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# UNPUBLISHED LETTERS OF CHARLES DARWIN

IN THE COLLECTION OF

CHARLES F. COX,

NEW YORK.

Thursday,

36 Gt. Marlbro' St.<sup>1</sup>

[ , 1838.]

My dear Owen,<sup>2</sup>

Many thanks for your note. I cannot let you leave<sup>3</sup> London without saying how

<sup>1</sup>"On March 7th, 1837, I took lodgings in Great Marlborough Street in London, and remained there for nearly two years, until I was married. During these two years I finished my Journal, read several papers before the Geological Society, began preparing the MS. for my 'Geological Observations,' and arranged for the publication of the 'Zoology of the Voyage of the Beagle.' In July I opened my first note-book for facts in relation to the Origin of Species, about which I had long reflected, and never ceased working for the next twenty years."—*Darwin's Autobiography*.

<sup>2</sup>Darwin first met Owen at Lyell's house, October 29th, 1836.—*Life of Richard Owen by his Grandson*. Vol. 1, p. 102.

There are no letters to or from Owen in Darwin's "Life and Letters," and there are only three from Darwin in "The Life of Richard Owen, by his Grandson." This is probably to be accounted for by the lamentable break which occurred in the previously close friendship between the two great men, after the publication of the "Origin of Species." In this connection, see the letter of July 5, 1844, to Mrs. Darwin, in which Owen was named as a possible editor of the great work in case of Darwin's sudden death before its publication; and note the subsequent partial erasure of the words, "*Professor Owen would be very good: but I presume he would not undertake such a work,*"—showing that Darwin did not at first anticipate the entire rupture of intercourse which was to result from Owen's opposition to his views.

<sup>3</sup>June 16th, 1838, Mrs. Owen says: "R's eyes are beginning to suffer from over-use," and that he cannot read or write in the evening.

very sorry I am to hear you are not well. I cannot help taking a part of this on my own conscience. I am afraid your working up *Macrauchenia*<sup>4</sup> must have done you harm. I think the new name a very good one. I have a revise of the description of *Toxodon*<sup>5</sup> ready, but I purposely do not send it. When you return, and are quite comfortable again, you will be able to look over the whole revise together.<sup>6</sup> I will see about getting a sheet of *Macrauchenia* set up. The manner, in which you mention in your letter the prospect of all the trouble, which your part must cost you, is to me deeply gratifying.

Good bye, Dear Owen. Let me know when you come back, & I sincerely trust, quite well again.

Yrs.

C. Darwin.

In August, 1838, Owen attended the meeting of the British Association at Newcastle, and afterwards went to Germany and Holland.—*Life of Owen by his Grandson*. Vol. 1, pp. 124-139.

Is the trouble with the eyes part of the illness referred to by Darwin, and was the tour on the Continent the journey from which he wished Owen might return "quite well again"?

<sup>4</sup>*Macrauchenia*, "a genus of fossil perissodactyls founded by Owen in 1838, upon remains of camel-like quadrupeds found in the Tertiary of South America."—*Century Dictionary*.

February 28th, 1838, Owen says: "My first number of Darwin's 'Fossils' (strange animals) is out and most unexpectedly the Geological Society has awarded me the Wollaston gold medal for that and other services to geology."—*Life of Owen*. Vol. 1, p. 121.

<sup>5</sup>"Amongst the descriptions which Owen made of the fossil mammalia collected by Darwin in the voyage of the 'Beagle' may be mentioned that of the *Toxodon* skull. \* \* \* The following account of the *Toxodon* in the autograph of Charles Darwin was found amongst Owen's papers."—*Life of Owen*. Vol. 1, p. 119.

<sup>6</sup>"We also find from the Diary that Darwin submitted the proofs of the 'Voyage' itself to Owen."—*Life of Owen*. Vol. 1, p. 121.

I did not receive with your note any engravings. But it [is] of no sort of consequence. I believe there is also a plate of head (redrawn) of great Armadillo, which you have not given me, as you intended.

[Addressed on back of note sheet]

Richard Owen, Esqr.  
Coll. of Surgeons.

26 Jan., 1842.  
12 Upper Gower St.,<sup>7</sup>  
Wednesday.

My dear Henslow,

Robert Brown called here yesterday morning asking me to write to you about the election of the Librarian of the Linn. Soc.—He thinks it very doubtful whether Dr. Seman's election (in favour of which he says you signed a paper) will be carried without great exertion on the part of his friends; & he further states that he thinks the interest of the Society is most deeply concerned in the issue. Dr. Seman will not practice as medical man—will therefore have nearly his whole time at the disposal of the Soc. In short he wants to persuade you to come up on the 15th

<sup>7</sup>“The house in which they lived for the first few years of their married life, No. 12 Upper Gower Street, was a small, commonplace London house, with a drawing-room in front and a small room behind, in which they lived for the sake of quietness. In later years my father used to laugh over the surpassing ugliness of the furniture, carpets, etc., of the Gower Street house. The only redeeming feature was a better garden than most London houses have.”—*Life and Letters*, Vol. 1, p. 299.

to vote & I want to persuade you for the sake of seeing you here. Brown also is very anxious for you to mention the case to Babington or any others whom there would be any chance of persuading to vote. Do see if you cannot come. I am commissioned to say by my wife how very glad she should be, if Mrs. Henslow could be prevailed on to come. It is a wretched time of year to leave home, but there is always some business for everybody in London, so do state the case to her with our united request.

As such antagonist powers as Brown & Lindley are so anxious for Seman's election it must be a clear case & do try & come.

Ever Yours,

C. Darwin.

Down, Bromley, Kent,<sup>8</sup>

Monday, March 30th, —46

Dear Sir,

I am much obliged for your note & very clear agreements, one of which I return signed. I am sorry (though I hardly expected it) that you can not aid me in my Third Part,<sup>9</sup>

<sup>8</sup>Married, January 29th, 1839. Moved to Down, September 14th, 1842.

"During the three years and eight months whilst we resided in London, I did less scientific work, \* \* The greater part of my time, when I could do anything, was devoted to my work on 'Coral Reefs,' which I had begun before my marriage, and of which the last proof-sheet was corrected on May 6th, 1842."—*Life and Letters*, Vol. 1, p. 69.

<sup>9</sup>The "Third Part" of the *Geology of the Voyage of the Beagle*, entitled "Geological Observations on South America," published by Smith, Elder & Co., 1846.

which I must publish as well as I can, by commission with you.

It is provoking that the rules of the trade will not permit you to sell the *Geolog. Parts* at reduced prices, for of course putting a new title page, as for a new edition, would do only for such gentlemen as Mess. Colburn & Co.<sup>10</sup> Whatever it costs my third Part shall be sold at not more [than] 10s, 6d,—I must economise in every possible way, for many engravings are wanted for it. Allow me once again to thank you for the uniformly, kind attention which I have on every occasion received from you, & believe me dear Sir

Yours very faithfully

C. Darwin

Friday night.

Down, Farnborough, Kent.

[1848?]

My dear Owen.

To my great dismay, there arrived this morning a request from L. Aukland, & Sir J. Herschel to me to draw up some Geological Instructions.<sup>11</sup> When men like Herschel & yourself give up your time to the task, I could not of course refuse, when asked. My object in all this is to ask you, whether you do not think the remarks

<sup>10</sup>Henry Colburn was the publisher of the first edition (1839) of the "Journal of Researches."

<sup>11</sup>This refers to "A Manual of Scientific Enquiry, prepared for the use of Her Majesty's Navy, and Adapted for Travellers in General," edited by Sir John F. W. Herschel, Bart., and published by authority of The Lords Commissioners of the Admiralty, 1849, to which Darwin contributed the chapter on "Geology," in which there is a section devoted to *Coral Reefs*. Owen contributed the chapter on "Zoology."

on Coral Reefs, which I sent you, appertain more properly to Geology than to Zoology. It struck me so when I was writing them, but I had not at that time any idea of the plan or that it included Geology. I shall have to treat on Subsidence generally, & I shd. think my remarks would most naturally follow there; but if you think they would be better under Zoology, I can refer to them.

I do hope that you and Mrs. Owen will be able to come to us tomorrow week: it will be a real pleasure to us & you will lose very little time. We shall dine at 7 o'clock on Saturday & your best plan of coming here will be to start by the Croydon Railway from London Bridge (do not be too late for the Train) at 4°.45' & stop at Sydenham Station; & thence take a Fly here. Forbes & hope Bell & Ramsay (as well as the Lyells earlier in the day) will come by the same 4°.45' Train and you had better take a 2 horse Fly together, for it is a long hilly stage of 10 miles. There is a good fly-place close to station over the Bridge. Do come; it will do you good, or if it does not, it will do me.

Ever yours

C. Darwin.

P. S.

I have just received your note with sincere grief. You need not leave the college till  $\frac{1}{4}$  past 4 o'clock & you might start early back on Monday: but I fear there is no hope. Should you take heart of grace, let me have a line. If you decide to return me the Coral M.S. please send it pretty soon.

Down, Farnborough, Kent.  
 Sunday [March 26, 1848]<sup>12</sup>

My Dear Owen,

I do not know whether your M.S. instructions<sup>13</sup> are sent in: but even if they are not sent in, I dare say what I am going to write will be absolutely superfluous, but I have derived such infinitely great advantage from my new simple microscope, in comparison with the one, which I used on board the *Beagle* and which was recommended to me by R. Brown,<sup>14</sup> that I cannot forego

<sup>12</sup>This date is supplied from post-marks on back of the letter.

<sup>13</sup>This refers to the "Instructions for Collecting and Preserving Animals," contributed to the "Manual of Scientific Enquiry," mentioned in the note to the next preceding letter. In Owen's chapter on "Zoology" there is a section "On the Use of the Microscope on Board Ship," which is said to "embody the experience of Mr. Charles Darwin, F. R. S." It describes in detail Darwin's simple dissecting microscope, referred to in this letter, and concludes with a note as follows: "A microscope such as is here described, and most of the apparatus, can be seen at Messrs. Smith and Beck's, opticians, of Colman Street, London." Doubtless Darwin supplied this section to Owen.

<sup>14</sup>"It strikes us now-a-days as extraordinary that he should have had no compound microscope when he went his *Beagle* voyage; but in this he followed the advice of Robert Brown, who was an authority in such matters."—*Life and Letters*, Vol. 1, p. 145.

In a letter to his sister Susan, dated Sept. 6th, 1831, when he was getting together the necessaries for the voyage, he says: "Tell Edward to send me up, in my carpet bag, \* \* \* my new microscope (about six inches long and three or four deep) which must have cotton stuffed inside."—Vol. 1, p. 206.

"Mr. A. B. Osborne writes: 'He was a dreadful sufferer from seasickness, and at times, when I have been officer of the watch, and reduced the sails, making the ship more easy, and thus relieving him, I have been pronounced by him to be 'a good officer,' and he would resume his microscopic observations in the poop cabin.'"—Vol. 1, p. 224.

"Most of his work was done with the simple dissecting microscope, but it was the need which he found for higher powers that induced

the mere chance of advantage of urging this on you. The leading point of difference consists simply in having the stage for saucers very large and fixed. Mine will hold a saucer 3 inches in inside diameter. I have never seen such a microscope as mine, though Chevalier's (from whose plan many points of mine are taken) of Paris approaches it pretty closely. I fully appreciate the utter absurdity of my giving you advice about means of dissecting; but I have appreciated myself the enormous disadvantage of having worked with a bad instrument, though thought a few years since the best. Please to observe that without you call especial attention to this point, those ignorant of natural history will be sure to get one of the fiddling instruments sold in shops. If you thought fit I would point out the differences, which from my experience, make a useful microscope for the kind of dissection, of the invertebrates, which a person would be likely to attempt on board a vessel. But pray again believe that I feel the absurdity of this letter, and I write merely from the chance of yourself

him, in 1846, to buy a compound microscope. He wrote to Hooker: 'When I was drawing with L. I was so delighted with the appearance of the objects, especially with their perspective, as seen through the weak powers of a good compound microscope, that I am going to order one; indeed, I often have structures in which the  $\frac{1}{30}$  is not power enough.'—Vol. 1, p. 349.

"My new microscope is come home (a 'splendid plaything,' as old R. Brown called it,) and I am delighted with it; it really is a splendid plaything."—*Letter to J. D. Hooker, May, 1847.*

Professor Francis Darwin informs me that this "splendid plaything" was a Smith & Beck compound microscope; that the simple one of his father's planning is still in his possession; and that the only other microscope his father had was a small Hartnack, which he much preferred to the Smith & Beck.



possessing great skill and having worked with good instruments, may not possibly be fully aware what an astonishing difference the kind of microscope makes for those who have not been trained in skill for dissection under water.

When next I come to town (I was prevented last time by illness) I must call on you, and report for my own satisfaction a really (I think), curious point I have made out in my beloved Barnacles. You cannot tell how much I enjoyed my talk with you here. Ever, my dear Owen,

Yours sincerely,

C. Darwin.

(Over)

P. S.—If I do not hear, I shall understand that my letter is superfluous. Smith and Beck<sup>15</sup> were so pleased with the simple microscope they made for me, that they have made another as a model: if you are consulted by any young naturalists, do recommend them to look at this: I really feel quite a personal gratitude to this form of microscope and quite a hatred to my old one.

[Addressed on back of letter]

Professor Owen

Royal College of Surgeons

Lincoln's Inn Fields

London.

<sup>15</sup>Mess. Smith & Beck, and their successors, R. & J. Beck, regularly listed "Darwin's Dissecting Single Microscope," and illustrated it in their catalogues. As late as February 5th, 1889, they had one in stock, and pronounced it "an extremely useful and serviceable instrument."

[Note paper with wide black border.]

Down, Farnborough,  
Saturday, Kent.

, [1849.]<sup>16</sup>

My dear Owen.

I am very obliged for your note & the C. Hunteri: the stupid carrier forgot to call on Thursday, as I ordered him. He will come on Monday to the College & he will, also, have to call at the Geolog. Soc., which I mention in case of your having sent the specimens there, expecting my attendance at the Anniversary. I have been prevented by being as usual<sup>17</sup> unwell. I have lost for the last 4 or 5 months at least 4-5 of my time, & I have resolved to go this early summer & spend two months at Malvern & see whether there is any truth in Gully & the water cure: regular Doctors

<sup>16</sup>This letter was written probably in February, 1849, the black border being in memory of his father, who had died November 13th, 1848.

<sup>17</sup>"Again in 1849, he notes in his diary:—'January 1st to March 10th health very bad, with much sickness and failure of power. Worked on all well days.' This was written just before his first visit to Dr. Gully's Water Cure Establishment at Malvern."—*Life and Letters*, Vol. 1, p. 350.

In the "Reminiscences" (Vol. 1, p. 131), it is said: "In 1849, when very ill, suffering from constant sickness, he was urged by a friend to try the water-cure, and at last agreed to go to Dr. Gully's establishment at Malvern."

He was at Malvern on March 28th, 1849. See Vol. 1, p. 372.

"In October, 1846, I began to work on 'Cirripedia.' \* \* \* \* \* Although I was employed during eight years on this work, yet I record in my diary that about two years out of this time was lost by illness. On this account I went in 1848 (1849?) for some months to Malvern for hydropathic treatment, which did me much good, so that on my return home I was able to resume work. So much was I out of health that when my dear father died on November 13th, 1848, I was unable to attend his funeral or to act as one of his executors."—*Life and Letters*, Vol. 1, p. 81.

cannot check my incessant vomiting at all. It will cause a sad delay in my Barnacle work, but if once half well I cd. do more in 6 months than I now do in two years.

I am quite delighted to hear how effectually you have done Sullivan's<sup>18</sup> work; I hope you will be rewarded by some treasures. I had already ordered your book on Limbs:<sup>19</sup> on a very small scale I have had some pretty homological work with the Cirripedia & now know certainly what the peduncle & shell is.

Yours most sincerely,

C. Darwin.

I never heard anything so astounding as the Log-Book of your H. M. S. Diddleus [sic]<sup>20</sup>.

[Letter paper with narrow black border.]

Down, Farnborough, Kent.

[Sept. 21, 1849.]<sup>21</sup>

Sir,

I trust to your kindness to forgive a stranger taking the liberty of addressing you. I have been

<sup>18</sup>B. James Sullivan, second lieutenant of the *Beagle* during the voyage of 1832-1836, afterwards Admiral and K. C. B.

<sup>19</sup>"On the Nature of Limbs," a discourse delivered on Friday, February 9, 1849, at an evening meeting of the Royal Institution of Great Britain.

<sup>20</sup>"Several persons of undoubted veracity declared that they had seen the 'Great Sea-serpent,' and brought much corroborative detail into their accounts, which were clearly given in good faith. The description given of a sea-monster which was reported to have been seen by the officers and crew of H. M. S. 'Daedalus,' attracted more than usual notice, for the position and intelligence of the observers guaranteed the truth of their story. Considerable correspondence ensued, and Owen made a strong attack upon the identification of the creature. \* \* \*"—*Life of Richard Owen, by his Grandson*. Vol. 1, p. 323.

<sup>21</sup>Entire date supplied from post-marks on back of letter.

for the last two years (at least such portions of it, as my health allowed me to work in) employed on a monograph, anatomical & systematic, of the Cirripedia; it was consequently with the greatest possible interest that I heard your admirable paper at Birmingham. I made a few remarks on the subject which will perhaps appear in the Athenaeum. In S. America I collected an allied form, parasitic on the Concholepas & which probably will be included in the same order with your's, but which I think must certainly form a very distinct family. I was very glad to hear from Mr. Taylor that your Paper will appear in the Annals<sup>22</sup> & then I shall be able to study it. I have no sort of pretension to claim any favour from you, but if you could at any time spare me one or two specimens in the shell preserved in spirits, it would be the most material kindness. I would pledge my honour not to publish anything so as to interfere with any further researches you might choose to make on the species. No one can be aware better than yourself after your excellent labours on the Mollusca, that when one is employed on a monograph, trifling points are found to be of interest, which are known to be so only to those employed on the class & it is on this ground

22 "To Mr. Hancock I am \* \* indebted for several long and interesting letters on the burrowing of Cirripedes."—*Preface to Darwin's Monograph on the Sub-Class Cirripedia*, 1851.

Mr. Hancock's Memoir on "*Alcippe Lampas*, the type of a new order of Cirripedes," appeared in the "Annals of Natural History," November, 1849.

"I am much indebted to this gentleman, who has been so eminently successful in his researches on the boring powers of marine animals, for giving me his opinion on several points connected with the present discussion."—*Darwin's Monograph*, Vol. 1, p. 346.

that I shd. so much like to dissect a specimen of your genus. I have now dissected species of all the genera of the Cirripedia, & have nearly finished the systematic part of the Pedunculata, but yet from the extreme slowness of rate at which my health allows me to work, my monograph will not appear for one or two years, so that I could not encroach on anything which you might choose to publish further on the subject. I trust that the great interest, which I have felt in your interesting discovery, will make you forgive my taking the liberty of asking you so great a favour as that contained in this letter.

I beg to remain,

With much respect

Yours faithfully

C. Darwin.

To

A. Hancock, Esq.

[Addressed on back of letter sheet]

Albany Hancock, Esq.

Newcastle-upon-Tyne.

[Letter paper with narrow black border.]

Down, Farnborough, Kent.

Sept. 29th, [1849.]

Dear Sir:—

I am truly obliged to you for your very kind letter & offer of specimens of *Alcippe*. You cannot imagine how much I shall enjoy seeing in your Paper & in actual specimens a new form of Cirripede; for I am wearied out with examining

scores & scores of closely allied common cirripedes. I should have written about *Lithotrya* in former letters, but I had hardly space, & I did not know whether you wd. like to have my views, on this point. I conclude that the *L.* forms its own holes from having seen numerous specimens (& 4 or 5 species), large and small, all exactly fitting their cavities. The calcareous cup is undisputably (I speak after careful examinations with dissolution in acids &c. &c.) formed by the cirripede & is common to most (I believe all) the species of genus. You are perfectly right that the calc. cup is never moved. But the cirripede inhabits (as far as I have seen) any cellular rocks, or corals, or shells (such as the cirripede *Conia*, with large tubes) & the pupa crawls into some minute cavity & there fixes itself forever, & then as it grows enlarges the hole to required size; this it effects by the edges of the valves & of the minute scales on the peduncle being sharply serrated; & as of course the serrated edge would soon be blunted, the calcareous scales on the peduncle are moulted with the membrane on which they are fixed & new sharp ones periodically formed. This (i. e. moulting of calc. scales) is a unique case, & I have no doubt is in relation to their boring necessities. I believe this is the way which my *Arthrobalanus*, which inhabits the *Concholepas*, also makes its cavities & its outer tissue is studded with elegant minute trifid & 4-fid points & the shell is apparently first perforated by other animals; but I have got to go over this again with *Arthrobalanus*, but I have had the misfortune to lose

nearly my whole stock of specimens, of which I collected thousands, for 15 years ago, in the Chonos Archipelago, I described its peculiarities in some detail.

I venture to predict that if you take the outer tissue of *Alcippe* & clean the corium from it & place it under the compound microscope, you will find the rasping minute points, & I believe you state that it inhabits shells abounding with cavities of *Cliona* &c. &c.

I am most particularly obliged to you for informing me of *Loven's* cirripede, of which I had not heard. I shd. be most grateful for a transcript of the paper & drawing,—or if the book be not above 2 lb. or too precious cd. it not be sent & I would gratefully repay postage, & thus save Mr. Alden the trouble of transcribing, to whom pray give my sincere thanks. I have in my collection this cirripede as I suspect: it is like an *Otion* & from not having calc. valves might be called an *Alepas*, but strange as it wd. appear to anyone who had not studied the internal structure of these animals, it is not even one of the *Pedunculata*, but belongs to the sessile division & forms a new genus between *Tubicinella* & *Coronula*. I shd. have been very sorry to have overlooked *Loven's* description.

I presume you have a superabundance of materials, but if at any time you wd. like to have my small collection of naked mollusca, made during my circumnavigation, they are at your service; but I fear specimens preserved for many years in

spirits must be almost useless. I think there are some new genera amongst them.

Once again allow me to thank you cordially for the very kind manner in which you have taken my request, & believe me, dear sir,

Yours sincerely obliged

C. Darwin.

To

A. Hancock, Esq.

I see in the Athenaeum they have omitted to express how valuable I thought your discovery & how interesting your whole paper. I am very curious to see what you say about the palpi. I cd. not follow the reading aloud, of this part, but if I understood right, the palpi are wonderfully different from anything I have ever seen in the Cirripedia. I think it possible that *Alcippe* & *Arthrobalanus* may turn out distinct orders. The metamorphosis is certainly different. My Larva has no thoraccic legs, whereas yours has. Mine is binocular, yours uniocular, &c. &c. Yet the 3 pair of cirri, the great lab- [paper torn] & habits are certainly strong points of resemblance.

I ought to apologise for the length of this letter.

[Addressed on back of letter-sheet]

Albany Hancock, Esq.

St. Mary's Terrace,

Newcastle on Tyne.



Down, Farnborough,  
Kent,  
Jan. 19th 1850.

My dear Sir,

In accordance with your permission, I have cleaned one of the large valves, which can be now drawn. The four little embedded pieces, now cleaned and separated, consist of (1.) a fragment of a scutum, (2.) a perfect upper latus, (3d) a perfect lower latus, 4th, a broken do. Your specimen is certainly now a hundred fold more instructive; & the 3 valves which were in their nearly proper position have not been in the least displaced. Many thanks for offer of sessile cirripedes, but I am not nearly ready for them yet. Forbes wrote to me that he understood that you had some valves from the Gault of Pollicipes besides those sent to me; if you have & wd. entrust them to me, I shd. be particularly thankful, as I want to see as many as possible. Mr. Fitch has sent me some beautiful specimens for description.

I fully thought I had asked you in my former note, & if I did not it was an accidental omission which I am very sorry to say will entail another note on you. It is to ask whether you will permit me to have some of your specimens drawn by Mr. J. Sowerby. I intend to have all the fossils, (if I get permission) which have any good characters, drawn. With respect to publication of the fossils, I have not yet thought: your mentioning the Palaeont. Soc.<sup>23</sup> makes me think whether

<sup>23</sup> "A Monograph of the Fossil Lepadidae, or Pedunculated Cirripedes of Great Britain," published by the Palaeontographical Society, 1851.

The "Fossil Balanidae and Verrucidae" was published by the same society in 1854.

my work wd. suit them. I hope immediately to receive a lot of fossils from Copenhagen, chalk, named by Beck & Steenstrup; which will be very valuable for comparison with the British specimens. Will you give me your idea about Palaeont. Soc.—I suppose, even if you thought it would suit, I had better wait till I have done the Sessile cirripedes, as the whole will of course be only a small Part. Pray forgive this trouble, & believe me

Yours very faithfully

C. Darwin

[Addressed on envelope]

J. S. Bowerbank, Esq.

3, Highbury Grove,  
London.

Down Farnborough Kent

Ap. 28th [1850]

My dear Owen.

I have received a letter from Capt. Sullivan at the Falkland Isls.<sup>24</sup> expressing his continued desire & zeal to collect fossil Bones on the coast of Patagonia<sup>24</sup> if he can get a vessel. He wants to know whether there is any prospect of one being sent him. Are you willing to remind Sir F. Beaufort?<sup>25</sup> Years may pass before such a chance again occurs of having a most zealous individual so

<sup>24</sup>Darwin himself visited these places in 1833 and '34, and in 1846, he published his paper "On the Geology of the Falkland Islands" in the *Journal of the Geological Society*.

<sup>25</sup>RearAdmiral Sir Francis Beaufort, Hydrographer to the Navy.

near at hand knowing actually where there are bones. Sullivan specifies November as the best month for the purpose: he says the Captain must have distinct orders to assist him: he would go to Gallegos first; St. Julians<sup>24</sup> for the Macrauchenia & other likely places & even Bahia Blanca:<sup>24</sup> it wd. take about 6 weeks,—he requires no remuneration for himself. I have no doubt without you are prepared to give a letter and get orders sent out to the Admiral of the Station, all will be forgotten;—whether you think this worth while, I know not. I shall write to Sullivan at once: but if you see or hear from Sir F. Beaufort & can give any answer, whether favorable or not will you please let me hear, that I may inform Sullivan, as he begs to hear. Failing a King's ship, he asks whether any Societies or Government wd. aid him to amount of 150 or 200 £; for with this sum he cd. get a Sealer to take him; but I feel assured this is hopeless,—at least as far as Societies are concerned. Sullivan says, & I am sure truly, that he cannot afford this sum himself.

I have to thank you much for sending me a note some time since, which I could show to Mrs. Dixon,<sup>26</sup> if I went to Watting: but travelling

<sup>26</sup>The widow of Captain George Dixon, the author of "*The Geology and Fossils of the Tertiary and Cretaceous Formations of Sussex*," published by Mrs. Dixon, in 1850, after the death of Captain Dixon, and edited by Richard Owen, who, according to the Introduction, had given "advice and most valuable assistance during the progress of the work."

*Xiphidium angustum* (Plate XXVIII, Fig. 9, of Captain Dixon's work) is a fossil pedunculate cirripede. Darwin was interested in it because he was then writing his "*Monograph of the Fossil Lepadidae*," published in 1851.

is so fatiguing to me, that it is not worth my while to go such a journey for a single species, which is all that I [\*] urgently. Perhaps you will be so [\*] if you have occasion to communicate with Mrs. Dixon, to ask her whether she has found & would lend me, a linear shell, named *Xiphidium angustum* & figured Pl. XXVIII, fig. 9. I certainly do wish much to see it; but not sufficiently to take me such a journey.

How I hope that your Lectures will be published, of which you sent me the prospectus.


Yours, my dear Owen, very truly,  
C. Darwin.

[Addressed on back of letter sheet]

Professor Owen  
Royal College of Surgeons  
Lincoln's Inn Fields  
London.

Down, Farnborough, Kent.  
Dec. 25th [1850]

My dear Sir,

As you have attended with such eminent success to the boring of animals into rocks, you will perhaps like to hear that I believe I now understand the boring of *Lithotrya*<sup>27</sup>, thanks to the enclosed drawing ( which please return) sent me by Steenstrup without text. I suppose the same explanation is applicable to *Arthrobalanus*,

\* Paper torn.

<sup>27</sup>See Darwin's *Monograph of the Cirripedia*, Vol. 1, pp. 336, *et seq.* Also Vol. 2, p. 535, note.

& I shd. think, Alcippe, (for the presence of the calcareous disc is not material to the change of place) but not, as far as I can see, to Clisia. Since receiving this same plate, I have had a good deal of rock, bored by L. dorsalis, given me, & I now find out, for the first time, the following important facts, (1) that the animal bores to its full depth when young & afterwards only increases the diameter of its hole. 2nd that a cup is only formed when the animal has ceased boring to a greater depth, but that before a cup is formed, a succession of little discs, exactly as represented, are deposited on one side of the hole, each new one, at each fresh exuviation, being placed 1-20 or 1-16 of [an] inch, or even more beneath that last formed; the disc or cup as I was always certain, never itself being moved. 3d The lowest disc is never at the bottom of the burrow, & this is faithfully represented in the Plate. Lastly, the skin of the peduncle at this bottommost part, at first after each exuviation is studded with minute calcareous beads, which are soon fairly worn away; and the beads are succeeded by hard horny star-headed points which are also much worn away, before a new moult; so that there is good wearing agency. (N. B. I found specimens with perfect coat underneath old coat nearly ready to moult, so no possible mistake). I shd. have said that as soon as the animal begins to increase much in diameter the chain of little discs are of course all worn away, so that no trace is left in full-sized specimens. In the drawing you will at once understand how the animal travels, by

imagining a set of ghosts or exuvia attached to each of the little discs, one above the other. I have seen a row of discs extending an inch in length. (The teeth on the valves & on the beads of the peduncle with their exuviation sufficiently explain the mere increase in diameter of the burrow.) I cannot explain in a letter how the discs are fixed; but it is as in all other Cirripedia, by a cement or tissue (for I hardly know which to call it,) which primarily debouches at the penultimate segment of the prehensile antenna of the larva (this cement is formed by a gland, strange to say, which is certainly part of the branching ovaria) & subsequently during life, in different Cirripedia, either through these 2 same orifices, or out of two fresh or only one fresh aperture placed symmetrically or irregularly or again through numerous apertures placed in a regular circle: so that it is nothing unusual in Lithotrya for the discs to be fixed symmetrically in a straight line. In Scalpellum the peduncle is attached to the thin stem of the Coralline by apertures, through which the cement debouches, placed quite symmetrically in a straight row along the ventral side, a new one being opened at each exuviation. But I must stop, & not weary you. I think the drawing will make you understand what I mean, better than my perhaps ill-expressed explanations. I have not yet looked at Alcippe! But do not suppose that I undervalue your kindness in having sent me the specimens! but I have been working like a

wretched slave at mere species<sup>28</sup> & have many more months work, & till I have completed this slavery, I have not heart to begin work of interest, for I think I shd. never get courage to resume the drudgery of describing species, & making out synonymms. I hope this letter will not bore you. Believe me, my dear sir,

Yours sincerely

C. Darwin.

P. S.—The accompanying spec. of, as I suppose, a *Cliona*, you can throw in the fire if of no interest to you,—from Northern part of Patagonia.

[Addressed on back of letter sheet]

A. Hancock, Esq.,

St. Mary's Terrace,

Newcastle-on-Tyne.

[Black-bordered note-paper.]<sup>29</sup>

Down, Farnborough, Kent.

June 8th [1851]

Dear Sir,

I am going to beg you to endeavour to procure me a very great favour from Mr. Alden—namely the loan of the volume, or of the Plate of Loven's *Alepa squalicola*, of which he most kindly sent me an outline tracing. Steenstrup sent me one specimen, which I dissected, after comparing

<sup>28</sup>See his complaints about the wrong done in Natural History by the theory that "some merit was due to a man for merely naming and defining a species."—*Life and Letters*. Vol. 1, pp. 364-372.

<sup>29</sup>This black border is for his daughter Annie, who died in April, 1851.

See his letter of April 29th to W. D. Fox. Vol. 1, p. 380.

it externally with the drawings, & now I am most anxious for Mr. G. B. Sowerby to copy two of the figures for my vol. for the Ray Soc. for which the Plates are now engraving. I do not know whether there is a copy in the Brit. Mus. & if there be, it would be very troublesome to obtain permission to have a copy made, & such wd. not be so accurate, as if Mr. Sowerby could have the plate at his own house. I have charged him in case Mr. Alden would confer this favour on me, to take the greatest care of it, to acknowledge its receipt, & to pay its return carriage, & not keep it long.

Mr. S. address is

29 Albert St.

New Camden Town  
London.

Now that I am in the way of begging favours, I will ask conditionally another; you once sent me a spirited sketch of an Ibla from Australia: have you more than one or two specimens: I have the greatest wish to possess the very base of the peduncle still attached to whatever it adheres, especially if the surface be smooth: it is too long a story to tell why, but hereafter if you look at my monograph you will admit the importance of the point.

[I also shd. be very glad to be permitted to soak & open a specimen to look at one other very curious point, in this most curious genus, & the specimen when dried again, would be hardly the worse for the cabinet. I have opened two specimens, but that is not nearly enough for the point



in question, & I know not whence to get any others.]\*

Pray forgive my giving you all this trouble. I see that you continue always hard at work. I have lately been reading with great interest your Papers in the Annals on the Bryozoa.

Believe me

Yours sincerely

C. Darwin.

(Over)

P. S.—Will you forgive my sending so untidy a note, but writing the above reminded me that some time since I purchased 4 specimens which until this minute I unaccountably have forgotten—but I see all are attached to a most rugged surface, if your's happen to be attached to anything smooth & you could spare the base of the peduncle, still attached, I shd. be very much obliged.

[Letter unaddressed, but evidently to Albany Hancock.]

Down, Farnborough, Kent,  
Dec. 25th [1852]

My dear sir,

You will probably remember that you called my attention to the following facts, that Verruca (-Clisia &c.) (1st) has the power of excavating a slight depression for itself; but that (2d) epidermis on a shell quite stops this process, &

\*This paragraph was written and then erased.

(3d) that under its middle there is sometimes a hollow sometimes with chalky matter. I have just been at work on the genus & find these three facts occurring in three different species from different quarters of the world. My object in writing is to ask you to look to one point in your collection; but first I will mention what results I have come to: I began with a very strong leaning to the view, which you advocate, that the excavation must be due to mechanical agency, but unwillingly I have been driven to hypothetical chemical action. My grounds of belief are as follows, & I should be grateful for your opinion,—viz.—

(1.) I can discover no sort of boring contrivance on margin of shell or on under side of basal membrane: and there is no difference in appearance in these parts, when an individual has bored & has not in the least bored: I have examined the [simple?] shell, & cleaned with potash, & after acid, with all powers.

(2d) Either the shell or basal membrane must, on mechanical theory, be wearing agent; & certainly, as far as the central hollow, it must be the basal membrane: but the basal membrane is united to the shell & animal's body by (besides corium & epidermis) only by a circle of fibres which Prof. Quekett, after most careful testing says are only ligaments: hence I think it impossible that the basal membrane can be moved (at least near the circumference, where the animal's cirri cannot reach) or again that the shell can be moved if we look at the basal membrane as the fixed point.

(3d) When a central hollow has been formed, the basal membrane (in this case generally brittle and cracked) is loose over this middle part, but was once certainly attached, as I have found the prehensile larval antennae in the middle, surrounded by the ordinary cirripedal cement, which certainly would require considerable mechanical power to separate from any object of attachment, & yet there is nothing whatever over the central portion of the basis, but the open sack: dissolution of the shell, on the other hand, to which the cement was attached, would perfectly explain the appearance.

4th. As you state, the epidermis of shells quite prevents the wearing, except where abraded or cracked: and I further find the epidermis of Bal. laevis (of which I send a valve), [in pencil] (not to be returned), is equally protective, now this membrane is so weak, that I cannot believe it could resist mechanical wear and tear, sufficient to wear into solid shell: so again Laminaria (when not uneven and so slightly ploughed up, like cracked epidermis) though not hard is not at all excavated: again I have specimens on two pieces of slate rocks (one rather soft) which contained no calcareous matter & were not in the least affected: whereas a third specimen of hard marble was excavated.

(5) The cement-ducts might pour out an acid over any part of the basis; but that they do so is a mere hypothesis: in Lepas fascicularis they must, I think, secrete some gas (carbonic acid gas?). I should have remarked that owing to the generally

reticulated state of the cement round the central hollow, lime dissolved under the central hollow might easily escape.

This is the state of the case, as far as I can make it out: will you forgive the length of this letter, and tell me what you think? and further will you see whether you have specimens of *Veruca* attached to any softish rocks or substances, without calcareous matter, & look & see if they act on them?

In two weeks' time, I shall positively at last, after a quite ridiculous lapse of time, look at your *Alcippe*, which I have never done yet! Have you anything new (or any fresh specimens to spare) on this most curious genus? I shd. be pleased to hear that time or inclination had led you to look at what I have said on the sexes of *Ibla* & *Scalpellum*, about which I remember once writing to you; & which facts appear to me curious. Again I beg forgiveness for the length of this letter & remain,

My dear sir,

Yours very faithfully

Charles Darwin.

A. Hancock, Esq.

I do not think my wretched, schoolboy M.S. on the outlandish *Mollusca* has been returned? Has it??

[Written in blue ink.]

Down, Farnborough, Kent.

Jan. 29th [1853 ?]

[Written across the top.]

Alcippe has no relation to my burrowing S.  
American little cirripede.

My dear sir,

I write in a hurry to catch to-day's post, to beg a favour and to apologise. For the favour first: I have been deeply interested by Alcippe, though I have not added much to your excellent description, excepting perhaps on the homologies, as compared with other cirripedes, of the sexual parts. I am almost driven mad by its generative system, & I write to ask whether you have any dry shells with Alcippe you cd. send me, as I think I could get some considerable good from them: I am most anxious to examine many specimens taken at different times of year: I shd. be most grateful if you could send me such by Post, allowing me to pay postage if heavy. Wd. it be possible to employ for me any fishermen to get the shells now?? though specimens taken later than those you sent me, would perhaps be most useful to me: but any now wd. be of greatest interest to me. My surmises are too vague & too long to tell in this note, & perhaps all a blunder, but I am dreadfully perplexed. Now for apologies,—can you forgive me when I tell you that I have cut up all the specimens you lent me? I fear I have been unreasonable, but I have trusted to the extreme

kindness you have shown me in all your correspondence. Will you forgive me?

Yours very truly,  
In Haste,  
C. Darwin.

[Letter unaddressed, but, like the next following, evidently to Albany Hancock.]

[Written in blue ink, similar to that of the foregoing letter.]

Down, Farnborough, Kent,  
Feby 10th [1853?]

My dear sir,

I trouble you with one line to say that amongst the few remaining & on those cut up & previously imperfectly as it turns out examined specimens, I have found plenty of male Alcippes,—indeed hardly any without some: so that I am in no want of more specimens at present. I shd., however, be very glad to have hereafter some few to distribute in a dry state on the continent, when I return the specimens in my possession: & indeed I shd. like a few more to examine the form of cavity; though I fancy I have made out this pretty well. You may imagine how peculiar the appearance of the male Alcippe is, when I mention that, though having had experience how diverse an aspect the males put on, I now know that I looked at a male, during the first day or two, & never dreamed it was a cirripede! I suppose after all

you have done in the anatomy of the mollusca no structure seems very difficult to you to make out, but I have found Alcippe one of the most difficult creatures I have ever attempted to make out.<sup>30</sup>

Yours very truly,  
C. Darwin.

[Written in blue ink similar to that of the foregoing letters.]

Down, Farnborough, Kent.  
Feb. 12th [1853?]

My dear sir,

I will begin a summary of what I have been able to make out on Alcippe, imagining you feel interest enough to read my scrawl: you must believe, that I express myself positively only for brevity's sake.

Mouth: every part peculiar, but strictly on normal type of Lepadidae: the rudimentary palpi, however, found only in Anelasma. I think your view on the row of hairs on Labrum being Branchial, must be given up; there are thinner but similar hairs on inner opposed tunic of sack together serving as a fence to prevent anything crawling into sack by the sides of Labrum.

Cirri. The organ you have called palp or first cirrus, certainly is the latter & not much more modified than in Anelasma: the thoracic segment

<sup>30</sup> My work on the Cirripedia possesses, I think, considerable value, as, besides describing several new and remarkable forms, I made out the homologies of the various parts. I discovered the cementing apparatus, though I blundered dreadfully about the cement glands, and

supporting this cirrus is confluent with mouth and forms the prosoma in normal manner. The second (N. B. this is properly the 3d segment of archetype crustaceans) thoracic segment is large and obscure: the 3d & 4th very distinct. In one monstrous specimen, the 4th segment bore an extra cirrus! showing that the segment is true. The 5th segment small, but quite distinct, & bears the pair of cirri nearest the mouth. The 6th segment equally distinct bears the middle pair of Cirri. The succeeding and outermost articulated organs are not cirri, but caudal appendages. The wonderful little cushion, is the inner ramus metamorphosed: the caudal appendages are never bi-terminous & hence have not cushions. It deserves notice that *Alepas cornuta* has the inner rami of the 5th and 6th pairs of cirri (& of no other rami) rudimentary, without muscles, & short. Those 3 pairs of organs in *Alcippe* are all in some respects in an embryonic condition.

Alimentary Canal: Oesophagus normal: biliary envelope thick & irregular as in *Anelasma*: rectum and anus none. I am positive of this latter fact, & it is the most curious point in the anatomy of the genus. *Alcippe* must always eject (as other cirripedes sometimes do) its excrement from mouth.

lastly I proved the existence, in certain genera, of minute males complementary to and parasitic on the hermaphrodites. This little discovery has at last been fully confirmed, though at one time a German writer was pleased to attribute the whole account to my fertile imagination."—*Autobiography; Life and Letters*, Vol. 1., p. 81.



Acoustic & olfactory orifices as in *Ibla*: eyes not discovered. Nervous system hardly examined.

Female organs of generation, all quite normal, as described under the *Lepadidae*. The ovigerous fraena are very large & are destitute (as in some species of *Pollicipes*) of glands: they possibly serve as Branchiae, as well as the universally admitted Branchiae in sessile cirripedes, of which they are the homologues.

Male Organs none, except a rudiment of penis in normal position between & on ventral side of 6th cirrus.

I have forgotten to remark that the external parts of animal, (capitulum & peduncle) do not especially differ from same parts in *Anelasma*, though very peculiar.

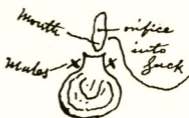
Metamorphoses: in 1st stage, I can add only the minute & lower antennae.<sup>31</sup> Most fortunately I detected some larvae just before their metamorphosis into *Alcippe*; they are peculiar in having their prehensile antennae seated almost at very end of quasi-bivalve shell: the abdomen & caudal appendages are peculiar; but they have six pairs of natatory thoracic legs & in all other respects resemble the pupae of *Lepadidae*.

Male Alcippe. Almost every female has from 1-3 or more males attached to her on both sides, externally, near the upper end of the horny

<sup>31</sup> "Mr. Hancock overlooked the inferior minute antennae."—*Darwin's Cirripedia*, Vol. 2, p. 548.

shield or disc on the peduncle, in two little bags, where I have put a cross.

The male is as transparent as glass, its lower end (answering to the peduncle) is three lobed & caricatures



the form of the same part in Alcippe: the other part (or capitulum) is flattened, much produced & of nearly same width throughout, with small orifice at upper end. Total length  $1/25$  of an inch. In the lower part we have an eye, & great testis & vesicula seminalis: in the capitulum we have nothing but a tremendously long penis coiled up & which can be exerted. There is no mouth, no stomach, no cirri, no proper thorax! The whole animal is reduced to an envelope (homologically consisting of 3 first segments of head) containing the testes, vesicula, & penis. In male Ibla we have hardly any cirri or thorax: in some male Scalpellums no mouth; here both negatives are united. I know it to be male of Alcippe from absolute identity of the pupae of both sexes & other reasons. I believe the males occur on every female: in one case I found 12 males & two pupae on point of metamorphosis permanently attached by cement to one female!

Excavation of burrow. The outer skin & even the whole thickened edge of orifice into sack,

<sup>32</sup> This diagram is (roughly) the same as Fig. 1, Plate xxii., of Vol. 2, of the "*Monograph on the Sub-Class Cirripedia*,"—Ray Society, 1854.

with its strong sharp spines, is periodically and often moulted: there is great difference in the state of the star-headed spines on the new underlying, & the old worn spines on the old membrane: this being the case, & there being good motive power, in the long & transverse & adductor muscles; I do not doubt the excavation is wholly mechanical. From the position of the larval antennae, Alcippe after metamorphosis must occupy a position fully as  
 33  
 much inclined, or more inclined than as drawn; & so would with only little change assume the position which it holds in the shell.



Though I cannot demonstrate, I can show good reason to believe, that it moves its position when first boring into the shell, just as *Lithotrya* does, a row of horny discs representing the calcareous overlapping discs in that genus. With respect to the edging or rim of shell, observed by you: it is quite inorganic. & has no more relation to Alcippe than the main part of the supporting coral reef has to a coral: when dissolved in acid, it is found to contain all sorts of rubbish, even of foreign animals; it is a kind of natural mortar. The rudely radiating dark lines and punctures in shell over peduncle of Alcippe are formed by a minute annelid.

33 This diagram is like Fig. 12, of Plate xxii., of Vol. 2 of the "*Mono-graph on the Sub-Class Cirripedia.*"

Affinities. I am dreadfully puzzled, & every day change, whether to form a family for its reception, as in one point of view it most amply deserves, or to be guided by its various close affinities to several genera amongst the Lepadidae & place it in that family. I have never repented in placing *Anelasma* there; but what to do in this far more puzzling case I cannot tell. I fear my long rambling letter will puzzle and weary you. Accept my very sincere thanks for allowing me to examine this most curious cirripede, & believe me, my dear sir,

Yours very truly,

Charles Darwin,

[Addressed on back of letter sheet]

Albany Hancock, Esq.

St. Mary's Terrace,

Newcastle-on-Tyne.

[Draft of letter apparently in the handwriting of Mr. Albany Hancock in answer to the foregoing.]

25 Feby. 1853.

My dear sir:—

I must apologise for having so long neglected to acknowledge the receipt of your long, interesting & obliging letter of the 12th inst., but the truth is I have been more than usually engaged of late, and have only just now found the requisite leisure. I rejoice to find that notwithstanding the limited supply of specimens at your command you have succeeded in making out so many interesting points in the history of *Alcippe*. You have thrown

much light upon the relationship of its various organs: the explanation you give of the cushion on the cirrus is very satisfactory, and so is the account of the parasitical males which are certainly very curious.

Regarding the rank that this animal should hold in the classification is undoubtedly a matter of no little difficulty, and must depend upon the view entertained of the relative value of the different organs. The cirri are certainly characteristic or important organs in the cirripedia, and as they are in direct relationship with the mouth, consequently with alimentary system they must bear strongly on the general economy. Any modification of these organs will influence the development of the parts about the mouth, as indeed is proved by *Alcippe*. Such modification would therefore appear of greater importance than variations of the capitulum or peduncle, the presence or absence of which latter does not seem to be of any signification. Looking in this way on the matter I arrived at the conclusion that the latter cirripede could not be placed with either the sessile or pedunculate group, both of which I believe nearly coincide in the characters of the cirri and mouth. Does not *Alcippe* differ in a more striking manner from the various genera of the *Lepadidae* than those genera differ from each other? If so, it can scarcely be placed in this family—and in truth can scarcely be called pedunculate,—and if placed in the order of which the *Lepadidae* is a member, the characters of the order will have to be altered.

I have said this much to explain the motive that induced me to place *Alcippe* apart from the two great groups of cirripedes; but my knowledge of the class is so imperfect that I have no great confidence in my own opinion on the subject. I am therefore well pleased that this curious animal is now in such competent hands, and have no doubt that you will find for it its proper place in the classification, how difficult soever the task.

I have not yet succeeded in procuring more specimens; but will assuredly have a supply this spring. I have spoken to two gentlemen who live on the coast and they have promised to do their best. I shall also be frequently at Cullervents myself.

Chas. Darwin, Esq.

[Written in blue ink like that of previous letters.]

Down, Farnborough, Kent.

March 30th [1853]

My dear Sir, I am much obliged for your note received this morning, with as full answers as you could send to my queries, & for a former note received some time since with excellent remarks on the classification of *Alcippe*. I have been very troublesome, but shall cause no more: & am truly obliged for all you have [done] for me. If in your power, I am sure you will kindly in course of summer get me a few specimens for the British Museum & for distribution.

I yet have a few specimens of other cirripedes of your's, in my possession. I have now finally finished with my S. American boring cirripede: & this has utterly confounded my previous confusion how to rank Alcippe & it: for they present some most remarkable similarity, for instance they are both bisexual, with the males remarkably alike,—& yet in what I must consider their fundamental organisation, & in their metamorphosis, they are so totally unlike that I cannot place them in the same order! My classification does not satisfy myself nor, I fear, you if you ever look to my volumes on this point.

Pray believe me,

My dear sir,

Yours truly obliged

Ch. Darwin.

The bosses on the cirri of Alcippe are hardish or crustaceous, they are all four opposed to each other & the little ridges on them are crenated; these facts made me suspect that their use was not for simple prehension, but for triturating the food: & now I find in my analogous S. American burrower, & in no other cirripede, that the oesophagus is provided with the most beautiful discs set with teeth, & brushes of hairs, worked by muscles, certainly for triturating food; which strengthens my notion.

[Letter unaddressed, but, like the next following, evidently to Albany Hancock.]

Down, Farnborough, Kent.

Aug. 24th, 1854.

My dear Sir:—

You may remember that you gave me permission most generously to dissect all your specimens of Alcippe lampas, which I obtained by dissolving the shell: but I have one or two in spirits not cut up. Shall I return them? or can you spare them for Brit. Mus.? I may mention that Mr. Bate has found Alcippe off Plymouth. I have some other specimens of yours not of much value, except one from Madeira. There is one, however, from Davis St. which I know is to be returned. I cd. return the whole lot by Post, without I have to return the Bottles. In this latter case is there anywhere in London, where parcels collect for you?

Allow me to thank you cordially & truly for the very great pleasure I derived from examining Alcippe lampas, which is described in full in my volume, now printed, & I presume soon to be published by the Ray Socy.—I have also discussed the excavatory power of Verruca, which subject I owe entirely to you. As there are several specs. of the Oxynaspis from Madeira, I have ventured to take 2 or 3 to give to Museums. With my sincere thanks & with much respect, I remain

My dear sir,

Yours sincerely,

Charles Darwin.



[Note Sheet.]

Sept. 12th

[Printed Heading]

Down,

Beckenham, Kent.

My dear Mr. Conway,

I must thank you for many things. Firstly for your letter, which has amused & interested me greatly. Secondly for the strange Debate, which I return in case you sh<sup>d</sup>. wish to lend it to anyone else. And thirdly for the Tribune, which I have been glad to read & which I suppose you do not want returned. The principle of Evolution has first-rate supporters in Morse & Gibs who are excellent naturalists. As for Prof. Swallows remarks they are worth very little.

I have had a very bad week & am ordered by my Doctor to do very little; so I will do no more than thank you again.

Yours very faithfully

Ch. Darwin.

I have heard of your intended book of the Scriptures of all times & nations, and I sh<sup>d</sup>. think it w<sup>d</sup>. be extremely curious & valuable.

[To Moncure D. Conway, Esq.]

Down, Bromley, Kent.

Dec. 4th [1868?]

My dear sir:-

I must trouble you with a few lines to thank you very sincerely for your note with some additional information as to Cave Rat & for your printed letter, which I well remember reading some years ago in your Journal. Several of your statements in your letter have interested me much, & the whole subject of these cave blind animals seems to me eminently curious. -- I am much obliged to you for telling me about Prof. Dana; pray give him my very kind remembrances; I believe no one, except his personal friends, will more rejoice at his perfect recovery than I shall. -- I am preparing a new corrected edition of my "Origin" & shall take the liberty to quote a few words from your letter as to Cave Rat. -- With sincere thanks & respect, pray believe me,

My dear sir,

Yours sincerely

C. Darwin.

[Addressed on back of note sheet]

Prof. B. Silliman, Jr.

Newhaven,

Conn.