

XIV.—*Letters from C. Darwin, Esq., to A. Hancock, Esq.*
Communicated by John Hancock, Esq.

The following letters from the late Charles Darwin, Esq., to the late Albany Hancock, Esq., are of so interesting a nature, that it has been thought they would be suitable for the pages of our Transactions, the more so as they specially refer to the discovery and description of an interesting form of burrowing Barnacle, *Alciippe lampas*, which was first observed on the Northumberland Coast by Mr. Hancock, and afterwards described by him in a paper communicated to the British Association, at the meeting held in Birmingham, 1849. The references given are chiefly to the Monograph of the Cirripedia by Charles Darwin, published by the Ray Society.*

No. 1.

Down Farnborough, Kent.

(Post mark Sept. 21st, 1849).

Sir,

I trust to your kindness to forgive a stranger taking the liberty of addressing you. I have been for the last two years (at least such portions of it as my health allowed me to work in) employed on a monograph, anatomical and 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 *Athenæum*.

In S. America I collected an allied form, parasitic in the Concholepas, and which possibly will be included in the same order with yours, but which I think must certainly form a very distinct family.

I was very glad to hear from Mr. Taylor that your paper

* Notice of the Occurrence, on the British Coast, of a Burrowing Barnacle belonging to a new Order of the Class Cirripedia. By Albany Hancock, Esq. *Annals and Mag. of Nat. Hist.* 1849, vol. 4, 2nd. ser. pl. 8, 9. And *Trans. Tyneside Naturalists Field Club*, vol. 1, p. 327. June 1849.

will appear in the Annals, and 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 a 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, and it is on this ground that I should so much like to dissect a specimen of your genus. I have now dissected species of all the genera of the Cirripedia, and have nearly finished the systematic part of the Pedunculata, but yet from the extreme slowness of the 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.

No. 2.

Down Farnborough Kent,

Sept. 29th, 1849.

Dear Sir,

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

I should have written about *Lithotrya* in former letter, but I had hardly space, and I did not know whether you would like to have my views on the point. I conclude that the *Lithotrya* forms its own holes, from having seen *numerous specimens*, and (four or five species) large and small, all *exactly* fitting their cavities. The calcareous cup is indisputably (I speak after careful examination, with dissolution in acids, etc., etc.) formed by the Cirripede, and is common to most (I believe all) the species of the genus. You are perfectly right that the calcareous cup is never moved. But the Cirripede inhabits (as far as I have seen) only cellular rocks, or corals, or shells (such as the Cirripede *Conia*,* with large tubes), and the pupa crawls into some minute cavity, and there fixes itself for ever, and then as it grows enlarges the hole to required size; this it effects by the edges of the valves and of the minute scales on the peduncle being sharply serrated; and 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, and new sharp ones periodically formed. This moulting of calcareous scales is a unique case, and I have no doubt is in relation to their boring necessities. I believe this is the way which my *Asthrobalanus*,† which inhabits the *Concholepas*, also makes its cavities; and its outer tissue is studded with elegant minute trifid and quadrifid points; and the shell is apparently first perforated by other animals; but I have got to go over this again with *Asthrobalanus*, but I have had the misfortune to lose nearly my whole stock of specimens, of which I collected thousands, for fifteen 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* and clear the corium from it and place it under the compound microscope, you will find the rasping minute points, and I believe you state that it inhabits shells abounding with cavities of *Cliona*, etc., etc.

I am most particularly obliged to you for informing me of

* = *Tetracilita*, Schumacher. See Darwin's Monograph, Balanidæ, p. 321.

† = *Cryptophialus*. Ibid. p. 563.

Lovén's Cirripede, of which I had not heard. I should be *most grateful* for a transcript of the paper and drawing, or if the book be not above 2 lbs. or too precious could it not be sent, and I would gratefully repay postage, and thus save Mr. Alder 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,* and from not having calcareous valves might be called an Alepas; but, strange as it would appear to any one who had not studied the internal structure of these animals, it is not even one of the Pedunculata, but belongs to the sessile division, and forms a new genus between Tubicinella and Coronula. I should have been very sorry to have overlooked Lovén's description.

I presume you have a superabundance of materials, but if at any time you would 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 requests, and believe me, dear Sir,

Yours sincerely obliged,

C. DARWIN.

To A. Hancock, Esq.

I see in the *Athenæum* they have omitted to express how valuable I thought your discovery, and how interesting your whole paper. I am very curious to see what you say about the palpi. I could not follow the reading aloud of this part, but if I understood right, the palpi are wonderfully different from anything I have seen in the Cirripedia. I think it possible that Alcippe and Arthrobalanus may turn out distinct *orders*. The metamorphosis is certainly different. My larva has no *thoracic* legs, where yours has; mine is binocular, yours uniocular, etc., etc. Yet the three pairs of cirri, the great labrum, and habits are certainly strong points of resemblance.

I ought to apologise for the length of this letter.

* = *Conchoderma*.

No. 3.

(Postal date Oct. 30th, 1849.)

Down Farnborough, Kent.

My dear Sir,

I have to thank you sincerely for many things. Your specimens arrived quite safe. I have as yet taken only a cursory glance at them; for I have an odiously tedious job of compiling long generic descriptions from my specific descriptions. When I have done in a fortnight's time, I will enjoy the treat of having a good inspection of Alcippe. I hope by that time your paper will be out, as it will save me much time in comparing every part with common Cirripedia; indeed I will wait till I can get the number with your paper. It is an immense time since I have seen a new form of Cirripedia. At the same time I will look over my Mollusca, and my few notes made at the time; and if they turn out of the slightest interest to you, I shall be heartily pleased by your acceptance of them. I will be careful of the specimens of Alcippe. Your sketches are *very* spirited. The Cirripede from Australia is the *Ibla Cuvieriana* (= *I. quadrivalvis*, Cuvier); that from Madeira is an unnamed species, which I have unwillingly been compelled to make into a new and insignificant genus. I have called it (supposing name be not used) *Machairis celata** (from being encrusted with bark of the Antipathes). If you have *any other* Cirripedes from foreign localities, and would allow me to examine them, it would be of great service to me.

Will you please to give my sincere thanks to Mr. Alder for the specimens, and for the great trouble he has taken in copying Lovén's paper. It is a most interesting Cirripede, and the type of a new family or order, for it has no relation to Alepas, the animal of which I know well. I must write to Loven; his description is unfortunately short. Will you add to your kindness by some time asking Mr. Alder to what place the Royal Academy of Sciences given in the title belongs. Lady Lyell translates the title as 'Extract from a Review of the Trans. of the R. Acad. of

* = *Orymaspis celata*, Darwin. Monog. of Cirripedia, Ray Soc., 1851, p. 134.

Sciences, 1st series, 1844, p. 192-4.' Secondly, will you be so kind as to tell me on what being (for I cannot read the word) your specimen of the *Ibla* is attached. And thirdly, whether you had any motive for calling your Cirripede 'Alcippe,' as perhaps I will change my long name of *Arthrobalanus* for a shorter one. Any time will do for an answer.

With respect to *Lithotrya*. The shells have relation to diameter of hole, but the shell-part of full-grown ones, *I believe*, project beyond their hole. This is hard to know, as peduncle shrinks much from drying. Holes are bored in all directions. The animal often *rises* a quarter of an inch in its hole from thickness of cup. Very young specimens have cups, *I believe* at earliest period. I cannot describe the whole process of fixing in letter, but I must think it quite impossible that any Cirripede can sink its basis in any object. I have thought that the larva of *Lithotrya* instinctively (and this not wonderful) creeps into the crevices of the coral-rocks to that depth, from which it can when nearly full-grown freely reach the surface; in the interval I believe it feeds on Infusoria in the water circulating in the crevices. I once thought that the larva of *Arthrobalanus* might have bored its hole with its prehensile antennæ, but I cannot now believe this. But there is another view or conjecture, which is perhaps the most probable, viz., that the larva (in second stage) boring a minute hole by an acid secreted from some gland, and through some duct and orifice in the prehensile antennæ (alluded to by me in *Athenæum*), by which afterwards the cement-stuff is poured out. This view would perfectly harmonise with the facts, of which I cannot doubt, that the Cirripede after metamorphosis can never alter its point of attachment; and secondly, the apparatus of minute points for enlarging its cavity in *Lithotrya*, *Arthrobalanus*, and *Alcippe*, is equally applicable.

But I shall utterly weary you with this discussion. Your statements about cavities of *Alcippe* make me doubt my view of the larva creeping into already existing cavities.

With my sincerest thanks,

Yours very faithfully,

C. DARWIN.

No. 4.

(Postal date Dec. 26th, 1849.)

Down Farnborough, Kent,

Dec. 25th.

My dear Sir,

I am very much obliged for your last very interesting letter, with your answers to all my queries, and the copy of your paper, which I am very glad of, though I take in the Annals.

I have not yet looked at Alcippe, for I have found my writing work run out, and I have lately received several new pedunculate species to describe. I have had such a misfortune in the loss of a parcel of Cirripedes from Copenhagen, amongst which was the curious *Alepas squalicola*!* the knowledge of which I owe to Mr. Alder's kindness.

I have of course read your description of Alcippe, and it is most clear and definite. I hope to put *Arthrobalanus*† in the same order with it, but it will be stretching a point to do so, and they must form distinct *families*, more distinct than any Pedunculate and any Sessile Cirripede are from each other. I hope before long to indulge in a look at Alcippe. I have had occasion to relook over many specimens of *Lithotrya*, and am as fully convinced as ever that the basal cup is fixed at a very early period, and is never moved. In one specimen several specimens were embedded in rock, parallel but in reversed positions (and others at right angles); and of the parallel ones the greater number had their capitulum (*i.e.* shells or valves) directed from the exterior surface of the rock inwards, so that they could never reach the surface, and must have lived in a subterranean cavity. I am as much as ever in the dark, whether the larva *creeps* in or *bores* in.

I had intended, but forgot, to ask you about the *Clitia*. All which I have seen (except some rare foreign species, which I have not yet touched) were fixed on *fuci* and stones; and if it be not asking too great a favour, I should be very much obliged if you would permit me to look at any fixed on shell. Several

* = *Anelasma squalicola*. Monog. of Cirripedia, Ray Society, 1851, p. 170.

† = *Cryptophialus minutus*, Darwin. Monog. Cirripedia, Ray Soc., 1854, p. 566.

months ago I had *one rather* careful inspection of the basal membrane, and was much surprised not to be able to see the prehensile antennæ of the pupa, or any orifices for the cement-stuff; yet from analogy I can hardly doubt that *Clitia* fixes itself like all other Cirripedes, in the manner briefly explained by me to the British Association.

In the case of *Coronula*, *Tubicinella*, and *Chelonobia*, I have *fancied* that the sinking was entirely or chiefly owing to the growth of the surrounding parts of the animal to which the Cirripedes were attached. I am much surprised about *Clitia*, and it shows me that there is even in common Cirripedes something about their attachment which I do not understand at all. I have seen, as I *believe*, that the cement could corrode through the membrane of its own peduncle, but not act on the calcareous scales supported by this membrane; so that your fact of the *Modiola* is still odder, and I hope you will allow me to quote it from you.

I have seen the larvæ of most of the species of *Anatifa*, and I think of *A. vitrea*,* but I have had either to dissect them out of the egg and just after their escape, and never as yet when naturally sent forth from the parent. In the state in which I have seen them they certainly had not any 'process or pedicel,' but exactly at the spot figured by you lies their mouth, which is very slightly prominent, without any trophi, and leading into an œsophagus running *anteriorly* and lost in cellular matter. If you would let me have a few of these specimens I should be *very much* obliged, and especially if you would give me any precise observations of your own on this 'pedicel,' for I do not in the least doubt that with all your experience in dissecting your observations would be more trustworthy than my own. Goodsir figures something like a masticating organ attached to base of legs of larvæ, which I could never see, and which, if such exist, would be a strange coincidence with *Limulus*.

No doubt you are aware that in all Cirripedes the larva from the stage you have figured becomes (so called) bivalve, hexapod, with prehensile antennæ, binocular, etc.; and when it attaches

* = *Lepas fascicularis*. Mon. Cirr., Ray Soc., 1851, p. 92.

itself it is in fact a natatory pupa, for it has no mouth, only a rudimentary shrivelled oesophagus, surrounded by the forming trophi of the young Cirripede. *Asthrobalanus* (= *Cryptophialus*) alone passes the first larva stage in egg, and appears when first born with prehensile antennæ, two eyes, etc.

But perhaps I weary you with these details; one forgets that others do not care so much for a subject as he who is at work on it.

I have not yet gone through my bottles for Mollusca, but will you be so good as to send me one line, to say whether you care for any naked Terrestrial (as *Vaginulus*, *Parmacella*), or aquatic naked Mollusca, or for any of Cuvier's "Tectibranches," as *Aplysia*, etc., or whether exclusively for the Nudibranch. I believe I have a few of each order. I fear that you will think I have written to you at unreasonable length.

Pray believe me,

My dear Sir,

Yours very sincerely,

C. DARWIN.

I begin to think I shall spend my whole life on Cirripedia, so slow is my progress, working only two to three hours daily.

No. 5.

(No postal date. 1850 in A. H.'s writing.)

Down Farnborough, Kent,

15th.

My dear Sir,

I ought to have sent you a line sooner to say that your specimens arrived safely. I will venture to keep the Madeira one till I commence reworking on the Pedunculata. I have marked outside the box to be 'returned to you' after I have taken a few.

The *Balanus* I will return almost immediately. It is one of the *very few* species which I dare name with little or no hesitation without opening (with the aid of some part exposed near the

basis). It is the *B. sulcatus* of Bruguière = *Lepas balanus*, Linn.* You sent me formerly specimens mingled with another species attached to a Pecten with the Clitias.

Do you know the latitude on the coast of Greenland? It would be valuable information for me. If you do even approximately, will you write it on slip of paper, without anything further, and send it me? I have this species from Iceland.

With respect to Lithotrya, I feel a conviction that if you had seen all the specimens which I have, you would not doubt that all the species bore, in whatever manner this may be effected.

Yours very sincerely,

C. DARWIN.

No. 6.

(No postal date.)

Down Farnborough, Kent,

May 12th.

My dear Sir,

Owing to a perhaps foolish habit of not reading periodicals when they come out, I have only just read your very interesting paper on the boring of Mollusca in the Annals; and this reminded me that you wished for more information regarding Lithotrya. I really do not know what to give. I have three specimens of Lithotrya, and I enclose one for you. I have picked out one that has lately moulted (this moulting of scales is unique in whole order of common Cirripeds), and therefore has the scales on peduncle, with the teeth pretty sharp. The valves, of course, are not moulted, but the old layers scale or are rubbed off. I have not *one* with the basal calcareous cup, though several have been lent me. I wish you could see the basal cup. I feel SURE it would confirm your opinion that it could not be the borer. I can see no reason yet to alter my opinion, that Lithotrya either crawls into the cavity, which it enlarges, or, if not, that the larva has the power of boring a hole, in which it fixes itself and

* = *Balanus porcatus*. Mon. Cirr., Ray Soc., 1854, p. 256.

undergoes its metamorphosis. I have several foreign species of *Clitia*, and I will attend to their to me quite wonderful boring powers.

Yours very sincerely,
C. DARWIN.

No. 7.

(No postal date. Marked "1850?" by A. H.)

Down Farnborough, Kent,
Sunday.

My dear Sir,

I send one line to beg you to keep my MS. as long as ever you like. I guessed why you did not write; it was wholly unimportant. I am sorry for the smash, and sorrier the species do not turn out more interesting; it is, however, as you say, curious about the Ranges.

The *Balanus* sent (for which many thanks) is the common *B. Cranchii* of British authors. I have never seen it from north of Tenby, in S. Wales, I will return it hereafter if requested.

I should be very glad to see the Greenland *Balanus*. Please state when sent whether to be returned. You know I must disarticulate a specimen for examination.

Have you several specimens of the Madeira little pedunculate Cirripede (which I named *Machairis*,* and have now changed into *Oxynaspis*), and if so, and you would *lend* or give me one for disarticulation, I should be *very glad*, as my specimens are all in *utter* state of *decay*, and several points of the dried animal remain unexamined by me. (I have this genus fossil from Chalk!)

I mean now to continue at the Systematic part till I have finished; a period which will arrive Heaven only knows when.

Many thanks for your letter. In haste.

Yours truly,
C. DARWIN.

* Monog. Cirripedia, Ray Soc., 1851, p. 133.

No. 8.

(Postal date Dec. 28th, 1850.)

Down Farnborough, Kent,

Dec. 25th.

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*, thanks to the enclosed drawing (which please return) sent me by Steenstrup without text. I suppose the same explanation is applicable to *Arthrobalanus* (= *Cryptophialus*, Darwin), and I should 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 *Clitia*.

Since receiving this same Plate I have had a good deal of rock, bored by *L. dorsalis*, given me, and I now find out, for the first time the following important facts:—(1) That the animal bores to its full depth when young, and 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 $\frac{1}{20}$ th or $\frac{1}{16}$ th of an inch, or even more, beneath that last formed; the disc or cup, as I was always certain, never itself being moved. 3rd, The lowest disc is *never* at the bottom of the burrow, and 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 should 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 . . . or exuviae 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 and on the beads on the peduncle, with their exuviations, sufficiently explain the mere increase in diameter of the burrow).

I cannot explain in a letter how the discs are fixed; but it is 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 antennae of the larva (this cement is formed by a gland, strange to say, which is certainly part of the branching ovaria), and subsequently during life, in different Cirripedia, either through these two 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, and 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*, and have many more months' work, and till I have completed this slavery I have not heart to begin work of interest, for I think I should never get courage to resume the drudgery of describing species and making out synonyms. I hope this letter will not bore you.

Believe me, my dear Sir,

Yours sincerely,

C. DARWIN.

P.S. The accompanying specimen of, as I suppose, a Cliona you can throw in the fire if of no interest to you. From northern part of Patagonia.

No. 9.

(No postal date. Marked 1850 by A. H.)

Down Farnborough, Kent,

Saturday.

My dear Sir,

I am ashamed at myself to think how long I have taken to send you my Mollusca. I have now got them in a bottle, and will send with them a catalogue of localities. There are about sixty packets, though some are duplicates. I send with them the *rudest* notes of colour and size made at the time. The colours are given by comparison with Pat. Symes' (?) nomenclature. The notes are those of an *ignorant schoolboy*, as I was almost then, and shamefully written. I would have copied them out if they had had any value. Will you nevertheless preserve these notes, for as one sometimes likes to see an old book, so I like to keep my wretched zoological notes. I fear my specimens can be of hardly any interest to you, they must be so shrunk from the spirits. I *think* there are one or two new genera. I will despatch the box with large bottle on next Wednesday, the first day our carrier goes. You will understand I do not want specimens ever returned. If there should be any part of MS. which you by chance should wish to read, I will with *pleasure* copy it.

Very many thanks for the Clitia. It has astonished me and convinced me of my ignorance. I entirely give up the burrowing of your Alcippe and my Arthrobalanus (= *Cryptophialus minutus*); I *only* do not give up Lithotrya, from its large misshapen cup being so ill-formed for burrowing, and from its having a beautiful rasping apparatus. How difficult it is to discuss any point by letter. I now see that I omitted to mention to you that *all round* the base, and therefore widest part of the head or shell on the top of the peduncle, there is a beautiful rasping rim or circular toothed saw; renewed, moreover, during every moult, when the shell and animal increases in size; and as the peduncle has great power to lengthen and shorten and twist itself about I cannot doubt, and if you were to fix a young Lithotrya at the

bottom of a deep hole of the diameter of a pin or straw, during growth the animal would be enabled to enlarge it to any extent. I confess I am quite puzzled by *Clitia*. It appears to me, from your specimens (which I must hereafter further examine), that the whole of the corrosion is effected round the margin of the base; that is, that no corrosion or wear goes on except round the growing basal edges. Did you come to this conclusion? I cannot doubt that the shell is so fixed that it cannot move; certainly there are no sharp points on basal membrane, as I have formerly examined it under high power. Does not your fact of the *Modiola* show that the action is effected by solution, or at least not mechanically.

You ask me about Goodsir's male *Balanus*. It is quite a mistake. His *male* *Balanus* is a *female* crustacean allied to *Bopyrus*, and his *parasite* is the male of this female.

But now comes the odd case. I have found two genera of Cirripedes with *males* separate and parasitic on the females. In these cases I am sure there can be no mistake, though I will not take up your time with details.

I have not *yet!* looked at *Aleippe*, for ever since writing last to you, my two-hour-per-day work has been occupied with a tiresome set of fossils. I have the curious *Alepas squalicola* sent me from Copenhagen, but I have not looked at it yet.

Yours very sincerely,

C. DARWIN.

No. 10.

(No postal date. "1851" by A. H.)

Down Farnborough, Kent,

June 8th.

Dear Sir,

I am going to beg you to endeavour to procure me a very great favour from Mr. Alder, namely, the loan of the volume or of the plate of Lovén's *Alepas squalicola*, of which he most

kindly sent me an outline tracing. Seenstrup sent me one specimen, which I dissected, after comparing it externally with the drawings, and now I am *most anxious* for Mr. G. B. Sowerby to copy two of the figures for my volume for the Ray Society, for which the plates are now engraving. I do not know whether there is a copy in the British Museum, and if there be, it would be very troublesome to obtain permission to have a copy made, and such would not be so accurate as if Mr. Sowerby could have the plate at his own house. I have charged him, in case Mr. Alder would confer this favour on me, to take the *greatest* care of it, to acknowledge its receipt, and to pay its return carriage, and not keep it long. Mr. S.'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.

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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.

P.S. Will you forgive my sending so untidy a note, but writing the above reminded me that some time since I purchased four specimens, which until this minute I unaccountably have

forgotten; but I see all are attached to a most rugged surface. If yours happens to be attached to anything *smooth*, and you could spare the *base* of the peduncle *still* attached, I should be very much obliged.

No. 11.

(June 22nd, 1851, by A. H.)

Down Farnborough, Kent,

June 22nd.

My dear Sir,

I write merely to thank you very much for your assistance regarding the book, and to request you to especially thank Mr. Alder when you see him for his kindness. I am much obliged for the offer of the Iblas, but being on a rugged support I do not think they would aid me.

Pray believe me, yours sincerely,

C. DARWIN.

You can return the MS. whenever you like, but I should be sorry to lose the pages, though of no value.

No. 12.

(25th Dec., 1852, by A. H.)

Down Farnborough, Kent,

Dec. 25th.

My dear Sir,

You will probably remember that you called my attention to the following facts, that Verruca (= Clisia, etc.) (1st) has the power of excavating a slight depression for itself; but that (2nd) epidermis on a shell quite stops this process; and (3rd) that under its middle there is sometimes a hollow, sometimes with chalky matter. I have just been at work on the

genus, and 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, and 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 and has not in the least bored. I have examined the single shell, and cleaned with potash, and after acid, with all powers.

(2nd) Either the shell or basal membrane must, on mechanical theory, be the wearing agent; and certainly, as far as the central hollow, it must be the basal membrane; but the basal membrane is united to the shell and animal's body by (besides corium and epidermis) only a circle of fibres, which Prof. Quekett, after most careful testing, says are *only ligament*: 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.

(3rd) When a central hollow has been formed, the basal membrane (in this case generally brittle or *cracked*) is loose over this middle part, but was once *certainly* attached, as I have found the prehensile larval antennæ in the middle surrounded by the ordinary cirripedal cement, which certainly would require considerable mechanical power to separate from any object of attachment, and yet there is nothing whatever over this 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 *Balanus lavis* (of which I send a valve, *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, and were not in the least affected; whereas a third specimen of hard *marble* was excavated.

(5) The cement-ducts *might* pour 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?). 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 *Verruca* attached to any softish rocks or substances, without calcareous matter, and look and 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 *Aleippe*, which I have never done yet! Have you anything new (or any fresh specimens to spare) on this most curious genus? I should be *pleased* to hear that time or inclination had led you to look at what I have said on the sexes of *Ibla* and *Scalpellum*, about which I remember once writing to you; and which facts appear to me curious.

Again I beg forgiveness for the length of this letter, and remain,

My dear Sir,

Yours very faithfully,

CHARLES DARWIN.

A. Hancock, Esq.

I do not think my wretched schoolboy MS. on the outlandish Mollusca has been returned? Has it?

No. 13.

(Postal date Jan. 12th, 1853.)

Down Farnborough, Kent,

Jan. 10th.

My dear Sir,

I am uncommonly obliged to you for taking so much trouble as to write at such length to me; though in truth, when I think of your many important pursuits in Natural History, I am ashamed to have lost you more than one good hour of time.

Your cautions and suggestions will be of considerable service to me, as leading to fresh observations, and making me explain some points more clearly. I will not take up your time in going into several points you notice in this letter, but they shall all be more or less attended to in my book.

I may just inform you that when a ribbed shell is cut through it can be seen that the marginal erosion* does not *graduate* into the central hollow; indeed if the whole base was simultaneously being eroded it is hard to see how the basal membrane and shell could be firmly attached. I quite agree that more specimens on calcareous and non-calcareous supports should be examined, and I will write to a naturalist in Devonshire to collect for me. I think, however, you did not understand that there were *several* specimens on the two slate-rocks and hundreds on the Laminariæ.

I am quite delighted at what you say about my little friends, the complementary males: I greatly feared that no one would believe in them; and now I know that Owen, Dana, and yourself are believers, I am most heartily content. I entirely agree with you on your remarks on cross-impregnation. Some years ago I set to work to collect facts on this head, but I have as yet done nothing with them. Such a view as yours is the only foundation, I am well convinced, to Steenstrup's rather wild Memoir on the non-existence of Hermaphroditism in Nature, though he extends the doctrine to mere physical organs!

Many thanks for the wretched MS. returned. I am quite sorry I asked for it, for I never dreamed that you had not long

* Diagram in illustration given in letter.

ago got what little good you could out of it. I shall be pleased at your doing whatever you liked with my specimens, etc.

You shall hear when I have * . . . with Alcippe. The other evening I read over your paper, and could not get to sleep for hours, from thinking of its curious and anomalous structure. I have some other specimens of yours.

With my sincere thanks,

Believe me, my dear Sir,

Yours sincerely,

C. DARWIN.

No. 14.

(No postal date.)

Down Farnborough Kent,

Jan. 29th.

My dear Sir,

I write in a hurry to catch to-day's post to beg a favour and to apologize. For the former 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, and I write to ask whether you have any dry shells with Alcippe you could 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 the year. I should be *most grateful* if you could send me such by post, allowing me to pay postage if heavy. Alcippe has *no* relation to my burrowing South American little Cirripede.

Would it be possible to employ for me any fishermen to get the shells now? though specimens taken *later* than these you sent me would perhaps be most useful to me; but any now would be of greatest interest to me. My surmises are too vague and

* A piece of the letter torn off here. Perhaps "grappled" is the word used.

too long to tell in this note, and 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.

No. 15.

(No postal date.)

Down Farnborough, Kent,

Feb. 10th.

My dear Sir,

I trouble you with one line to say that amongst the few remaining and on the cut up and 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 should, 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; and indeed I should 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, and never dreamed it was a Cirri-pede! 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.

Yours very truly,

C. DARWIN.

No. 16.

(Postal date 12th, 1853.)

Down Farnborough, Kent,

Feb. 12th.

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, not strictly on normal type of *Lepadidæ*; 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 the labrum.

Cirri.—The organ you have called palpi, or first cirrus, certainly is the latter, and not much more modified than in *Anelasma*. The thoracic segment supporting this cirrus is confluent with mouth, and forms the prosoma in normal manner. The *second* (N.B. This is properly the third segment of archetype crustacean.) thoracic segment is large and obscure; the third and fourth very distinct. In one monstrous specimen the fourth segment bore an extra cirrus! showing that the segment is true. The fifth segment small, but quite distinct, and bears the pair of cirri nearest the mouth. The sixth 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 on the inner ramus metamorphosed. The caudal appendages are never biramous, and hence have not cushions. It deserves notice that *Alepas cornuta* has the inner rami of the fifth and sixth pair of cirri (and of no other rami) rudimentary, without muscles, and short. These three pairs of organs in *Alcippe* are all in some respects in an embryonic condition.

Alimentary Canal.—Œsophagus normal; biliary envelope thick and irregular, as in *Anelasma*; rectum and anus *none*. I am

positive of this latter fact, and it is the most curious point in the anatomy of the genus. Alcippe must *always* eject (as other Cirripedes sometimes do) its excrement from the mouth.

Acoustic and olfactory orifices as in Ibla. Eye not discovered. Nervous system hardly examined.

Female Organ of Generation.—All quite normal, as described under the Lepadidæ. The ovigerous fræna are very large, and are destitute (as in some species of Pollicipes) of glands; they probably serve as branchiæ, as well as the universally-admitted branchiæ in sessile Cirripedes, of which they are the homologues.

Male organs none except a rudiment of penis in normal position between and on ventral side of sixth cirrus.

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

Metamorphoses.—In first stage I can add only the minute and lower antennæ. Most fortunately I detected some larvæ just before their metamorphosis into Alcippe. They are peculiar in having their prehensile antennæ seated almost at the very end of the quasi-bivalve shell; the abdomen and caudal appendages are peculiar; but they have six pairs of natatory thoracic legs, and in all other respects resemble the pupæ of Lepadidæ.

Male Alcippe.—Almost every female has from one to three or more males attached to her on both sides, externally, near the upper end of the horny 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, and caricatures the form of the same part in Alcippe; the other part (or capitulum) is flattened, *much produced*, and of nearly the same width throughout, with a *small* orifice at the upper end. Total length $\frac{1}{2}\frac{1}{5}$ th of an inch. In the lower part we have an *eye*, and great testes and vesicula-seminalis. In the capitulum we have nothing but a tremendously long penis coiled up, and 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 three first segments of

* Sketch given in letter.

head), containing the testes, vesiculæ, and 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 the male of *Alcippe* from absolute identity of the pupæ of both sexes and other reasons. I believe the males occur on every female. In one case I found *twelve* males and two pupæ on point of metamorphosis permanently attached by cement to one female!

Excavation of Burrow.—The outer skin, and even the whole thickened edge of the orifice into the sack, with its strong sharp spines, is periodically and often moulted. There is a great difference in the state of the star-headed spines on the *new* underlying and the *old* worn spines on the old membrane. This being the case, and there being good motive power in the long and transverse and adductor muscles, I do not doubt the excavation is wholly mechanical. From the position of the larval antennæ, *Alcippe* after metamorphosis must occupy a position fully as much inclined, or more inclined, than as drawn,* and 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, and 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 the shell *over* the peduncle of *Alcippe* are formed by a minute Annelid.

Affinities.—I am dreadfully puzzled, and every day change, whether to form a family for its reception, as in one point of view it amply deserves, or to be guided by its varied close affinities to several genera amongst the *Lepadidæ* and 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 yet tell.

I fear my long rambling letter will puzzle and weary you.

* Sketch given in letter.

Accept my very sincere thanks for allowing me to examine this most curious Cirripede, and believe me,

My dear Sir,

Yours very truly,

CHARLES DARWIN.

No. 17.

(No postal date.)

Down Farnborough, Kent,

Feb. 25th.

My dear Sir,

Whenever you have a few minutes leisure I should be very much obliged for answers to two questions, if you can answer them. (1) At what depth is *Alcippe* found? (2) At about what date was the shell taken which you sent me in spirits with all the specimens of *Alcippe*. I want to know, because most of the males were well filled with spermatozoa. I fear I wrote to you at too great length in my former letter.

Pray believe me,

Yours very truly obliged,

CHARLES DARWIN.

The dried specimens you sent me by post lately, swarmed with males, but all too dry and shrivelled to do much good with.

No. 18.

(No postal date.)

Down Farnborough, Kent,

March 30th.

My dear Sir,

I am much obliged for your note received this morning, with as full answers as you could send to my queries, and 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; and 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 and for distribution.

I yet have a few specimens of other Cirripedes of yours in my possession.

I have now finally finished with my South American Boring Cirripede; and this has utterly confounded my previous confusion how to rank *Alcippe* and it; for they present some most remarkable similarity, for instance, they are both bisexual, with the males remarkably alike, and yet, in what I must consider their fundamental organization, and in their metamorphosis, they are so totally unlike that I cannot place them in the same orders! My classification does not satisfy myself, nor, I fear, you, if ever you look to my volume on this point.

Pray believe me, my dear Sir,

Yours truly obliged,

CH. DARWIN.

The bosses on the rim of *Alcippe* are hardish or crustaceous, they are all four opposed to each other, and 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; and now I find in my *analogous* South American burrower, and in no other Cirripede, that the œsophagus is provided with the most beautiful discs, set with teeth, and brushes of hairs, worked by muscles, certainly for triturating food, which strengthens my notion.

No. 19.

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 the British Museum? 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 Straits, which I know is to be returned. I could 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 and truly for the very great pleasure I derived from examining *Alcippe lampas*, which is described in full in my volume, now printed, and I presume soon to be published by the Ray Society.

I have also discussed the excavating power of *Verruca*, which subject I owe entirely to you.

As there are several specimens of the *Oxynaspis* from Madeira, I have ventured to take two or three to give to Museums. With my sincere thanks, and with much respect,

I remain, my dear Sir,

Yours sincerely,

CHARLES DARWIN.

No. 20.

(No postal date.)

Down Bromley, Kent,

May 25th.

My dear Sir,

I am really very much obliged to you and Mr. Storey,* and am quite ashamed at having caused so much trouble, but I was very curious to obtain this information. My present work leads me to wish to get as accurate information as I can on what some call the economy of nature, and the point in question seemed to me deserving of attention, as aiding in shewing how far the struggle with other species checked the extreme possible

* John Storey, F.B.S.E., was a careful and accomplished botanist, and formerly Honorary Secretary of the Tyneside Naturalists' Field Club.

northern range of any species. It seems odd that dwarfing should be so frequent on mountains, and so rare, or at least not equally conspicuous, at the extreme northern lowland limits of a species.

I hope that you will be so kind whenever you see Mr. Storey to present to him my sincere thanks for all the trouble he has so kindly taken for me, and pray believe me,

My dear Sir,

Yours very sincerely,

CHARLES DARWIN.