

Friday or Monday next. We shall not remain long at the pits, but could show them to you and possibly, if you could accompany us, might have to leave you there, as I fear there might not be room in the carriage of Mr. Meeson, who proposes to take us to some other pits in the neighbourhood. The Grays pits are however the great features, and these I shall be happy to show you and to join you again there.—Believe me to remain, very truly yours,

JN. PRESTWICH.

The following letter from Charles Darwin refers to the same discovery :

DOWN, 19th July 1855.

DEAR LUBBOCK—I had a note from Lyell this morning, in which he says you have found the first *Ovibos moschatus* ever discovered fossil in England !

I must congratulate you on such a capital discovery. Considering the habits of *Ovibos*, and the nature of the drift-beds, I declare I think it one of the most interesting discoveries in fossils made for some years. . . . I congratulate you, and may this be the first of many interesting geological observations.—Yours very truly,

CH. DARWIN.

I wish you could have come here on Tuesday. Adios !

With such encouragement as this from the great men of science, it is no wonder that his young enthusiasm was fostered and grew keener than ever.

In 1856 he wrote a paper in the *Transactions of the Entomological Society* on some Entomostraca, collected in the Atlantic Ocean by Dr. Sutherland, and in the following year on eight new species which he found in the English Channel during a holiday spent at Weymouth.

Here, for the first time, he met Mr. and Mrs. Busk, who soon became, and till their death remained, close and intimate friends.

somewhat too technical to be of interest to others than entomologists. They sufficiently show, however, the reputation that he was already acquiring among contemporary scientists not only for careful and industrious, but also for entirely original work. The following portion of a letter from Charles Darwin refers to the same paper :

DOWN, *Sunday Morning*, 1857.

DEAR LUBBOCK—At the Philosophical Club last Thursday, I overheard Dr. Sharpey speaking to Huxley in such high and warm praise of your paper, and Huxley answering in the same tone that it did me good to hear it. And I thought I would tell you, for if you still wish to join the Royal Society, I should think (Sharpey being influential in Council and Secretary) there would be no doubt of your admission. Even if you were not admitted the first year it cannot be thought the least disgraceful. I am not aware, but perhaps you have been already proposed.—Believe me, dear Lubbock, yours sincerely,
C. DARWIN.

Bankers. It was adopted by them, and Lubbock drew up the rules, which have been in operation ever since. They were sent down by each Bank to their Country Correspondents, and met with general approval.

The following letter from Charles Darwin is worthy of quotation, as showing both how highly the famous evolutionist already appreciated the works and gifts of Lubbock, who was still, it is to be remembered, only in his twenty-sixth year, and also as indicating the astonishment, which we must all share, that with such various calls on his time he was able to accomplish so much.

DOWN, Feb. 15, 1860.

MY DEAR LUBBOCK—Many thanks for *Anthropological Review* returned. Thanks also about buds and ovary. I wish I had remembered your discussion. I have now alluded to it in 2nd Edition. Taking the whole sense of Müller's pages, especially one passage further on, I still think he meant to say that buds and germs were essentially the same, but it is far more doubtful than I supposed. I have been reading your address to Ent. Soc. ; and the number of first rate papers to which you refer is quite appalling. How do you find time to search up so much matter ? I have nothing else to do, and do not hear of half so many papers. It is very unfair of you ! Do you take in the *Zeitschrift für Wissen. Zoolog.* ; if so, can you lend me vol. xvii. p. 1, with Landois' "On Noises of Insects" ?

Also can you lend me Desmarest on "Crustacea,"—a thick pinkish volume, if you have it. I want to look at sexual differences. I have been looking at your papers and figures in March and May, and have been fairly astonished (for I had nearly forgotten) at the wonderful structure of the geniculated antenna of male ; but I wish you had figured both antennae, *i.e.* the pair, in their proper position : I should have liked to have given a copy in a wood cut.

If you ever arrive at any definite conclusion, either wholly or partially for or *against* Pangenesis, I should very much like to hear; for I settled some time ago, that I should think more of Huxley's and your opinion, from the course of your studies and clearness of mind, than of that of any other man in England. H. Spencer's views, I hear from him, are quite different from mine: he says he shall think over the subject, but apparently he does not yet quite understand what I mean.

There is a rather nice Review of you in last *Athenaeum* and a very unnice one of my book; I suspect, from two or three little points, by Owen.—Ever yours very truly,
C. DARWIN.

This year (1860) was remarkable in the annals of science for the publication of Darwin's great work on the *Origin of Species*. Writing to Dr. (Sir J.) Hooker on March 3, 1860, he gives the following table of those who went with him in his conclusions: ¹

Geologists.	Zoologists and Palaeontologists.	Physiologists.	Botanists.
Lyell Ramsay	Huxley J. Lubbock	Carpenter Sir H. Holland (to large extent)	Hooker R. C. Watson
Jukes H. D. Rogers	L. Jenyns (to large extent) Searles Wood		Asa Gray (to some extent) Dr. Boott (to large extent) Thwaites

The *Origin of Species* raised a storm of controversy, and even of obloquy, on the head of an author so greatly daring as to disturb the ideas of the creation story in which mankind had hitherto—or at all events until the slightly earlier publication of Lyell's *Geology*—been brought up. (Lubbock was, of course, on the

¹ *Life and Letters of Charles Darwin*, vol. ii. p. 293.

the satisfaction of knowing how ably his son acquitted himself in the new rôle of parliamentary candidate. Every reference to his speeches is couched in the terms of congratulation. It is evident that the line which he struck out for himself in his first venture in public speaking, as distinguished from lecturing, was that which he adopted throughout his career. He spoke simply, lucidly, with a conviction that carried his audience with him, and without any rhetorical arts. His father wrote to him, in regard to his first speech at Maidstone, the following brief note, which has a sad interest in that it is the last he ever penned :

HIGH ELMS, FARNBOROUGH, KENT,
Wednesday 26th, 1865.

DEAR JOHN—I wish to add my tribute of applause. Your excellent speech at Maidstone appears to me to be in the best possible taste and admirably suited to the occasion. Go on and prosper.—I am, dear John, yours most affectionately,
J. W. LUBBOCK.

His ever kind friend, Charles Darwin, had written on the previous day a letter of highest encomium on his address :

DOWN, BECKENHAM, KENT,
February 25, 1865.

MY DEAR LUBBOCK—Although you will be overwhelmed with congratulations, I must write to say how heartily I rejoice over your success. Your speech at Maidstone struck me as quite excellent, and I fully expect to see you a great man in Parliament, as you are in Science. But even in the moment of triumph, I must let one little groan escape me for poor deserted Science. Anyhow, I know that you will always love your first-born child, and not despise her for the sake of gaudy politicks.

I wrote to ask you a question about savages and suicide before I had heard of Maidstone ; otherwise,

of course, I would not have troubled you. If, in the course of a few weeks, you can inform me, I should be glad, but the point is not very important for me.

Once again, I do most sincerely congratulate you.—
Ever most truly yours, CH. DARWIN.

From his political opponent, Sir William Hart Dyke, he received a very kindly humorous letter saying that he had been a little surprised at first to hear that there was a chance “of the ‘Sensation Drama’ of Lubbock *v.* Dyke being enacted,” but assuring him that neither “during the Performance of the Piece nor after the Curtain dropped” should any ill feeling on Sir William’s part enter into the contest.

It was an assurance that was perfectly fulfilled.

John Stuart Mill’s is one of the distinguished names that appears on Lubbock’s committee in his electoral campaign.

In spite, however, of the fervour with which his speeches were received, and the congratulations accorded them, the “forlorn hope” failed, and he was handsomely defeated. The majority against him, odd as it may now seem, was larger than it would otherwise have been in consequence of the opinions on the Antiquity of Man expressed in his *Prehistoric Times*, which had appeared during the contest. He was urged to keep the book back till after the election, but thought that such a course would be scarcely honourable.

As a curious illustration of the state of popular opinion at the time, it may be mentioned that at Tonbridge, even as late as 1871, a meeting was held to reply to a lecture he had recently

The following letter contains Darwin's comments after the perusal of the proof sheets :

HAREDENE ALBURY, GUILDFORD,
August 12, 1871.

MY DEAR LUBBOCK—You will see where we are, and where we remain for 3 weeks more.

I hope the proof sheets having been sent here will not inconvenience you. I have read them with infinite satisfaction, and the whole discussion strikes me as admirable. I have no books here, and wish much I could see a plate of *Campodea*. I never reflected much on the difficulty which you indicate, and on which you throw so much light. I have only a few trifling remarks to make. At p. 44 I wish you had enlarged a little on what you have said of the distinction between developmental and adaptive changes ; for I cannot quite remember the point, and others will perhaps be in the same predicament. I think I always saw that the larva and the adult might be separately modified to any extent. Bearing in mind what strange changes of function parts undergo with the intermediate states of use, it seems to me that you speak rather too boldly on the impossibility of a mandibulate insect being converted into a sucking insect ; not that I in the least doubt the value of your explanation. Cirripedes, passing through what I have called a pupal state, *as far as their mouths are concerned*, rather supports what you say at p. 52.

At p. 40 your remarks on the Argus pheasant (tho' I have not the least objection to them) do not seem to me very appropriate as being related to the mental faculties.

If you can spare me these proof sheets when done with, I should be obliged, as I am correcting a new Edition of the Origin when I return home, tho' this subject is too large for me to enter into.

I thank you sincerely for the great interest which your discussion has given me, and with thanks for your congratulations on an event that gives us great satisfaction.—Believe me, yours very sincerely,

CH. DARWIN.

I return by this post the sheets.

He found time, moreover, to look abroad,

phrase goes, by the atmosphere of adulation with which he was surrounded. It would have been little wonder, indeed, had his valuation of his own talents been a little distorted. We have seen the estimate in which his contributions to science were already held, at a comparatively early period of his life, by the greatest scientists of the day, and it is a judgment that must forever silence the cheap cynicism of that criticism which proclaimed him "a great scientist among bankers, a great banker among the scientists." Darwin, Lyell, Tyndall, Huxley, to name only a few, have given their liberal witness to the worth of his original contributions to the natural science of which they were common devotees. The comment of the first-named alone on the *Prehistoric Times* were enough to establish the fame of any man of science.

I cannot resist telling you how excellently well, in my opinion, you have done the very interesting chapter on savage life. Though you have necessarily only compiled the materials, the general result is most original. But I ought to keep the term original for your last chapter, which has struck me as an admirable and profound discussion. It has quite delighted me, for now the public will see what kind of man you are, which I am proud to think I discovered a dozen years ago. I do sincerely wish you all success in your election [to Parliament] and in politics; but after reading this last chapter you must let me say: oh dear! oh dear! oh dear!—Yours affectionately,

CH. DARWIN.

The above was written in June 1865. There is no doubt that *Prehistoric Times* did open the public eye to an appreciation of Sir John, as Darwin predicted, but in the wonderful mass of

to the necessity of its introduction were not a little curious. A clerk of Stuckey's Bank had overpaid his account, and had deceived the auditors by falsifying the books, so that his frauds remained for a considerable time undetected. To every one's surprise it appeared that this was no offence in the eye of the law. This Bill for the first time made it a legal offence, and it has proved very useful, as many clerks who robbed their employers by means of false entries had previously escaped scot-free.

He also contributed two articles to *Nature*, on the relation between insects and flowers, of which Mr. Darwin writes as follows :

DOWN, BECKENHAM, KENT,
September 26th, 1874.

MY DEAR SIR JOHN—I have read your two articles in *Nature* and they seem excellently done; but my object in writing is to caution you, unless you have good evidence, about C. K. Sprengel's notion of Bees being deceived by a nectar-lip nectary. As far as my memory goes, Orchids are his best case, and I think I have shown that he is here mistaken, and my conclusion has been supported by subsequent observations.

I suppose you do not want more cases of coloured calyx, but our common *Polygala* is a remarkable case, as the calyx during flowering season is bright-coloured, and *then turns green* whilst it protects the seed-vessel after the flowering season is over.—Yours very sincerely,
CH. DARWIN.

In the autumn he attended the meeting at Belfast of the British Association, and gave one of the lectures. The subject was related to that of the articles in *Nature*. Professor Tyndall was President for the year, and the following extract from a letter of his indicates his estimate of the lecture :

ment which he chiefly favoured, and on Friday he gave in it a most interesting account on the manner in which the seed of a certain grass, the *Stipa*, buries itself in the ground."

Probably, however, the encomium which gave Sir John most satisfaction of all was the following from him, whom he ever piously regarded as his father in science, Charles Darwin. We may all echo one sentence of that great man's letter, "How on earth you find time is a mystery."

DOWN, BECKENHAM, KENT,
August 2nd, 1881.

MY DEAR LUBBOCK—I have read with pleasure your Address. You have piled honours high on my head.

I have scribbled such thoughts and remarks, as would have occurred to me if I had read your Address when published. I fear that this will be of little or no use to you, except perhaps, in one or two cases, by leading you to make further enquiry.

I had put a pamphlet on one side for you, as I think that you would like some time to read it, and it has occurred to me that from this excellent *résumé* of Dr. Adler's work (which, no doubt, you have read) you might easily make a short abstract for your Address, for I think that parthenogenesis deserves special notice in recent scientific work.

I have torn out a page for you to illustrate and strengthen what you say about inoculation.

My suggestions and criticisms are poor affairs, but they are the best which I could send.

This Address must have cost you much labour, and I congratulate you on its virtual completion. How on earth you find time is a mystery to me.—Yours very sincerely,
CH. DARWIN.

In the afternoon of the first day of the Association's meeting he had walked to the Cathedral with Huxley. At the entrance they met Professor Henry J. Smith, who put up his hands with a look of mock surprise. "Ah," said Huxley, "you did