "I have never met one person who was not delighted with your writing." Will send half of his edition on Orchids [The Various Contrivances by which Orchids are Fertilized by Insects] in a few days, and the rest will soon follow; fears it can never be popular, but asks Gray not to judge too severely by the first half, "for, if I do not deceive myself the two last chapters are better." Believes he has been very foolish to publish in popular form; Murray has thought of some arrangement for an American republication, as

[Sir Charles] Lyell's new book is to appear in America; but with "my less important book" it seems "quite out of the question"; "The North seems going on generally victorious; and thank God there is distinct ground broken as to Slavery question; but we stupid English cannot yet believe that you will ever be a single Union again" Hopes Gray will ask his pupils to look carefully to gradation in sexes in American Hollies; feels he is wrong about some of the Melastomes; it would be good, if a Rhexia grew in a garden, to cover up a plant with net and see if it seeded as well as uncovered plants; thanks Gray for [John Stuart] Mill's pamphlet; "[Sir Joseph] Hooker has been here for 3 days and we had lots of pleasant talks. I am always full of admiration and love for him."

A L S. 4 pp. 20 cm x 13 cm. No. 65 [108]

Apr. 25

Thanks Gray for 2 notes, one of which was "very savage against England. I cannot help feeling that we shall drift into war; what a curse it will be to us anyhow; for you seem to be getting to like war. - I wish I had known when I read the correspondence that Mr. [Charles G.] Loring was your father-in-law. I should have read it, if possible, with still greater interest. We must keep to Science, I fear, for we both seem to be getting to think each other's country['s] conduct worse and worse. But I should like to know whether General [Benjamin Franklin] Butler is in your honest opinion, a bad man." Gray's remarks from [Jeffries] Wyman about the Incas came "very appropriately", for he is at present summing up all facts on this subject and gives facts on both sides; regrets now that he has always intentionally evaded the case of man, but has put in a note on such facts as he has heard of; has sent two copies of his Linum paper; hopes the case may interest Gray, as it has him; Amsinckia turns out with him variable only in length of pistil; forgot to ask about inter-marriage; has heard, from statistical returns, that Ohio has legislated against cousins marrying; asks if this is true; asks if Gray ever formed any theory why, in a spire of leaves, the angles go $1/2$, $1/3$, $2/5$, $3/8$, and not $1/4$ or $1/5$; "This seems to me most marvellous. There must be some explanation." His "good friend" [Hugh] Falconer has been "twitting" him that these angles go by as fixed a law as that of gravity, and never vary; fancies that the packing of organs in the very early bud may cause general alternation in the parts of the flower and consequent interruption in the species; was very sorry to see Falconer's letter in the Athenaeum which was "so violent toward [Sir Charles] Lyell. We have had lately sharp sparring in the Athenaeum. Did you see the article on Heterogeny . . . written I believe, certainly by [Sir Richard] Owen"! it was in Review on [William Benjamin] Carpenter, who seems to have been silly vexed at Owen calling me Carpenter's master; it was like his clever malignity. Under the cloak of a fling at Heterogeny I have sent a letter to Athenaeum in defence of myself, and I take sly advantage to quote Lyell's amended verdict on the Origin [of Species]." Hopes Gray will have time to look at rostellum of Gymnadenia this summer because, in the Botanical Garden at Edinburgh, a Mr. J[ohn] Scott has been experimenting on foreign genera and finds that the rostellum stimulates some kinds to

protrude their tubes, but that these tubes only creep along its surface to the stigma and never penetrate the rostellum; Scott has written asking for Gray's detailed observations; has lately found some Primroses with 3 pistils, but waited so that he could "peep" into the ovarium; put in pollen and afterward found the tubes exerted, and attached to, and apparently penetrating, the ovules, "but never by the micropyle!" Has now no doubt that Gray was perfectly right about fertilization of *Cypripedium*; a friend lent him a plant of *Cypripedium*, he put a very minute bee into the labellum and covered the orifice with wet paper; this precaution was superfluous, for the edges of the orifice of labellum were folded over so the bee could not crawl out; watched the bee crawl out by one of the "windows opposite the anthers" and with his back toward them, against which he firmly pressed it, owing to the "elastic wool" opposite the anthers; "It was pretty to see under lens how the whole thorax and base of wings was smeared with pollen. I put him back into the labellum five times and five times I saw his back smeared. As you know he must pass under the stigma (with its spires directed towards the apex as you describe), for there is no other passage; and as I expected when I cut open the flower I found the stigma well smeared with pollen. It was beautiful."

A L S 6 pp. 25 cm x 17 cm. No. 51

[109]

May 9

Thanks Gray for sending the new part of his "Statistics" which he has taken a lively interest in reading; asks a question about the note at page 387 - to how many genera the 49 species belong - because he wants to know how large the proportion of monotypic genera is, in the disjointed species, to the whole Flora; he began to work out this point in all the cases he met of much disjointed species, but "failed from want of knowledge"; tried, also, to "make out" whether the disjointed species would not on average belong to small families, but again failed for "want of knowledge", though the cases in which he could find out something confirmed his expectation that species having disjointed ranges would belong to small genera; "Your list of the trees made my mouth rather water to know what proportion had sexes in some degree separated, - on which subject I wrote you a ridiculously long letter some weeks ago." Is glad Gray is going to attack introduced plants in the next number; "I may mention that two or three years ago I compared the proportions of the British introduced species to the native Flora and it was in several cases ridiculously close; I then took your first Edition and did the same, but the proportions here were very different; but I think this point would be just worth looking to, for chance of some result." Has just looked at his "old useless" notes and sees he made out in Gray's Manual [of the Botany of the Northern U S.] 206 introduced plants, and of them compositae form 1/8, and so do, as he thought, Gray's indigenous compositae; gives [Hewett Cottrell] Watson's list of proportions of introduced and indigenous Compositae, Umbelliferae, Labiatae and Leguminosae for Britain; "I happened to stumble on these results first, and was inclined to think something of them; but I suppose all was chance or errors." Thinks the standard proportion ought to be for the world in the same latitude, and not the standard of the individual country; "Though why I should trouble you with an old exploded notion of mine, I know not."

A L S. 8 pp. 20 cm x 13 cm. No. 9

[110]

May 18

Returns Appleton's letter; "It is provoking that" Appleton "does not publish 2d Edition of The Origin of Species?]. . . . what a battle you do seem to have been fighting on the Origin of Species. . . . Whatever amount of truth my book may contain, the saving of it . . . will surely be wholly due to a very small body of men. Had it not been for [Sir Charles] Lyell, [Sir Joseph] Hooker, yourself and two or three others, . . . my book and the whole subject would have been mere flash in the pan. For the attacks are now being incessant and very bitter." [Adam] Sedgwick and Prof. Clarke attacked him "savagely at Cambridge Phil[osophical] Soc[iety] but "dear old [John Stevens] Henslow (though he goes but little way with me) stood up manfully for the subject as legitimately within bounds of science, and produced excellent effect." Prof. [John] Phillips has lectured at Cambridge and [Andrew] Murray has read a paper at Royal Society of Edinburgh, both against him; "And thus I could go on for many more!! But the effect on me is that I will buckle on my armour and fight my best. You seem to have done so already in grand style. And I believe Hooker will, as certainly will Lyell and [Thomas Henry] Huxley. But it will be a long fight. By myself I should be powerless." Feels his weak health "acutely", as he cannot work hard; so bitter is the feeling of some that neither [William Henry] Harvey nor [Isaac Bayley] Balfour have ever read Hooker's "Australian Essay"; "Is this not incredibly paltry?" Makes him "savage" to think of the "slighting way in which [Sir Richard] Owen alludes to Hooker's essay in Edinburgh Review; Lyell is working hard at geological history of Man, and it is really marvelous how rapidly curious facts are turning up." Expects Lyell's essay, discussing The Origin of Species, will make a great "commotion"; hopes most sincerely Gray may publish his "stunner of an answer to Bowen, Agassiz, & Co.".

A. L. S. 6 pp. 20 cm x 13 cm. No. 14

[111]

May 30

Down, Beckenham, Kent.

Asks Gray to send seeds of *Nesaea verticillata*; wishes to raise seedlings from "illegitimate unions" to see if seedlings are sterile like true Hybrids and like the "illegitimate offspring" of *Lythrum*; the fact seems "all important" to him.

A L S. 1 p. 20 cm x 13 cm. No. 121

[112]

June 18

Thanks Gray for his "valuable" letters; "I am quite conscious that my speculations run quite beyond the bounds of true science." Has not received the last part of Gray's Silliman ['s Journal of Science] papers, [Sir Joseph] Hooker has, and will lend him his if Gray has not another copy; Gray's remarks on the head will be of real use when he returns to the subject; "A man must be blind not to see how cautious a reasoner you are." Expresses thanks for Gray's remarks on disjoined species; "I daresay I may be quite in error. I saw so much difficulty even theoretically and so much impossibility practically from my ignorance, that I had given up notion till I read your note to your article. I had only just copied out a few striking cases out of Hooker's Himalayan Journal and turned to

[Ernest Gottlieb] Steudel to see what the genera were. The notion was grounded on the belief that disjoined species had suffered much local extinction and therefore . . . I inferred that Genera and Families with very few species (i.e. from Extinction) would be apt (not necessarily always) to have narrow ranges and disjoined ranges. You will not perceive, perhaps, what I am driving at and it is not worth enlarging on, - but I look at Extinction as common cause of small genera and disjoined ranges, and therefore they ought, if they behaved properly and as nature does not lie, to go together! I have not the least doubt that the proportions of British naturalized plants were due to simple chance; but I thought it was just worth mentioning to you; I had from your former Edition of Manual [of the Botany of the Northern U S] quite given up idea." Thanks Gray for telling him about the trees; "now with your facts, and those from Britain, N[ew] Zealand, and Tasmania, I shall have fair material for judging." Thinks Gray's fraction of 95/132 a "striking coincidence"; "I thank you much for your remarks about my crossing notions, to which I may add, I was led by exactly the same idea as yours, viz that the crossing must be one means of eliminating variation, and then I wished to make out how far in animals and vegetables this was possible." Papilionaceous flowers are "almost dead flowers" to him and he cannot experiment, as castration alone often produces sterility; is surprised at what Gray says about Compositae and Gramineae; from what he has seen of the latter they seemed favorable for crossing, and from observations by several scientists on the adhesive pollen he had concluded that the Compositae were likely to be crossed; will be glad to hear of any observations Gray makes on the early fertilization of plants in these 2 orders, as they would save him from "great blunders"; it has seemed to him, from several published remarks on this subject in various genera, that early fertilization has been "inferred" from the early shedding of the pollen, which he thinks is clearly a "false inference"; thinks another cause of the belief of fertilization in the bud is the "not-rare abnormal early maturity of the pistil", as described in Gärtner [?]; has hitherto failed in "meeting with" detailed account of regular and normal impregnation in the bud; Podostemon, Subularia, and Leguminosae under water are the strongest cases against him, as far as he knows; "It is really pretty to see how effectual insects are; a short time ago I found a female Holly 60 measured yards from any other Holly and I cut off some twigs and took by chance 20 stigmas, cut off their tops and put them under microscope; there was pollen on every one and in profusion on most! Weather cloudy and stormy and unfavorable, wind in wrong direction to have brought any." Is delighted to see that "we now absolutely agree" on Fumariaceae, for he never supposed the structure of their flowers to do more than prove an occasional cross, perhaps only once in several generations; "But have you attended to one point. plant a cabbage or radish of 2 distinct varieties moderately near each other, and the proportion of mongrelised plants is immense; indeed sometimes hardly any come from seed raised under such circumstances. I have counted proportions - how the stigma of each flower is surrounded not only by its own stamens. with pollen shed as soon as flowers are open, but by a multitude of other flowers with pollen of the same variety; and yet this mongrelising takes place to an enormous extent I believe a cross is so beneficial, that the pollen of a distant variety

has a perpetual action over the plant's own pollen. You will see the inference which I should draw in regard to Fumariaceae." Will send Gray a copy of his abstract when it is published next winter, though he will not give abstract on facts in regard to crossing, for they are too many.

A. L. S. 12 pp. 20 cm x 13 cm. No. 9 - A [113]

June 30

Down, Beckenham, Kent.

Has received Gray's 2 letters, and has forwarded "the enclosure, with the page inserted", to Nature with the hope the editor will publish it; thanks Gray for sending the *Sarracenia* which he soaked, then stuffed with cotton wool, so that now he has an excellent idea of the plant; "It is as wonderful a case as any Orchid *Drosera* or *Dionaea* and I cannot say more in honour of a plant." Notices that the bud is brightly colored to attract insects; has read with interest Gray's semi-theological review and has got the book; thinks the review will be satisfactory; "The more I reflect on the subject the more perplexed I grow." Asks Gray to observe *Pinguicula*; the margins of its leaves have power of movement when excited by solid objects such as bits of glass or nutritious fluids, but best of all by the 2 stimulants combined; believes the purpose of this movement, although he is not yet sure, is to push flies further onto the leaf, when washed by rain into the narrow channel formed by the naturally involuted edge; [postscript in Darwin's handwriting:] "I do not think that I wrote a bit too strongly about your article on me."

L. S. 3 pp. 20 cm x 13 cm. No. 109 [114]

July 1

Has been "baddish for 2 or 3 weeks", but is better, and means to "amuse" himself by "scribbling" a few lines on Orchids; has received Gray's note on *Platanthera Hookeri* and on diversity of forms of *Cypripedium*; wishes Gray could spare time to write a paper on the latter; *Platanthera Hookeri* is really beautiful and quite a new case; "It is almost laughable the viscid discs getting so far apart that the [word illegible] of the flower has to be divided into two bridal chambers!" Has added a note to the German Edition about this, and a few words on *Cypripedium* on Gray's authority; has written Trübner to send Gray the 6 copies; "I wish you would let me pay for them, but you are so punctilious that you would fling without permission first granted, the money across the Atlantic in the same shameful manner in which you did the 3 L." His son, George, who is an entomologist, has been watching Orchids with "enthusiasm and indomitable patience", and has made out clearly that it is a fly which fertilizes *O[rchis] maculata*; "It was pretty to see the pollinia attached to their spherical eyes, and after the act of depression parallel to and . . . above the proboscides. But the most remarkable case is that of *Hemimium Monorchis*; he has brought me 24 species of very minute Hymenoptera with pollinia attached to all, and always to the same exact spot, viz. to the exterior base of femur of front legs. Nothing has given me such an idea of close adaptation of form of whole flower; the labellum hangs obliquely downwards and the minute insects enter between its edges and the large viscid disc on

one side; and in retreating they hit their prominent femurs against the under side of the disc. So closely fitted is the flower to the insect that my son saw several times insects after entering in a wrong position come out, change their position and re-enter." Is astonished at the success of his book with botanists; [M.J.] Berkeley has received it in London R[evue] ? "rather egregiously", and [Sir Joseph] Hooker writes "strongly"; has done little lately except some crossing of plants; has made a great series of crosses on the peloric flowers of Pelargonium, but doubts whether he will get such good results as he at first hoped with respect to sterility of hybrids; [Charles] Naudin writes he is going to publish on this subject this autumn and his papers give him [Darwin] the idea he does not know what has been done in Germany; finds *Rhexia glandulosa* requires insect agency to set seed, but sees as yet no probability of dimorphism; Gray mentioned some genus in which he found 2 forms like *Primula*, and a third form with both pistil and stamens short; asks whether all flowers on the specimen were thus classified; wishes much to know because *Lythrum* is trimorphic.

A. L. S. 5 pp. 20 cm x 16 cm. No. 69

[115]

July 3

Down, Beckenham, Kent.

Thanks Gray for sending his book, How Plants Behave; "it is a capital idea, capitally executed. - It has in many ways delighted me, and I am even more delighted to hear that you think of publishing . . . on the subject." Asks if Gray can "support" his idea that tendrils become spiral after clasping an object from stimulus from contact, inasmuch as they become spiral when they have clasped nothing; is now correcting proofs of his small book on [The] Expression [of the Emotions in Man and Animals] and when that is done hopes to begin on *Drosera*; is thinking of republishing all his "quasi-botanical papers" with 2 or 3 new ones; hopes it will be in time for Gray; is astonished at Mrs. Gray's "spirit and audacity in going all the way to California, though to be sure this is not much after the Nile. It makes my blood run cold to think of such expeditions." Thanks Gray for the engraving of the ape-man, which he is glad to possess, though he is surprised it was thought worth painting and engraving; he saw in Nature that Dr. [A.S.] Packard was in London, wrote to him, care of the editor, but heard he had left for Paris; whether he ever received his invitation to Down, he does not know.

A. L. S. 4 pp. 20 cm x 13 cm. No. 107

[116]

July 14

His son is ill again with scarlet fever; "With respect to *Pogonia*, it would be a very curiously anomaly, if insects open the anther for nectar. You say nothing about the rostellum; from vanilla I should expect that viscid matter would be forced under lip of anther. Insects ought to be watched at work. . . . Ought not these cases to make one very cautious when one doubts about the uses of all parts." Believes structure of all singular flowers is governed in relation to insects; asks how Gray would have "worked" the North American Orchids; is glad Gray's "harassing" lectures are over; [Sir Joseph]

Hooker is very anxious about Mrs Hooker and has started on a health tour to Switzerland; has heard today of the sudden death of Prof [H.G.] Bronn, just as he had finished translating his [Darwin's] Orchid book [The Various Contrivances by which Orchids are Fertilized by Insects]; has just made an observation which has surprised him and which he will have to repeat several times to prove its accuracy; he stated in his Primula paper that the long-styled form of *Linna grandiflorum* was utterly sterile with its own pollen; has lately been putting pollen of the 2 forms on the division of the stigma of the same flower, and it strikes him as "truly wonderful" that the stigma "distinguishes" the pollen and is penetrated by the tubes of the one and not of that of the other, nor are the tubes exerted; the stigma of the one form acts on and is "acted by" pollen which produces not the best effect on the stigma of the other form; concludes that the 2 forms of this one species may be said to be generically distinct.

A. L. S. 4 pp. 20 cm x 16 cm. No. 70 [117]

July 16

Down, Beckenham, Kent.

Asks if Gray will allow his two sons, George and Francis, to call on him when they come to Boston toward the latter part of their visit to the United States; they already have a good many letters of introduction, but if Gray should know of any "pleasant and good natured persons" to whom he could give them letters of introduction he would appreciate it; has received a delightful notice of his Descent of Man which must have been written by Gray

A. L. S. 3 pp. 20 cm x 13 cm. No. 101 [118]

July 21

Torquay, Devon.

Is "at the sea" for his daughter's sake; thanks Gray for 5 hybrid pamphlets and for his "pleasant and useful" note; is now writing his Orchid paper; is glad to hear what Gray says about *Cypripedium* and that he will look at *Spiranthes*; suggests he look at a flower just opened and not visited by a moth; hopes [Sir Joseph] Hooker will send *Arethusa*; is astonished at *Drosera filiformis* "misbehaving"; his own experiments were all tried in the late summer; asks if Gray expected rapid or visible movement; the minutest atom of raw meat placed on a single gland which is covered with viscid secretion shows motion best, but if the gland is dry no movement takes place; hopes to finish observations on *Drosera* this autumn; from what he has seen of *Cypripedium* insigne his difficulty is to know what induces an insect to place its head so far back in the flower; asks Gray to watch for a little time for the bare chance of an insect's visit; [makes diagrams of 2 forms of *Primula*]; would be particularly obliged, if Gray knows of analogous cases, if he would name them and allow him [Darwin] to quote him, and if they are hardy plants and good seeders he would experiment on them; as no one has aided the subject of natural selection and the knowledge of his book so much as Gray, he must tell him something pleasant after the many attacks for neglecting "Induction", "Baconian philosophy", and other subjects - that John Stuart Mill, held in England as the "highest authority on such subjects", said lately to a friend that the "reasoning throughout is in the most exact accordance with the strict principles of logic",

and the "method of investigation . . . followed is the only one proper to such a subject". Is glad of the state of Gray's country; never doubted the North would conquer the South after carefully reading [Frederick Law] Olmsted's last book; wonders what is to follow; cannot believe, from letters in the Times, the South would ever have "fellow-feeling" enough with the North to allow a government in common; asks if the North could endure a Southern president; considers the whole affair a great misfortune in the progress of the world, but would not regret it so much if he could persuade himself that slavery would be annihilated; "your president [Abraham Lincoln] does not even mention the word in his address. I sometimes wish the contest would grow so desperate that the North would be led to declare freedom as a diversion against the enemy. In 50 or 100 years your posterity would bless the act." Hooker is "overworking himself", and he seldom hears from him. [Includes diagrams of 2 forms of Thyme].

A. L. S. 3 pp. 18 cm x 13 cm. No. 61

[119]

July 21

Down, Farnborough, Kent.

Thanks Gray for acceding to his request about marking the close species; does not quite understand from [Sir Joseph] Hooker's note whether the sheets have arrived, but fancies not; Hooker read over Gray's letter and seems to have been "as much struck with it" as he has been; it shows in a most "striking manner" the geographical affinities of species and the difficulty of ascertaining what are species; "Your discussion on connecting and separating forms seems to me so philosophical, that I much hope that someday you will be as good as your word and write an 'Essay on Species'. I hope, also, before end of year to hear that you have found time to write on the geographical distribution of the United States plants; and if my letter caused you to do this some year or two before you otherwise would have done it, I shall congratulate myself in private, at having done good Botanical work." Some of his immersed seeds of radishes, beets, capsicum, oats, rhubarb, lettuce, carrots, celery, and onions have come up after 82 to 85 days' immersion.

A. L. S. 4 pp. 20 cm x 13 cm. No. 3

[120]

Aug. 15

Now that he can do nothing, he "maunders" over old subjects, and Gray's approbation of his "Climbing Paper" gives him "great satisfaction"; made his observations when he could do nothing else and much enjoyed it, but always doubted whether they were worth publishing; "I demur to it not being necessary to explain in detail about the spires in caught tendrils running in opposite directions; for the fact for a long time confounded me and I have found it difficult enough to explain the cause to 2 or 3 persons. One botanist has published that he could detect a difference of structure in the tendrils at the points of reversal of the spire!" Thanks Gray for *Specularia* seed; "We continue to be deeply interested in American affairs; indeed I care for nothing else in the Times. How egregiously wrong we English were in thinking that you could not hold the South after conquering it. How well I remember thinking that Slavery would flourish for centuries in your Southern States. My women read much about [it] to me."

Suggests books for Gray to read; this is a longer note than he has written for weeks; is trying a system of cure - "eating very little of anything, and that almost exclusively bread and meat."

A. L. S. 4 pp. 20 cm x 13 cm. No. 87 [121]

Aug. 24

Down, Farnborough, Kent.

Has forwarded Gray's recent letter to [Sir Joseph] Hooker, who has since started with Dr. [John] Lindley's eldest son, on a tour in Germany; is sorry Gray had such trouble about the *Dytiscus*; appreciates list of close species Gray sent; has read that species of larger genera are more closely related to each other than are species of smaller genera; a very good entomologist and Hooker and [George] Bentham do not believe in this, but several facts make him think there might be some truth in it; concluded from Gray's list and Manual of the Botany of the Northern U. S. that where many organic forms are allied, making what is called a genus, some of them are apt to be more closely allied than are the species in the smaller genera; arrives at the same conclusion from H[ewett] C[ottrell] Watson's marked list of British Flora; Hooker thinks very well of [Alphonse Louis Pierre] de Candolle's new work on geographic distribution [*Géographie botanique raisonnée*, 1855]; "I have met several (chiefly amongst animals) cases, so many that I can hardly think it purely accidental, in which, when the species of a genus differed in some organ or part, which is usually constant in the species of the same genus, then that one or more of the species individually varied in some degree in this same organ or character." [Gives examples in plant and animal kingdoms].

A. L. S. 6 pp. 20 cm x 13 cm. No. 10 [122]

Sep. 4

Cliff Cottage, Bournemouth, Kent.

"My poor Boy (whose waxen face blushed up to the eyes at the thought of writing to a live Professor) has this day made a marked step and has taken several walks of a few hundred yards; and my wife is recovering well and her skin well peeling. - We have taken two houses here, so I hope and trust this dreadful fever will not spread." Is glad Gray intends to publish some separate notes on Orchids which he has so "capitally worked out"; is pleased to hear about *Goodyera* and *Gymnadia tridentata*; Gray's account makes him think the latter in like *Bonatea speciosa*, "and often and often have I speculated what on earth could be meaning of its wonderful horn-like stigmas and projecting anthers." Suspects its structure may have been arrived at by a process somewhat analogous to that which apparently has produced the "wondrous nectary of *Angraecum sesquipedale*"; "It would appear that self-fertilization is commoner than I thought; since publishing I have found that *Neottia nidus-avis* fertilizes itself, if insects fail to do the job." Thanks Gray for *Houstonia* seed; is glad to hear, but disappointed, about the *Specularia* pollen-tubes; "I cannot resist sending you a diagram about *Lythrum*." Finds it has 3 kinds of stigmas and 3 kinds of pollen, with the stamens of the same height on 2 of the 3 forms producing the same sort of pollen, and he cannot doubt they are fitted to fertilize the stigma

of that height; "I conclude so from watching the Bees; but hope to prove it by my crosses. So that we have 3 hermaphrodite forms each depending on half the stamens of either one of the two other forms " This strikes him as a "very curious" case; the 3 forms co-exist in about equal numbers; believes that [John] Lindley, in Vegetable Kingdom, describes certain structures of flowers wrongly - the so-called calyx, with its 12 bundles of spiral sepals, appears to consist of 6 narrow sepals and 6 modified petals, all cohering, and the colored petals belong to an inner whorl and are modified stamens; asks if Gray can tell him of any flowers with fertile anthers of different colors; believes this would be a pretty sure guide to dimorphism or trimorphism; "All my semi-botanical work, as you know, has been connected with insects, and now I am almost sure (but I find it a disgusting truth that with me first observations are generally all a blunder) that flowers have led me to a curious little discovery with respect to the best-known insect in the world, the Hive Bee. I saw the other day to my dismay (see Origin [of Species]) Hive Bees sucking the common red clover, but it was a second crop, which I am told produces shorter flowers; but many of the Bees never attempted this, but always inserted their heads between the flowers and sucked at holes bitten through the corolla. - The same bee always followed the same practice. And apparently those which suck at the mouth of the flower have a longer proboscis than the other bees, which suck through the holes. . . . Since writing the above by Jove I have found I have as usual at first blundered about the proboscis; but if you had seen the Bees, the blunder was almost excusable." [Encloses diagrams of long-styled, mid-styled, and short-styled *Lythrum Salicaria*].

A. L. S. 7 pp. 20 cm x 13 cm. No. 68

[123]

Sep. 16

Asks for information on cases of dimorphism like that of *Primula*; [makes diagram of anthers and stigmas of 2 forms of *Primula*]; the individual plants are divided into 2 sets or bodies, like males and females in quadrupeds, but both in *Primula* are hermaphrodite; would be glad to know of other analogous cases; asks if the 2 forms are ever borne on the same plant; Thyme is different, as the one form is simply female; [makes diagram of anthers and stigmas of *Linum*]; is almost sure pollen of one kind of *Linum* is sterile on its own stigma but good on the stigma of another plant, and pollen of another kind of *Linum* is good on its own stigma; hopes further study of this subject will throw light on *Hybridistum*; has some seeds of *Primula* raised in scanty numbers from stigmas fertilized by homomorphous union - by pollen from plant of same form; asks if Gray has looked at *Spiranthes*; his species is different; he watched bees fertilize them at Torquay; has almost finished his paper on Orchids and will then go back to work on cocks and hens, fowls, and rabbits; has just been looking at *Dionea* in aid of his *Drosera* work; "How curious it is to see a fly caught and how beautiful are the adaptations compared with *Drosera*."

A. L. S. 4 pp. 20 cm x 16 cm. No. 73

[124]

Oct. 16

Expresses thanks for 2 letters and some pages of Silliman['s Journal of Science] with several notices he was glad to see; Lythrum Salicaria is "coming out so clear" he does not care much for the other species, but will be very glad of seed of Nesaea; is disappointed with "those odious Melastomatads", yet feels "sure there is something very curious to be made out about them"; has looked at [J Trimble] Rothrock's observations on Houstonia; they are "capital" in some respects, but not sufficient about reciprocal Latitudination; with a few experiments of his own he feels he could give all the facts; is at home again and working steadily on Variation under Domestication [The Variation of Animals and Plants under Domestication], but "pottering over plants is much better sport"; worked at Bournemouth a bit at "my old friend Drosera", testing all sorts of fluids that do not act on ordinary organic compounds but do act on nervous systems of animals; declares he is coming to the conclusion that some plants must have something closely analogous to nervous matter, as acetate of strychnine stopped all movement and acetate of morphia greatly dulled and retarded movement; is interested in such a number of natural hybrids being found between 2 species of Verbascum and linking V[erbasum] Thapsus and Lychnitis closely together; [Sir Joseph] Hooker was horrified when he told him this; thanks Gray for sending an article from the Daily News which was "read aloud in family conclave"; verdict was that the North was fully justified for going to war with the South, but that as soon as it was plain there was no majority in the South for reunion, the North might have made peace and agreed to a "divorce" after the victories in Kentucky and Tennessee; thinks it is curious there is belief in the United States in annexation of the South, while in England it is the almost universal opinion that this is utterly impossible; deplors the war and says that already it has fostered widespread feeling in favor of aristocracy and monarchism; "no one in England will speak for years in favor of the people governing themselves"; asks Gray not to be "indignant", and not let Mrs. Gray be "more indignant than she can help"; inquires about a Mr. Flag, of New York, who has cultivated wild Maize.

A L. S. 6 pp. 20 cm x 16 cm. No. 81

[125]

Oct. 19

"I received yesterday your article on Climbers and it has pleased me in an extraordinary and even silly manner. You pay me a superb compliment, and as I have just said to my wife I think my friends must perceive that I like praise, they give me such hearty doses. I always admire your skill in reviews or abstracts, and you have done this article excellently and given the whole essence of my paper. I dare say you are right about the climbing roses; I never thought about spring shoots behaving differently from others." Has had a letter from F[ritz] Müller, in S. Brazil, who has been "stirred up" to observe climbers and gives him some curious cases of Branch-climbers in which branches are converted into tendrils and then continue to grow and throw out leaves and new branches, after which they lose their tendril character; his health is better, has not vomited for about 5 weeks, due to not having eaten anything but toast and meat for the last 2 months, but cannot recover mental strength and does no regular work; has had some flowers crossed for him this summer and has lately been counting the seeds;

Gray might like to know that *Mitchella* "behaves" exactly like the Cowslip; asks if he told Gray a year or two ago that *Pulmonaria* offers a curious case - the long-styled form being absolutely sterile with its own pollen, while the short-styled is almost perfect with its own pollen; has also ascertained that plants raised from dimorphic species fertilized by their own pollen, are themselves generally sterile and are often dwarfs, so that they offer the closest analogy with Hybrids - the first cross and the product both being more or less sterile; this seems to him a very curious fact; does not know when he will be able to publish any of these results, for he has resolved whenever able to do anything, to publish his next book; has not heard very lately from [Sir Joseph] Hooker who returns tomorrow to Kew; Hooker's illness has been very serious; "To me the loss of his correspondence has been very great." His [Darwin's] wife has read aloud to him Stephens' two books on Central America; "What a remarkably pleasant writer he is! and how singularly deficient in the spirit of a naturalist. Who is he and is he still alive? You are enough to stimulate a dead man to work." [2 pp. are in another handwriting].

A. L. S. 4 pp. 20 cm x 13 cm. No. 93

[126]

Oct. 19

Is pleased Prof. Henry goes so far with the Darwinian theories; does not think the Atlantic [Monthly] article will be made to appear as if from Gray; feels it must be now either rejected or printed, as so much time has elapsed; is thinking of the larger scheme of getting all 3 articles published with Gray's name, but is a poor judge of such things himself because of mixing so little with people, and [Sir Joseph] Hooker is away and [Thomas Henry] Huxley is in distress; is writing [Sir Charles] Lyell, who admires Gray's articles, and who is a good judge in all publishing affairs, and if he thinks there is any chance of a sale he [Darwin] will consult Murray; he will tell Murray of a book and 2 other brochures, "all poor", which have appeared against The Origin [of Species]; thinks a pamphlet on their own side might sell; feels it would be indispensable to have Gray's name and titles on the title page, and very advisable to have some remark on the title showing its bearing on Natural Theology or Design; is afraid it is "ten to one" against Gray's articles succeeding; asks Gray to send a title, subject to Murray's approval; fears there is no chance of Gray's having time to add some criticisms on [William] Hopkins; all 3 articles together might be too long; seem much too good to be almost lost in a periodical; "Murray says the Origin goes on selling well. I had a letter a week ago from [George Henry Kendrick] Thwaites of Ceylon; he was at first much opposed to us, " and now approves; "Even [William Henry] Harvey . . . is not nearly so savage against me as . . . when he published his foolish pamphlet. Such cases give me much confidence that Natural Selection is not very far from truth." Thinks it might be "worth while" for Gray to send title and say whether he would add any criticisms, as in this case he would delay publication, "if it is ever to take place."

A. L. S. 4 pp. 21 cm x 16 cm. No. 32

[127]

Nov. 4

[Down], Beckenham, [Kent].

"I send by this post rest of sheets, some not pressed but corrected. Title & Index shall be sent hereafter."

A. Postal card. S. 1 p. 8 cm x 6 cm No no. [128]

Nov. 18

Is anxious for information which possibly may be gained in Southern United States; old writers often insist that differences of constitution go with complexion, and wishes to know whether there is any truth in this; "It has occurred to me that liability to such disease as yellow-fever would answer my question in the best possible way." Asks if Gray knows of any one of a "scientific mind" to whom to apply to ask whether any observations have ever been made or published on Europeans, without any cross with negro blood, of dark complexion and black hair, being more liable, or less, to be attacked with yellow fever than persons of light complexion; he has just published a little notice in Gardeners' Chronicle on the fertilization of Leguminous plants which rather bears on "our" Funariaceus description.

A L S. 4 pp. 20 cm x 13 cm. No. 19 [129]

Nov. 23

Thanks Gray for stamps sent his son, Leonard, and for Nesaea seeds; has not received the first notice on the Orchid book [The Various Contrivances by which Orchids are Fertilized by Insects], although Gray writes he has sent the second one to Silliman's Journal of Science; an article on "Supernaturalists" by the Duke of Argyll, in the last Edinburgh Review, which brings in Orchids, is "clever", but he does not see that it really reviews any of the difficulties of Theology; considers a review on Max Müller - on the origin of language - by his [Darwin's] brother-in-law, H[ensleigh] Wedgwood, and his daughter, in the last Macmillan's Magazine, and [Henry Walter] Bates' paper on "Butterflies of Amazonia" are "well worth reading"; no one has brought so clearly before his mind as has Bates the process of segregation of varieties into species; [Sir Charles] Lyell's book "is not yet out", but Murray sold 4,000 copies recently at auction; has nearly finished a long chapter on the simple facts of the variation of a few cultivated plants and would be very much obliged if Gray would tell him whether the fruit of wild *Fragaria virginiana* is much larger than that of *Fragaria vesca*, whether he knows anything of the *Fragaria grandiflora*, and how far south the *Fragaria virginiana* ranges; believes his son, Horace, is a "prophetic type of future naturalists, as Agassiz would say", because he expresses "pity for the poor people who formerly believed in" no variation in species of animals and plants. "Read in Times today the great news of [George B.] McClellan's dismissal from the army. Good God what will be the end of all?"

A L S 3 pp. 26 cm x 21 cm. No. 49 [130]

Nov. 24

Gray's facts on Northern Range astonish him and will be pre-eminently useful for his purpose; is delighted Gray intends to attack naturalized plants; "The last sentence in your letter at first surprised me and troubled me, . . . viz. 'that a considerable part of our Alpine plants are not known in our Arctic continental regions'.

I did not perceive that you had added but are connected with Scandinavia through Labrador, etc. - And this made me happy again. But looking at the Globe is it not rather a forced expression to exclude Labrador from your 'Arctic continental regions'? . . . You may confound some one else as you did me. . . . You say that only method to make you work is 'to show you the way'. . . . The very best workman sees blemishes in his work which other and poorer workmen cannot ever perceive. . . . If I had written a paper half as good as yours how conceited I should have been!" He cannot "get over" his surprise at Gray's naturalized and agrarian plants not being variable, and asks Gray to please keep this point in mind.

A. L. S. 4 pp. 20 cm x 13 cm. No. 5 [151]

Dec. 11

Is hastening to say he will gladly pay half of the cost of Gray's pamphlet, will distribute some copies, and leave others in the agents' hands; will put a few advertisements in newspapers, and see what he can do to get it noticed in a few periodicals, "but I am a bad man for that, as I live so retired." Must hear, somehow, to whom the consignment of copies was sent, and Gray can tell exact sum to repay him; fears it will be impossible to get the pamphlet known in England so as to sell many; if Gray, upon reflection, would not like to go to the expense, he must not consider himself bound; "Just to exemplify use of your pamphlet, Bishop of London was asking [Sir Charles] Lyell what he thought of the Review in Quarterly [Journal of Science], and Lyell's answer was, Read 'Asa Gray's in Atlantic [Monthly]'." Will write [Thomas Henry] Huxley about [Chauncey] Wright's article, but has "no hope", as Huxley said a "good strong anti-Darwinian article as what he wanted." Knows nothing about payment, but will inquire and let Gray know; heartily acquiesces either in better or cheaper editions; is sure [Sir Joseph] Hooker will help in getting pamphlet known.

A. L. S. 4 pp. 20 cm x 13 cm. No. 33 [152]

Dec. 14

Sends Gray a note from [Thomas Henry] Huxley which says Huxley will be glad of [Chauncey] Wright's article, if approved of; he [Darwin] believes no other publication would accept it, for all have treated of The Origin [of Species]; feels sure the [Natural History] Review will have "great merit" and "be worthy" of Wright's article; thinks it would be a "great pity" if it were not published, for, as Gray has said, it is so good; calls attention to the fact that Huxley asks whether Gray would ever contribute to his Review [Natural History Review] but fears Gray is pledged to Silliman's [Journal of Science]; is anxious to hear Gray's final decision on the manner of publishing his 3 articles; still thinks No. 2 the best, though he believes most readers would prefer No. 3; would like to see [William Henry] Harvey's letter, for he has had much pleasant correspondence with him; is returning the Tickner and Fields letters, as Gray might wish to refer to them; there is a "nice, but too flattering", article on The Origin by a young man of whom many think most highly.

A. L. S. 2 pp. 20 cm x 13 cm. No. 39 [153]

[?] 23

Letter will be brief because his daughter is very ill; is glad Gray has decided to publish, and insists that he [Darwin] bear whole risk of loss; [encloses list of names of those to whom he would distribute copies]; has just received a letter from Prof. [Jeffries] Wyman; has not read it, but sees it is profoundly interesting;

For Reviews

Annals [and Magazine] of Natural History
Edin[burgh] New Philosophical Journal
Athenaeum

Saturday Review [of Politics, Literature, Science and Art]

Gardeners' Chronicle

Natural History Review

[illegible]

Geological Society

Royal Society

Linnaean Society

Athenaeum Club

Flemian (a Review) [?]

Private

S[amuel] P[ickworth] Woodward

H[ewett] C[ottrell] Watson

[Sir Joseph] Hooker

[George] Bentham

[William Benjamin] Carpenter

[Thomas Henry] Huxley

[Sir John William] Lubbock

Self

[John Stevens] Henslow

[Adam] Sedgwick

[William] Hopkins

B[ishop] of Oxford

[Samuel Wilberforce]

[Sir John Frederick William]

Herschel

Whendall [?]

[Sir Charles] Lyell

C[harles] Kingsley

Barbary

R[obert] Chambers

[Thomas Vernon] Wollaston

Sir H[enry] Holland

[Hugh] Falconer

[William Henry] Harvey

Mrs. Darrock [?]

L[eonard] Horner

[John] Phillips

H[enry] D[arwin] Rogers

Trottnick [?]

Al[phonse Louis Pierre]

de Candolle

Os[wald] Heer

Thuran "of Calcutta"

[Edward] Blythe "of do."

[George Henry Kendrick]

Thwaites "of Ceylon"

A. L. S. 2 pp. 20 cm x 13 cm. No. 12

[134]

n.d.

[Place of writing unknown; first page missing].

Appletons have sent him £ 50 for Variation [of Animals and Plants] under Domestication which he thinks "very handsome" and which he owes to Gray; has not heard from [Sir Joseph] Hooker, but read in the [Gardeners'] Chronicle that when he was elected President for next Congress he was received with great applause; has seen hardly a soul for a long time "except the [Thomas Henry] Huxley's and two detachments of

[Prof. Charles Eliot] Nortons. I then verified a grand generalization, which I once propounded to you, that all persons from the U[nited] States are perfectly charming. . . . I often think with pleasure of your visit here." Asks Gray to observe whether the beards of Germans, when differing in tint from hair of the head, are of a lighter or redder tint, and if they often differ in tint.

A. L. S. 2 pp. 20 cm x 13 cm. Incomplete. 1st p. or pp. missing. No no. [135]

n.d. Is glad Gray will look at Rubiaceae and hopes he will find time to make a few experiments; thanks him for notes about his Hollies and hopes he will "look a little to them. There is to me incomparably more interest in observing than in writing; but I feel quite guilty in trespassing on these subjects, and not sticking to varieties of the confounded Cocks, Hens and Ducks." He hears [Sir Charles] Lyell is "savage" at him; knows he will not be able to resist Linum next summer; "What you say about our keeping in our intrenchments and firing long shots about Design has made me laugh. I suspect I am more cowardly than you, as I ought to be, as I do not feel sure of my ground. Here is my answering long shot about the cream-jug-nose: I should believe it to have been designed . . . until I saw a way of its being formed without design, and at the same time saw in its whole structure . . . evidence, of its having been produced in a quite distinct manner, i.e. by descent from another cream-jug whose nose possessed, perhaps some quite distinct use. When I think of my beloved Orchids, with rudiments of five anthers, with one pistil converted into a rostellum, with all the cohesion of parts, it really seems to me incredibly monstrous to look at an Orchid as created as we now see it. Every part reveals modification on modification." Will send Gray his Orchid opusculum.

A. L. S. 2 pp. 25 cm x 21 cm. Incomplete. pp. 1-4 missing. No. 51-A [136]

n.d. "Mosely is president of Section D at Montreal." [Letter and signature in another handwriting].
1 p. 20 cm x 13 cm. No no. [137]

1860
Feb. 20 Asa GRAY, [Cambridge, Mass.]. To Charles DARWIN,
[Down, Bromley, Kent, England?].

Encloses letter received from D. Appleton & Co.; is sending Appleton sheets of Edition 2 [of Origin of Species] with Darwin's additions; has delivered to Appleton the "Historical Paper" he promised them, "trusting to their promise of 5 prints and to their honor for more if they are not molested by reprinters which we shall keep off. The offer of check for 50 £ (which I might send to Mrs. Darwin for pin-money since you scorn it) tempts me, but I think it wise to wait & hope for more." Has mailed a copy of his review; has sent it to Agassiz who is "childishly apt to be offended at any opposition, but I have, as you see, been very careful to avoid all cause of personal offence."

A. L. S. No. 37 [138]

Enclosure: Letter from D. Appleton & Co., New York, N. Y., Feb. 17, 1860, to [Asa Gray], Cambridge, Mass., "We can't say what we will do respecting the notes & additions till we see them, but we shall be anxious to make our edition conform to any future English Edition"; no one can hold an American copyright unless he be a citizen of the United States; propose to pay 5% on retail price, as Gray has suggested, "as there is no reason why a work without any legal rights should pay the same as one that is secured by law"; desire to act liberally; regret there is no protection for foreign authors, "think it a monstrous shame", but must take things as they exist; are willing to send Darwin 50¢, "and very likely that would be as much as he could receive by the sales."

Enclosures: $2\frac{1}{4}$ pp. of notes and corrections [on Origin of Species?].

Enclosure: Printed form filled in by hand, statement, from [D. Appleton & Co., N.Y.] to Asa Gray "for Mr. Darwin", of sale of Origin of Species to May 1, 1860, shows "1750 sold, at 5% on \$1.25 - \$109.37 L22.00" . .



BIOGRAPHICAL NOTES

- Agassiz, Alexander, 1835-1910. Son of Louis; American naturalist and capitalist; developed Calumet and Hecla copper mines, bringing him great wealth which he devoted to zoological research and to endowment of Harvard Museum of Comparative Zoology, Cambridge, Mass.; author of zoological works, mostly on deep sea animals.
- Agassiz, Louis Jean Rodolphe, 1807-73. Swiss-American naturalist; professor of natural history, Neufchatel and Harvard; founder of Harvard Museum of Comparative Zoology, Cambridge, Mass.; author of zoological works.
- Argyll, Duke of, 1823-1900. English author and statesman; member of Gladstone's cabinet; liberalist; defender of theism; author of religious, political, and scientific works.
- Baer, Karl Ernst von, 1792-1876. Russian naturalist; founder of modern embryology; professor at University of Königsberg; author of scientific works.
- Bates, Henry Walter, 1825-92. British naturalist and explorer; gave plausible explanation for protective coloring of animals and insects; explored upper Amazon, bringing back some 8,000 new species; Alfred Russell Wallace with him part of time; portion of his valuable collections in British Museum; his rare beetles bought by Renee Oberthun, of Rennes, France; assistant secretary of Royal Geographical Society; author of scientific works.
- Bentham, George, 1800-1884. English botanist; author of handbooks on Flora and a work with Hooker.
- Berkeley, Miles Joseph, 1803-89. English botanist and authority on fungi and plant pathology; contributed much to knowledge of fungus pests of crop plants.
- Bessey, Charles Edwin, 1845-1915. American botanist; professor of botany, Iowa State College and University of Nebraska; president of Society for Promotion of Agricultural Science; president of Nebraska Academy of Sciences; acting chancellor of University of Nebraska; Fellow of American Association for Advancement of Science; among first to use laboratory methods in teaching botany; author of textbooks and miscellaneous articles.
- Bishop of Oxford, see Wilberforce.
- Blyth, Edward, 1810-73. English zoologist.
- Boott, Francis, 1792-1863. English physician.
- Bowen, Francis, 1811-90. American philosopher; professor of political economy, and Alvord professor of natural religion, moral philosophy and civil polity, Harvard; owned and edited North American Review; author of many works on history, biography, political science, logic, religion, and philosophy.

Biographical Notes

- Brace, Charles Loring, 1826-90. American philanthropist; social worker in missions at Five Points and on Blackwells Island, N. Y.; founded Children's Aid Society and the first newsboys' lodging house; traveled Europe, studying reform schools and prisons; author of several works.
- Bridgman, Laura, 1829-89. American blind mute; attended Perkins Institute for the Blind, at Boston, Mass., where Dr. Samuel G. Howe undertook her education; learned to read raised letters, interchange thoughts with others, and to associate words with objects; studied geography, history, and algebra; played piano, and sewed; received and answered letters from all parts of the world; became skillful teacher of the blind and deaf and dumb; thought deeply about religious matters, and reasoned with discrimination.
- Bronn, Heinrich George, 1800-1862. German geologist; professor of natural history, Heidelberg; author of scientific works; translated into German The Various Contrivances by which Orchids are Fertilized by Insects, by Darwin.
- Brown, Robert, 1773-1858. Scottish botanist; naturalist on Flinders' surveying expedition to Australia; discoverer of Brownian Movement - rapid vibratory movements of minute particles suspended in fluid; author of first British botanical work to treat of plant arrangement in philosophical manner.
- Buckle, Henry Thomas, 1821-62. English historian; chief work, History of Civilization, of which only two volumes were completed; died, while traveling, in Damascus.
- Burchell, William John, 1782-1863. English explorer and scientist; schoolmaster at St. Helena; explored Africa, where he collected 63,000 natural objects and much astronomical and meteorological material; explored Brazil, many animal and plant species, discovered by him, bear his name.
- Butler, Benjamin Franklin, 1818-93. American lawyer, soldier, and statesman; served in both houses of Massachusetts state legislature; major general in Union Army during Civil War in United States; governor of Massachusetts.
- Butler, Samuel, 1835-1902. English philosopher, artist, archaeologist, and miscellaneous writer, exhibited at Royal Academy; wrote several books controversial to theories of Darwin, his friend; his one novel was posthumously published.
- Cairns, John Elliot, 1823-75. Irish economist; his Slave Power (1862), a defense of the North in Civil War in United States, made great impression in England; theoretical economist of school of John Stuart Mill; author of many works on economics.
- Candolle, Alphonse Louis Pierre Pyramus de, 1806-93. Son of Augustine, Swiss botanist, whose work he continued.
- Carlyle, Thomas, 1795-1881. Scottish essayist, philosopher, and historian; author of many works, mostly of a historical character.

- Carpenter, William Benjamin, 1813-85. English naturalist, physiologist, writer, and editor; lecturer and professor at various institutions; made three voyages to North Atlantic and Mediterranean in study of biology; author of many works on physiology.
- Chambers, Robert, 1802-71. Scottish publisher and author of many works.
- Colenso, John William, 1814-83. Bishop of Natal, South Africa; mastered Zulu language, prepared grammar and dictionary, and translated Prayer-book and part of Bible; became convinced of improbability of many statements in Bible; deposed from his See as result of The Pentateuch and the Book of Joshua Critically Examined (7 parts, 1862-79); visited England, conferred with Archbishop of Canterbury, and pleaded cause of dispossessed Zulu chief; author of works in algebra and arithmetic, and many of religious character.
- Cooke, Josiah Parsons, 1827-94. American chemist and mineralogist; professor of chemistry and mineralogy, and founder and director of chemical laboratory, Harvard; first instructor in America to use laboratory in undergraduate course; worked out atomic weight of antimony.
- Cooper, Susan Fenimore, 1815-94. Daughter of James Fenimore; American miscellaneous writer.
- Dana, James Dwight, 1813-95. American naturalist; scientific observer on United States exploring expedition, under Charles Wilkes, visiting Antarctic and Pacific oceans; associate editor of American Journal of Science; professor of natural history and geology, Yale; author of several works.
- Darwin, Erasmus, 1731-1802. Grandfather of Charles Robert Darwin; English physician and poet; evolutionary theories expressed in Zoonomia.
- Darwin, Sir Francis, 1846-1925. Son of Charles Robert Darwin; English botanist; assistant to his father; reader in botany, Cambridge, Eng.; foreign secretary of Royal Society; president of British Association; edited Life and Letters of Charles Darwin and More Letters of Charles Darwin; author of botanical works.
- Darwin, Sir George Howard, 1845-1912. Son of Charles Robert Darwin; English astronomer and mathematician; Plumian professor of astronomy and experimental philosophy, Cambridge, Eng.; president of British Association; recognized as authority on cosmogony; author of several works.
- Darwin, Sir Horace, 1851-1928. Son of Charles Robert Darwin; English civil engineer; chairman of Cambridge Scientific Instrument Co.; Fellow of Royal Society.
- Darwin, Leonard, 1850-? Son of Charles Robert Darwin; English economist and eugenist; major in Royal engineers.

Biographical Notes

- Daubeny, Charles Giles Bridle, 1795-1867. English chemist and botanist; professor of chemistry and botany, Oxford; author of works on volcanoes and atomic theory.
- Draper, John William, 1811-82. American chemist, physiologist, and writer; born in England; professor of chemistry and physiology, Hampton-Sydney College, Virginia, and University of City of New York, made important discoveries in spectrum analysis and photography, author of many works of scientific and historical character.
- Du Bois-Reymond, Emil Heinrich, 1818-96. Physiologist, born in Berlin, Germany, son of French parents; results of years of research, including theory of electrical nature of muscle action, were published, developed methods and apparatus for study of physiology.
- Falconer, Hugh, 1803-65. English palaeontologist, botanist, and physician to East Indian Company in Bengal; superintendent of botanical gardens in India; steadily carried on research work which resulted in remarkable discoveries of fossil Fauna and Flora; continued researches in Europe after retirement; edited nine parts of an illustrated work.
- Forbes, Edward, 1815-54. English zoologist; research work in ocean life advanced science of palaeontology; professor of botany, King's College, London; curator of Geological Society museum; palaeontologist with British Geological Survey; vacations spent in deep sea dredging, making notable discoveries in fossils; his valuable collections are in College Museum, Edinburgh, where he was professor of natural history; author of many scientific works.
- Gärtner, Rudolf, 1817-80. German publisher.
- Gray, John Edward, 1800-1875. English naturalist; Fellow of Royal Society.
- Harvey, William Henry, 1811-66. Irish botanist; authority on algae; spent several years in South Africa; author of several scientific works.
- Houghton, Samuel, 1821-97. Irish scientist. Fellow of Royal Society.
- Heer, Oswald, 1809-83. Swiss geologist and naturalist; pioneer in palaeobotany; distinguished for researches on Miocene Flora; professor of botany, University of Zurich; directed attention to Tertiary plants and insects of Switzerland; director of Botanical Garden, Zurich; author of many works.
- Henslow, John Stevens, 1796-1861. English botanist and geologist; during tour in Isle of Wight, with Adam Sedgwick, developed interest in geology; professor of mineralogy and botany, Cambridge, Eng., took holy orders; introduced Darwin to Capt Fitzroy, of H. M. S. Beagle, author of many works.
- Herschel, Sir John Frederick William, 1792-1871. Son of Sir William; English astronomer; spent four years at Cape of Good Hope, charting southern heavens; president of Royal Astronomical Society; made valuable contributions to development of photography; author of several works on astronomy.

- Hochstetter, Ferdinand Christian von, 1829-84. Austrian geologist.
- Holland, Sir Henry, 1788-1873. English physician; Fellow of Royal Society; physician in ordinary to Queen Victoria and Prince Albert; author of many scientific works.
- Hooker, Sir Joseph Dalton, 1817-1911. English botanist and traveler; assistant surgeon on Sir James Ross' Antarctic expedition; journeyed to India, Palestine, Morocco, and United States; director of Kew Gardens, London; president of Royal Society; together with Lyell, first induced Darwin to make public his views on origin of species; author of many works.
- Hopkins, William, 1793-1866. English mathematician and geologist.
- Horner, Leonard, 1785-1864. Scottish geologist and educationist; Fellow of Royal Society.
- Huxley, Thomas Henry, 1825-95. English scientist; assistant surgeon in Royal Navy; studied deep sea life near Australia and New Guinea; collected and classified marine life on basis hitherto unused; professor of natural history, Royal School of Mines, London; introduced laboratory method in study of biology; zealous advocate of Darwin's views on evolution; author of many works on scientific research.
- Jukes, Joseph Beete, 1811-69. English geologist; naturalist on H. M. S. Fly, on expedition to Torres Strait, New Guinea, and east coast of Australia; served on geological surveys of Great Britain and Ireland; professor of geology, Royal College of Science, Dublin, Ireland; author of many works.
- Kant, Immanuel, 1724-1804. German philosopher; championed liberty and progress; author of many works.
- Kingsley, Charles, 1819-75. English clergyman and writer; canon of Westminster; chaplain to Queen Victoria; one of initiators of Broad Church Movement; zealous advocate of various schemes for improvement of conditions of English working classes; author of many novels, poems, and sermons.
- Lamarck, Jean Baptiste Pierre Antoine de Monet, Chevalier de, 1744-1829. French naturalist, conchologist, meteorologist, and evolutionist; keeper of herbarium, Jardin des Plantes, Paris; professor of zoology, Museum of Natural History, Paris; supported doctrine of derivation of species from other species; first to grasp theory of organic evolution in its entirety; work important as forerunner to that of Darwin; author of many works.
- Lecoq de Boisbaudran, Paul Emilé, 1838-1912. French chemist; known for pioneer work in spectroscopy, through which he discovered samarium and dysprosium, which he isolated.
- Ledebour, Karl Friedrich von, 1785-1851. German botanist and traveler; author of book on plants of Russia.

Biographical Notes

- Leibnitz, Gottfried Wilhelm von, 1646-1716. German philosopher and scholar; attained great eminence in science of mathematics; declined professorship at university in Nuremberg; custodian of public library, Hanover; in addition to law, science, and philosophy, gave much attention to theological questions, seeking earnestly, and without success, to unite Protestant and Roman Catholic churches, and Litheran and Reformed churches of Prussia; discovered differential calculus; made his discovery public earlier (1684) than did Newton.
- Lieber Francis, 1800-1872. German-American political philosopher; twice imprisoned in Germany for liberal sentiments; originated and edited Encyclopedia Americana; professor of history, political economy, and constitutional history, South Carolina College, and Columbia University, New York, N. Y.; author of many works of political character.
- Lincoln, Abraham, 1809-65. Sixteenth President of United States; served in Illinois state legislature; practised law; served in United States Congress; stoutly opposed policies of Stephen A. Douglas; Civil War President; assassinated by John Wilkes Booth.
- Lindley, John, 1799-1865. English botanist, writer, and editor of botanical and horticultural journals; organized first flower shows held in England; was influential in having Royal Gardens at Kew preserved when Parliament sought to abolish them; attempted several schemes of natural classification of plants; author of many botanical works.
- Lubbock, Sir John William, 1803-65. English astronomer and mathematician; member of Royal Astronomical Society; given medal by Royal Society for investigation of tides; studied lunar theory; developed method for calculating orbits of comets and planets; took part in establishing British Almanac; applied theory of probability to life-insurance problems; author of many scientific works.
- Lyell, Sir Charles, 1797-1875. English geologist; devoted time and fortune to geological research; made scientific tours over Europe and United States; summarized evidence in favor of theory that the race of man was much older than was currently believed; together with Hooker, first induced Darwin to make public his views on origin of species; author of many scientific works.
- McClellan, George B., 1826-85. American general; served in Mexican War; major general and commander-in-chief of Union Army during Civil War in United States; governor of New Jersey.
- Mansell, Henry Longueville, 1820-71. English philosopher; carried further Kant's and Hamilton's theories; reader on moral and metaphysical theology, Bampton lecturer, Waynflete professor of moral and mental philosophy, professor of ecclesiastical history, Oxford; Dean of St. Paul's, London; author of many works.

- Martineau, James, 1805-1900. English philosopher and theologian; follower of Bentham and disciple of Kant; materialist; exponent of doctrine of Christian Theism; professor of philosophy, Manchester New College, and principal of same; author of many works on philosophical and religious themes.
- Mason, James M., 1798-1871. American jurist and statesman; served in Virginia house of delegates and in Federal Congress; withdrew from Senate at outbreak of Civil War in United States, and cast his lot with Confederacy; appointed Confederate minister to England; seized, on high seas, together with John Slidell, by Commander Charles Wilkes, of U. S. Navy, while on British steamer, Trent; on way to Europe; held prisoner by Federal authorities until released at demand of England.
- Meehan, Thomas, 1826-1901. American botanist, nurseman, writer, and editor; hybridized fuchsia and produced new race, at age of thirteen; at fifteen, made and published his scientific discovery relating to stamens of portulaca.
- Mill, John Stuart, 1806-73. English philosopher, political economist, and writer; editor of Westminster Review, in which many of his own articles appeared; leader in movement for woman's suffrage in England; served as member of Parliament; moving spirit in discussion groups which were considered radical, but many of whose suggestions have since been adopted and are now regarded as conservative; author of many works of a political character.
- Moseley, Henry Nottidge, 1844-91. English naturalist; Fellow of Royal Society
- Müller, Hermann, 1829-83. German naturalist.
- Müller, Max, 1823-1900. German philologist; brought philology and mythology to public attention; edited fifty-one volumes of translations of sacred books of the East; leading exponent of symbolic interpretation of mythology; made literary contributions to philology.
- Murchison, Sir Roderick Impey, 1792-1871. Scottish geologist and geographer; found clue to discovery of Silurian system; explored several parts of Europe; carried out geological survey of Russian empire; foretold discovery of gold in Australia; encouraged geographical science, and kindled spirit of adventure among those engaged in Arctic exploration and African discovery; director of geological survey of Royal School of Mines, and of Museum of Practical Geology, London; author of scientific works
- Murray, Andrew, 1812-78. Scottish naturalist.
- Nägeli, Karl Wilhelm von, 1817-91. Swiss Botanist; contributed to knowledge of functions of various plant parts; author of many scientific works.
- Newton, Sir Isaac, 1642-1727. English mathematician and philosopher; made notable contributions to calculus; broke up white light into colors of spectrum; invented a reflecting telescope; formulated law of universal gravitation and laws of motion; professor of mathematics, Cambridge, Eng.; president of Royal Society; author of several scientific works.

Biographical Notes

- Norton, Charles Eliot, 1827-1908 American writer, editor, and educator; joint editor, with James Russell Lowell, of North American Review; professor of history of art, Harvard; president of Archaeological Institute of America; author and editor of many works
- Olmsted, Frederick Law, 1822-1903. American landscape architect; in cooperation with Calvert Vaux, prepared design for Central Park, New York, N. Y., and for United States Capitol grounds and terrace; designed park systems of many cities in United States, and acted as commissioner of Yosemite National Park; planned laying out of Jackson Park, Chicago, for Columbian Exposition; appointed by President Lincoln on commission to inquire into sanitary condition of United States Army during Civil War in United States.
- Owen, Sir Richard, 1804-92. English naturalist and comparative anatomist; superintendent of natural history department, British Museum; organized Museum of Natural History, South Kensington; made acquaintance of Cuvier, in Paris, with whose name Owen's is connected in science of zoology; research work embraced classes of animals from sponge to man; author of many works on living and fossil animals.
- Oxford, Bishop of, see Wilberforce.
- Packard, Alpheus Spring, 1839-1905 American naturalist, entomologist, and zoologist; assistant to Agassiz, at Harvard; took part in several scientific expeditions; state entomologist of Massachusetts; professor of zoology and geology, Brown University, Providence, R I, classified insects; founder and chief editor of American Naturalist; author of many works
- Parsons, Theophilus, 1797-1882. Professor of law, Harvard; author of many works on law and religion
- Phillips, John, 1800-1874 English geologist; Fellow of Royal Society
- Pictet de la Rive, François Jules, 1809-72 Swiss zoologist and palaeontologist.
- Rogers, Henry Darwin, 1808-66. American geologist; professor of chemistry and natural philosophy, Dickinson College, Pa.; engaged in state surveys of Pennsylvania and New Jersey; together with his brother, brought before Association of American Geologists and Naturalists conclusions on physical structure of Appalachian chain and on elevation of great mountain chains; researches included general count of geology of United States and of coal fields of North America and Great Britain; professor of natural history and geology, Glasgow, Scotland; author of scientific works.
- Royer, Clémence, 1829-1902 French political economist, philosopher, archaeologist, and anthropologist; advocate of women's rights
- Sedgwick, Adam, 1785-1873. English geologist; professor of geology, Cambridge, Eng.; studied rock formation of Devonshire.

- Silliman, Benjamin, 1779-1864. American scientist and editor; professor of chemistry, Yale; first president of American Association of Geologists and Naturalists; founder and editor of American Journal of Science, known as Silliman's Journal; one of founders of National Academy of Science; author of works on chemistry and travel.
- Slidell, John, 1793-1871. American lawyer and statesman; state congressman, Louisiana; minister to Mexico; member of United States Senate, from which he withdrew during Civil War in United States; appointed Confederate minister to France; seized, on high seas, together with James M. Mason, by Commander Charles Wilkes, of U. S. Navy, while on British steamer, Trent, on way to Europe; held prisoner by Federal authorities until released at demand of England; negotiated French loan for Confederacy.
- Spencer, Herbert, 1820-1903. English philosopher; projected a scheme of philosophy based on principle of evolution in its relation to life, mind, society, and morals; author of many philosophical works.
- Sprengel, Christian Conrad, 1750-1816. German botanist and educationist.
- Thwaites, George Henry Kendrick, 1811-92. English botanist and entomologist; Fellow of Royal Society
- Vries, Hugo de, 1848-?. Dutch botanist; professor at University of Amsterdam; his mutation theory opened up new period in history of evolution; author of works expounding his theory.
- Wagner, Rudolph, 1805-64. German anatomist and physiologist; studied under Cuvier; professor of zoology and comparative anatomy in Erlangen and Göttingen; discovered germinal vesicle of human ovum; studied nerves and brains of human beings; author of many scientific works.
- Wahlenberg, George, 1784-1814. German botanist.
- Wallace, Alfred Russell, 1823-1913. English naturalist; spent many years in traveling, especially in South America, with Henry Walter Bates, and to Asiatic islands; his observations of animals and plant life led him on the track of natural selection; exponent of evolution; received gold medals of Royal Geographical and Linnaean societies, and first Darwin medal of Royal Society; author of many scientific works.
- Walsh, Benjamin Dann, 1803-69. English entomologist
- Watson, Hewett Cottrell, 1804-81. English botanist and phrenologist.
- Wedgwood, Hensleigh, 1803-91. Brother in-law of Charles Robert Darwin; English philologist.

Biographical Notes

Wilberforce, Samuel, 1805-73. English prelate; Dean of Westminster; Bishop of Oxford; Bishop of Winchester; although a High Churchman, did not support Oxford Movement; advocate of moderation; active and influential in House of Lords; established theological colleges; author of many works, mostly of religious character.

Wilkes, Charles, 1801-77. American naval officer; attached to depot of charts and instruments, Washington, D. C.; set up first fixed astronomical instruments for observations; carried on research work in islands of southern Pacific; explored Antarctic and Fiji Islands; as Commander of U. S. steamer San Jacinto, seized Confederate commissioners, James M. Mason and John Slidell, on board British steamer Trent; published account of exploring expedition.

Wollaston, Thomas Vernon, 1822-78. English entomologist and conchologist.

Woodward, Samuel Pickworth, 1821-65. English naturalist.

Wright, Charles, collector for Asa Gray.

Wright, Chauncey, 1830-75. American mathematician and philosopher.

Wyman, Jeffries, 1814-74. American physician; lectured on comparative anatomy at Lowell Institute; professor of anatomy, Harvard; collected specimens for his museum on numerous expeditions to various parts of world; author of many treatises on anatomy of animals.

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