

J. E. GRAY, CHARLES DARWIN, AND THE
CIRRIPEDES, 1846-1851

By A. E. GUNTHER

THE exchanges, correspondence or other, between John Edward Gray (1800-1875), Keeper of Zoology at the British Museum from 1840, and Charles Darwin (1809-1882), are so few that the little there is assumes unusual significance. It is however, a fortunate circumstance that their one and only working collaboration reflected favourably on the character and generosity of both men. This concerned an investigation into a sub-class of the Crustacea, the *Cirripedia*, comprising the barnacles (the acorn shells and forms related to them). The common goose-barnacle, *Lepas anatifera*, had long drawn attention to itself by adhering to the bottoms of ships, and so acquired a high nuisance value which prompted further study. The first naturalists associated with investigations of its structure and classification from the eighteenth century onwards include several famous European names: among the French, Cuvier, Blainville and Lamarck who removed it from the mollusca into the crustacea; and among the English, John Hunter, Everard Home and W. E. Leach. It was Leach (1790-1836) who, having a large collection at his disposal in the British Museum, at Montagu House, provided the *Encyclopaedia Britannica*, Supplement of 1819, with the first English classification (1). It was this collection that gave both Gray and Darwin much of the material for their work, and since this paper is concerned with the relations between them rather than with the specimens, a word needs be said of the different course of their early careers.

Both came from families possessing intellectual and scientific background, but whereas the Darwins were well to do, Gray's father, from no fault of his own, had fallen on difficult times, and his means scarcely allowed even the education of their eldest son. That John Edward, the younger, received an education at all was due largely to his own initiative and to an ability that was recognized by his teachers. Darwin's early life, on the other hand was cushioned by a family of independent means that gave the boy both a public school education and years at both Edinburgh and at Cambridge Universities.

Gray was eight years older than Darwin. Following a medical education and various ups and downs he found regular employment at the British Museum in Montagu House in 1824 at the age of 24, the year that Darwin went to Edinburgh. In 1828, finding himself unfitted by nature for life as a

physician, Darwin was sent to Cambridge where, in 1831, he took a degree and in December of that year joined the 'Beagle' as a naturalist. On his return to England in 1838 until 1841, he was Secretary of the Geological Society in London, and then, in 1842, moved with his family to Down, Farnborough, Kent, where he lived for the rest of his life. By contrast, Gray remained an assistant to the Keeper of the Natural History Department at 15/- a day until 1836 when he was taken onto the staff and given a salary. In 1840 he was promoted Keeper. In those first fifteen years he concerned himself with building up a modern *systema naturae* through the issue of a series of Lists, or embryonic Catalogues, which were to include all animal groups. His productivity was little short of amazing. In 1825, for instance, the year he first issued a *Synopsis of the Genera of Cirripedes* (2), he prepared no less than thirty papers including a *Synopsis of Reptiles and Amphibians* and a draft of one on *Mammals* (3). Considering how much else he was doing his first attempt at classifying the *Cirripedes* provided a useful basis of discussion. His complaint of previous workers was that they failed to consult or quote the work of others, a criticism that included Leach's attempt at naming the Museum's large collection. His classification was included in the *Encyclopaedia Britannica* of 1819/24, a copy of which is heavily annotated and captioned in Gray's hand (4). Gray's paper was honoured by a summary in Ferussac's *Bulletin des Sciences Naturelles . . .* (5) the next year, and in Oken's *Isis* in 1834 (6). Gray's last observations on the *Cirripedes* were recorded in 1833, (7) as a note on their reproduction from the coast of Devon, and in 1848 (8) from material supplied to him by others.

From 1838, on his return to England, to 1846, Darwin was principally engaged on working over his notes of the 'Beagle' voyage. Then, as the result of a conversation with Gray, whom he had got to know as a frequent reader of papers at the Zoological Society, he decided, after some hesitation, to accept Gray's suggestion, that he should engage in a study of the *Cirripedes*. He was further encouraged in this by Gray's offer of handing the subject wholly over to him, including specimens and documents. The subject, as he was later to quote Agassiz had become 'a pressing desideratum in zoology', and it must have been perfectly clear to Gray that the increasing demands of the Keepership would make it impossible for him to undertake the time consuming work a complicated group required. Darwin started work for his monograph about September 1846. In December 1847, after eighteen months, he wrote to Gray asking that he should be granted permission by the British Museum Trustees to examine the Museum samples in his home at Down. The following is a copy of Darwin's letters in Gray's hand (9); and it refers equally to stalked

and acorn (balanomorph) barnacles on which Gray had worked, and which were loaned:

In case the Trustees are inclined to do me the honour of acceding to my request, I pledge myself to take the utmost care of the collection and to do nothing whatever to the specimens without your express permission.

I will only further add that Mr. Cuming and Stutchbury and yourself have placed their most valuable collections at my disposal for description and that I have a fair collection of my own.

How much a monograph of this order is wanted, you know it far better than any one in England, are well aware. In fact the whole of the specimens are in almost a complete state of chaos, as Agassiz has remarked, "a monograph of the Cirripedia is now a pressing desideratum in zoology". How far I am capable of this undertaking you must decide; if I fail it will not be for want of labour.

I apologise for the length of this letter and beg to thank you for the kind assistance you have already given me.

I remain,

your very faithfully

Charles R. Darwin

To J. E. Gray Esq.

The following is a draft, in his own hand, of Gray's submission to the Trustees: (10)

B.M. Zoological Department, Reports, Minutes etc. 1847-1848 ff. 84, 84vo.

Copy in Gray's hand.

December the 28, 1847.
Down, Farnborough, Kent.

My dear Gray,

You are aware that I have been attending for the last 14 months to the anatomy of the various genera of Cirripedia. Having, as I hope, now acquired a fair knowledge of their fundamental structure, it is my intention to publish a monograph of this difficult order. The object of this letter is to ask you to request the permission of the Trustees to describe the public collection of the Museum. This involves however the absolute *necessity of my having the collection*, not all at once, but in groups *at my house here*. I find by experience that each species takes me between 2 and 3 days and each new genus as many weeks. Every portion requires examination under the micro-

scope and all the member organs under a high compound power. The shells also require soaking and cleaning. I have resolved not to describe any species without I can do it thoroughly. I am well aware that my request is an unusual one; but I would most respectfully beg to call the attention of the Trustees to the fact that specimens are sent out to be mounted and that one specimen of every species of Cirripedia must be disarticulated for the character to be ascertained and the parts of the mouth to be dissected. The portions thus dissected I prepare in spirits between two plates of glass. If the Trustees think me worthy of their confidence I will give to the Museum all such preparations (whether made from my own or the public collection) and all my entire shells including many new species as soon as my work is completed. I would further beg to call the attention of the Trustees to the fact that their entire collection, contained in 8 or 10 drawers, will thus be named and arranged without the loss of the valuable time of the officers, though I fully believe that you could do the work in half the time I could, yet I am convinced that, to examine and classify the public collection in the order it should be done, it would take a year.

B. M. Zoological Department, Reports, Minutes etc. 1847-1848 ff. 82, 83
In Gray's hand (draft)

Mr. Gray at the request of Mr. Darwin lays the accompanying letter before the Trustees.

Mr. Gray would at once have granted Mr. Darwin's request, for there can be no doubt of his high scientific and general character; that great increase in the knowledge of these very anomalous animals will be derived from his examination, but that Mr. Darwin states that "it involves the absolute necessity of having the collection (not all at once but in groups)" at his own house: and the collection much increased in value and interest by the labour he will bestow upon it. For though it is in fact a preparation of the collection for exhibition, yet Mr. Gray did not feel that it exactly came under the permission of the Trustees by which he is allowed [to] send out the Mammals, Birds and fishes etc. to be preserved or Insects to be set.

Mr. Gray may state that the collection excites very little public attention for it has been kept in drawers under the cases for the last 5 or 6 years, and during that time has been consulted only by a single person, Mr. Stutchbury of Bristol for private examination until lately when Mr. Darwin has been often making use of it.

Should the Trustees consider it consistent with the rules of the Museum

to grant Mr. Darwin's request, Mr. Gray will take every precaution of marking the specimens and keeping a catalogue thereon so as to ensure their proper return of which Mr. Darwin's high character would of itself be a sufficient guarantee.

Attached: Darwin's letter to Gray of 28 December 1847, from Down.

At their meeting on 29 January 1848 (C. 7454) the Trustees gave Gray authority to allow the specimens of the Museum's collection of *Cirripedes* to go on loan to Down. On 17 February Darwin's letter of thanks for so exceptional a favour was placed before the Trustees at their next meeting (C. 7470). Darwin's letter read: (11)

In Gray's hand.

Down, Farnborough
5 February 1848.

My dear Gray,

I beg you will take the first opportunity to lay before the Trustees my most sincere acknowledgement of the great honour they have done me in entrusting to my care the collection of *Cirripedia* for the purpose of disarticulating and mounting the soft parts in spirits of one specimen of each species and of naming and describing the collection. I beg to assure them, as far as my utmost endeavours can assure the result, their confidence shall not be misplaced. I hope to add many other species to the collection from my own and several private collections already handed over to me.

Allow me to repeat to you my thanks for your kind and generous assistance.

Believe me
your very sincerely
Charles Darwin.

From June 1848 to November 1849 a large number of *Cirripedes*—upwards of some 500—dried and in spirits were sent to Down, each shipment carefully receipted with Darwin's initials (12).

Early in 1848 Gray's constitutional urge to publish whatever came into his mind got the better of him. He had one or two unpublished observations on *Cirripedes* made before Darwin had started work. These with the stimulus of their exchanges led Gray to prepare two papers (or rather notes) for the Zoological Society which he read at the meeting of 14 March 1848, and took the occasion of mentioning some of Darwin's conclusions (8). Darwin's reaction

to Gray's again taking up the *Cirripedes* was natural. He had, in the months previous, been approached by more than one of his friends who reminded him of Gray's reputation for irascibility. These warnings, coupled with Gray's papers to the Society and the mention of his own work, brought him to ask Gray to make his intentions clear. On his return to the Museum, Gray's first action was to allay Darwin's fears: (13)

B.M. Zoological Department, Reports, Minutes etc. 1847-1848 ff. 254, 255.

Draft

British Museum

26 August 1848

My dear Darwin,

I am very sorry that any person should have so misunderstood me as you inform me that they have done.

When I read the papers at the Zool. Society (8), I stated that you were engaged on the anatomy and study of these animals, and cited some of the important discoveries you had made and further stated that I brought forward the paper giving the Synonima of the genera and species and (my arrangement of the genera) as they existed in my MS Catalogue on which I had been working for some time as by so doing I should be able to facilitate your labours.

The paper was solely the MS of the Catalogue which you saw before you commenced the study of the subject.

You requested me to assist you in your work by giving you the Synonima etc. and I considered that it would be more satisfactory for you to quote them from a paper printed in the proceedings of the Zoological Society than from my MS. especially as you have the same specimens to examine as those on which I have worked and which are marked with the result of my labours.

But as you appear to think I may run counter to you I shall withdraw the papers as I find they have not yet been sent to the Press. I certainly did not believe that they would in any way interfere with you and that you had any desire that I should not publish an abstract of the observations which I had made on this group on which I have devoted many months or rather years, all of which I have more readily communicated to any who have enquired about them. I informed you at the time you first spoke to me on the subject what I had done [or] was doing with them, but since that period I have abstained from any further researches, and was merely about to record the

observations I had made and can only repeat [the] thought that by so doing I was helping you.

Believe me my dear Darwin,

Yours very truly

PS. I find I must let the reset papers read in March remain as they are in type as the specimens are figured on the plate of another animal.

What changes Gray made in the two papers is not known, since Darwin's work was not mentioned in the printed version. They fill page 44 of the *Journal*, have no illustration and are followed by a plate, on the next page, on another subject.

Darwin's reply follows (14):

In own hand.

Down, Farnborough, Kent
August 29th 1848

My dear Gray,

I thank you for your letter. Although I have nothing to add to what passed in our last interview, it may, perhaps, be satisfactory to you to have it in writing. To those who expressed their opinion to me, that you intended to anticipate my work, I stated that I had undertaken the task to your suggestion, as is most strictly true, for the idea had never before crossed my head;—that I had at first refused your most liberal offer of putting the entire subject into my hands;—and that when I changed my mind, I had met the most cordial assistance in my application to the Trustees. (This I have mentioned to several other people, and shall feel bound to state it publicly.) I also distinctly stated that you had communicated to me information of all kinds on the Cirripedia, and that, as you had been employed for many years on these animals, you had a *perfect* right to anticipate me, though I was unwilling to believe it, as I owed on this subject so much to you. I had resolved not to mention to you these communications (the first of which I received some months since) but now when coming to the determination of the species, I felt anxious to know what you intended doing, and I think you will admit that it was natural that I should wish that what little novelty there yet remained in the subject, should be the reward of my work, which I assure you has been to my utmost every day. I certainly should not have dreamed of undergoing the labour of making out all the close species, if I had supposed that the most striking, and therefore most interesting and easy forms, were to be described before me; and this, I hope, you will consider a

sufficient apology for my having spoken to you on the subject. You are perfectly correct in stating that I had urged you to give me the synonymy (which I hope now to be able to make out to the extent which appears to be necessary) and I remember, before I thought of undertaking all the species, that I asked you to name such species as I might dissect. I shall certainly communicate the substance of your very liberal letter and the assurances that you are far from wishing to anticipate me, to the four persons above alluded to. I sincerely hope that you will not connect with me any disagreeable impressions on this subject, and that it will be forgotten, for I assure you, that I shall not forget the tenour of your letter to the Trustees on my behalf.

Pray believe me
Yours very sincerely,
C. Darwin.

To J. E. Gray Esq.

Whether these letters were followed by further exchanges between Darwin and Gray is not on record, but at least the Museum's specimens continued to be sent to Down, and Darwin's *Monograph on the sub-class Cirripedia* . . . appeared in 1851 (15). In the Preface Darwin makes generous and unstinted acknowledgement both of Gray's original recommendations and of his contributions, whether personal or through the Trustees. Later generations which come to associate Darwin's name primarily with the concept of evolution, must see this monograph as an outstanding achievement of a kind that Gray, in all his thousand publications, was never able to equal. Yet it would leave any man proud to whom the opportunity had been given to make a contribution to so important a stage in Darwin's development.

The only other exchanges on record between Gray and Darwin are four letters, the first in the present writer's possession, and three others in the Darwin Archive in the Cambridge University Library. The first from Darwin to Gray is dated 1 July (no year) but was evidently written to collect information for his work on *The Variation of Animals and Plants under Domestication*, published by John Murray in January 1868. If Gray replied to this letter there is no evidence in Darwin's work that such information as he may have had to contribute was included.

The other three letters are from Gray to Darwin, being written in April 1871. What initiated them is not clear. Darwin's *Descent of Man* appeared in February 1871, and since Gray made no contribution to the subject of Sloths

and Lemurs in that work one may suppose that some question or other passed between the two after Gray had read Darwin's text.

These four letters are so fragmentary in relation to their subjects that they do not call for further comment, but advantage is being taken of this paper to place them on record.

Letter from Charles Darwin, to J. E. Gray

Down Bromley Kent
July 1st (undated)

Mr dear Gray

You once told me that you would help me in my Essay on variation. I want *much* some information on a point of Geographical Distribution, and to be allowed, to give information on your authority. It is, whether there are genera of Echinoderms, starfish etc. which have species (especially if closely related) in the northern and southern seas, but have not any one species in the Tropical seas? Or whether they are very closely related and representative genera in the north and south, without any closely related genus within the Tropics?

I am quite ignorant about the range of Echinoderms and perhaps all the genera have very confined ranges. Could I find information on this head in any publication?

Pray forgive, if you can, the trouble, and believe me.

Yours very sincerely

Ch. Darwin.

University Library, Cambridge: Darwin Archive, f. 95/96

Letter from J. E. Gray to Charles Darwin, 2 April 1871

British Museum.

My dear Darwin,

The two toed Sloth and the black crested three toed Sloth *Bradypus torquatus* are both sexes alike. But the females [of the] *ai* (*Arctopithecus* Gray) are covered with uniform fur varying in length and colour in the different species. The males have a patch of short shiney very soft hair between shoulders. In three out of the four species in the B.M. this patch is large and of a bright orange yellow color with a central black streak and black spots on the edge. In the other (*A. flaccidus*) the patch is small and pure white with a short black central streak. In the female of *A. griseus* has a small pure white tuft of very soft hair like a powder puff on each side of the back over the loins almost hid from view by the longer hairs of the fur. I have not

found these puffs [tufts] in the females of the other species. Buffon who knew the male of one species, called the *ai a dos brulé* which Lesson has named *Acheus ustus*. Temminck thought the spot was produced by the long hair being worn off. Wagler in 1813 [Isis 1831] suggested they were the males and lately I have been able to verify the fact.

With kindest regards,
Ever yours sincerely
J. E. Gray

I did not learn to write with my left hand until I was more than 70, so excuse its badness.

Signed by Gray only.

British Museum
3rd April 1871.

My dear Darwin,

I think I recollect having made a mistake in my letter to you yesterday. I quoted "Isis" 1813, it ought to be 1831 and Wagler thought it was the female had the yellow dorsal spot.

Buffon called the male sloth *L'ai adult* [adulte], vol. xiii to vi and Daubenton too in his description calls it *ai de dos brulé* vol. xiii, p. 62. He says it has the appearance as if the hair of the back had been burnt.

I have sent you on an American pamphlet I have received today.

Yours sincerely,
J. E. Gray (left hand).

University Library, Cambridge: Darwin Archive, f. 98

Letter from J. E. Gray to Charles Darwin, 7 April 1871

British Museum

My dear Darwin,

I do not know if the following observations are new to you as an instance of great differences between ♂ and ♀ and the great variations in the ♂ and stability in the females. *Lemur macaco* Linn. female only lately brought to Europe as far as we know is always brown with white whiskers *L. leucomystax* Bartlett, P.Z.S. [1862, p. 347]. Males, except when pure white all over, always have the head, hands and feet, tail, underside of body and limbs black, nape and ring round the base of tail white. The [colour] of the fur is red chestnut, reddish grey; or pure white [and] all the intermediate shades. The white backed variety sometimes has a black patch on each shoulder and on the front of the thighs. The patches vary in size in different

specimens, sometimes very large, even so large as [to] make the entire animal black *L. niger*, Geof. [actually Schreber].

Ever yours sincerely,

J. E. Gray.

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NOTES

- (1) W. E. Leach, Cirripides. *Encyclopaedia Britannica, Supplement*, 3, pp. 168-171 (1819).
- (2) J. E. Gray, A synopsis of the genera of Cirripedes. Thomson, *Ann. Philos.*, 10, pp. 97-107 (1825) (*List of Works* No. 15).
- (3) J. E. Gray, List of the Books, Memoirs and Miscellaneous Papers (*List of Works* Nos. 19a, 23, 1825).
- (4) Gray's copy in B.M. Zoological Department, Tracts, Zoology, 5, No. 24.
- (5) Ferussac, de, Synopsis des Genres de Cirripedes 8 (2): 285-288 (1826).
- (6) L. Oken, *Oken's Isis*, 5, pp. 488-489 (1834).
- (7) J. E. Gray, On the reproduction of *Cirripeda*. *Proc. Zool. Soc. Lond.*, Oct. 22. Pt. 1, pp. 115-116 (1833). (*List of Works*, No. 138).
- (8) J. E. Gray, Description of a new species of *Anatifa*, and On *Thaliella* . . . *Proc. Zool. Soc. Lond.*, 16, p. 44 (1848); also *Ann. Mag. Nat. Hist.*, 2, p. 456 and 3, 236, 237 (1848). (*List of Works*, Nos. 345, 346).
- (9) British Museum, Zoological Department, Reports, Minutes etc. 1847-1848, ff. 84, 84v.
- (10) *Ibid.* ff. 82, 83.
- (11) *Ibid.* f. 87.
- (12) *Ibid.*, 1848, ff. 250-252; 1849, ff. 251-253.
- (13) *Ibid.*, ff. 254, 255.
- (14) *Ibid.*, ff. 256, 257.
- (15) C. Darwin, *A monograph on the sub-class Cirripedia*. Ray Society, London, (1851).