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THE DARWIN-BATES LETTERS

CORRESPONDENCE BETWEEN TWO NINETEENTH-CENTURY TRAVELLERS AND NATURALISTS

Part I

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The Darwin-Bates Correspondence

[PLATE I]

A list of the letters published here, noting missing letters supposedly written that have not been found. Also noted are the books in which some letters have previously been published. For the key, see p. 4, below.

Bates's Letters

1. Missing.
3. Missing.
5. 18 March 1861.
7. 28 March 1861.
9. Missing. 23 September 1861.
11. Missing.
13. Missing.
15. 6 January 1862.
16. 11 January 1862.
18. 25 January 1862.
20. Missing.
22. Missing.
24. [17 April 1862].
25. 30 April 1862.
28. 19 May 1862.
30. 14 June 1862.
32. 17 October 1862. *Clodd.*
35. 24 November 1862. *Clodd.*
37. Missing.
39. Missing.
41. Missing, but a postscript has been found.
44. 8 April 1863.
46. 20 April 1863.
48. 2 May 1863. *Clodd.*
49. 29 September 1863. *Clodd.*
51. 24 October 1863.

Darwin's Letters

2. 22 November [1860]. *More Letters.*
4. Missing.
6. 26 March [1861]. *Clodd.*
8. 4 April [1861]. *Clodd.*
10. 25 September [1861]. *Clodd; Life & Letters.*
12. 3 December [1861]. *Life & Letters.*
14. Missing.
17. 13 January [1862]. *Clodd.*
19. 31 January [1862].
21. 27 [February 1862].
23. 16 April [1862].
26. 4 May [1862]. *More Letters.*
27. 9 May [1862].
29. Missing.
31. 15 October [1862].
33. 18 October [1862]. *Clodd.*
34. 20 November [1862]. *Clodd; Life & Letters.*
36. 25 November [1862]. *More Letters.*
38. 15 December [1862].
40. 12 January [1863].
42. 26 January [1863].
43. 4 March [1863].
45. 18 April [1863]. *Life & Letters.*
47. 30 April [1863]. *More Letters; Clodd.*
50. Missing.

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It is easy to understand why Charles Darwin and Henry W. Bates developed a close and loyal friendship. Bates was anxious to become acquainted with the already famous natural scientist who had stirred up such a sensation a year before by publishing the controversial *Origin of Species*. Darwin was delighted to hear from the young and obscure entomologist who had so recently returned from eleven years of experience in Brazil on the Amazon, which Darwin had visited nearly thirty years before. After their first exchange of correspondence, Bates wrote frequently to Darwin asking for advice. Darwin wrote often asking Bates for technical information about savage people, climate, plants, animals, and especially beetles, bugs and butterflies, all information he wanted for use in his studies. Much of this correspondence has been preserved and is presented here. It began in 1860 and continued until Darwin's death in 1882.

Darwin's name and fame are well known to all, but Bates's history is relatively obscure. A short recital of it is given here in order to make this study more understandable and more interesting. Henry Walter Bates was born 8 February 1825 in Leicester in the heart of the English Midlands and the centre of a large agricultural area. His birthday lacked four days from being sixteen years after that of Charles Darwin and Abraham Lincoln. Bates's grandfather and father were both hosiers, the trade in which young Henry was apprenticed from the age of thirteen until he was twenty. He had very little formal schooling but devoted what little spare time he had at his disposal to self-improvement at the liberally managed Mechanics' Institute in Leicester. For recreation he scoured Charnwood Forest with his brothers for insects and, before he was eighteen, he made his first scientific contribution, 'On Coleopterous Insects Frequenting Dark Places.' It was published in the *Zoologist* on 3 January 1848 in the first number of that journal to appear. During this period he met and became a good friend of Alfred Russel Wallace, then English master at the Collegiate School in Leicester. Even then Wallace was intrigued by the problem of the origin of species. Wallace was a botanist, but they often collected together. Both men were enthusiastic collectors and both experienced growing dissatisfaction with their restricted collecting possibilities. Both men read Humboldt and Lyell as well as Darwin's recently published journal. After reading Edwards's *A Voyage Up the River Amazon Including a Residence at Pará*, Wallace proposed a joint collecting expedition to the Amazon, their expenses to be paid by shipping home and selling duplicate scientific specimens.

They arrived at Pará, which is now called Belém, in May 1848. In the course of time they went up the Amazon to Rio Negro and Manaus, several thousand miles from the mouth, where they parted company and went



Plate I

Henry Walter Bates (1825–1892)

This oil painting by Thomas A. Sims, R.A., hangs in the Director's office of the Royal Geographical Society. A brass plate on the frame is inscribed : 'Henry Walter Bates, F.R.S., Assistant Secretary, R.G.S., painted on his return from the Amazons by Thomas Sims'. Bates was thirty-four years old when he returned in 1859.

(By courtesy of the Royal Geographical Society)

separate ways in order to cover more ground. Wallace returned to England after four years, but Bates remained another seven years living under the most trying conditions, subsisting on a miserable, deficient diet, nearly dying of yellow fever, being robbed of his money and yearning for civilized entertainment and companionship with relatives and friends. His entire trip netted him about £800 profit, but he had collected and sent home nearly 15,000 specimens, 14,000 of them insects and 8,000 of them new to science. Bates returned home in July 1859.

The *Origin of Species* appeared in November of that year and the correspondence between Darwin and Bates began soon thereafter. Much of Bates's subsequent history is revealed by this correspondence. He busied himself with scientific pursuits, described his collections and did free-lance writing for others. He spent the last twenty-eight years of life, from 1864 until his death in 1892, as assistant secretary to the Royal Geographical Society of London.

Bates was disappointed not to have obtained a place in zoology at the British Museum, but, despite much doubt and many misgivings on the part of the Council, he was appointed for a six-months probationary period. He was then in his fortieth year, had never been robust and was still suffering from the effects of his expedition. He was a distinguished entomologist, had won the warm friendship of Wallace and was strongly recommended by Darwin. It was only the assurance of Bates's publisher, John Murray, that Bates was a good business man, that induced the Council of the Society to appoint him.

All of the Darwin letters to Bates are part of the Darwin material purchased by the author from Messrs. Scribner's Sons of New York in 1933. Some of the Bates letters to Darwin are among the Darwin papers deposited by the owner, Sir Robin Darwin, C.B.E., in the University Library, Cambridge, and are reproduced by his kind permission; others are in volumes 82 and 83 of the permanent collection of Darwin papers in the possession of the University Library, Cambridge. Seven letters from Bates to Darwin and five letters from Darwin to Bates have already been published, at least in part, in a 'Memoir' by Edward Clodd in the reissue of the *Naturalist on the River Amazons* in 1892. Published here, including the above, are thirty-six letters from Darwin and thirty-nine from Bates. The correspondence lasted from the first letter on 20 November 1860 until 17 February 1882, two months before Darwin's death.

The correspondence is irregular, being enthusiastic at some times and very desultory at others. There are obvious omissions due, in part at least, to Darwin's habit of destroying correspondence after it had accumulated, a habit he apparently abandoned in 1862. Much of the correspondence is heavy, technical, and lacking in personal and family interest. There is much here to show how Darwin worked, where he got his information

and whom he asked for advice. One almost gets the impression that Darwin, in his enthusiasm, in his naïveté and in his unique position as a senior scientist, took advantage of Bates's good nature. There is no reason to believe that Bates ever resented so much attention.

Requests for information about other letters of the Darwin-Bates correspondence were published in the *Bulletin of the History of Medicine and Allied Sciences*, *Science*, and *The Times Literary Supplement*. No information about further letters was included in about twenty replies.

This correspondence is published to give the thoughts and show the interplay of ideas and the moulding of opinions of these two men. The letters have been transcribed as accurately as possible, preserving the peculiarities and idiosyncrasies of writing of both men. Both men rarely spelled out 'and' but used the symbol '&' instead. They often used 'shd' and 'wd' instead of 'should' and 'would.' Both men used capital letters for most important nouns and for specific as well as generic names. Their punctuation has been preserved. Darwin particularly separated sentences with a dash as well as a period, he used a semicolon or parenthesis where modern usage calls for a comma and he used the long 's' as the first one in 'ss'. Darwin did not indent and indicated paragraphs only by a short line above. Darwin's hand writing at best is very poor and difficult to read. Salutations grew less formal. Darwin used, 'My dear Sir,' then 'My dear Mr. Bates,' and later 'Dear Bates'. Bates always wrote, 'My dear Mr. Darwin', or 'Dear Mr. Darwin'. Darwin was satisfied to indicate the month and day and invariably omitted the year. The year when added is shown in square brackets. The names of flora and fauna have been checked in *Webster's New International Dictionary* and geographical terms and place names in standard atlases.

If the stationery is printed or embossed, the heading is here printed in capital letters; if the heading is written, it is given in small letters.

References in the text are made to *Life and Letters of Charles Darwin* by Francis Darwin, London, John Murray, 1887, as *Life and Letters*; *More Letters of Charles Darwin* by Francis Darwin, London, John Murray, 1903, as *More Letters*; *The Naturalist on the River Amazons* by Henry Walter Bates, London, John Murray, 1863, as *Naturalist*; the *Memoir of the Author* by Edward Clodd included in *The Naturalist on the Amazons*, reprint of the unabridged edition, London, John Murray, 1892, pp. xvii-lxxxix, as *Clodd*; and *Webster's New International Dictionary of the English Language*, Springfield, Mass., G. and C. Merriam Company, 1960, as *Webster*.

The author expresses his deep appreciation to several friends, scholars and librarians, without whose technical knowledge and unfailing cooperation this study would not have been possible. These include

Sir Gavin de Beer, F.R.S., Miss Maria Skramovsky of the British Museum (Natural History), Mr T. A. Walden, Director of the Leicester Museums and Art Gallery, Mr George H. Goodwin, Jr., of the American Museum of Natural History, and Mr David Kronick, formerly of the Cleveland Medical Library Association, who generously tapped the resources of their institutions for identifying and photo-copying innumerable references. Sir Gavin de Beer was especially helpful in interpreting difficult portions of Darwin's handwriting.

A table of the letters published has been prepared in which the missing letters are indicated. Those letters that have been published before are also marked.

The study is divided into two parts for convenience. The first includes letters written between 22 November 1860 and 24 October 1863 and concerned principally with comments about the *Origin of Species*, Bates's book of travels, Bates's questions about style, contents and scope of his book, the choice of a publisher and the book's acceptance. The second part is somewhat different because Bates is occupied with affairs of the Royal Geographical Society.

The beginning of the acquaintance is not known because the first letter in the series, one from Bates to Darwin, has been lost. To judge from Darwin's reply, it was long and thoughtful and covered many subjects to which Darwin reacted enthusiastically and answered enthusiastically. Darwin's reply was dated 22 November without indication of the year. It must have been 1860 because Darwin 'is working on a new edition' of the *Origin*, the third edition of two thousand copies, published in April 1861. The second edition, which is not so marked, of three thousand appeared in 1860. The first edition of 1250 copies was published on 22 November 1859. These dates and figures are from R. B. Freeman's *The Works of Charles Darwin*, London, Dawson, 1965, pp. 21-22.

2.

Nov. 22 [1860]

Down Bromley Kent

My dear Sir

I thank you sincerely for writing to me & for your very interesting letter. Your name has for very long been familiar to me, & I have heard of your zealous exertions in the cause of Natural History. But I did not know that you had worked with high philosophical questions before your mind. I have an old belief that a good observer really means a good theorist & I fully expect to find your observations valuable.

I am very sorry to hear that your health is shattered; but I trust under a healthy climate it may be restored. I can sympathise with you fully on this

score, for I have had bad health for many years & fear I shall ever remain a confirmed invalid.—I am delighted to hear that you, with all your large practical knowledge of Nat. History, anticipated me in many respects & concur with me.—As you say I have been thoroughly well attacked & reviled, (especially by entomologists, Westwood, Wollaston & A. Murray have all reviewed & sneered at me to their hearts' content) but I care nothing about their attacks ; several really good judges go a long way with me, & I observe that all those who go some little way tend to go somewhat further. What a fine philosophical mind your friend, Mr. Wallace has, & he has acted, in relation to me like a true man with a noble spirit.—I see by your letter that you have grappled with several of the most difficult problems, as it seems to me, in Natural History — such as the distinctions between the different kinds of varieties, representative species etc. Perhaps I shall find some facts in your paper on intermediate varieties in intermediate regions,—on which subject I have found remarkably little information.—I cannot tell you how glad I am to hear that you have attended to the curious point of Equatorial refrigeration. I quite agree that it must have been small ; yet the more I go into the question the more convinced I feel that there was during the Glacial period some migration from N. to S.—The sketch in the Origin gives a very meagre account of my fuller M.S. essay on this subject.—

I shall be particularly obliged for a copy of your paper when published ; & if any suggestions occur to me, (not that you require any) or question I will write & ask.—Pray believe me, with respect & good wishes,

My dear Sir
Yours sincerely
C. Darwin

I have at once to prepare a new Edition of the Origin, & I will do myself the pleasure of sending you a copy ; but it will be only very slightly altered.—

Cases of neuter ants, divided into castes, with *intermediate gradations*, (which I imagine are rare) interest me much. V. Origin on the Driver ants p.—241— (please look at the passage).

The above letter is published in full in *More Letters*, vol. i, pp. 176–177. A foot-note on page 177 commenting on the letter states : ‘ Probably Bates’s paper entitled ‘ Contributions to an Insect Fauna of the Amazon Valley,’ Trans. Entomol. Soc., Vol. V., p. 335, 1858–61. It was read June 21, 1861.’

J. O. Westwood had reported in the *Gardener’s Chronicle*, 1859, p. 997, that cross-bred specimens did not breed true. Darwin replied in a letter to the same journal, 21 January 1860, p. 49, objecting that Westwood had given no reference to variability of the Swedish Turnip. Francis Darwin states in *Life and Letters*, vol. ii, p. 267, that this was one of the very few cases in which his father was drawn into anything resembling a controversy.

T. V. Wollaston's review appeared in the *Annals and Magazine of Natural History*, 1860, **5**, 132-143. Darwin wrote in a letter to Lyell, published in *Life and Letters*, vol. ii, p. 284: 'I am perfectly convinced (having read this morning) that the review in the *Annals* is by Wollaston; no one else in the world would have used so many parentheses. I have written to him and told him that the "pestilent" fellow thanks him for his kind manner of speaking about him. I have also told him that he would be pleased to hear that the Bishop of Oxford says it is the most unphilosophical work he ever read. The review seems to me clever, and only misinterprets me in a few places. Like all hostile men, he passes over the explanation given of Classification, Morphology, Embryology, and Rudimentary Organs, &c.'

Andrew Murray's review was dated 16 January, read 20 February, and published in the *Proceedings of the Royal Society of Edinburgh*, 1862, **4**, 274-292. In another letter to C. Lyell on 6 January 1860, Darwin wrote: '... I will send A. Murray's paper whenever published. It includes speculations (which perhaps he will modify) so rash, and without a single fact in support, that had I advanced them he or other reviewers would have hit me very hard. I am sorry to say that I have no "consolatory view" on the dignity of man. I am content that man will probably advance, and care not much whether we are looked at as mere savages in a remotely distant future.'

There is a Darwin letter in the Dana Biomedical Library of the Dartmouth Medical School addressed to 'My dear Sir' but obviously meant for Murray, in which Darwin states, '... I have read your review. I am sure I have no grounds whatever to complain, but on the contrary: to thank you for the general manner of speaking of me.' This letter was edited with comments by the present writer and published in the *Dartmouth Alumni Magazine* (January 1965).

It is obvious that Darwin often wrote boldly and bravely about his reviewers in correspondence with a third party, but was actually conciliatory to them in personal correspondence.

The passage taken from the second edition of the *Origin*, 1860, Chapter 7, 'Instincts,' page 241, reads as follows: 'I believe that natural selection, by acting on the fertile parents, could form a species which should regularly produce neuters, either all of large size with one form of jaw, or all of small size with jaws having widely different structure: or lastly, and this is our climax of difficulty, one set of workers with one size and structure, and simultaneously another set of workers of a different size and structure:—a graduated series having first been formed, as in the case of the driver ant, and then the extreme forms, from being the most useful in the community, having been produced in greater and greater numbers through the natural selection of the parents which generated

them : until none with an intermediate structure were produced.'

A Bates letter in answer to the above and Darwin's reply are obviously missing because Bates makes no mention of Darwin's letter. It is assumed that Bates's letter of 18 March 1861 is in answer to a second in which Darwin asked specific questions.

5.

King St Leicester
18 March 1861

Dear Sir

At last the Ent. Society have printed my paper & I am enabled to send you a copy according to promise

I do not know whether the perusal will repay your trouble, & I cannot point out especial passages embodying the facts most useful & interesting to you ; they are scattered over the whole,—perhaps the concluding observations & those under the head of *T. Hierocles* are most to the point.

I think there are about 3 points of interest arising from the review of the Species of *Papilio*, & the genus from the precision of the specific characters & the great amount of material existing in collections is well calculated to illustrate them.—These are ;

1 The derivation of the Amazonian fauna. I confess I was not prepared for the result to which I was obliged to arrive after a close examination of the species & their distribution—viz : that the Guiana region must have been the seat of an ancient & peculiar fauna transmitted through vast lapses of time ; & that thence was derived the fauna of the Amazon valley.—Also that it was still so rich in endemic species. Surely I am right in deriving the conclusion that there can have been no great extinction here during the glacial epoch.

2 The widely different variability of species when under different local conditions in localities widely apart—Let us suppose 4 species A B C D living at localities 1 & 2. A will be not in the least modified : B constant at 1 will be instable at 2 : C will have become changed at 2 in all its individuals, but the change is so small that all will admit the difference to be that of a variety : whilst D will have become changed so much more considerably at 2 that every author will treat the form as a perfectly good species. Yet all the points of difference between D1 & D2 are similar to those between B1 & B2 & C1 & C2, only they are greater in degree or more numerous

3 The permanency of local varieties after they have become established. It is still the favourite argument of our best naturalists that varieties will always return to their normal form & that they will interbreed & produce fertile offspring. This argument is derived from the observation of varieties produced by domest[ication]—a false guide—such varieties are too rapidly made, to be compared with the slow alteration of the whole organism which takes place in nature & affects, I have no doubt, at length the reproductive elements.—In the genus *Papilio* there is a set of local varieties all connected by fine gradations of

differences ; & yet in one well established case two of these varieties exist in contact & do not show the slightest tendency to amalgamate.—It is a case exactly parallel to what would be if we were to find in the wild state a series of graduated local varieties between the horse & the ass ;—I thought it likely I should find in the Natural History of the Horse and ass some data to prove the parallel & turned to a paper of Blyth lately published on the varieties of wild ass. I was surprised to find how little was satisfactorily known on the subject & how uncertain & vacillating is the state of our knowledge of the species & varieties of these conspicuous animals.

With [re]gard to the varieties of species involving modifications of their reproductive elements, a learned entomologist a friend of mine, Mr. Baly has found in the chrysomelidæ what he calls *specific differences* in the male organs of generation ; he finds they vary between very closely allied forms,—but then he reasons I think falsely,—he says that the fact of the difference in these organs proves distinctness of species & thus he is proving to be distinct species forms which all Entomologists had agreed to consider as varieties.—It is very amusing to read (In the proc. Ent. Soc. I think September or October [last ?] that he had received a large series of Donacæ from N. America,—there were many doubtful forms amongst them & he has applied the test of these organs to prove whether they are distinct species or no.

I hope you will excuse this rather rambling letter & favour me with your opinion on my paper at your earliest convenience

Yours sincerely
H W Bates

The paper referred to at the beginning of this letter is ' Insect Fauna of the Amazons Valley,' *Trans. Entom. Soc.*, 1861, **5**, 223–228, 335–361. Baly's paper is entitled ' Description of New Species of Sagro : remarks on that genus and the characters of Cheiloxenai, a new genus belonging to the same family,' *Trans. Entom. Soc.*, 1858–61, **5**, 230–260.

Webster states that *Papilio* is a Linnean genus originally comprising all of the butterflies but as now used is restricted to the typical swallow-tailed butterflies and some of their allies. It comprises many subgenera and is the type of the family Papilionidae. Chrysomelidæ is a family of small beetles that feed chiefly on the leaves of plants.

6.

March 26 [1861]

Down Bromley Kent

Dear Sir,

I have read your papers with extreme interest & I have carefully read every word of them. They seem to me to be far richer in facts on variation, & especially on the distribution of varieties & subspecies, than anything which I have read. Hereafter I shall reread them, & hope in my future work to profit by them

& make use of them. The amount of variation has much surprised me. The analogous variation of distinct species in the same regions strikes me as particularly curious. The greater variability of the female sex is new to me. Your Guiana case seems in some degree analogous, as far as plants are concerned, with the modern plains of La Plata, which seem to have been colonised from the north, but the species have been hardly modified.—

(I have been particularly struck with your remarks on the Glacial period. You seem to me to have put the case with admirable clearness & with crushing force. I am quite staggered with the blow & do not know what to think. Of late several facts have turned up leading me to believe more firmly that the Glacial period did affect the Equatorial Regions ; but I can make no answer to your argument ; & am completely in a cleft stick. By an odd chance I had only a few days ago been discussing this subject, in relation to plants, with Dr. Hooker, who believes to a certain extent ; but strongly urged the little apparent extinction in the Equatorial regions. I stated in a letter some days ago to him, that the Tropics of S. America seem to have suffered less than the Old World. There are many perplexing points ; temperate plants seem to have migrated far more than animals.—Possibly species may have been formed more rapidly within Tropics than one would have expected. I freely confess that you have confounded me : but I cannot yet give up my beliefs that the Glacial period did to certain extent effect the Tropics.—)

Would you kindly answer me 2 of 3 questions if in your power.—When species (A) becomes modified in another region into a well marked form (C), but is connected with it by one (or more) gradational forms (B), inhabiting an intermediate region ; does this form (B) generally exist in equal numbers with (A) & (C), or *inhabit an equally large area* ?—The probability is that you cannot answer this question ; though one of your cases seem to bear on it.—

Has Mr. Baly published on Chrysomelidae ? if so, or when he does, would you give me reference to his paper ?

In Butterflies, in which the sexes are differently coloured, is the male or female most beautiful in *our* eyes ? Do you know in Tropics any strictly nocturnal Moths with gaudy colours ? As with Birds, have you ever noticed that female butterflies make any selection of the male with which they copulate ? Do several males pursue same female ? Are butterflies attracted by gay colours, as it has been asserted Dragon-flies are. Any authentic facts on the courtship of Butterflies would be *most thankfully* received & quoted by me.—But I can see how very improbable it is that anything shd have been observed.—(You will, I think, be glad to hear that I now often hear of naturalists accepting my views more or less fully ; but some are curiously cautious in running risk of any small odium in expressing their belief.) With cordial thanks & respect.

Believe me my dear Sir

Yours sincerely

C. Darwin

P.S. Have you recieved copy of new Edit. of Origin ?

This is obviously in answer to Bates's letter of 18 March. It contains also the first of a very long series of technical questions concerning which Darwin asks for help.

This letter was published in full by *Clodd*, pp. xxxii-xxxiii. The portion enclosed in brackets was published in *More Letters*, vol. i, pp. 464-65.

Baly published 'Descriptions of some new species of Chrysomelidae,' *Trans. Entom. Soc.*, 1856-58, 4, 339-352. He later published 'Descriptions of Six New Species of Chrysomelidae from the East', *J. Entom.*, 1862, 1, 93-97.

7.

King St Leicester
28 March 1861

My Dear Sir

I received your kind letter today & think it better to reply at once as I am going to London on Monday for a few days study at the Insect room Brit. Mus. & shall not have another opportunity very soon. It gives me very great pleasure to find that my paper is likely to be useful to you. I am quite convinced that insects offer better or clearer illustrations of the problems you occupy yourself with than any other class of animals or plants. It is so easy with them to obtain great series of examples & have them before you in a small compass, which is one advantage they have.

I do not think the result I have come to regarding the persistence of the Guiano-Amazonian fauna can be shaken. I can see *no traces* of a migration of high-temperate-zone forms across the region. Now there are a number of genera of insects characteristic of the high-temperate-zones, which are now common to S. America & N. America with Europe; but they have not a single representative in Amazonia, although some of them have representatives along the Andes of New Granada & Peru. Your hypothesis only requires (as you have stated) a migration along belts of latitude & therefore I do not think that the conclusion I have come to affects your position much after all.—I am further confirmed in my view that no migration took place across Guiano-Amazonia by the fact that there is a migration going forward now, or has been within the historic period of species of a migratory genus of Butterflies, but although one or other of the forms has obtained a footing in most other regions of the world there is no trace of them in that country. I allude to the species of the genus *Pyrameis* of which there are two English species *P. atalanta* & *P. cardui*. The geographical distribution of the species of this genus is very peculiar & has often excited attention. The following is a sketch of the facts *P. atalanta* is found throughout Europe & N. America as far South as Mexico. On the Southern slopes of the Himalaya & in Madeira (!) it exists as *P. Callirhoe* considered by some as a local var. In *Java* it presents us with a further var. in *P. Dejeanii*: & in the Sandwich Islands another allied form, *P. Tammeamea*; It appears to go no further South than *Java*, & is *not* found in S. America. *Pyrameis cardui* is found throughout Europe, Africa to the cape of Good Hope; Asia & and asiatic islands to Australia & New Zealand, without varying.—In America it is found throughout the United States. In Venezuela it exists only as a var. *P. Leachiana*, throughout N. America it is found in conjunction with *P. Huntera* a very closely allied species; but the two have different areas, although existing together in some parts; To the west of the Andes from California to Chili there exists another form of *Cardui*, viz *P. Carye*;

and in the S. of Brazil another, *P. Myrinna* ; in the Amazonian region not a trace of any of these forms is to be found. From these facts it would appear 1st that Northern temperate forms more readily cross the equator in the old world than in the new : 2nd that they are *subjected* to far greater modifying or transforming influences in the New world than in the old, 3rd that the only passage made use of by them in the New world is the chain of the Andes. I should add that the species of *Pyrameis* are strongly migratory ; many accounts at least of immense migrations of *P. cardui* having been published.—

When I read your book first, the chapter on Geogr. distr. of course struck me very much ; but a doubt occurred to me whether the resemblance in the faunas & floras between the N. & S. temperate zones was of such a nature as to require the explanation you have given. I am quite ignorant of the nature of the curious resemblance in the plants of Tierra del fuego & am quite inclined to think my doubt is unfounded in this respect. However, two considerations have presented themselves to me. 1st there are in several classes of animals & plants groups which seem to have originated at a remote epoch of the worlds history, & which, in consequence of the *limited nature of their variability* have gone on repeating nearly similar specific forms in all countries where they exist up to the present time. In plants I am not aware it is any other class than the inferior Cryptogamia which are in this position ; but in insects there are several groups. Two occur to me at the present time the Hesperiadæ in the Lepidoptera & the Libellulidæ (Dragonflies) in the Neuroptera. Similar species of these are found all over the world. I will not dwell too long on the subject or my letter will run to great length. I wish to suggest whether it is possible that the Tierra del fuego plants belong to such a group as I have described. I know that they are not found (as you have stated) in the tropical zones, but are the genera found there ; & are they of such a homomorphic nature (to coin a word) as I have described.—The other consideration is that there are genera peculiar to the high temperate zones of both hemispheres which present in South America compact groups of species so very dissimilar to those of North America that it is out of the question that they could have been derived thence so lately as the glacial period. I will give you a strong & very intelligible case viz. that of the Chilian species of *Carabus*, of which yourself discovered several of extreme beauty. The genus *Carabus* at the present time is not found in the Northern hemisphere, further south than 28° nor in the Southern further North than 33°.—The Chilian species number 11 & form a group distinguished by peculiarities of form sculpture ;—Now there is not a species that at all approximates those of N. America ; there is an utter dissimilarity between the two groups. The nearest approach to the Chilian forms is presented in species of the South of Europe (perhaps some may occur in E. Asia). How did the Chilian *Carabi* get there, & why are they so different from those of the rest of the world ?—To me this is a most interesting fact ; it seems to point to a far more ancient Glacial epoch, than the post-tertiary one. The *Carabi* may be studied by any one in five minutes by consulting the “Bowringian Collection” in the British Museum, where they are lucidly arranged.

With regard to the question you ask me—whether an intermediate local form B is numerous & widely dispersed between the ranges of its extreme forms A & C : the facts that I have on the subject are numerous & rather ill digested at present. They are complicated in themselves & difficult. I know many instances of two local forms separated by a wide space without apparent natural barriers, untenanted by intermediate forms. There are many others (closely allied) which exist together on their mutual frontiers without blending. There

are others which are very polymorphic in a central region, whilst in other localities E. W. N. & S. segregated into several well defined local varieties or admitted species. I will mention now one case only that meets your question. It is one whose correctness I am quite sure of. On the dry soils, supporting for the most part a thinner forest growth, of the hilly regions of Guiana & Venezuela there exists a conspicuous *Heliconia*, *H. Melpomene*, it is abundant also in the central parts of the lower Amazons where the Hilly sandy country occurs & is found on the N. & S. Shores in extreme profusion. In the moist alluvial plains Eastward to Pará & Westward to Peru & Bolivia, not a single individual is to be seen. In its place, occupying exactly the same sphere as it were, is *H. Thelxiope* a form so strikingly peculiar in colours that no one has ever doubted it to be a perfectly distinct species; it swarms in individuals & is nearly constant in its specific characters. Now wherever this form comes into contact with *Melpomene* there exists a number of intermediate varieties many of which have been described as species: they *are rare, & very restricted in range*. They have puzzled Lepidopterists & it has been almost settled that they were hybrids. I am convinced they are not *hybrids*; I never saw *Melpomene* & *Thelxiope* in copula; and besides these pretended hybrids exist at Demerara & Cayenne, where *Thelxiope does not occur*. I am thoroughly convinced that *Thelxiope* is a local variety of *Melpomene*, having all the appearance of a species, but created by the influences to which it has been subjected, out of *Melpomene*; & that the intermediate forms are the gradations. I have found *Melpomene* in copula with these forms & perhaps it may sometimes be so with *Thelxiope*, but that would not affect the case much, I think.

The group from which I have supplied this example is very interesting, —being restricted to S. America, which swarms with its species; & apparently a modern creation. I am now studying it with a view to writing upon it & I think I have got a glimpse into the laboratory where Nature manufactures her new species.—

The other question you ask me relates to sexual selection.—I *have* seen *Papilios* attracted by bright colour in the forest, namely by the scarlet sepals of certain plants having inconspicuous or no flowers. I have also seen repeatedly many males following a female.—On this subject I will point out to you the following facts. In the *Æneas* section of the genus *Papilio*, the males are generally of extremely brilliant colours, velvety black, vivid green, carmine, & red with opalescent reflection,—the females are plainer & so different from the males that they were generally held to be distinct species until I took them in copula. Now it is not *all* the species that present this disparity. In one *P. Panthonus* the two sexes are exactly alike in colours, the male only being a trifle brighter; from this species the divergence may be traced getting wider & wider from species to species up to *P. Sesostris* or *P. Childrenæ*, where the dissimilarity reaches its highest point. There is exactly the same phenomenon presented in the genus *Epicalia* & others. The females, however vary—in localities,—& although I thoroughly believe in your theory of sexual selection yet I think that local circumstances have some effect on colours. I do not however think that sunlight is the direct cause of vivid colours, although beautifully coloured male butterflies are almost always in the sun (genus *Catagramma*) whilst their drab partners are in the shade. I think the causes lie in the abundance of food, warmth & moisture of the atmosphere & even in the sluggish state of the atmosphere because the brightest coloured butterflies are not generally found within the influence of the Atlantic sea breezes, but in the sultry valleys of the Andes & the centre of the continent.

These causes operate on the larva & so by correlation on the perfect insect. I cannot say that I have noticed female butterflies pointedly selecting their male partners. The extremely rich male butterflies of the genus *Catagramma* live & sport together in the sun light all day, whilst their plainer coloured females are confined to the shades of the forest. The males too appear to be very much more numerous than the females,—I think *there is no* doubt now about this significant fact there must be many hundred males to one female of these butterflies, & in another genus *Cybdelis* where the males are also distinguished by rich colours the males are immensely more numerous than the females. In another beautiful genus, *Megistanis* no female has yet been found although the males are very numerous. In *Callithea*, however where the males are the most richly coloured of all butterflies, both sexes are found in equal numbers, but *here the females* are not *much* behind the *males in beauty*. The males which sport together all day in the sunlight disappear about 4–5 P.M. & I have watched them then flying off to the forest (towards the summits of the trees) where doubtless some of them find their mates.—There are no strictly nocturnal Lepidoptera gaily coloured in the tropics : There is no scarcity of brilliantly col^d. moths but they are all dayfliers.

Perhaps the above facts will illustrate a little the problems of sexual disparity & beauty of colours. There is no phase of your theory I like better than its explanation of the subtle adaptations of organic beings. There is one topic which Entomology will help to illustrate, viz. that of mimetic analogies. I have an immense number of facts on this subject. Some of these resemblances are perfectly staggering,—to me they are a source of constant wonder & thrilling delight. It seems to me as though I obtain a glimpse of an *intelligent motive* pervading nature, as well as of the mighty never-resting wonder working laws that regulate all things.—

I received the copy of 3rd edit. of the "Origin" a most valued present for which accept my thanks.—I received a letter from my friend Wallace last week He was going to Timor.

Yours sincerely
H W Bates

There is a stupid error in my paper, the note referring to "Recent & ancient fossil shells." The secretary took upon himself to correct the proof, I saw it but forgot to erase it—

Believe me my dear Sir
H. W. Bates

New Granada is not mentioned in the *Rand McNally Cosmopolitan World Atlas*, 1959 or in the *National Geographic Atlas of the World*, 1963. In 'A Pronouncing Gazetteer of the World' from *Webster*, New Granada is described as : in Colonial times, a Spanish vice-royalty in N. W. South America occupying region now in Venezuela, Colombia, Panama, and Ecuador. The Sandwich Islands are now called the Hawaiian Islands.

Many of the terms, too numerous to mention, used in these letters are not in *Webster*. Since they are highly technical and of interest only to entomologists, no further attempt has been made to identify them.

Definitions are given, however, of more important and more general terms to make the content more understandable and more interesting.

Lepidoptera is the order of insects that consists of butterflies and moths ; Hesperidae are a large family of Lepidoptera consisting of the skipper butterflies ; and Heliconides, a large genus of butterflies ; Neuroptera is an order of insects which includes lace-wing flies, ant-lions, and allied forms ; Carabus is a genus of large ground beetles typical of the Carabidae which are a large family of beetles mostly of active predaceous and largely terrestrial habits (the ground beetles); Cryptogamia in older systems of classification refers to a sub-kingdom embracing all plants that do not produce flowers or seeds, as ferns, mosses, algae, etc., contrasted with Phanerogamia.

Miss Maria Skramovsky of the British Museum states that the Bowringian collection of insects was assembled by John Charles Bowring (1821-1893), a British entomologist who presented his collection of about 230,000 specimens to the British Museum in 1863. According to the Museum Catalogues, Bowring possessed the Tatum collection of Geodephaga and Major Parry's collection of Anthripidae. He purchased Chevrolet's entire collection of Longiacornia which was founded on that of Dejeau. Bowring also purchased Jekel's collection of Rhynchophera. Bowring himself collected energetically while abroad in China and also employed people to collect for him.

The following mutilated note on an unused envelope is in Darwin's handwriting and is identified by Darwin, who wrote on it in red pencil, the same as he used to mark two passages in Bates's letter of 28 March 1861 : ' Bates '

Bates : Valuable Glacial : very good
S. America Coc points to ancient glacial period. Chalk
glacial & per . . . u (Permian ?) so.—
. Very good on intermediate vars. in intermediate zones.
. Admirable on Sexual Selection. Butterflies
attracted by bright sepals.'

The date ' 28 March 1861 ' is written on the envelope in ordinary pencil, in another hand.

8.

Apr. 4th [1861]

Down Bromley Kent

My dear Sir

I have been unwell, so have delayed thanking you for your admirable letter. I hope you will not think me presumptuous in saying how much I have been struck with your varied knowledge, & with the decisive manner in which you bring it to bear on each point,—a rare and most high quality, as far as my experience goes.—I earnestly hope you will find time to publish largely : before the Linn. Soc. you might bring briefly out your views on species.—Have you ever thought of publishing your travels & working in them the less abstruse parts of your Nat. History ? I believe it would sell, & be a very valuable contribution to Nat. History.—You must also have seen a good deal of the natives. I know well it would be quite unreasonable to ask for any further information from you ; but I will just mention that I am now & shall be for a long time writing on Domestic variation of all animals. Any facts would be useful : especially any showing that *savages* take any care in breeding their animals ; or in rejecting the bad & preserving the good—or any fancies which they may have that one coloured or marked dog etc. is better than another.—I have already collected much on this lead, but am greedy for facts.—You will at once see their bearing on Variations under Domestication. ((Your observations on Carabus, with respect to Glacial period, seem very important.—I daresay you know that some geologists have speculated on a Permian & even on a Chellean Glacial period.—When considering plants of Australia, an impression crossed me that there must have been an ancient migration from N. temperate to S. temperate regions.—I feel sure that if you saw lists of plants in T. del Fuego, & on *isolated* mountains of India, Java, Borneo, Abyssinia, S. E. Australia, & Fernando Po, you would see that there *must* have been a very recent migration. This view is largely supported by plain geological facts.—I have a rather long M.S. discussion, well copied out, which Hooker has read, & which if you thought it worth your while, you might with welcome read ; but I doubt whether it would be worth your while.—Hereafter when I come to Geograph. Distrib. (& God knows when that will be) I will deeply consider all your most valuable remarks.—

Thank you for facts on intermediate var. in intermediate regions : I can see how complex this case is ; & I hope before I come to the subject you will have largely published. But the case you give is excellent.—)

Hardly anything in your letter has pleased me more than about sexual selection. In my large M.S. (& indeed in Origin with respect to tufts of hairs on breasts of Cock Turkey) I have guarded myself against going too far ; but I did not at all know that male & female butterflies haunted rather different sites. If I had to cut up myself in a Review, I would have worked & quizzed Sexual Selection : therefore, though I am fully convinced that it is largely true, you may imagine how pleased I am at what you say are your beliefs.—This part of your letter to me is a quintessence of richness.—The fact about Butterflies attracted by coloured sepals is a rather good fact, worth its weight in Gold.—It would have delighted the heart of old Christian C. Sprengel,—now many years in his grave.—

I am glad to hear that you have especially attended to "mimetic" analogues—a most curious subject.—I hope you will publish on it. I have for a long time wished to know whether what Dr. Collingwood asserts, is true, that the most striking cases generally occur between insects *inhabiting the same country*.—

Believe me
Yours most truly obliged
Ch. Darwin

This letter is quoted complete by *Clodd*, page xxxv, except for the portion in double parentheses. He attributed it to 1860.

Darwin states that he has been unwell and so has delayed his answer. It took him six days! This letter contains Darwin's suggestion that Bates should publish an account of his travels. *The Naturalist on the River Amazons*, published in 1863, was the result.

Darwin again is plying Bates with questions for material he wants for use in *The Variation of Animals and Plants Under Domestication*. This book was published in 1868, seven years later.

Darwin's statement from the *Origin* is as follows: ' . . . the tuft of hair on the breast of the turkey-cock, which can hardly be either useful or ornamental to this bird :—indeed, had the tuft appeared under domestication it would have been called a monstrosity ' (Chapter IV, Natural Selection : second edition, 1860, page 90).

10.

Sept. 25 [1861]

DOWN
BROMLEY
KENT, S.E.

My dear Sir

Your letter, like every one that I have received from you, has been a mine of wealth & has interested me greatly. But first for the most important point, viz your Book of Travels, & I heartily rejoice that you intend publishing. I shd think that you could not have a more respectable or pleasant publisher than Mr. Van Voorst.—At the same time, I apprehend, there can be no doubt that Murray has *much* greater power of getting large distribution. Murray has the character of being a very liberal paymaster & I am sure I have found him most liberal & pleasant to deal with. ((Of course every publisher will be cautious with a new author. If your Book should turn out popular (& there is some little more luck in this), it would be a thousand pities that it shd not be in hands that would press its sale. I cannot remember that Mr. V. Voorst has ever had any large sale for any of his Books.—On other hand Mr. V. V. would better appreciate your scientific character than Mr. Murray.—This is all the advice I can give ; except that)) I rejoice I went to Murray with the *Origin*.—By the way here is case in point ! Colburn did not value my *Journal*

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of Researches & would never, I am sure, have published a second Edit. I took it from Colburn & sold it to Murray & it has large & *great* sale up to present day.—If you shd decide on Mr. Murray, & if you would so like I shall be most happy to write to Mr. Murray & can most truly tell him how much I appreciate the force of intellect & knowledge & style of your letters to me.—What terms he would offer you I cannot conjecture : he would, no doubt, wish to see some M.S. ((As an old hackneyed author let me give you a bit of advice, viz to strike out every word, which is not quite necessary to current subject, & which would not interest a stranger. I constantly asked myself, would a stranger care for this ? & struck out or left in accordingly.—I think too much pains cannot be taken in making style transparently clear & throwing eloquence to the dogs.)) I hope that you will not think these few words impertinent.—(I would sell only 1st Editions.)—((Now for a few words on Science.—Many thanks for facts on Neuters. You cannot tell how I rejoice that you do not think what I have said on subject absurd.—Only 2 persons have even noticed it to me.—viz the bitter sneers of Owen in Edinb. Review, & my good friend & supporter Sir C. Lyell, who could only screw up courage to say “Well you have manfully faced the difficulty.”—

What a wonderful case of *Volucella*, of which I had never heard ; I had no idea such a case occurred in nature : I must get & see specimens in the Brit. Museum.—I hope & suppose you will give a good deal of Nat. History in your Travels ; everyone cares about Ants—more notice has been taken on Slave Ants in the Origin than of any other passage.—

I fully expect to delight in your Travels. Keep to simple style as in your excellent letters,—but I beg pardon I am again advising.

What a capital paper yours will be on Mimetic resemblances.—You will make quite a new subject of it.—I had thought of such cases as a difficulty, & once when corresponding with Dr. Collingwood, I thought of your explanation ; but I drove it from my mind, for I felt that I had not knowledge to judge one way or the other. Dr. C., I think, states that the mimetic forms inhabit same country ; but I did not know whether to believe him—What wonderful cases yours seem to be.—Could you not give a few woodcuts in your Travels to illustrate this ?—I am tired with hard day's work, so will run, except to give my sincere thanks & hearty wishes for the success of your Travels.—))

My dear Sir
Yours sincerely
C. Darwin

A letter from Bates between 4 April and 25 September is missing. The date of letter no. 9, 23 September 1861, was obtained from a folder in the Bates collection in the University of Cambridge Library. It contains Bates's letter of 30 April 1862, and bears a note : ‘ +letter of 23 Sept. 1861 on pale blue paper ’. In this missing letter, Bates apparently announced his intention of writing a book and asked Darwin about publishers. Bates also discussed neuters and described the case of *Volucella*. He discussed also his forthcoming work on mimetics.

Darwin's letter of 25 September 1861 was quoted extensively by Clodd, page xxxvi, who attributed it to 1860. The portion in double parentheses was printed in *Life and Letters*, page 380.

Of the publishers involved, Henry Colburn, Great Marlborough Street, London, published Darwin's *Narrative of the Surveying Voyages of His Majesty's Ships Adventure and Beagle between the Years 1826 and 1836*; Vol. III, *Journal and Remarks 1832-1836* by Charles Darwin, Esq., M.A., 1839; and the reprint entitled, *Journal and Researches into the Geology and Natural History of the Various Countries Visited by H.M.S. Beagle under the Command of Captain Fitzroy, R.N., from 1832-1836* by Charles Darwin, Esq., M.A., F.R.S., Secretary to the Geological Society 1840.

Darwin's other books were published by John Murray, Albemarle Street, London, over a period of nearly forty years.

V. Voorst specialized in publishing beautifully illustrated books on natural history, but went out of business.

The following statement is copied from a foot-note in *More Letters*, vol. i, 1903, page 196: 'Volucella is a fly—one of the Syrphidae—supposed to supply a case of mimicry: this is doubtless the point of interest with Bates. Dr. Sharp says, (*Insects*, Part II, in the Camb. Nat. His. Series, 1899, p. 500), "It was formerly assumed that the *Volucella* larvae lived on the larvae of the bees, and that the parent flies were providentially endowed with a bee-like appearance that they might obtain entrance into the bees nests without being detected." Dr. Sharp goes on to say that what little is known on the subject supports the belief that the "presence of the *Volucella* in the nests is advantageous to both fly and bee."'

A Bates letter, answering Darwin's of 25 September, is missing, an 'extremely interesting one & valuable references.'

12.

Dec. 3^d [1861]

Down Bromley Kent

My dear Sir

I thank you for your extremely interesting letter, & valuable references,—though God knows when I shall come again to this part of my subject.—One cannot of course judge of style when one merely hears a paper, but yours seemed to me very clear & good.—Believe me that I estimate its value most highly. Under a general point of view, I am quite convinced (Hooker & Huxley took same view some months ago) that a philosophic view of nature can solely be driven into naturalists by treating special subjects as you have here done.—Under a special point of view I think you have solved one of the most perplexing problems which could be given to solve. I am glad to hear from Hooker that the Linn. Soc. will give Plates, if you can get drawings; but I suppose they might be drawn on stone or copper.—*Pray excuse* me for again saying if

ever you want to do so, I shall be pleased to send it, for any aid in Natural History.—

Do not complain of want of advice during your Travels : A daring part of your great originality of views may be due to the necessity of self expression of thought. I can understand that your reception at the B. Museum would damp you ; they are a very good sort of men, but not the sort to appreciate your work. In fact I have long thought that *too much* systematic work of description somehow blunts the faculties. The general public appreciates a good dose of reasoning, and generalisations with new & curious remarks on habits ; final causes etc. etc., far more than do the regular naturalists.—

I am extremely glad to hear that you have begun your Travels. (I thought from Glacial Letters admirably written). I am very busy, but I shall be *truly* glad to render any aid which I can by reading your 1st chapter or two. I do not think I shall be able to correct style,—for this reason, that after repeated trials I find I cannot correct my own style till I see the M.S. in type. Some are born with a power of good writing, like Wallace ; others like myself & Lyell have to labour very hard & long at every sentence. I find it very good plan, when I cannot get a difficult discussion to please me, to fancy that some one comes into the room, & asks me what I am doing ; & then try at once & explain to the imaginary person what it is all about.—I have done this for one paragraph to myself several times ; & sometimes to Mrs. Darwin, till I see how the subject ought to go.—It is, I think, good to read one's M.S. aloud.—But style to me is a great difficulty ; yet some good judges think I have succeeded, & I say this to encourage you.—What *I think* I can do will be to tell you whether parts had better be shortened.—It is good I think to dash “in medias res,” & work in later any descriptions of country or any historical details which may be necessary.—Murray likes lots of woodcuts—give some by all means of Ants.—The public appreciate Monkeys,—our poor cousins,—What sexual differences are there in monkeys ? Have you kept them tame ? if so about their expression.—I fear that you will hardly read my vile handwriting, but I cannot without killing trouble, write better.—

You shall have my candid opinions on your MS., but remember it is hard to judge from M.S.—one reads slowly & heavy parts seem much heavier.—A first rate judge thought my Journal very poor ; now that it is in print, I happen to know, he likes it.—I am sure you will understand why I am so egotistical.—

I was a little disappointed in Wallace's Book on the Amazon ; hardly facts enough.—On the other hand, in Gosse's book, there is not reasoning enough to my taste.—Heaven knows whether you will care to read all this scribbling.—M.S. can be sent by Book Post, if marked to be printed.—Had you not better register it ?

Many thanks for Wallace's letter : he rates me much too highly & himself much too lowly.—That was an admirable paper of his in Linn. Journal.—But what strikes me most about Mr. Wallace is the absence of jealousy towards me ; he must have a really good honest & noble disposition, a far higher merit than mere intellect.

With cordial good wishes & thanks

Yours sincerely
C. Darwin

I am glad you had pleasant day with Hooker : he is an admirable good man in every sense,

This letter was reproduced in *Life and Letters*, vol. ii, pp. 378-80.

An important letter with discussion of Bates's important study on Mimetics in butterflies, which was read before the Linnean Society on 21 November 1861, and published in the Society's *Transactions*, 1862, **23**, 495-566.

Darwin refers to Wallace's letter of 24 December 1860 to Bates from Ternate, discussing the *Origin of Species* and Darwin's theory in the highest terms, and published in Wallace's *My Life*, 1905, vol. i, page 373. Wallace wrote: 'I know not how, or to whom, to express fully my admiration of Darwin's book. To *him* it would seem flattery, to others self-praise: but I do honestly believe that with however much patience I had worked and experimented on the subject, I could *never have approached* the completeness of his book, its vast accumulation of evidence, its overwhelming argument, and its admirable tone and spirit. I really feel thankful that it has *not* been left to me to give the theory to the world. Mr. Darwin has created a new science and a new philosophy: and I believe that never has such a complete illustration of a new branch of human knowledge been due to the labours and researches of a single man. Never have such vast masses of widely scattered and hitherto quite unconnected facts been combined into a system and brought to bear upon the establishment of such a grand and new and simple philosophy.'

Wallace's book is *A Narrative of Travels on the Amazon and Rio Negro*, London, 1853.

Philip Henry Gosse (1810-1888), an English naturalist, wrote *A Naturalist's Sojourn in Jamaica* (1851).

An exchange of letters between Darwin's of 3 December [1861] and 6 January 1862 is apparently missing. In this letter, Bates accepts Darwin's suggestions for the first chapter of the travel book that Bates had apparently mailed to him.

15.

King St Leicester
6 Jan'y 1862

My Dear Sir

I have now finished the rough draught of 5 chapters of my book & send you by post the 2nd for perusal according to your kind offer to do so. I do not know how I shall repay you for all this.

Your suggestions on 1st chapter shall be all carried out. Your good opinion has delighted my Father & Brother as well as myself. The recommendation to alter the *arrangement* [missing word] putting the historical sketch last is just the kind of correction that I feel most to need. As to your statement that the argument on the effects of climate on dress is original, you are certainly too generous. I find there is something to the same effect in your "Journal"

[popular ed. p 381] as my notions were suggested whilst on the Amazons the two conclusions have been derived independently & therefore my remarks will be interesting ; but I must quote your passage in a note. [letter torn, word lost] loose statement of the rocks being cretacean is derived from Mr Wallace's book ; No [word lost] was found & therefore it is useless to employ the word. I shall feel obliged to introduce a little Geology—so much as I observed which contributes to explain the *latest changes* & therefore connects itself with Geograph. Distribution of the Fauna. I must consult some Geologist before I do it.—One of my little theories is that part of the Delta of the Amazons was land probably an island whilst most of the alluvial plain was under water ; & that the Amazon waters have broken through it. I shall be careful however not to advance too much in the book.

As to explaining the " We " [yòur] remark shows me that there must [be] something very abrupt & unsatisfactory in my commencement. I will add a straight forward preface explaining the object of journey of Mr Wallace & self &c&c.,—or would you incorporate the explanation in the text of 1st chapter.

Please criticize this 2nd chapter without reserve. I am not satisfied with the arrangement of the matter nor with the argument on the text of Burmeister's remarks. But I know you will be candid with me as before.

Yours sincerely
H W Bates

C. Darwin esq.

This letter has been damaged. Words in square brackets are supplied by the author.

Bates's remarks about the effects of climate on dress refer to the colour and pattern of insects and plants and not human apparel. Bates wrote in *Naturalist*, vol. i, p. 21 : ' Mr. Darwin was led to the same conclusion many years ago, when comparing the birds, plants and insects of the Galapagos Islands, situated under the Equator with those of Patagonia and Tropical America. The abundance of food, the high temperature, absence of seasons, of extreme cold and dearth, and the variety of stations, all probably operate in favouring the existence of a greater number and variety of species in tropical than in temperate latitudes. This, perhaps, is all we can say with regard to the influence of climatal conditions.'

The explanation for the trip to South America by Bates and Wallace is given in the Preface.

Here is what Bates wrote about Hermann Burmeister (1807–1882), a German naturalist and traveller in South America, in *Naturalist*, vol. i, page 53 : ' A German traveller, Burmeister, has said that the contemplation of a Brazilian forest produced on him a painful impression, on account of the vegetation displaying a spirit of restless selfishness, eager emulation,

and craftiness. He thought the softness, earnestness, and repose of European woodland scenery were far more pleasing, and that these formed one of the causes of the superior moral character of European nations.'

16.

King St Leicester
11 Jany 1862

My Dear Sir

It grieves me very much to hear of your illness. I beg of you to throw my M.S. aside & not give a moment's thought to the subject until you are perfectly restored.

I go to town on Monday to spend a few days—to study at the B. M. & also to attend the Linn. Soc. meeting, where I shall exhibit the box of mimetic butterflies. I have arranged these in such a manner that any Naturalist may understand them

Yours sincerely
H W Bates

If you should wish to say anything whilst I am in town—a note will find me addressed to

John O'Groats Hotel
Rupert Street
Haymarket

Nothing is known of the nature of Darwin's indisposition or how Bates learned about it. It seems not to have been too severe because Darwin replied two days later.

17.

Jan 13th [1862]

Down Bromley Kent

My dear Sir

(I have been very bad for fortnight & could not read your M.S. before today & yesterday. It is, in my opinion excellent—style perfect—description first-rate (I quite enjoyed walking in forests) & good dashes of original reflexions I must write very briefly.—Remember the large sale of a Book depends much on chance,—on whether public mind occupied,—other books coming out etc., etc., etc., but I feel assured that your Book will be a permanently good one, & that your friends will always feel a satisfaction on its publication.—I will write when you like to Murray.)

Would you add a notice by simile to kind of mysterious sounds heard in forest ? but be vague & very brief in any simile, for description as it now stands is good.—Did the native look round & shrink or hide when he trembled & heard sound ? This is capital.—

Matadors very good—Better than very good.—At p. 9 ought you not to enlarge one sentence to show why the diversity of classes of Lianas interested you ? Is it because it is showing that many Families have thus become modified,—something in same way as Marsupials, Edentata, Carnivora, Rodentata, & Quadrumana have here in another manner become “climbers” ?—How are moths & sphinxes in Tropics ? Did you inquire ? Is the little Heron insectivorous ? State somewhere for me whether any of the Mammals & Birds *often* & *long* kept in confinement in native home, breed ? I like much discussion on Burmeister. I like all.—*I am very weak* & tired.—Do not think you have anything to thank me for—it has been pleasant.—Go on as you have begun & you will surely succeed.—

Yours very sincerely

C. Darwin

Hooker is much interested by what I told him about your conclusions of colours of Butterflies & Tropics.

Clodd dated this letter 1860 and quoted the portion in parentheses, page xxxvii.

Darwin was commenting on the manuscript of Bates's Chapter II, which occupies pp. 44–85 of *Naturalist*, Volume I. The mysterious sounds in the forest were vividly described by Bates (pp. 72–73), who ‘found the natives generally as much at a loss in this respect as myself. Sometimes a sound is heard like the clang of an iron bar against a hard, hollow tree, or a piercing cry rends the air. . . . With the natives it is always the Curupira, the wild man or spirit of the forest, which produces all noises they are unable to explain. . . . At one time I had a Mameluco youth in my service, whose head was full of the legends and superstitions of the country. . . . Whenever we heard any of the strange noises mentioned above, he used to tremble with fear. He would crouch down behind me, and beg of me to turn back. He became easy only after he had made a charm to protect us from the Curupira’. This was the native to whom Darwin referred.

The ‘Matador’ was a parasitic plant—Sipó Matador or Murderer Liana—of which Bates gave an excellent description (pp. 53–55). Bates's interest in the diversity of classes of Lianas (climbing and twining plants) was mentioned on pp. 48–49 of the book, which evidently corresponded to p. 9 of his manuscript, to which Darwin referred. The little heron was kept as a pet by the Brazilians, and walked about the floors of houses ‘picking up scraps of food or catching insects’ (*Naturalist*, p. 82); perhaps Bates added the information about its diet after receiving Darwin's letter.

Some insight into the situation in the Darwin household at this time is gained from referring to a letter by Darwin to Rev. J. Brodie Innes dated 24 February 1862 and published as part of the 'Darwin-Innes Letters, The Correspondence of an Evolutionist with His Vicar, 1848-1884' (*Ann. Sci.*, 1961 (1964), **17**, 211-212). Darwin wrote; '..... We have had the Influenza here very badly—16 were sick in this house and at one time six in bed. Etty keeps capital; but now we have Horace failing badly with intermittent weak pulse, like four of our other children previously. It is a curious form of inheritance from my poor constitution, though I never failed in exactly that way.'

18.

King St Leicester
25 Jany 1862

My Dear Sir

I found the returned MS & your kind letter when I came home after 8 days stay in town chiefly occupied by studies relative to the travels. I hope your good opinion of the 2nd chapter will be equally merited by the rest. I will now trouble you to write to Mr Murray as there is quite enough ready for him to form an opinion of the work.

I will not write more at the present time—The complaint which I heard in London has afflicted your family has extended to here—I am now suffering from a kind of influenza with some symptoms of an ague but it has not upset me completely at present

Yours sincerely
H W Bates

19.

Jan 31 [1862]

Down Bromley Kent

My dear Sir

I hope the enclosed note will be satisfactory to you, as far as it goes. You see that he wishes to see as much M.S. as you can let him have.—When it comes to negotiations, remember my advice about disposing only of 1st Edit. He will probably offer to take risk & give you a certain share of profit of sale. I believe it is most usually thought that Mr. Murray can be quite trusted. You must remember that as yet your name is not known to the general public, which necessarily always makes a Publisher cautious. I heartily wish you all the success which I am sure you deserve. If I can do anything whatever for you, it will give me real pleasure. In Haste

Yours very sincerely
C. Darwin

This letter apparently refers to Murray's reply to Darwin's note recommending Bates and his book. Bates's answer describing the terms offered by Murray is missing.

21.

27 [February 1862]

Down

My dear Sir

I will write again, but I do not want to lose a post to say that the terms are *very* favourable : I never heard of such terms being offered for first work.—You may depend he thinks very highly of your Book.—I have always agreed for each Edition separately (except for my Journal which I disposed of all at once for much worse terms), but I think the terms are so good, I would let Murray do as he likes.—You might say you should prefer agreeing for only one Edition. I am *heartily* glad.

Yours
C. Darwin

There was a lapse of six weeks between this letter and the following one, but the context offers no reason to believe that there were intervening letters, except that Bates most certainly answered the letter of 27 [February].

23.

April 16th [1862]

(Heading torn off)

My dear Mr. Bates

Dr. Hooker is coming here tomorrow night to stay till Monday, is there any chance of your being able to spare time to come here during this time : it would give me great pleasure if you could. If you can spare the time or want a little rest, come without writing. You must come to Bromley, Kent, by Railway ; but I am sorry to say Bromley is six miles from this house.—On Monday I could send you back to Bromley with Hooker.—I hope to get Mr. Lubbock over on Sunday or Saturday evening.—I only heard this morning when Hooker was coming or I would have given you longer notice.—A bit of rest would do you good.

Yours very sincerely
C. Darwin

Bromley is 10 miles from London & you can start from the Victoria Station, Pimlico or from London Bridge.—do *not* get out at " Shortlands for Bromley ", but at the proper Bromley station.—

24.
London

Gregory's Hotel
Rupert Street
Thursday evening
[17 April 1862]

My Dear Sir

I could not resist your invitation to the beautiful weather, although I had made other arrangements for Easter, intending to have a trip South about the middle of next week. Nothing could be more delightful to me than to visit you. I have arrived so far, en route, & intend to take an early train for Bromley tomorrow I see a train from Pimlico at 9.55—I will try that & arriving at Bromley I shall get a boy to carry my bag & walk to Down having a great desire to loiter about the Kentish lanes—the country is quite new to me

I have been very poorly lately & shall be poor company.

Yours sincerely
H W Bates

Both of these letters were written in 1862. In that year, 16 April was Wednesday, 17 April was Thursday and 20 April was Easter.

25.

King St. Leicester
30 April 1862

My Dear Mr. Darwin

I arrived here late Saturday night much fatigued. I wish to tell you what I learnt at the British Museum relative to the insects of South Temperate S. America & New Zealand. The *Carabi* of Chili & Tierra del Fuego are a remarkable case. There are 11 species known & I examined 8 of them, I have sent for a German Monograph which will tell me all about them. They form a subgenus *Ceroglossus* of Solier & as a group they are quite distinct from all other *Carabi* their nearest relationship being with S. European species. The genus *Carabus* is absent from Tropical America, N. Zealand, Australia & the Malay archipelago. I will not be quite sure before seeing the Monograph that none have any near resemblance with species of N. Temperate Zone; I believe however there is no near affinity & therefore that no *Carabus* crossed the tropics during the recent Glacial epoch. It is inconceivable that these *Carabi* should have crossed & that their near allies in the North (2 or 300 species) should have undergone an entire modification, amounting to subgeneric value since the crossing, as Dr Hooker suggested. Could the genus have originated in Chili independently? The great genus *Calosoma* nearly allied to *Carabus* but sharply distinct is almost cosmopolitan & appears of higher antiquity than *Carabus* for O. Heer, (über die fossile *Calosomen*, just out) finds many in tertiary strata of Europe & N. America & not a single *Carabus*. Could *Carabi* have segregated from *Calosoma* in the North & in the South independently? I think it highly improbable & have no doubt you think so.

What do other groups say? I was surprised at the poverty of the British Museum in Chilian Butterflies I thought it was rich. In your journal you mention flocks of many species off Patagonia I only find 3 from E. side of S. America from Buenos Ayres to Falkland islands. 46 species in all have been described. All accounts agree that Chili is poor in butterflies but still there must be more than 46 for Britain has 66. Of these 46 I found only 12 in B. M. but I referred to descriptions & was able to get a good knowledge of 13 more. These teach us much the same lesson as the Carabi with a few other things in addition. There are species *very closely* allied to European & Californian ones for instance a "meadow brown" *Epinephile Janiroides* very near our *Janira*. The genus or subg. *Epinephile* is quite unknown in tropical America but is present in California, Canada (& U.S.?) & N. Temperate zone old world, but the Chilian sp. comes nearest European sp. The genera are generally the same as N. Temperate, but the species in 6 cases form groups peculiar to Chili. In one genus the species are *very closely allied* to species of *mountainous Tropical America*. One solitary sp. is common to Chili, S. Brazil (30^d S. lat.) & Venezuela & is totally absent, genus & species, from Amazon region. 2 or 3 quite tropical species exist in middle Chili as local varieties.

The *Cicindelidæ* of which 8 are recorded confirm the above with the exception that there appears to be no species more nearly related to N. Temperate than to Tropical American sp.

The New Zealand insect fauna is wonderfully scanty I find only 7 Butterflies & 3 *Cicindelidæ* recorded all of which I examined. The *Cicindelæ* belong to a group which occurs in New Guinea but I do not know where else. All the Butterflies but one (a comⁿ Australian sp.) are peculiar to the country; one forms a peculiar N. Zealand genus The rest have a generic resemblance to those of N. Temperate zone; two of them come nearer to European species than they do to any Australian or equatorial asian but they are of such a nature that it is inconceivable how they could be modifications of Northern species which crossed the equator so recently as during the Glacial epoch.

It would be very desirable to publish in some journals a complete analysis of the insect fauna of these S. Temperate countries; but the great deficiency of our English collections makes me afraid of undertaking it. I never felt more painfully the confused state of the B. M. collections & the loose manner in which additions have been made. Nothing w^d be easier than to obtain a very large set of Chilian Insects for there are resident collectors & Continental Museums seem to get supplied. Do you think it would be worth while to analyse the Chilian Carabi & adduce confirmations *from other groups*?

I send you the references which I promised. At the B. M. one day Mr Pascoe showed me the case of Dimorphism he announced. It is in the males only—two forms of male very different in *structure*. Of course this has nothing to do with dimorphism in flowers but does it not throw light on the first origin of neuter ants,—2 forms of female?—

I set an artist to work on the plates of mimetic butterflies which it appears will be very expensive.

Please give my kind regards to Mrs. Darwin & family.

Yours sincerely
H. W. Bates

I find this precious bit in a Vienna periodical just to hand.

“Dedicamus hocce genus eximium cel. Domino Henrico Bates, Darwinianæ doctrinæ propugnatori acerrimo &c”. I don't know how the author got to known I was a Darwinian.

I am told that the vacant place at B. M. will be filled by a young man who has Prof. Owen's protection & that it will be little or no use my becoming a candidate.

The Latin can be translated : ‘We dedicate this rare genus to Master Henry Bates, keen and excellent defender of the doctrine of Darwin’

The German monograph referred to is ‘Ueber die fossilen Calosomen,’ Leonhard v. Braun, *N. Jahrb.*, 1861, pp. 52-58. There is a note scribbled in ink near the signature, perhaps in Darwin's handwriting : ‘in Portfolio Ch. 7, extract on character of (?) Beetles, Neuter forms.’ Calosoma is a genus of predaceous ground-beetles, family Carabidae. Cicindelidae is a family of active predaceous beetles, the tiger-beetles of which Cicindela is the typical and largest genus.

A scribbled note in Darwin's handwriting found with Bates's letter of 30 April 1862 reads :

‘Do not believe Carabi could have segregated—very strange not copied during glacial period—Birds just same puzzle & sea-shells—

Only caution about land shells altering so much i.e. copied in small details, yet in grand details so constant—Do not consider glacial as too short or recent : Forbes believed that it fully corresponded to a stage in Tertiary : Geology—I think it would be worthwhile—what are differences of Carabi in other parts of world : is the Chilian [word lost] distinct say than Siberian or N. American or Himalayan if there found.’

The following notes were written by Bates and evidently used in preparing the above letter. Darwin has noted on each sheet—‘H. W. Bates, May 1862’.

Wingless Coleoptera in the Caucasus Dr. Fr. Kalenato, Meletemata Entomologica, Fascic. 1.2. Petrop. 1845—(The work is advertised by Williams & Norgate—it is not in the Ent. Soc. Library, London—I have seen a very good abstract & it appears Kal. goes very deeply into the geograph. distrib. of insects in the Caucasian lands. He was sent out by the Imperial Botanic Gardens, S. Petersburg). “The snow zone begins at 1400 Klafter (8,400 ft.) and ends at 2000 Kl. (12,000 ft.?).

The insects are few, nearly all beetles, *all peculiar* to the snow zone & *all wingless*. The genera are as follows :

Platychrus	Philonthus
Feronia (omaseus	Lathrobinm
Pseudomaseus & Platysma)	Xantholinus
Nebria	Nister (K?)
Calathus	Nelops (K?)
Amara	Elmis
Frechus	Hydraena
Fachys	Ochthibius
	Meriones
	Otiorhynchus

Carabici—feeding on the Onisci,
Mysiapoda & Mollusca
of the snow region.”

It wd be desirable to know whether arctic beetles of these genera are wingless, but I cannot at present think of any treatise on the subject ?

Coleoptera of the Azores

H. Drouel has given an account of this ‘ Revue & Magazin de Zoologie ’ 1859. I could not find this work in London. I have seen only a short abstract—57 species of beetles are given all except one, have a strong European character & many are common to the Azores & Europe : one (Calathus fulviper) I have hunted out & find to be a N. European insect. A thorough analysis is desirable & could easily be made by a good European Coleopterist. The solitary tropical beetle is a common American one found throughout Brazil & W. Indies but *not* in N. America ; it presents local varieties in America, it appears well established in Azores, found in big trees in 3 islands.

[Upper left hand corner of this page is lost]

L. Cervus varies in the number of leaf-like [?] expanded joints of the antennæ from [?] to 6. This feature was always considered of specific importance & many species were founded on the other *differences accompanying*—viz. L. pentaphyllus—L. Fabiani—L. Pontbrianti & L. Turcicus—They are now believed to be all one sp. by Kraatz—Berliner Entom. Zeitschrift 1860 p 68 sq.—I notice their varieties are local being found in S. & S. E. Europe.’

26.

May 4th [1862]

Down

My Dear Mr. Bates.

Heartly thanks for your most interesting letter & three very valuable extracts. I am very glad that you have been looking at the S. temperate insects. I wish that the materials in the B. Museum had been richer, but I should think the case of the S. American Carabi, supported by some other case, would be worth a paper. To us who theorise I am sure the case is very important. Do the S. American Carabi differ more from the other species, than do, for instance, the Siberian & European & N. American & Himalayan (if the genus exists there); if they do, I entirely agree with you that the difference would be too great to account for by the recent Glacial period. I agree, also, with you in utterly rejecting an independent origin for these Carabi.— There is a difficulty, as far as I know, in our ignorance whether insects change quickly in time; you would judge of this by knowing how far chiefly allied Coleopterae generally have much restricted ranges, for this almost implies rapid change. What a curious case is offered by Land Shells, which become modified in every sub-district, & have yet retained the same general structure from very remote geological periods. When working at glacial period, I remember feeling much surprised how few Birds, no mammals & very few sea-mollusca seemed to have crossed, or deeply entered, the intertropical region during the cold period. Insects, . . .

The last portion of this letter has been lost. Fortunately, it is published in its entirety in *More Letters*, vol. i, p. 200, and the following is taken from that source :

. . . . from all you say, seem to come under the same category. Plants seem to migrate more readily than animals. Do not underrate the length of Glacial period: Forbes used to argue that it was equivalent to the whole of the Pleistocene period in the warmer latitudes. I believe, with you, that we shall be driven to an older Glacial period.

I am sorry to hear about the British Museum; it would be hopeless to contend against any one supported by Owen. Perhaps another chance might occur before very long. How would it be to speak to Owen as soon as your own mind is made up? From what I have heard, since talking to you, I fear the strongest personal interest with a Minister is requisite for a pension.

Farewell, and may success attend the *acerrimo propugnatori*.

P.S. I deeply wish you could find some situation in which you would give your time to science; it would be a great thing for science and for yourself.

Arthur William Edgar O'Shaughnessy (1844–1881) finally got the appointment. He is listed in the *Dictionary of National Biography* as a poet. In June 1861, he was appointed a junior assistant in the library

of the British Museum, and in August 1863 he was promoted to an assistantship in the zoological department. This transfer gave great offence to naturalists and was condemned by a resolution passed at a meeting of the Zoological Society.

27.

May 9th [1862]

DOWN
BROMLEY
KENT, S.E.

Dear Bates

I have been talking in London with Sir C. Lyell & he told me facts showing that there almost certainly must have been a Glacial MIOCENE period! In N. Italy he has seen huge boulders embedded with Miocene shells!—

I saw at the B. Museum the admirable mimetic case of Philippine Fugicon & Cureulio—Really wonderful.—The gentlemen at the Museum sucked in your doctrine in explanation as easily as mother's milk. I saw, also, there Mr. Baitz [?] & he showed me the genetic organs of closely allied Chrysomelidæ—on my word it is a very curious & unpleasant case, for he vows that the parts never vary.—Dr. Günther, I find, is the candidate for place in the British Museum, & I apprehend no one would have a chance against him.—I hear also with much surprise that a candidate would have to pass a civil-service examination.—Good night

I am tired—

Yours sincerely
C. Darwin

28.

King St Leicester
19 May/62

My Dear Mr. Darwin

The discovery of signs of a glacial period in Miocene times is very remarkable as I daresay you have thought, inasmuch as the whole tertiary epoch has been always considered a time of greater warmth than the present in the Northern Hemisphere. But if it turns out a cold epoch intervened in the middle of the tertiary it would only compel us to enlarge the lapse of time allowed to the whole.

I have just received the German monograph on Chilian Carabi. It is very complete. The author is of high reputation. He concludes that the 11 species form only a section of the genus although very closely allied amongst themselves & distinct as a body from all Carabi of the Northern Hemisphere. He anticipates no future discovery to modify today's conclusions on the Geographical distribution of the genus. No Carabus has been found within the tropics & none beyond the Southern tropic in Eastern Hemisphere. He goes into many inquiries arising out of the subject but strange to say does not start the question "Whence came these isolated Chilian Carabi?"

I never thought of the modifications in horny genital apparatus of closely allied Chrysomelæ being a difficulty [with the] theory. Quite the contrary it seems that they were a strong support of it. Mr Baly has found that an English Chrysomela believed on other grounds to be a [word lost] in these organs from its supp [word lost]. He therefore again separates it & there is an end of the matter. Like many other naturalists who aim only at separating species *neatly* in their collections & monographs, Mr Baly never thinks of the possibility of gradual modification and consequently never looks in Nature for it. I believe, as regards the total specific form, $\frac{3}{4}$ ths of the species in all large groups will be found, when specimens are collected over a wide area, to be incapable of sharp definition from their nearest allies. [word missing] modifications have scarcely ever been [called] for, [illegible word] in the whole specific characters of special organs. Mr Janson, an [ento]mologist tells me there is a difference [in genital] [a]pparatus, between two English [Carabi] C. hybrida & C. maritima. Now [modern en]tomologists, on the most conclusive [evidence] have re-united these two. It appears however the two are more distinct in England than on the Continent! This is what I should expect from observations on S. American insects. There is a gradual divergence amongst varieties of a species over a wide area. Who would think of examining the genital apparatus through all the graduated series of var^s of these Cicindelæ? Thanks for the copy of "orchids." I have read it through with great pleasure. It is very clear.—In Ann. Nat. Hist. for June you will see a note of mine on a new way of regarding local varieties. I have sent in an application for British Museum situation but it will be of little use. Something else may turn up soon.

Yours sincerely
H W Bates

I go tomorrow to London, for three days chiefly to see the artist about finishing the plates for Linnean Transactions. I shall call on Murray. Can I do anything for you. The address 43 Harmond St Hampstead road will find me.

The German monograph was *Ueber die neuesten entdeckungen im hohem norden, von dr. Oswald Heer, Vortrag gehalten den 28, January, 1869, auf dem rathaus in Zurich.* Druck und verlag von F. Schulthess, 1869.

This letter has been mutilated and portions of it are lost. Those parts enclosed in parentheses have been supplied by the author to the best of his ability.

On the various contrivances by which British and Foreign Orchids are fertilized by Insects was published in London in 1862.

Bates refers to 'Contributions to an insect fauna of the Amazons valley' (*Ann. Nat. Hist.*, 1861, **8**, 40-52; 147-152; 212-219; and 471-479). Darwin apparently sent Wedgwood's query to Bates but the letter is missing.

30.

King St Leicester
14 June 1862

My Dear Mr Darwin

Adjoined is a sort of answer to Mr Wedgwood's query & I shall be very glad if it prove of any use. I am truly sorry to hear of your being again more unwell than usual ; but this under the circumstances of the interruption to your work which it causes, is a national concern.

My book "progresses" it is true, but slowly. How thoroughly ashamed I am to have bragged to you how quickly I could write it. I am at it every day as many hours as I can stand ; but what takes me one day to write, takes 5 to alter.

Please do not make use of the facts about generative organs in beetles which I gave you. Conversing in London again with those gentlemen I find such a chaos of statements that the facts are not to be depended upon—

Yours sincerely
H. W. Bates

31.

DOWN
BROMLEY
KENT, S.E.

Oct 15th [1862]

My dear Mr. Bates

I want to hear a little news of you & your Book, & how you & it go on.—We have had a wretched summer & have returned home about a fortnight.—One of my poor Boys, Leonard, was fearfully ill for two months from effects of scarlet fever & on our journey to sea-side, Mrs. Darwin sickened with a fever & we were detained 3 weeks at Southington. My health has suffered considerably, but I am now slowly at work again.—

When at leisure pray let me have a line, telling me what you have been doing.—By the way the other day a Mr. Edwin Brown of Buxton sent me Proc. of N. Ent. Soc. with a letter, in which he tells me that he is working at a genealogical classification of genus *Carabus*.—In answer I told him that you had thought of something of the kind.

Pray believe me
Yours very sincerely
C. Darwin

Although four months went by since Bates's last letter, there is no reason to believe that any correspondence has been lost because Darwin complains about Bates's long silence. Darwin seems to have been sick and there was sickness in the family during much of this period, and so Bates obviously did not want to bother him.

32.

King St Leicester
17 Oct 1862

My Dear Mr Darwin

I was as pleased as a child this morning to have a letter from you again. [Mr Wallace two months ago told me of the illness in your house & knowing the distress you would be in I did not like to trouble you with letters. You do not say whether Mrs Darwin and Leonard are quite recovered.]

Regarding my book. I am thoroughly ashamed of myself after so much bragging at the beginning not to have finished the work after 12 months employed on it. I think I told you that it would be only external stimulus that would impel me on with it I felt so disinclined to write. I hoped, however, having once commenced, a liking for the task would set in but it has not been so. I have been working & bodging against inclination ever since April last. You will be glad to hear that now, 620 pages are finished out of the 700 of which the work is to consist. Two thirds of the M.S. have been delivered to Murray : after the last receipt M. writes "it keeps up to the mark." With the autumnal weather a better activity has arisen & I am writing rapidly.

I must plead a little excuse for slowness, an interruption caused by re-writing & passing through the press my memoir in *Linnean Transactions*. This took me 3 hours a day for six weeks in June to August. It fills 72 pages quarto. I took great pleasure in this. I shall get sharply scolded, I expect, for expense caused to the Society in altering proofs ; but I wanted to make the treatise worthy of the high honour of its place in the *Transactions*. It was written only as a review of species with ordinary introductory remarks for the "Proceedings." I feel now that the memoir does not express my thoughts with the force & clearness that I think I could impart were I allowed to rewrite again the whole. I shall be most anxious to have your deliberate criticism on it. I should think it is now about ready for distribution. I have matter for another better treatise on the origin of species out of local varieties.

Mr Edwin Brown is manager in a large bank at Burton. I have known him 21 years : he was my earliest Naturalist friend. I have always looked on him as a man of extraordinary intellectual ability. I have given him my notions on Carabi. He is amassing material (specimens) at a very great expense. He has never travelled : this is a great deficiency for the relations of species to closely allied species & varieties cannot, I think, be thoroughly understood without personal observation in different countries. He has very little leisure & perhaps will not be able to devote the enormous time & anxious thought to the subject which are required to work it out.

It was a damper not to see my book advertised in Murray's lists for November, December. Should you advise me to recommend him to begin printing ? The correction of proof would sooner help me to finish than hinder me. There are 3 or 4 more engravings required, which he will put in hand doubtless when he has the additional M.S. which I shall send him in 5 or 6 days. Please give my kind regards to Mrs Darwin & family.

Yours sincerely
H W Bates

This letter was reproduced by *Clodd*, lxi, in its entirety except for the portion in square brackets.

The illness in the house is partially described by Darwin in a letter (*More Letters*, vol. ii, p. 288) to Hooker from Cliff Cottage, Bournemouth, on 11 September 1862: ' . . . My wife is almost well, thank God, and Leonard is wonderfully improved. . . . Good God, what an illness scarlet fever is! The doctor feared rheumatic fever for my wife, but she does not know her risk. It is now all over.'

33.

Oct 18 [1862]

DOWN
BROMLEY
KENT, S.E.

My dear Mr. Bates

Many thanks for your very friendly note.—I am not *at all* surprised at the turn which your Book has taken. Everyone finds the same thing. It is dull work, but must be borne. People differ about being hurried by Proofs: I hate it, so shd recommend you not to be in too great a hurry. Murray would be an excellent adviser when you had better begin. After M.S. sent in, proofs seldom arrive for first fortnight, & then the flow is steady. I am most anxious that yours shd be a real good Book; so do not hurry over proofs—; yet I have known some very fine men correct too much, & flatten their style.—I find it good to correct in pencil & read aloud, & if it sounds well, not to plague more over it. But you write a capital style.

I was very glad you have finished paper for Linn. Soc. When I go to London, I must enquire for the Post; for it is not sent till enquired for. You need not ask me to read it carefully; I shall be sure to do that.—Not having been in London I have not seen a paper which you told me would appear in *Annals*. I am glad to hear about Mr. Brown: I thought he was something remarkable. Mrs. Darwin & my Boy are well; but my children are cause of never ending anxiety to me. This is a stupid note, but I am tired with a long day's work. With hearty good wishes for your work through the press.—

Dear Bates

Yours very sincerely
C. Darwin

34.

Nov 20 [1862]

Down Bromley Kent

Dear Bates

I have just finished several reads of your Paper. In my opinion it is one of the most remarkable & admirable papers I ever read in my life. The mimetic cases are truly marvellous & you connect excellently a host of analogous facts. The illustrations are beautiful & seem very well chosen; but it would have saved the reader not a little trouble if the name of each had been engraved

below each separate figure ; no doubt this would have put the engraver into fits, as it would have destroyed beauty of Plate. I am not at all surprised at such a paper having consumed much time. I rejoiced that I passed over the whole subject in the Origin, for I shd have made a precious mess of it. You have most clearly stated & solved a wonderful problem.—No doubt with most people this will be the cream of the paper ; but I am not sure that all your facts & reasoning on variations & on the description of complete & semi-complete species is not really more, or at least as valuable a part.—I never conceived the forces nearly so clearly before ; one feels present at the creation of new forms.—I wish, however, you had enlarged a little more on the pairing of similar varieties ; a rather more numerous body of facts seems here wanted. Then again what a host of curious miscellaneous observations there are,—as on related sexual & individual variability you give ; these will someday, if I live, be a treasure to me.—

With respect to mimetic resemblance being so common with insects : do you not think it may be connected with their small size ; they cannot defend themselves ;—they cannot escape by flight at least from Birds ; therefore they escape by trickery and deception ? I have one serious criticism to make & that is about title of paper ; I cannot but think that you ought to have called prominent attention in it to the mimetic resemblances.—Your paper is too good to be largely appreciated by the mob of naturalists without souls ; but rely on it, that it will have *lasting* value, & I cordially congratulate you on your first great work. You will find, I shd think, that Wallace will fully appreciate it.—How gets on your Book ?—Keep your spirits up. A Book is no light labour. I have been better lately & working hard ; but my health is very indifferent. How is your health ? Believe me Dear Bates

Yours very sincerely
Ch. Darwin

Excuse my poor M.S. paper.—

This letter is printed in *Life and Letters*, vol. ii, pp. 391–393, and in *Clodd*, xlv.

This letter and the following one refer to ‘ Contributions to an Insect Fauna of the Amazons Valley ’, *Trans. Linn. Soc.*, 1862, **23**, 495–566, in which the now familiar subject of mimicry was founded. The paper was read on 21 November 1861.

35.

King St Leicester
Nov 24 1862 [?]

My Dear Mr Darwin

I am now well repaid for the labour of writing the paper in Linn. Trans. having received your warm & discriminating approval. Dr Hooker has also written a most friendly letter on the subject valuing the treatise highly. Mr Wallace & I discussed the matter last time he was in London & he thinks me entirely right. As for the “ mob ” of Naturalists, I don’t care much about

them. There is only one man in Europe who will be able to follow my reasonings species by species & variety by variety ; this is Dr C. Felder of Vienna & I shall be glad to hear from him upon the subject. As to ordinary Entomologists they cannot be considered scientific men but must be ranked with collectors of postage stamps & crockery.

I wish to have more criticisms from you on the subject, when you are quite at leisure (what an idea !).—I am quite sure I could now put the case much more strongly. But you say the argument is quite clear so I must rest contented. I now believe one of the forms of *Leptalis* (*L. argochloe*) is not an immediate descendant from *L. Theonoe*, as the other varieties are, but has originated (in a similar way) somewhere Westward in Andean Valleys and has wandered to the Amazons. I would be tedious here to give reasons. Does it occur to you that a great deal may be unexpectedly learnt by thus thoroughly going into one small group of natural objects ? The more I study them the more I am surprised at the wonderful revelations which spring from them : much more than is explained in my treatise.

There is a foot-note to this letter of 24 November 1862, which is written at the head of the paper : ‘ I am vain enough to wish to see the paper noticed in some widely circulated publication, thinking it may introduce my book. Do you know anything of Sir J. Herschel or his address ? I hear he writes those capital monthly summaries of science in the “ Cornhill ” & would like to send him a copy.’

The rest of the letter has not been found. Bates referred to Baron Cajetan Felder (1814–1894), jurist, statesman and entomologist.

36.

Nov. 25 [1862]

Down Bromley Kent

Dear Bates

I should think it was not necessary to get a written agreement.—I have never had one from Murray. I suppose you have a letter with terms ; if not, I shd think you had better ask for one to prevent misunderstanding.—I *think* Sir C. Lyell told me he had not any formal agreement.—I am heartily glad to hear that your Book is prospering. Could you find some place, even foot-note (though these are in nine cases out of ten objectionable) where you could state, as fully as your materials permit, all the facts about similar varieties pairing,—at a guess how many you caught, how many noted in your collection.—I look at this fact as very important ; if not in your book, put it somewhere else, or let me have cases.—

I entirely agree with you on enormous advantage of thoroughly studying one group.—I shd doubt Sir J. Herschel reading or reviewing nat. History : his address is “ Collingwood Hawkhurst, Kent ”.—

I have already drawn Asa Gray's attention to your paper ; but I fear it is out of his line, as he contributes only Bot. Reviews to N. American Journal.—I will see whether a suggestion to one of Editors of the Nat. Hist. R. will do any good ; but as you are aware it is very unusual to review papers.—I wish I had spare strength or time to review your paper ; but in truth I have lost several months of time this whole summer, that I must work on my own work when well enough.—

I really have no criticism to make ; style seems to me very good & clear ; but I much regret, that in title or opening passage that you did not blow loud trumpets about what you were going to show.—Perhaps paper would have been better more divided into sections with Headings.—Perhaps you might have given somewhere rather more of a summary on the proof of segregation of varieties & not referred your readers to the descriptive part, excepting such readers as wanted minute details.—But these are trifles : I consider your paper as a most admirable production in every way. Whenever I come to variation under natural conditions (my head for months has been exclusively occupied with domestic varieties), I shall have to study & restudy your paper & no doubt shall then have to plague you with questions.—I am heartily glad to hear that you are well.—I have been compelled to write in a hurry, so excuse me, & believe me,

Yours very sincerely
C. Darwin

This letter is published in *More Letters*, vol. i, pp. 214-5. See Bates's letter of 25 January 1862, page 20, asking Darwin to write to Murray ; also Darwin's letter of 31 January.

A letter from Bates mentioned by Darwin on 15 December is missing.

38.

Dec. 15th [1862]

DOWN
BROMLEY
KENT, S.E.

Dear Bates

Many thanks for the paper & references on pairing of var., a subject which interests me much.—If I do not hear to contrary I will keep the paper sent, though I do not see in it much on pairing of varieties.

I would not on any account give you trouble to copy the passages out of Zoologist ; when I want the subject I can consult the volume in London.—I am sorry for all your trouble & delay about artists etc ; but it is a law of nature that they will give trouble & it is of no use fighting against a law of nature.

Good luck to you.

Yours very sincerely
C. Darwin

By any chance did you ever come across Indians badly pressed by famine, who were then compelled to cook in *new* ways & try *new* vegetable productions ?—If by any odd chance you have, I shd be glad to hear ; otherwise do *not* write to say that you do not know.—

Bates probably answered these questions, conscientious as he was on all such matters, but his letter is missing.

40.

Jan 12th [1863]

DOWN
BROMLEY
KENT, S.E.

Dear Bates

Would like to say that I have just heard from Asa Gray, that if he had a copy of your paper he would *endeavour* to get Prof. Haldemann to review in Silliman's Journal of Science. *If* you have a spare copy and think it worth chance, post it by Book Post to :

Prof. Asa Gray
Cambridge
Massachusetts
U. States

I may as well tell you, as you will be sure to find out from resemblance to my letter that I have sent little Review of your Paper to Nat. Hist. Review.— But I have reason to believe Editors will modify some parts.— If you send a copy to Asa Gray tell me ; otherwise do not trouble yourself to write.—

By the way here is question sometime for you to answer, *if you can*, Do Bees or Lepidoptera visit flowers of Melastomatae ; if you shd remember what genera of plants, please state ; on a mere chance whether you can answer I know. I hope Book prospers

In Haste
Yours
C. Darwin

This refers to Bates's article on mimetic analogy, which Darwin reviewed in the *Natural History Review*, 1863, p. 219, and Gray reviewed in the *American Journal of Sciences and Arts* (Silliman's Journal), November, 1863, **86**, 285–290.

41. A letter dated between 15 and 26 January 1863 is missing, but a postscript to it has been preserved and is presented here.

P.S. I have committed the folly of getting married & the lady & self have both a strong desire to live in or near London, I am going to try hard to get employment to add to my small independent income sufficient to live here & devote all time to science

First vol. of book nearly finished printing. All illustrations done but 3 or 4 & the map which require 4 or 5 days more. M.S. finished except a few lines to the last & toughest discussion & preface. I am told 1250 copies to be struck off but do not know if this be correct. The illustrations are extremely beautiful I have watched over nearly all and some of them are quite in an original style for books of travel

The marriage certificate in Somerset House shows that Bates, then aged 37, was married in London on 15 January 1863 to Sarah Ann Mason, aged 22. The year of the marriage is incorrectly given as 1861 in *Clodd* and the *Dictionary of National Biography*.

42.

Jan 26 [1863]

Down Bromley Kent

Dear Bates

I congratulate you very sincerely on your marriage. Judging from my own experience it is the best & almost only chance for what share of happiness this world affords. I hope you may succeed, for the sake of science, in getting fixed near London.—

I am heartily glad to hear that your great labours over your Book are drawing to a close ; I know that I for one shall read it with real interest.—Pray thank Wallace, when you see him about Melastomas : I have in truth given more trouble on this, than case deserves, & am truly obliged to you. The fact is I cannot endure being beaten by a beggarly flower to the degree, which these confounded Melastomas have beaten me.—I am pleased to hear of discussion on species at Forbes too.—With my renewed congratulations & hopes for your happiness.

Believe me
Yours sincerely
Ch. Darwin

43.

Down Bromley Kent
Mar 4 [1863]

Dear Bates

I heard from Asa Gray a week ago, & must have the pleasure of sending you a copy of part of his letter. "The copy of Bates's paper addressed to me by the author has come. I am by no means clear that I shall pass it on to Haldemann, certainly not just yet. I have run thro' it. It is *fascinating*. He at least is a full believer in Nat. Selection & the illustrations he brings to your aid are the prettiest possible. I keep it in pickle along with Decandolle on oaks & some other matters to make the subject of the evening for our Scientific Society which meets with me a fortnight hence, when I hope to torment Agassiz delightfully, and then I will myself draw up an abstract of the interesting matter for Silliman, but it is too late for the March number."

I have been rather extra unwell for the last ten days so write this by dictation. [The rest is in Darwin's handwriting.] Don't trouble yourself to acknowledge this. I hope Book goes on well.—

Yours sincerely
C. Darwin

The first part of this letter is in Francis Darwin's hand writing.

Gray refers here to Alphonse Louise-Pierre Pyramus de Candolle (1806–1893). Gray delighted in baiting Agassiz for his refusal to accept Darwin's theory.

44.

King Street
Leicester
8 April 1863

My Dear Mr Darwin

I have been very busy for the last 10 days with all the little details connected with a change of residence & am now in Leicester packing up my collections & library for removal to London. This will account for my not writing before to thank you for the very beautiful review of my essay in the "Natural History review". By this days post I send a copy of my book of travels for your acceptance. I finished all my work connected with it 6 weeks ago & am now quite tired of it, the delays having been so numerous & tedious. I found I had written for about 200 pages more than was necessary & so was obliged to leave out much matter including a long, unfinished account of origin of species by segregation of races in support of your theory.

Mr Murray seems very slow to push my book, friends here complaining that it has never been properly advertised. The expenses were very great (over 700£) & the price in consequence is fixed high. Very few copies will be sent to reviewers so that there will be slow progress. Your most well-timed remarks at the end of your little review will be the best advertisement I have had or expect to have.

Yours sincerely
H W Bates

I remain here till the 14th

45.

April 18th [1863]

DOWN
BROMLEY
KENT, S.E.

Dear Bates

I have finished Vol. I. My criticisms may be condensed into a single sentence, namely that it is the best book of Natural History Travels ever published in England. Your style seems to me admirable. Nothing can be better than the description on the struggle for existence & nothing better than the description on the Forest scenery. It is a grand book, & whether or not it sells quickly it

will last. You have spoken out boldly on Species ; & boldness on this subject seems to get rarer & rarer.—How beautifully illustrated it is. The cut on the back is most tasteful. I heartily congratulate you on its publication.—The Atheneum was rather cold as it always is, & insolent in highest degree about your bending facts.—Have you seen the “ Reader ” : I can send it you, if you have not seen it.—I have got spare copies of my article in N. Hist. Review, which I will send you on Monday & you can use or burn them as you like. I look forward with much pleasure to Vol. 2.—

Yours very sincerely
Ch. Darwin

I will keep copies of Review till I know your London address.

Reproduced in *Life and Letters*, vol. ii, p. 381.

The ‘ cut on the back ’ is a mystery to the author, because his copy of the first edition has been beautifully bound in calf skin and the original cover has been lost !

46.

22 Harmond Street
Haverstock hill N.W
April 20 1863

My Dear Mr Darwin

I have at length returned from Leicester & the above will be now my address for a long time. Your last note to me at Leicester stated you had just received my book & were commencing to read. I hope now you have finished & are ready to pronounce sentence. With regard to the other matter you mention, namely my prospects ; I must be open with you as I have been heretofore. My total income £123 £100 of which is allowed to me by my brothers on account of my withdrawing from their small business (which was not large enough to support 3 of us) & leaving £1000 of my capital on loan, the other £23 is interest on shares.

Now I hope to add by scientific or literary work sufficient to enable me to live modestly. I should consider myself well off if I could gain an additional £150.

There is perhaps some slight chance of my getting in at British Museum which of course would fix me for life. I can get plenty of Entomological work from private persons but it is of a tedious, mechanical nature & would prevent me from undertaking original research.

The house I have taken is a very *small* one, but retired & semi-rural. Mrs B. is a plain domesticated woman so there you have it all.

One of my reviewers “ hopes that I have made my expedition answer in a pecuniary point of view ” It certainly did not answer for the total savings of my 11 years work did not exceed £800

Yours sincerely
H W Bates

I go to Murray today to ascertain the trade result of my book

C. Darwin
at Rev. C. Langton's
Hartfield
Tunbridge Wells

April 30th [1863]

Dear Bates

You will have received before this the note which I addressed to Leicester, after finishing Vol. I, & you will have received copies of my little review of your paper. By the way I heard yesterday from Asa Gray that his article on same is delayed till next number in Silliman's Journal.—I have now finished Vol. 2, & my opinion remains the same ; that you have written a truly admirable work, with capital original remarks, *first-rate* descriptions, & the whole in a style which could not be improved. My family are now reading the book & admire it extremely ; & as my wife remarks, it has so strong an air of truthfulness.—I had a letter from a person the other day, unknown to you, full of praise of the book.—I do hope it may get extensively heard of & circulated ; but to a certain extent this, I think, always depends on chance.

I suppose the clicking noise of surprise made by the Indian, is that which the end of tongue applied to palate of mouth & *suddenly* withdrawn makes ?

I have not written since receiving your note of April 20th, in which you confide in me & tell me your prospects. I heartily wish they were better & especially more certain ; but with your abilities & powers of writing it will be strange if you cannot add what little you require for your income.

I am glad that you have got a retired or semi-retired situation. What a grand ending you give to your book contrasting civilization & wild life ! I quite regret that I have finished it : every evening it was a real treat to me to have my half hour in the grand Amazonian forest, & picture to myself your vivid descriptions. There are heaps of facts of value to me in a Nat. Hist. point of view.—It is of great misfortune that you were prevented giving the discussion on Species.—But you will, I hope, be able to give your views, & facts somewhere else. Once again I congratulate you & believe me

Yours very sincerely

C. Darwin

We shall stay here till Wednesday & then move for a week to J. Wedgwood's Esq.

Leith Hill Place

Dorking

Surrey

We have come for change for my Boy & own health-sake.

This letter was published in *More Letters*, vol. i, pp. 240-241 and also by *Clodd*, p. xliii.

May 2 [1863]

My dear Mr. Darwin

I have received both your letters containing remarks on my book & need not say how well satisfied I feel with them. From all quarters, strangers & friends, the opinions are favourable. It is most curious ; all find pleasure in the book,

Darwinians, Calvinistic church ministers, Dissenting parsons, hard-headed men of business; women, old men & boys; philosophic naturalists & species grubbers. It makes one feel contented to be the author on the score of literary fame alone but more substantial rewards are not wanting for Mr Murray has given me a handsome sum (£250) although the edition (1250) is far from sold out. He has behaved in the most friendly manner to me the friendliness visibly increasing every day. I cannot forget that all this is due primarily to yourself who gave me so favourable introduction to a publisher. Not only that, if it had not been for your spurring me on I am quite sure the book would never have been written. As long as I live I shall remember these things & wish it were within the limits of etiquette to make them public. Murray said to me on Wednesday "I am very glad I am the publisher of the book".

Your former letter in which was a quotation from a letter of Asa Gray's gave me much pleasure I find there is no end to the instruction that may be drawn from these cases of mocking butterflies: every time I reflect on them some new deduction flows forth, so that I think I have matter enough for another paper which might be illustrated by the same plates. Mr Wallace came last Sunday to spend the evening with me & after examining all my specimens of these mockers, came to the conclusion that all Nature does not furnish so plain & striking a case of the origin of species & of new & complex adaptations to new conditions, by the simple process of variation & Natural Selection. But my first task will be to write a paper on the general subject of origin by segregation of local races.

Believe me

Yours sincerely

H W Bates

49.

22 HARMOOD ST HAVERSTOCK HILL N.W.
29 Sept 1863

My dear Mr. Darwin

I am very much concerned to hear of your increased illness; but I expected it having been told by various persons throughout the summer that you were not so well this year. Let us hope that Malvern wells will give you a little relief so as to enable you to work cheerfully & correspond with your friends again. If it would not be annoying I should like to send you a letter with Natural History chit chat now & then, regardless of your answering me punctually.

Many thanks for the loan of Asa Gray's reviews. Really my little paper in the Linnean has been greatly honoured to be reviewed by Darwin & Asa Gray. How capitally Gray has done it. I did not think it possible that the complex details of variation &c which I gave could be so briefly and luminously brought to a focus. I think I must write to him, to thank him.

[The review in the Times of my book has caused quite a commotion. I consider it the best that has yet been written. It is also of great general importance because it is a public concession on the part of the highest literary tribunal, of the claims of philosophical natural history to the attention of the public. My old Father happens to be on a visit to me & the review came very apropos, causing great elation in our little family circle. My Father is an old man of business who thinks everything right that is said by the Times; & who begins now to see that his son really has written a goodish book.

The longest review that has yet appeared is in the *Revue des deux mondes* by Forgues. It is also most excellently done & I think shows a closer examination & higher appreciation of the book than any that has yet appeared in England except that of the *Times*.

You ask me what I am doing. I have been commissioned by Mr W. Wilson Saunders to write a Monograph of the Mantidæ (a remarkable family of Insects). The work is to appear in the Ray Society series, in 4to, illustrated by 20 plates by Westwood. This has occupied me the last 4 months & will continue to occupy me for 18 months longer. Mr Saunders pays me (moderately) for the work & leaves me all the credit. This work leaves me time for other things, such as short articles & I had commenced one on the whole subject of local variation, intending to incorporate details of facts of new varieties interbreeding with counterparts, which you require. But the Monograph is so much pleasanter work just now that I have laid this paper aside for the present. I will not write more just now]

Yours sincerely
H W Bates

The portion of this letter enclosed in square brackets was published in *Clodd*, p. lxx. Darwin's health was poor at this time. The following letter shows that Darwin told Bates about his illness in a letter that is missing.

51.

22 HARMOOD ST. HAVERSTOCK HILL N.W
24 Oct 1863

My dear Mr. Darwin

I just send you a line to say that I posted the tract containing Asa Gray's criticisms immediately after receiving your note. The sad news conveyed in your letter has caused great sorrow to myself & those of our friends to whom I have communicated it. Of course much correspondence is out of the question until you are better therefore several small matters I wished to mention must remain for another time.

Yours sincerely
H W Bates

Francis Darwin states in *Life and Letters*, vol. iii, pp. 1-2 : 'The work was more than once interrupted by ill-health and, in September (1863), what proved to be the beginning of a six months' illness forced him to leave home for the water-cure depressed, in spite of the hopeful opinion of one of the most cheery and skilful physicians of the day. Thus he wrote to Sir J. D. Hooker in November : "Dr. Brinton has been here (recommended by Busk) ; he does not believe my brain or heart are

primarily affected, but I have been so steadily going downhill, I cannot help doubting whether I can ever crawl uphill again. Unless I can, enough to work a little, I hope my life may be very short, for to lie on a sofa all day and do nothing but give trouble to the best and kindest of wives and good dear children is dreadful.” ’

This is the end of Part I and the beginning of a lapse in the correspondence for fifteen months, due largely to Darwin's ill-health. It also concludes most of the discussion about Bates's book. Bates's habits began to change materially about this time when he went to the Royal Geographical Society.

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THE DARWIN-BATES LETTERS CORRESPONDENCE BETWEEN TWO NINETEENTH-CENTURY TRAVELLERS AND NATURALISTS

Part II

By ROBERT M. STECHER, M.D.*

The Darwin-Bates Correspondence

A list of the letters published here, noting missing letters supposedly written that have not been found. Letter 54 has previously been published by Clodd. For the full reference, see Part I of this paper.

Bates's Letters	Darwin's Letters
53. 28 January 1865. <i>Clodd.</i>	52. Missing.
55. 22 March 1865.	54. Missing.
56. 29 March 1865.	57. Missing.
59. Missing.	58. 26 April [1866].
61. 19 December 1866.	60. 18 December [1866].
62. 11 March 1867.	
63. Missing.	64. 30 March [1867].
65. Missing.	
	66. Missing.
67. 18 February 1868.	68. 19 February [1868].
69. 21 February 1868.	
70. 26 February 1868.	
71. 12 March 1868.	72. 18 March [1868].
	73. 15 April [1868].
74. 20 April 1868.	75. 22 April [1868].
	76. Missing.
77. 20 May 1868.	78. 21 May [1868].
79. 28 May 1868.	80. Missing.
81. 10 September 1868.	
82. 29 September 1868.	83. Missing.
84. 8 June 1869.	
85. 17 January 1870.	

* Department of Medicine, Western Reserve University Medical School, Cleveland, Ohio.

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|-----------------------|-------------------------|
| 86. 20 May 1870. | |
| 87. 15 November 1873. | 88. 16 November [1873]. |
| | 89. Missing. |
| 90. 7 February 1874. | |
| 91. 1 October 1874. | 92. Missing. |
| 93. 11 January 1877. | 94. 6 February 1877. |
| 95. 25 January 1879. | 96. January 1879. |
| | 97. [December 1880]. |
| | 98. 3 January 1881. |
| | 99. 7 January 1881. |
| | 100. 17 February 1882. |

THE Darwin-Bates correspondence has been divided into two parts for convenience. Part I, from November 1860 to October 1863, has been already published in this journal (*Ann. Sci.*, 1969, 25, 1-47).

Part II resumes after a lapse of fifteen months. Bates during this period probably was very busy adjusting himself to his new duties as Secretary to the Royal Geographical Society and doing free-lance writing. This was a period of relative inactivity for Darwin. He published no new books between *On the various Contrivances by which British and Foreign Orchids are fertilized by Insects* (London, 1862) and *The Variation of Animals and Plants under Domestication* (London, 1868) and only ten short papers. It is recorded in *Life and Letters* (vol. iii, p. 1) that a six-months' illness, beginning in September 1863, forced him to leave home for a water-cure at Malvern and he remained ill for some time after.

The correspondence was reopened by Darwin, but his letter has not been found.

53. ROYAL GEOGRAPHICAL SOCIETY
15, WHITEHALL PLACE, S.W.
Jan 28 1865

My dear Mr Darwin

The receipt of a letter from you was an unlooked-for pleasure. I have had news of your health from time to time, having seized all opportunities of asking from persons likely to know, & had heard lately of your slight improvement. Let us hope it may continue.

You are very kind to enquire after my personal affairs. I have no doubt Dr Hooker has kept you well acquainted with what is done & said in Natural History circles & the perusal of the Journals &c keeps you well-informed about

the rest. Perhaps there may be a few Entomological items bearing upon Darwinian views, which have not yet fallen in your way. I was much gratified on receiving the Berlin "Bericht für Entomology 1862" (you know the Natural History reports appear in Wiegmann's Archives) to find at the very commencement a flattering notice of our paper on Mimetic Butterflies. These reports originally were written by Erichson & are now written by Gerstaecker. They are usually very skilfully & not very mercifully done. Gerstaecker has seized all the essential points of my paper & repeats them with an evident bias in their favour. Being the highest Entomological tribunal I think you will like to have the testimony of this "Bericht" to the absence of, at any rate, any important errors in my facts & arguments.

You will be glad to hear that I like my present position very much. I should have preferred a Natural History appointment but I had no chance of one & the birth of our sweet little child with expectations of another forced upon me cogent arguments for accepting the first thing that offered. I hope besides to do a little in improving this great society & assisting Naturalists in travelling. The Mantida monograph progresses, but about this I will speak in a future letter.

My dear Mr Darwin
Yours sincerely
H W Bates

This letter was published in *Clodd*, page lxxvii.

'Wiegmann's Archives' was 'Archiv für Naturgeschichte' (Berlin), edited by A. F. A. Wiegmann for six years, then by Wilhelm Ferdinand Erichson, and later by Karl Eduard Adolf Gerstaecker (1828-1895), zoologist.

The Mantida monograph was 'Descriptions of Fifty-two New Species of Phasmidae from the Collection of Mr. W. Wilson Saunders with remarks on the Family' by Henry W. Bates. This was read before the Linnean Society on 15 June 1865 and published in *Trans. Linnean Society*, 1866, **25**, 321. The subject was mentioned in Bates's letter dated 29 September 1863. He wrote: 'You ask me what I am doing. I have been commissioned by Mr. W. Wilson Saunders to write a Monograph of the Mantidie (a remarkable family of insects). The work is to appear in the Ray Society Series in quarto illustrated by 20 plates by Westwood. This has occupied me the last 4 mo and will continue to occupy me for 18 months longer. Mr. Saunders pays me (moderately for the work and leaves me all the credit).'

A letter from Darwin mentioned in the next communication has not been found.

55.

ROYAL GEOGRAPHICAL SOCIETY

15, WHITEHALL PLACE, S.W.

March 22 1865

My dear Mr Darwin

It gave me great pleasure this morning to see a letter in your handwriting showing that you had recovered your previous tolerably good state of health. The news will be welcome to all our friends when I tell them.

Pray do not send your copy of the Heliconidæ paper as I have still a few left & will send one to Mr Walsh, to whom I am indebted for copies of his papers lately arrived. I have read every line of his with great pleasure,—just a little deadened perhaps by the diffuseness of his style & want of closeness in his otherwise just reasoning.

I will make use of this opportunity to tell you of a small discovery of mine lately made in working out the species of a very long genus of Longicorn Coleoptera. It is with regard to the abrupt & profound modifications in sexual parts between *very* closely-allied species. You will perhaps recollect my telling you some time ago of a series of dissections made by a friend of mine of the male organs in Chrysomelidæ, he having found great differences from species to species & even separated what were previously considered to be varieties, on the ground of modifications in these organs. In my Longicorns the parts in question are the *accessory* organs, that is the terminal abdominal segment from which the organ protrudes when necessary. The genus is Colobthea—one of those genera of which so many exist in all orders, in which the species seem to have been endlessly multiplied by nature. Nearly all the species can be distinguished by the form of the accessory organs in the sexes & I have found most profound modifications in what would be otherwise considered as *local* varieties. I have no doubt whatever that the species on dissemination over a wide area & breaking up into local varieties undergo modifications of these organs very readily & that this fact has operated greatly in the multiplication of species in nature for it is difficult to conceive the variously formed males of these closely-allied races to be equally adapted to their own females & to the females of their sister races. There is a physical obstacle here in the way of amalgamation with the parent or sister forms of segregated local varieties or races. I shall publish my notes on the subject in the Annals & Mag. Nat. Hist. in the course of my papers on Amazones Longicornes.

Yours sincerely

H W Bates

I am expecting "cartes" by every post from my photographer & will send a pair on their arrival

Benjamin Dawn Walsh (1808–1849) is quoted extensively in the *Descent of Man* on entomological subjects, particularly in regard to sex ratios and sexual characteristics.

The article referred to is Bates's paper on Mimetics, 'Contributions to an Insect Fauna of the Amazon Valley. Lepidoptera: Heliconidæ' (*Linn. Soc. Trans.*, 1862, **23**, 495–566).

Mr. G. R. Crone, Librarian, the Royal Geographical Society, London, suggests that 'Cartes' refers to a type of small photograph, popular at that time, not much larger than a visiting card or *carte de visite*. He states that the Society has a photograph of Bates, 4" × 2½" in size, which could be a copy of the one in question. It is not dated, but shows him as a younger man than in his other pictures. Bates would have been about forty years of age at that time.

56. ROYAL GEOGRAPHICAL SOCIETY
15, WHITEHALL PLACE, S.W.
March 29 1865

My Dear Mr Darwin

Enclosed is the promised portrait. Will you do me the favour to send me yours for my album; the larger one you gave me in 1862 now hangs up in my study.

I sent a copy of "Mimetic Butt^s." to Mr Walsh. We are overwhelmed here with North Pole business.

Yours sincerely
H W Bates

Darwin probably acknowledged Bates's photograph, but his letter is missing.

The 'North Pole business' refers to negotiations for an Arctic expedition. The Admiralty rejected the proposals made at this time and it was not until ten years later that the expedition, under Captain Sir George Nares, R.N., set out on the British Arctic Expedition of 1875-76. A portion of the North Greenland Coast was explored and a sledging party struggled to 83° 20' 26" N. Nares received the Founder's Medal, R.G.S., in 1877. He had been recalled from command of the famous Challenger to lead this expedition. Owing to an outbreak of scurvy this expedition accomplished much less than had been hoped for.

58.

[right corner of letter torn off]

April 26th [1866]

My dear Bates

I shd be greatly obliged to you if you could answer 4 questions.—Perhaps Mr. Rye wd be so very kind as to aid you or undertake the task. Prof. Victor Carus is translating into German my book on Coral Reefs & says he cannot find the proper Title & Date of publication of some books to which I refer (left corner torn away) & which I read some 30-40 years ago at the Geographical

Soc. or at the Admiralty. I refer to a "Memoir on the Pacific, appendix 1835 by Adm. Krusenstern." It is clear that I here refer *in English* to some book: what is real Title?—Secondly, I frequently refer to "Freycinet's Hydrographical Memoir", & V. Carus asks, "is this Vol 8th of his Voyage autour du monde sur l'Uranie et la Physicienne?" I cannot remember, but rather think there is a separate Hydrographical work by Freycinet.—

Thirdly I refer to the charts of the Natura & Anambas Islands published in the "Voyage of the Favourite", & V. Carus asks whether I refer to La Place's "Voyage de la Favorite"? Do you know of any voyage of the Favourite? I think that I remember consulting La Place.—

Fourthly & lastly was there a voyager of the name of *Surville* I refer to such in reference to Solomon Isl.—V. Carus thinks it is misprint for D'Urville; which I do not believe. Cans you *forgive* me & aid me?

Yours most truly
Ch. Darwin

Julius Victor Carus (1823–1903) was a German zoologist and zoological writer, editor of the *Zoologischen Anzeiger* and translator of Darwin's works. *The Structure and Distribution of Coral Reefs* (1842) appeared as part of *Charles Darwin's gesammelte Werke* (1876–1878). No separate mention of a German translation has been found.

Admiral Adam Johann Krusenstern (1770–1846) was a Russian traveller, admiral and hydrographer in England. His book has not been found in the British Museum (Natural History).

Louis Claud Desauîses de Freycinet (1779–1842) was a French navigator and author of *Voyage de découvertes aux Terres Australes* and *Voyage autour du monde exécuté sur l'Uranie et la Physicienne* (names of the corvettes) during 1817–1820. There are two volumes on *Navigation and Hydrographie* (Paris, 1826).

Natura Besar is an island between Malaya and Borneo. Anambas is in Indonesia.

J. F. M. de Surville (1717–1770) and J. S. C. Dumont d'Urville (1740–1842) were navigators, mentioned in Darwin's *Coral Reefs* on pp. 63 (note) and 90 respectively.

Bates must have answered this letter and there may have been more correspondence between this letter in April and Darwin's next in December.

60.

Dec. 18 [1866]

Down Bromley Kent

Dear Bates

Will you kindly do me a favour?—Mr. Blyth tells me that in 23 Vol. of your R. G. Journal, there is a letter from Commander A. Butakoff to Sir R. Murchison, in which the tameness of the deer on recently visited Isle (?) in Lake Aral is

mentioned. Now if the paper is quite short, will you have it copied for me, with the man's name—proper volume, date of vol. & page.—If passage is long, can you send me Vol. by Post & it shall soon be returned, as I want it soon.—Forgive me troubling you and believe me

Dear Bates

Yours very sincerely

Ch. Darwin

'Mr. Blyth' was probably Edward Blyth (1810–1873), who was interested primarily in mammology and ornithology. Commander, later Admiral, Alexis Boutakoff's statement on the subject, August 19 (31), 1852, is quoted in full: 'The largest of these islands, Nicholas the First, is covered with steppe-wood (saksaul, *anabasis ammodendron*, a piuris orientalis, jonquil, etc.) and its only inhabitants were innumerable Saigaks, a sort of antelope. There were no vestiges of human beings, and the best proof that none have ever been there was the circumstance that the Saigaks, generally very timid and watchful, did not fly from us, but, on the contrary, looked at us with a sort of curiosity. Such confidence could not last long: and, after two months difficult navigation, living on salt food, exposed to constant heat and hard labor, we were but too happy to feed on the dainty flesh of these innocent animals'.

Admiral Boutakoff was awarded the Patron's Gold Medal, 1867, by the Royal Geographical Society for introducing steam-navigation on the Aral Sea.

This letter obviously was written in 1866 because Bates's reply, following, was dated the next day. It contained the extract quoted here and comments on the name of the deer.

61.

ROYAL GEOGRAPHICAL SOCIETY

15, WHITEHALL PLACE, S.W.

December 19th 1866

My Dear Mr Darwin

On the other side is the extract you require. I presume "saigak" means the same as "saiga", therefore the animals were the saiga antelope. The islands were new *discoveries*; the Kirghises of the Aral shores did not know of their existence.

Yours sincerely

H W Bates

I am now reading Lyells 1st vol. New Ed. Principles, with intense pleasure & great profit.

The book was the *Principles of Geology*, (3 vols.).

The extract referred to has not been found.

The Aral Sea is the fourth largest lake listed in the *National Geographic Atlas of the World* with an area of 25,300 square miles. It lies east of the Caspian Sea.

Saiga is a desert-living antelope found on the Khirges steppes, according to Webster.

Bates writes Kirghises but the *Encyclopaedia Britannica*, 14th Edition, uses Khirges. Since Bates is writing, his spelling is used.

Darwin's Journal (p. 17) states that Darwin was staying with Erasmus, 13 to 21 February. He obviously had a long visit with Bates as implied from the following letter, in which the parts in square brackets are added in red pencil, apparently by Darwin.

62.

ROYAL GEOGRAPHICAL SOCIETY

15, WHITEHALL PLACE S.W.

March 11 1867

My dear Mr Darwin

I promised when you were here to look up a few cases of sexual ornamentation etc in insects & send the particulars to you. Here they are.

1. Gay-coloured butterflies.

The tropical American genus *Epicallia* is a good case. The males are amongst the most gaudy of all butterflies; the females are generally very much plainer. I know both sexes of 12 species: in one [A.] both sexes are similar (male less gaudy) in pattern & colours [gaudy colours or plain] & this pattern is that of the great majority of the females of the genus: in 9 other species the two sexes are so different that Entomologists formerly placed them in separate genera, but the male of one of the 9 is totally different in colour & pattern from the other 8 males although as gaily coloured as they: in the remaining 2 species both males & females are gaily coloured but males more so than females.

I think this case will interest you: the fact of the females of 10 species being of the same type of colouration gives us a clue to the ancestral type, from which the males have diverged by sexual selection, & this type is that of both sexes in several allied genera found in various parts of the world. The fact, again, of 2 species having females gaily-coloured & very different from the females of the rest of the species points to the inheritance of gay colours being partaken of by both sexes in the two species whilst in the rest male has inherited male colours & female female. The males & females in this genus do not frequent separate haunts as in many other genera; but the females fly slower & nearer the ground than the males. The facts afforded by *Epicallia* are not isolated in Entomology, other genera & natural groups of species within genera afford similar illustrations.

In some genera when the males are much more gaily coloured than their females the males are immeasurably more numerous than the females & spend most of their days in the open sunlight whilst their obscurer partners are

confined to the shade of the woods (Genera, *Catagramma*, *Eunica*, *Megistanis* &c). In other cases the females have clearly diverged from the common type of coloration while the males have remained unchanged in this respect ; groups of species of *Pieris* come under this category.

2. Armature of males.

I have looked through my collection of horned genera of *Lamellicora* beetles : viz. *Copris*, *Phanceus* and *Onthophagus* and find 5 specimens with their cephalic horns broken or edge of clypeus chipped. As we generally select perfect specimens in collecting this is only an indication of what might be found if attention was drawn to the subject. The male horns & thoracic bosses are so wonderfully developed in many species that they must have been drawn out by a long course of Natural Selection & therefore must be of some use to the species ; but no one has yet recorded, to my knowledge, a case of males fighting : true, the species come out of their holes only at night. Excessive variability in size of horns and bosses I find is the rule, but not a rule without exception. For instance, the most wonderfully horned species of *Onthophagus* in the world viz. *O. rangifer* of the Zambezi region, shows in abt. 100 specimens I have inspected (collected at random by a non-Entomologist) show no imperfectly developed males: the female is without armature in this species. The rule in *Onthophagus* is, however, a gradual degradation of horns &c from fully-developed males down to males so degenerate that they are scarcely to be distinguished from females. Another fact is that the species in *Onthophagus* cannot be naturally classed according to the horns of the male : in each natural group of species there are species with one cephalic horn & others with a pair like the bull.

In *Phanaeus* & *Copris* there are species with horned females scarcely to be distinguished from degenerate males.

If you wish to ask me any more special questions about these matters, I shall be glad to try to answer them

Yours sincerely

H W Bates

Wallace brought forward your enquiry regarding gay caterpillars before the Entomological Society last Meeting & several practical men are looking out for explanations.

If gay, colours transferred to female : if plain the male has retained primordial colour of ancestors.

64.

DOWN
BROMLEY
KENT, S.E.

March 30 [1867]

Dear Bates

Would not the tabulating the Horned Beetles be very troublesome ; if not I certainly shd like to hear the result. But in truth it would be a pity for you to waste or take up much time over the job, for some general remarks would do very well for my object.

Your remarks in answer to my lady-friend (Miss Tillet, daughter of late Mr. Tillet of Betly Hall) are interesting & fairly satisfactory ; but it would have been better if it could have been stated what "other objects" they first

mocked ; or if it could be shown that some species mocked full-coloured Heliconididae, for then as the latter gained their splendid colours so could the workers.—Not that I feel a shadow of doubt about the truth of your theory—it must be true. Wallace told me in a letter of the pretty case of the white moth & the young Turkeys. I suppose you have, of course, seen his letter to Field ; but I enclose a couple of copies.—Many thanks about Junonia—whenever I go to B. Museum, I will ask to see the genus & will look at the differences & similarities in the sexes : it seems a capital case. You have indeed given me much valuable information.—

Dear Bates

Yours very sincerely

Ch Darwin

I have just finished having read aloud your Amazon Book, & liked it better 2nd time even than 1st time.

I shall send your letter to Miss T. as she begged me to.

This letter obviously refers to the previous one from Bates, which has not been found, and apologizes for all of the trouble Bates has gone to on Darwin's behalf.

An interesting collection of fifteen long-horned beetles is pictured in the *Land and Wildlife of Tropical Asia*, Dillon Ripley and the Editors of Life, Life Nature Library, New York, 1964. It is entitled, 'Wallace's Delight : the Long-horned Beetles' (pp. 140-141).

The Tillets, father and daughter, are not mentioned in *Life and Letters* or *More Letters* and are not known to Sir Gavin de Beer or Miss B. Maria Skramovsky.

The 'pretty case of the white moth & the young Turkeys' is recounted by Wallace (*My Life*, vol. ii, p. 4). Wallace wrote : 'I had just been preparing for publication my rather elaborate paper on "Mimicry and Protective Colouring," and the numerous cases in which specially showy and slow-flying butterflies were known to have a peculiar odour and taste which protected them from the attacks of insect-eating birds and other animals led me at once to suppose that the gaudily-coloured caterpillars must have a similar protection. I had just ascertained from Mr. Jenner Weir that one of our common white moths (*Spilosoma menthrostri*) would not be eaten by most of the small birds in his aviary nor by young turkeys. Now, as a white moth is as conspicuous in the dusk as a coloured caterpillar in the daylight, this case seemed to me so much on a par with the other that I felt almost sure my explanation would turn out right.'

A copy of Wallace's letter to *The Field* was laid in Darwin's letter. It is a loose sheet with no date or other identification save Wallace's name and address. It is printed in *My Life* (vol. ii, p. 4). In it Wallace

asks people to offer different coloured caterpillars in bird feeding trays and note which ones birds eat and which ones they reject.

No reply from Bates has been found.

The following letter was obviously a reply to one Darwin had written to Bates concerning the sex distribution of various insects.

67. ROYAL GEOGRAPHICAL SOCIETY
15, WHITEHALL PLACE, S.W.
Feby 18 1868

My Dear Mr Darwin

I could not myself remember any case of female insects being very conspicuously more numerous than the males & therefore put the question as you desired to the Entomological Society last night. Some fact & good opinions were elicited :—

Mr Stainton said that observations of numerical proportion of the sexes, in collecting were quite unreliable, for it was a well-known fact that in collecting *Tineina* moths the females were nearly in all species very scarce, whereas in breeding the females proved to be more numerous—as 2 to 1 to the males. Mr F. Smith also stated that observations in the field were of little value. He quoted some most interesting experiments he was making in breeding some British sawflies—all the individuals turning out to be females. These I think will prove to be cases of parthenogenesis : Mr Smith will write to you himself.

Other members gave these cases :—

Tomiscus villosus (bark of the oak Coleoptera)

no male found by Ratzeburg during many years observation

Tomiscus dispar—an English species

males exceedingly rare

Miana arcualla—an English moth—

males fly in great numbers over damp meadows in many places—females only one or two in a season

Apabania muliebris

an English Neuroptemes insect

hundreds of females found during last two years—no male yet found

In Hemerobiidæ according to Maclachlan females are much more numerous found than the males.

One of the above cases I have mentioned by mistake—it being the reverse illustration—males more numerous than females.—Plenty of cases can be mentioned of males occurring more numerous ; but after the strongly expressed opinions of Stainton & Smith & the case of the *Tineina* (which was confirmed by other able observers, Mr F. Bond among them) I must say I shall hereafter hesitate to accept any fact, without reason, of sexual disparity in numbers without the species are actually bred. Although the cases of vast majority of ♂ butterflies in hot countries seem pretty clear, yet it is possible the females may, in some unaccountable manner, elude observation.

Perhaps in selecting species for figuring in your next work, it would be better to chose the most wonderful, altho' they may not be British. The Chilian stag beetle *Chiasognathus*, would surprise people more than our common insect. I am now reading your two Volumes. Pangenesis I have read once & must read again before I thoroughly understand it. A friend tells me the same.

Yours sincerely
H W Bates

Henry Tibbatts Stainton (1822–1892) was author of *A Manual of British Butterflies and Moths* (2 vols., 1857–9) and *The Natural History of the Tineina* (13 vols., 1855–73). He worked with Maclachlan on editorial activities.

Frederick Smith (1805–1879) wrote many papers on insects listed in the Royal Society's *Catalogue of Scientific Papers* between 1864 and 1877.

Julius Theodor Christian Ratzeburg (1801–1871) of Berlin occupied himself principally with insects injurious to forests.

Robert Maclachlan (1837–1904) was treasurer and former secretary to the Entomological Society of London and later president.

Frederick Bond (1811–1889) wrote many short notes about insects and birds between 1866 and 1871, but no monographs or extensive articles.

Darwin's two volume work was *The Variation of Animals and Plants under Domestication*, published on 30 January 1868. 'Pangenesis' is the next to the last chapter of Volume II.

68.

DOWN
BROMLEY
KENT, S.E.

Feb. 19th [1868]

My dear Bates

I am in utter confusion ; will you kindly clarify my mind.—Westwood divides *Saltatoria orthopt.* (Modern Clopt. Vol. 2, p. 439) into :

Achetidae = ?

Gryllidae = ?

Locustidae = ?

Now you speak (Amazons Vol. I, p. 250 of Achetidae, Locustidae and Acridiidae.—Now will you homologize these families for me, & tell which terms will be *most generally* understood, which I care more about than which are most strictly correct. Again, & very important for me, you say that (p. 252) in the Achetidae (I suppose of Westwood likewise) "the wing-cases are symmetrical, both have straight edges a distinct portion is *not* set apart for sound-producing" Now how can I reconcile this with

Westwood (p. 440) who gives drawing of the two wings which are not strictly symmetrical, & one of which is furnished "with a distinct portion" for sound producing.—

Is it the same species of cricket having both wings similar? I should be grateful if you would kindly aid me as far as you can, & excuse me troubling you.

Yours very sincerely

C. Darwin

Harris places *Platyphyllum concassum*, the "Katy-did", into Gryllidae.— Is this the Gryllidae of Westwood? I suppose the "Field cricket" belongs to Achetidae of old authors.—

Your *Chlosocatus* cannot belong to the Locustidae of Westwood, but I suppose to his Grillidae.

It seems strange that Darwin made no mention whatever of the careful and detailed letter of 18 February. The letter of 19 February may possibly have been written before the first one had been received.

Darwin's reference to Westwood is not very exact. He apparently meant *An Introduction to the Modern Classification of Insects* (J. O. Westwood, 2 vols., London, 1839-40).

69.

ROYAL GEOGRAPHICAL SOCIETY

15, WHITEHALL PLACE, S.W.
Feb 21 1868

My dear Mr Darwin

Mr Westwood very unfortunately tried to introduce names for the families of leaping Orthoptera, different from those used by several authors of Monographs.

His

Achetidæ = Achetidæ of majority of authors, but

Gryllidæ of some [Three words, now illegible, have here been added in pencil, perhaps by Darwin.]

Gryllidæ = Locustidæ of majority

Locustidæ = Acridiidæ — do —

(Former)

You might use English names for first & last "Cricket" & "grasshopper" — but the second have no English name.

In *Amazons* vol I, 250, I aimed at showing there was a gradation of perfection in the stridulating organs at base of wing-cases. What I stated about Crickets was the result of visits to Baker's ovens at the time, dissection of fresh specimens and examination of many foreign Achetidæ; and on looking at a few species I have at hand I still say that the *edges of base of wing cases of Achetidæ* are not forced out of symmetry; i.e. not produced into lobes of different shapes & widely different structure,—as they are in Locustidæ, where also the organs advance gradually from genus to genus (speaking generally) to high degree of elaboration.

On looking at Westwood's figures of Achetidæ, quoted in your letter, I find he does not figure two wing-cases of a male but one of a male & one of a female.

It is however probable that in some true crickets (Achetidæ) there may be some differences in the two wing-cases; *see text* of Westwood. It would be advisable therefore to avoid making too sweeping an assertion about their symmetry throughout the family; this is not essential to the argument, which is that there is a gradation in assymetry & a step by step advance to a wonderful musical instrument.

Want of symmetry in the same pair of wings of insects is an exceedingly rare phenomenon and how it could first occur by variation in these orthoptera, so as to give Nature the first chance of a musical instrument, is a curious question. I think it occurred through the overlapping of the inner edges of base of wing-cases, caused simply by the shape of the wings & body, without any reference to a musical instrument being afterwards elaborated.

Yours sincerely
H W Bates

70.

ROYAL GEOGRAPHICAL SOCIETY

15, WHITEHALL PLACE, S.W.
Feb 26, 1868

My dear Mr Darwin

I have examined more than 100 species of all the genera of horned Copridæ & find there is no absolute difference in size between male & female. This conclusion would not perhaps be arrived at if a good series of each species were not at hand for comparison, as the Copridæ vary unusually in size & ill-developed males smaller than females are very common; but fully developed individuals of both sexes always agree pretty well in size.

I have looked at a good many Dynastidæ & find the males in some genera a little *longer* than the females; in bulk they are much greater than the females, owing to horns & their supports, but the difference in length & breadth exclusive of horns, when they come to be measured is not much. The genera *Dynastes* & *Megasoma* may be safely quoted as those in which this disparity in size is less subject to doubt.

I have now no longer a large collection of Orthoptera; but will profit by my next visit to B.M. to notice if there is any sexual difference in colours.

I have, since I last wrote, re-examined a male house cricket & can confirm my former statement that there is no difference in shape of the two wing-cases at the part where the stridulating organs are situated. There is also very little difference in the form of the organ itself on the left & right wing-cases, one process is probably *scored* across & the other smooth. I must examine them again under microscope.

We have gained a powerful convert to Darwinianism in Mr Von Kiesenwetter one of the school of learned & rigid systematic Entomologists that has flourished for many years at Berlin. He has written a very lucid & able article illustrating origin of species by examples in the genus *Oreina* (Chrysomelidæ) in Berlin Entom. Zeitschrift 1867 pt 3-4

Yours sincerely
H W Bates

Ernst August Helmuth von Kiesenwetter wrote 'Entomologische Beiträge zur Beurteilung der Darwinischen Lehre von der Entstehung der Arten' (*Berliner Entomologische Zeitschrift*, 1867, 2, 327-349).

This letter obviously continues the discussion of the previous one and is based on more and different data.

71. ROYAL GEOGRAPHICAL SOCIETY
15, WHITEHALL PLACE, S.W.
March 12, 1868

My dear Mr Darwin

I have again examined a few genera of Trop. American butterflies to ascertain the gradation in divergence of sexual coloration & give you the results as promised.

1. *Junonia* (including *G. Precis* of some authors) on re-examination I think the genus is better divided into *three* classes, instead of *four* as I gave in former letter :

1st class of species—in which there is no difference whatever in colours or pattern & both sexes are destitute of rich or strongly contrasted colours.

The majority of species in this large genus come under this class.

2nd class—Similar colours & pattern but male richer & brighter in colour

1. *Junonia Onone*— 2. *J. Radinna* 3. *Jun. clelia*

3rd class sexes quite different in colours : male having rich, deep & contrasted colours.

1. *Junonia orithya*

2. „ *andremiaja*

2. *Aeneas* group of *Papilio*, slow-flying insects abounding in Tropical American forests & highly characteristic of the fauna

1st class, both sexes having same colours & pattern, but males generally darker & richer in tint ; in one species no difference of tint

at least 4 species

2nd class, sexes strongly contrasted, males with green or blue & white & crimson patches on a black ground—females simply white & crimson patches on black ground.

At least a dozen species.

3rd class, still more strongly contrasted than class 2, the males having the crimson patches superbly glossed with opalescent shades—about a dozen species.

4th class—contrast reaching its acme in the same direction as the gradation from 2nd to 3rd classes.

Two or three species.

3. *Epicalia*—same as described in my former letter to you. This genus illustrates the principle of females preserving a tolerably uniform type of colouration whilst the males, similar to females in some species, diverge greatly in others.

I have found, however, a much better illustration of this in the genus *Eubagis*.

4. *Eubagis*—A very numerous genus abundant in Tropical America & peculiar to the fauna

Some species are alike in both sexes & their pattern is after the general type of the great group of butterflies to which the genus as well as *Epicallia* belongs, but in the majority of the species of *Eubagis* the males assume metallic colours & in some species reach a high degree of beauty, the females preserving the same plain colours. Curiously enough, in one or two species the female has a trace of metallic lustre! Female of one specie far more like male of 2nd specie than is the male.—

I have noted a few more genera presenting similar phenomena but will not trouble you with them now, as I hope to show you them next Sunday morning.

The females of these butterflies cannot be called absolutely plain; it appears to me that in the gradual heightening of the male beauty through long ages of sexual selection the female sex has occasionally inherited some portion of the beautified colours & propagated them. I can show you cases where the male varies in ornamental colours & where selection is in all probability *now* going on: one case, in particular, is that of a most magnificent local variety of a common Tropical American species, *P. of Aeneas* group.

I hope your health withstands the excitement of London and that I shall see you on Sunday.

Yours sincerely
H W Bates

72.

March 18 [1868]

My dear Bates

It has occurred to me that you must occasionally come across Missionaries or Doctors who have long lived intimately with Savages: in this case, if you can, oblige me by leading conversation to the notion of savages about the beauty of women, & secondly & *more especially* how far the women have any indirect influence in getting men, whom they prefer or admire, to court them or purchase them from their parents. You will at once see my object.—I have many facts, but am greedy for more.—I most thoroughly enjoyed my morning's talk with you.—This obviously requires no answer.

Most sincerely yours,
C. Darwin

This letter was written from his aunt Sarah Elizabeth Wedgwood's home, where Darwin stayed for three weeks in March 1868.

Darwin makes no mention of Bates's three long letters, but they were presumably discussed during the morning's talk that Darwin enjoyed.

73.

DOWN
BROMLEY
KENT, S.E.

April 15 [1868]

My dear Bates

Will you look at p. 2 of enclosed note (which please return for it has been & is precious to me), at passage with two blue lines, & tell me the name of the one *Papilio*, in which there is no difference in colour in the two sexes. Also should you call this one species a beautiful or plain one, either relating to quite other Butterflies or to the Genus *Papilio*?—

As I am writing I will ask another question: in your communication to W. H. Edwards in the *Proc. Ent. Soc.* of Philadelphia, you speak of the *females* of *Argynnis* *Diana*, *Sagona*, *Paphia*, *Papilio* *Turnus*, as departing from the type of their family.—Now are these more gaudy than their males? The black female of *P. turnus* cannot be considered so. With *Argynnis* are the above females mimetic? I suppose you do not know whether in the case of these species the males or females appear most numerous. I hear from Walsh & Edwards that with *P. turnus* the males are as 4 to 1 to the females. H. Doubleday has just made up the good way of estimating number of sexes, viz by priced German listing & the results are striking; thus with Butterflies, out of 114 sp. & var in which the sexes differ in price (of course there is no difference in very common species) the males in every case but one are cheapest; so that according to this standard, on an *average*, there ought to be for every 100 females 143 males.—To return to the female Butterflies, Walker thinks in the *Pieridae* that in all cases when the female is most beautiful she is mimetic.—

My M.S. on *Lepidoptera* has run to greater length than I like, but has interested me much: & I owe 4/5 of my information to your great & unvarying kindness.—

Yours very sincerely
Ch. Darwin

Darwin was referring in the second paragraph to Bates's 'Notes upon the variation of sexes in *Argynnis* *Diana* (communicated to Wm. Edwards, Esq., in a letter dated Oct. 20, 1864)', *Proc. Ent. Soc. Philadelphia*, 1865, 4, 204–207.

74.

ROYAL GEOGRAPHICAL SOCIETY
15, WHITEHALL PLACE, S.W.
April 20 1868

My dear Mr Darwin

I have found amongst my old papers a rude drawing of the conspicuous caterpillar which you intend to mention so enclose it. I believe it is natural size; the black is not *deep* enough, otherwise I think the colour is accurate.

Ann. of Sci.—Vol. 25, No. 2.

h

The species of *Aeneas* group of *Papilio* in which the sexes show no difference in colours or pattern are

P. <i>Triopas</i>	}	males simply have deeper & richer ground colour than females
P. <i>Chabrias</i>		
P. <i>Montezuma</i>		
P. <i>Panthonus</i>		
P. <i>Ascanius</i> —I see no difference in tint of ground colour.		

P. Ascanius in both sexes is handsome ; I think more conspicuous & gay in colouring than any of the other four species here mentioned. The whole *Aeneas* group is remarkable for its rich, strongly-contrasted & conspicuous colouring ; but the rule is—less richness in tint in ♀ than in ♂, although the ♀ has often more variety of colour. It is very clear there is no adaptation of female colouring to disguise in this group.

The female of *Argynnis Diana* & *A. Sagana* are of olive-brown or blackish colours relieved by lighter marks. Regarded absolutely one cannot consider them less beautiful than their mates ; but there is no doubt, here, adaptation for purposes of disguise has taken place ; Entomologists of the countries where they are found alone could say what they imitate ; I have no idea myself ; but *A. Sagana* ♀ is coloured much like many common butterflies of *Adolias* group found in same countries.

Yours sincerely
H W Bates

A capital suggestion that of H Doubleday !

75.

Ap 22 [1868]

DOWN
BROMLEY
KENT, S.E.

My dear Bates.

Hearty thanks.—You are the man to answer a question fully & clearly.—I return the drawing which on the whole is capital for me.—I have nearly finished my M.S. on Lepidoptera & will then have it copied by a man who writes an excellent hand.—Your most kind offer of reading it has made all the difference to me : if I had not known this I shd have been tormented the whole time with doubts. I have tried to be cautious and to avoid blunders, but when one writes on a subject, with which one is not familiar, mistakes almost inevitably occur.—

Yours very sincerely
C. Darwin

Two-thirds of the M.S. relates to facts communicated by you to me, or published by you.—

Darwin's manuscript on the Lepidoptera has not been identified. It is not listed in the bibliography in *Life and Letters*.

77. ROYAL GEOGRAPHICAL SOCIETY
15, WHITEHALL PLACE, S.W.
May 20 1868

My dear Mr Darwin

I think I can get ♂ and ♀ of the *Onitis furcatus* & also males & females of two or three species of *Geotrupes* at Mr Janson's (Dealer) ; he has promised to oblige us but I must go to his place to select them as soon as I can spare an hour.

Yours sincerely
H W Bates

These insects are very cheap—a few pence each for the more curious *Geotrupes* with conspicuous sexual differences—Would you like more than one or two species ? (Janson lets you pay, or ask him to send account) Do not hurry

Darwin's letter of inquiry has obviously been lost.

Edward Wesley Janson (1821–1891) studied Coleoptera and became a bookseller. He was curator of collections, librarian and secretary of the Entomological Society at various times. He is quoted in the *Descent of Man*.

78. DOWN
BROMLEY
KENT, S.E.
May 21 [1868]

My dear Bates

Cordial thanks.—You must not hurry yourself. You understand that I always want male & female. I shd like

Typhaeus—English species

Copris (*not* English sp.)

Geotrupes 2 sp.

Onitis

4 or 5 or 6 other genera, some differing much, some differing little according to sex in external appearance.

I heard in Chile *Chiasognathus Grantii* squeaking loudly so I wd gladly pay £1 for a pr, if they can be bought : I brought home a dozen sp. T. but gave them all away.

If the beetles do not come named, & they prove interesting, I dare say you cd put me up to getting them named. Will you pay Mr. Janson for me ? or ask him to send me a mem ? anyhow I shd like his address so that I might apply for more specimens if the result proves interesting, which I don't expect. Should you be able to come down the early part of next month & spend the Sunday here ; it wd give us very great pleasure to see you & it wd make it pleasant to Mr. Jenner Weir who has been extraordinarily kind & whom I want to invite here.

With many thanks

Yours sincerely
Charles Darwin

We cd send to station on Saturday evening & on the Monday morning.

This letter was written by Francis Darwin and signed by Darwin. It is the first time he has signed his full name.

79.

ROYAL GEOGRAPHICAL SOCIETY

15, WHITEHALL PLACE, S.W.

May 28 1868

My dear Mr Darwin

Being our Anniversary week I have been very much occupied, even my leisure hours taken up by foreign visitors & medallists; otherwise I should have written before to tell you that I went to Janson on Saturday afternoon & looked out a few beetles for you. He wrote the tickets specifying name & sex whilst I was there & I hope has fixed them to the right specimens. In *Geotrupes* there is scarcely any external difference between ♂ and ♀ other than a row of denticulations along the *under surface* of the *anterior tibiæ*: these tibiæ have the usual row of teeth on the outer edge in both sexes but ♂♂ have in addition this row of small irregular sized teeth *quite underneath*. I picked you two females & one ♂ of one *Geotrupes*: a ♂ & ♀ *Geotr. Typhaeus*, & ♂ & ♀ *Lethrus*, a most extraordinary being closely allied to *Geotrupes*, its habits described (I think) in Kirby & Spence's "Introduction." Luckily I found at Janson's both sexes of *Onitis furcifer*. He promised to try to get *Chiasognathus* for you. The *Dynastidæ* are remarkable for stridulating organs, their classification being founded to some extent on these organs: as an example I send you ♂ & ♀ *Oryctes*. Janson is a very good Coleopterist & a *very fair* dealing man: he will supply you with anything in his power.

Yours sincerely
H W Bates

The Annual (Anniversary) Meeting of the Royal Geographical Society was held on 25 May of that year.

Bates refers to *An Introduction to Entomology; or, Elements of the Natural History of Insects, comprising an account of Noxious and Useful Insects, of their Metamorphoses, Food, Strategems, Habitations, Societies, Notions, Noises, Hybernation, Instinct, etc. etc.*, seventh edition, 1856, by William Kirby, M.A., F.R.S., F.L.S., rector of Barham, and William Spence, Esq., F.R.S., F.L.S.

81.

ROYAL GEOGRAPHICAL SOCIETY

15, WHITEHALL PLACE, S.W.

Sept 10 1868

My dear Mr Darwin

On returning from a trip with my children I found your kind invitation. Unfortunately I cannot avail myself of it this time, on account of the arrears of work staring me in the face after a brief holiday. I have the 1st number of a

new geographical magazine to bring out in November, a Manual of Geography in December (I wish they may get it) besides other indispensable work to do. So I must work on Sundays & week days for a few weeks.

Yours sincerely
H W Bates

Darwin's letter of invitation has been lost.

The Royal Geographical Society revised its publication policy by purchasing the copyright of *The Geographical Magazine* and changed the old *Proceedings* which ceased with the end of Volume 22. The new name adopted was *Proceedings of the Royal Geographical Society and Monthly Record of Geography*! The new *Proceedings*, edited by Bates, started as a monthly on 1 January 1889.

82.

ROYAL GEOGRAPHICAL SOCIETY

15, WHITEHALL PLACE, S.W.
Sept 29 1868

My dear Mr Darwin

Have you heard of a Dr. Semper, a Naturalist, who spent 7 years in the Philippines & Caroline Arch^{ip}; & who published some observations on coral reefs in Kolliker & Siebold's *Zeitschrift*?

He is now in London for a few days & much wishes to be allowed to visit you for an hour or so. I find him a most pleasant young man, brim full of new & interesting information concerning volcanic phenomena, geographical distribution of animals &c. He sent large collections to Europe.

If you are able to see him any day during the next 10 or 12, please write to me & I will convey the answer to him.

Yours sincerely
H W Bates

Professor Karl Gottfried Semper (1832-1893) was a naturalist who was Professor of Zoology in Wuerzburg. He wrote on travels in the Philippine Islands. He was the author of 'Ueber Generationswechsel bei Steinkorallen und über das M. Edwards'sche Wachsthumsgesetz der Polypen. (Zugleich ein Beitrag zur Fauna der Philippinen)' (*Zeit. für Wissenschaft. Zoologie*, 1872, 22, 235-280).

84. ROYAL GEOGRAPHICAL SOCIETY
15, WHITEHALL PLACE, S.W.
June 8 1869

My dear Mr Darwin

I am ashamed to say that my brother Entomologists have almost nothing to offer you in statistics of sexes of bred insects. Last evening there was a Meeting of the Society & I took the opportunity of asking all the practical Entomologists individually if they had done anything towards answering the appeal made to them some months ago & I found they had done nothing. There was only one exception & I am promised the results of enumeration by one gentleman a Mr. Buckler, which as soon as I receive I will forward to you.

I trust this glorious weather finds you well & in happy working condition.

Yours sincerely
H W Bates

I had a fearfully busy season—in Geography

A letter from Darwin inquiring about sexes of insects is missing. W. Buckler is referred to in Bates's letter of 11 January 1877 as an exciseman. William Buckler was a trained oil and water-colour artist who became deeply interested in entomology. He described and pictured many British larvae alone and with his great friend, the Rev. John Hellins. Buckler was a very religious man and he wrote on the fly-leaf of Stainton's Manual: 'For thou, Lord, hast made me glad through thy works, and I rejoice in giving praise for the operations of Thy hands.' Buckler wrote *The Larvae of the British Butterflies and Moths* (9 vols., Ray Society, 1886-1901).

85. 40 Bartholomew Rd, Kentish Town
15 Whitehall Place
Jan 17 1870

My dear Mr Darwin

I have sent to the post stamped & registered, your packet of Manuscript, which I hope will reach you safely. You will see my suggested alterations all marked lightly in pencil in their places & will notice that they nearly all relate to the orthography of scientific names of Insects—a very small matter!

As to the *facts* there is only one which I do not clearly understand: it is the rudiments of horns in ♀ of *Onitis fuscifer*. I conclude, however, that you consider the indented or flattened (retuse) front part of thorax as the rudiment of horn formation. Well, it is so in effeminate males of *Phaneus orthophagus* & so forth & I presume you are correct in so expressing the fact with regard to *O. fuscifer*: but still some qualification in terms seems necessary.

Other facts I have noted are additions to your stores. Of course I would not think of giving you new facts of these classes in which you have a super abundance already. But in one case—sexual dissimilarity of colours in Coleoptera—you have clearly not enough. I have recently been working at *Longicorns*, a group in which an enormous amount of modification for mere ornament has taken place & I find that there are here many cases of sexual disparity in colours. The genera *Mallaspis*, *Pyrodes* & *Esmeralda*, large & very beautiful *Prionida* Longicorns offer the most striking examples. In *Esmeralda* the two sexes have been placed in different genera partly on account of difference of colour. Many other genera in the tribe offer less striking cases.

The reasoning I had not ought to touch & have not touched in the M.S.—I almost always agree with you & have less scruple on this account to suggest a modification of your views of females not being more dull-coloured by selection which will bring your opinion & Wallace's nearly into harmony. It is this—the necessity of females being dull-coloured for protection is true, but they have not been *made* dull from former brighter hues, but have simply been *kept* dull by natural selection steadily eliminating all tendency to brightness. This will not disagree with your clenching & true argument against Wallace that females of a genus are truer in colour to the generic type than males.

Yours sincerely
H W Bates

The packet of manuscript probably refers to an early draft of Darwin's *Descent of Man* which appeared in February 1871.

86.

ROYAL GEOGRAPHICAL SOCIETY
15, WHITEHALL PLACE, S.W.
May 20 1870

My dear Mr Darwin

I have been having some conversation with the Editor of the "Academy" about Mr Wallace's last book & the appearance of backsliding from the Darwinian theory which it contains. Other sincere friends of the pure truth have expressed a little surprise & bewilderment at the same phenomenon. The views of friend Wallace are so plausible & suit so well wide-spread prejudices that you no doubt think with me they might be controverted. But who is to criticize them? No one but yourself. I do not think anyone else would have the present insight into the fallacy but yourself; to others it would require much study & labour to marshal the arguments. I said so to Mr Appleton & he begged of me to write to you in support of a request he is going to make to you to write him a short article as review of the book.

When you were last in town I spoke to you about some sentences I had written on man, interwoven in last chapter of Mrs. Somerville's book. It weighs on my conscience to think that you took too much notice of what I said: for I do not really think there is much in the matter worthy of your attention.

Yours sincerely
H W Bates

This letter is quoted in full in *Clodd*, lxxxiii. It refers to Wallace's *Contributions to the Theory of Natural Selection*, which was dated March 1870. Wallace is most gracious and generous to Darwin in the preface but then states as follows : ' Another reason which led me to publish this volume at the present time is that there are some important points on which I differ from Mr. Darwin, and I wish to put my opinions on record in an easily accessible form, before the publication of his new work, (already announced) in which I believe most of these disputed questions will be fully discussed.' Darwin's *Descent of Man* was published on 24 February 1871.

Darwin certainly felt no malice nor did he express any criticism of Wallace in a letter of appreciation for Wallace's generous remarks about him. His letter to Wallace, dated 20 April [1870], is published in full in *Life and Letters* (vol. iii, p. 121).

The book referred to by Bates is *Physical Geography*, by Mrs. Mary Fairfax Somerville (1780-1872), which he revised in 1870.

Charles Edward Cutts Birch Appleton (1841-1879) was the editor of the *Academy* referred to. The *Academy* was a literary periodical, ' a Monthly Record of Literature, Learning, Science and Art ' , which appeared on 9 October 1869, and Appleton remained editor until his death.

87.

ROYAL GEOGRAPHICAL SOCIETY

1, Savile Row
Burlington Gardens,
W.

NOV 15 1873

My dear Mr Darwin

Enclosed is Wallace's reply. You will perceive that he is at present unaware of the scope & nature of the revision required, & I should think they ought to be *very exactly* defined if he is employed ; otherwise he would be likely to query the reasoning.

Your sincerely
H W Bates

Note that the Royal Geographical Society has moved from 15, Whitehall Place, S.W., to 1, Savile Row, Burlington Gardens, W.

88.

DOWN
BECKENHAM, KENT

Nov 16th [1873]

My dear Bates

Very many thanks for all your kind assistance.—I will write tomorrow in full detail to Wallace & I hope all will go smoothly.

Yours very sincerely
Ch. Darwin

This exchange of letters in 1873 is explained by correspondence published by James Marchant in his *Alfred Russel Wallace: Letters and Reminiscences* (London, 1916, vol. i, pp. 281–282). Darwin apparently had written to Wallace asking if he would care to revise the *Descent of Man*. In a letter dated only ‘Wednesday morning’, but evidently written in November 1873, Wallace acknowledges a letter from Darwin. He asks Darwin to forget the whole matter because he does not particularly wish for the work. On 18 November Wallace wrote again stating that he understood what was required and would do the work to the best of his ability. He said that he had helped Sir Charles Lyell on his last three or four editions and had been paid at the rate of 5/- per hour. He thought that this was not quite enough and offered to do it for Darwin at 7/- per hour. He asked that the MSS. be copied out because ‘. . . . it adds considerably to the time required if there is any difficulty in deciphering the writing, which in yours (as you are no doubt aware) there often is’. He also stated that his hasty note to Bates ‘was not intended to be shown to you or anyone’.

Darwin wrote to Wallace on 19 November that Mrs. Darwin had suggested that their son George would be most pleased to undertake the work. The second edition of the *Descent of Man* appeared in the autumn of 1874.

90.

ROYAL GEOGRAPHICAL SOCIETY

1, Savile Row,
Burlington Gardens,
W.

Feb 7 1874

My Dear Mr Darwin

All the books you asked for were fortunately “at home”, & a clerk has already packed them up & sent them addressed as you wished. With regard to the classified catalogue, I believe I have authority from the Council to present

copies to any Institution or Individual whom I may think would make a good use of them ; there will be no need, therefore, to trouble you for the amt. nominally charged.

I have not at present learnt Dr Staley's whereabouts ; nor has any one from the Sandwich Is. turned up here by happy chance.

Yours sincerely
H W Bates

The classified catalogue probably refers to *Classified Catalogue of the Library of the Royal Geographical Society to December 1870* (London, John Murray, 1871).

Darwin obviously wrote to Bates requesting the loan of books, but the letter has not been found. Dr. Staley was Bishop Thomas Staley who wrote 'On the Geography and recent volcanic eruption of the Sandwich Islands' (*Geogr. Soc. Journ.*, 1868, **38**, 361-369).

91.

ROYAL GEOGRAPHICAL SOCIETY

1, Savile Row,
Burlington Gardens,
W.

Oct. 1 1874

My dear Mr Darwin

Mr Maclachlan, Treasurer & former Secretary of the Entom. Soc., has taken charge of Mr Barber's communication & thinks it will be accepted by the Society for publication with a coloured plate. The phenomenon described is not new, the Society having long ago received communications of a similar nature regarding *Tieres brassica* &c. but Mr Barber's facts are rather more striking than usual.

Thanks for your kind enquiries regarding my family. I rejoice to say they are all well & have returned invigorated from a long & happy holiday.

Mr Belt's address is

Thos Belt
Cornwall House
Ealing W.

Yours sincerely
H W Bates

Mr. Barber was really Mrs. M. E. Barber who wrote 'Notes on the peculiar habits and changes which take place in the larva and pupa of

Pipilio nireus,' Communicated by Charles Darwin, M.A., F.R.S., &c. (*Trans. Ent. Soc.*, London, 1874, pp. 519-521, pl. XI (coloured)).

Thomas Belt [? -1878] was a careful observer of zoological and botanical as well as geological phenomena. He was trained as a mining engineer and had travelled extensively making frequent visits to Siberia and Southern Russia between 1873 and 1878. He died in Denver, Colorado.

93.

40, BARTHOLOMEW ROAD
LONDON, N.W.
Jan 11 1877

My dear Mr Darwin

Enclosed is the Extract about the snake-like caterpillar, which is not repeated in the "Nat. on Amazon".

On enquiry I find that the Rev. Mr. Hellins, goal chaplain Exeter, is the possessor of only a portion of the magnificent collection of drawings of British Caterpillars in all stages. They were drawn by a Mr Buckler, an Exciseman, who holds the larger portion. Buckler is a rabid anti-Darwinian & they say would not lend his drawings for any Darwinian purpose, but the Reverend is far more liberal & will very likely lend them if wanted.

Hoping you continue well, notwithstanding the sudden lowering of the temperature

Yours sincerely
H W Bates

Darwin apparently wrote to Bates asking for information about a caterpillar. The letter is missing. This is the first letter between the men for over two years.

The Rev. John Hellins, M.A., (1829-1887) was a great friend of Buckler and assisted the Ray Society by writing many additional descriptions which appeared in two volumes of Buckler's *The Larvae of the British Butterflies and Moths*, already published.

94.

February 6 1877

From Mr. C. Darwin, Down, Beckenham

Please return me Prof. Weismann's letter, as it contains reference about eggs. I shd be curious to hear some time how you succeed in reputation about the drawing.—

Feb. 6th

C.D.

The card is addressed as follows :

H. W. Bates, Esq
R. Geograph. Soc.
Savile Row
London.—

Postmarked February 6 1877

A portion of a Darwin letter to August Weismann is quoted in *Life and Letters* (vol. iii, p. 231). No date is given but it was written in 1877 inquiring about the colour of caterpillars and birds' eggs.

95.

ROYAL GEOGRAPHICAL SOCIETY

1, Savile Row,
Burlington Gardens,
W.

Jan 25 1879

My dear Mr Darwin

Mr Galton & other men of high esteem in the Royal have suggested to me the propriety of going in for the Fellowship, under the new Rules.

Of course it is impossible to decline such a chance if there is one for me. Are you quite free to propose me? It is possible you may hesitate on the ground that you have already proposed a number of men this year: if so do not think I shall be hurt if I am told to wait another year

Yours sincerely
H W Bates

96.

DOWN,
BECKENHAM, KENT
RAILWAY STATION
ORPINGTON, S.E.R.

Jan 26th 79

My Dear Bates

It will give me *real* pleasure to propose you for the Royal. You will see that you yourself must not circulate the certificate,—not that there can be the least impropriety in your asking any of your friends.—Will you therefore fill up all the necessary particulars, giving a pretty full list of your publications & schools to prepare to me. At the same time give me a list with addresses of such men as you wd like to sign, & I will then write & forward certificate to

each.—*I do not know who are on the Council* & it is a confounded bore that some of the best men, to whom I shd naturally apply, such as Hooker & Huxley are excluded. This makes it the more necessary that I shd have some sort of list from you.—

Ever yours sincerely
Ch. Darwin

You must not be surprised if you are not elected the first time, for hardly anybody is thus elected.—

Bates was elected F.R.S. on 2 June 1881.

Darwin wrote to Gladstone proposing a memorial or pension for Wallace. The next three letters refer to this matter. Wallace discussed his affairs quite frankly in *My Life* (vol. ii, pp. 394 and 395). He stated that he had mentioned his financial worries quite confidentially to Mrs. Fisher, then Miss Buckley, who told Darwin.

Francis Darwin wrote in *Life and Letters* (vol. iii, p. 228) that his father's letter to Sir Joseph Hooker of 17 December 1879 first suggested a Government Pension for Alfred Russel Wallace.

97.

Down Beckenham Kent

[December 1880]

My dear Bates

After signing the memorial, please put it in enclosed envelope & post it without loss of time to Sir J. Hooker.—

Yours very sincerely
Ch. Darwin

P.S. I am inclined to think that you had better annex your official Title to your signature, but please do as you think best.—

98.

DOWN
BECKENHAM, KENT
RAILWAY STATION
ORPINGTON, S.E.R.

Jan 3rd—1881
(after our 2nd Post)

My dear Bates

I am rather alarmed about the Memorial for Wallace.—Mr Seldon wrote that he had forwarded it to you.—Have you received it & on what day did you

forward it to Sir J. Hooker? Pray forgive me troubling you.—I am much disappointed in not having been able to get the M. into Mr. Gladstone's hands some days before meeting of Parliament.—

Yours very sincerely
Ch. Darwin

Mr. Seldon has not been identified.

99.

DOWN
BECKENHAM, KENT
RAILWAY STATION
ORPINGTON, S.E.R.

Jan 7th 1881

My dear Bates

I write one line, as I am sure that the news will please you, viz that I have just received a note from Mr. Gladstone himself, saying that he "will recommend Mr. Wallace for a pension of £200 a year". The Memorial was sent in only on the 5th & answer received today! Wonderfully kind of Mr. Gladstone.— I am weary of writing notes, though beyond measure pleased.—

Ever yours
Ch. Darwin

100.

DOWN
BECKENHAM, KENT
RAILWAY STATION
ORPINGTON, S.E.R.

February 17th, 1882

My dear Bates

Will you be so good as to sign the enclosed certificate for Mr Meldola, who does not expect to be elected until 2 or 3 years have elapsed?—Please return in enclosed envelope.—But if for any reason you object, I will mention the fact to no one. I shall ask Sir J. Lubbock if he will sign & then send it in to the R. Soc.—My dear Bates, yours very sincerely

Ch. Darwin

Raphael Meldola (1849–1915) was a professor of chemistry and a well-known entomologist. He translated and edited Weismann's studies in the theory of descent (1882–1883), which included a prefatory notice by Darwin. He was elected F.R.S. on 4 June 1886.

Darwin died on Wednesday 19 April 1882.

Thus ends the correspondence of the two eminent naturalists, a correspondence that had lasted over twenty years. The record, incomplete as it is, shows, nevertheless, that it was sometimes spirited and intense, but interrupted and desultory at other times. It shows how Darwin worked. He learned early the technique of appealing directly to the man who knows, or who originally made the discovery, rather than poring tediously through reference works. Early in the acquaintance, Bates appealed frequently to Darwin for advice about writing, style, publishing and publishers. Bates repaid Darwin many times over by his meticulous attention to detail in answering Darwin's numberless questions, particularly about entomology.

The correspondence is almost entirely technical and, therefore, is of more interest to working scientists, biologists, entomologists and geologists and to historians of science than to the general reader. It is almost entirely devoid of personal references. The only exception to this was comment on both sides about Bates's marriage. From reading these letters, one would never guess that Bates, as a young man, wrote in one of his books: 'I am as fond of Latin as women are of satin', that he was a life-long admirer of Gibbon's *Decline and Fall of the Roman Empire*, which he read over and over throughout his life, that he joined a glee club and learned to play a guitar in order to accompany himself with his favourite songs. 'His love of melody, whether in music or poetry, never left him and on the rare occasions that his health permitted, he refreshed himself at the opera: his catholic taste enabled him to enjoy the music of composers as diverse as Wagner and Sullivan', Clodd recorded. Clodd characterized him further when he wrote: 'but even more than the gentle voice, the winning smile and the affectionate greeting, the friends of this sincere, this guileless, this self-reliant man will cherish, as the chief lessons of his life—especially in an age of *Sturm und Drang*, of pushing to the fore, of clamour for priority of discovery of a new sun spot or asteroid—the wholesomeness of possessing the soul in patience, of work done in quiet and of finding a like impulse and content in the thought that, so far as a man's work is sterling and contributory, "natural selection" will take care of it.'

Bates continued to serve as Assistant Secretary of the Royal Geographical Society for nine years after Darwin's death. He died of influenza, complicated by bronchitis, on 16 February 1892. He was buried four days later in St. Marylebone Borough Cemetery, Finchley. His wife was buried with him five years later.