

May, 1868 (No. 28); "On Geological Time," Part II.; "Tables of Eccentricity of the Earth's Orbit," which appeared in the *Philosophical Magazine* of August 1868 (No. 29); "On Geological Time," Part III.; "Inquiry into the Effects of Icebergs, Interglacial Periods, etc., with the Suggestion that the Warm and Cold Periods of the Glacial Epoch explain the commingling of Mammalia of Sub-tropical and Arctic Types in the Cave and River Deposits" (No. 30).

These papers Croll seems to have communicated to Mr. Charles Darwin, F.R.S., in a letter which, unfortunately, has not been preserved. Mr. Darwin's reply, however, was carefully preserved by Croll, and the following interesting and instructive correspondence thereupon ensued:—

DOWN, BROMLEY, KENT,
19th September 1868.

DEAR SIR,—I hope that you will allow me to thank you for sending me your papers in the *Philosophical Magazine*. I have never, I think, in my life, been so deeply interested by any geological discussion. I now first begin to see what a million means, and I feel quite ashamed of myself at the silly way in which I have spoken of millions of years. I was formerly a great believer in the power of the sea in denudation, and this was perhaps natural, as most of my geological work was done near sea coasts and on islands. But it is a consolation to me to reflect that as soon as I read Mr. Whittaker's paper on the escarpments of England, and Ramsay and Juke's papers, I gave up in my own mind the case; but I never fully realised the truth until reading your paper just received. How often I have speculated in vain on the origin of the valleys in the chalk platform round this place, but now all is clear. I thank you cordially for having cleared so much mist from before my eyes. With sincere respect, I remain, dear sir, yours very faithfully,

CHARLES DARWIN.

EDINBURGH, 23rd September 1868.

Charles Darwin, Esq., M.A., F.R.S.

DEAR SIR,—I am delighted to find that you are so well pleased with the two papers which I sent.

I have taken the liberty of forwarding to you by book post two other papers which may interest you, which please to accept.

I am sorry that it is not within my power to send you a copy of a paper "On the Eccentricity of the Earth's Orbit and its Relation to the Glacial Epoch," which appeared in the *Philosophical Magazine* for February 1867; and "On the Physical Cause of the Submergence of the Land during the Glacial Epoch," *Philosophical Magazine* for April 1866.—I am, yours very truly,

JAMES CROLL.

DOWN, BROMLEY, KENT,

24th November 1868.

DEAR SIR,—I have read with the greatest interest the last paper which you have kindly sent me. If we are to admit that all the scored rocks throughout the more level parts of the United States result from true glacier action, it is a most wonderful conclusion, and you certainly make out a very strong case; so I suppose I must give up one more cherished belief. But my object in writing is to trespass on your kindness and ask a question, which I daresay I could answer for myself by reading more carefully, as I hope hereafter to do, all your papers, but I shall feel much more confidence in a brief reply from you. Am I right in supposing that you believe that the Glacial periods have always occurred alternately in the northern and southern hemispheres, so that the erratic deposits which I have described in the south parts of America and the glacial work in New Zealand could not have been simultaneous with our Glacial period? From the glacial deposits occurring all round the northern hemisphere, and from such deposits *appearing* in South America to be as recent as in the

North, and lastly, from there being some evidence of the former lower descent of glaciers all along the Cordilleras, I inferred that the whole world was at this period cooler. It did not appear to me justifiable without distinct evidence to suppose that the north and south glacial deposits belonged to distinct epochs, though it would have been an immense relief to my mind if I could have assumed that this had been the case. Secondly, do you believe that during the Glacial period in one hemisphere, the opposite hemisphere actually becomes warmer, or does it merely retain the same temperature as before? I do not ask these questions out of mere curiosity, but I have to prepare a new edition of my *Origin of Species*, and am anxious to say a few words on this subject on your authority. I hope that you will excuse my troubling you.—Pray believe me, very faithfully yours,

CHARLES DARWIN.

EDINBURGH, 2nd December 1868.

Charles Darwin, Esq., M.A.

DEAR SIR,—Under another cover I send you a rough abstract of my views on change of climate. Along with that I enclose a copy of my papers on the subject, so that you can refer to some points that I could not well explain in the MS. without extending it to an unsuitable length.

I am sorry I cannot make you a present of the small volume. You may, however, keep it beside you as long as you wish, for I have another copy to which I can refer.

Should you find any points not clearly stated, I shall be delighted to afford you further explanations. And if you find, as no doubt you will, some points where you have reason to believe that I am in error, I shall take it kindly indeed if at your leisure you will drop me a note on the subject, expressing your opinion freely. It is in subjects like this, so new and so complicated, one always feels anxious lest he may go off the path,—I am, yours very truly,

JAMES CROLL.

DOWN, BROMLEY, KENT,

4th December 1868.

MY DEAR SIR,—As you may be anxious about the book, I write to say that I have received it, the MS. and your note. I will soon read the MS., and as you do not object, will perhaps keep the book till Christmas, as my second son, who is a mathematician, and who was extremely interested by your last papers, and who wished to read the others, will then be at home.—Pray believe me, yours truly obliged,

CHARLES DARWIN.

Croll had written to Professor Tyndall for a copy of one of his articles, and received the following characteristically kind reply:—

9th November 1868.

MY DEAR SIR,—If I had a separate copy of that article, I would gladly send it to you. I will write a note to the sub-editor and ask him to send you the date.

It gave me pleasure to hear Mr. Darwin express the delight he experienced in reading one of your recent papers. I had been staying with him for a day or two. He is, for him, exceedingly well.—Yours very truly,

JOHN TYNDALL.

I am glad the notice in the *Proceedings* pleased you. Are you a Fellow of the Royal Society?

31); on "The Influence of the Gulf Stream," which appeared in the *Geological Magazine* of April 1869, and in *Scientific Opinion* on April 21 and 28, 1869 (No. 32); on "Mr. Murphy's Theory of the Cause of the Glacial Climate," which appeared in the *Geological Magazine* of August 1869, and in *Scientific Opinion* on September 1, 1869 (No. 33); and "On the Opinion that the Southern Hemisphere loses by Radiation more Heat than the Northern, and the Supposed Influence which this has on Climate," which appeared in the *Philosophical Magazine*, September 1869, and in *Scientific Opinion* on September 29 and October 6, 1869 (No. 34).

It is truly wonderful how, after a day's work at his office, Mr. Croll was able to carry on any independent investigation at all. But it is still more wonderful, considering the disconnected and spasmodic manner in which he was compelled to do this, that he was able to embody his results in the series of clear, logical, and precise papers which he published during this year. These papers bear no trace of any incoherence or want of connection; on the contrary, they exhibit an amount of sustained mental vigour and consecutive, clear, logical thought, which, to the reader, would rather indicate a vigorous bodily and mental state, capable and indicative of a continuous and connected mental effort.

The following correspondence with Mr. Darwin is highly interesting:—

DOWN, BROMLEY, KENT, S.C.

10th January 1869.

MY DEAR SIR,—I write one line to say that I am ashamed of myself for having kept your book so long, partly for my son's sake, and partly for my own sake, as I have not yet come to the place where I want to quote it. If I hear from you, I will send it at once; if I do *not* hear, I will keep it for about ten days more, and will then send it registered, so you shall get it safe.—With sincere thanks, yours very faithfully, CHARLES DARWIN.

DOWN, BROMLEY, KENT, S.C.

31st January 1869.

MY DEAR SIR,—To-morrow I will return registered your book, which I have kept so long. I am most sincerely obliged for its loan, and especially for the MS., without which I should have been afraid of making mistakes. If you require it, the MS. shall be returned. Your results have been of more use to me than, I think, any other set of papers which I can remember. Sir C. Lyell, who is staying here, is very unwilling to admit the greater warmth of the southern hemisphere during the Glacial period in the north; but, as I have told him, this conclusion, which you have arrived at from physical considerations, explains so well whole classes of facts in distribution, that I must joyfully accept it. Indeed I go so far as to think that your conclusion is strengthened by the facts in distribution. Your discussion on the flowing of the great ice-cap southward is most interesting. I suppose that you have read Mr. Moseley's recent discussion on the force of gravity being quite insufficient to account for the downward movement of glaciers. If he is right, do you not think that the unknown force may make more intelligible the extension of the great northern ice-cap? Notwithstanding your excellent remarks on the work which can be effected within a million years, I am greatly troubled at the short duration of the world, according to Sir W. Thomson, for I require for my theoretical views a very long period *before* the Cambrian formation. If it would not trouble you, I should like to hear what you think of Lyell's remarks on the magnetic force which comes from the sun to the earth. Might not this penetrate the crust of the earth, and then be converted into heat? This would give a somewhat longer time during which the crust might have been solid, and this is the argument on which Sir W. Thomson seems chiefly to rest. You seem to argue chiefly on the expenditure of energy of all kinds by the sun, and in this respect Lyell's remark would have no bearing.

My new edition of the *Origin* will be published, I suppose, in about two months; and, for the chance of your liking to have a copy, I will send one.—With my very sincere thanks for all your kind assistance, I remain,
yours very faithfully, CHARLES DARWIN.

I wish that you would turn your astronomical attention to the consideration whether the form of the globe has not been periodically slightly changed, so as to account for the many repeated ups and downs of the surface in all parts of the world. I have always thought this cosmical cause would some day be discovered.

EDINBURGH, 4th February 1869.

Charles Darwin, Esq. F.R.S.

DEAR SIR,—Your favour, with book, came duly to hand; and I am glad to hear that some of the papers had been of a little use to you. I am very much pleased to hear that you consider the facts in distribution favourable to some of the views expressed in my paper on "Climate."

I have not as yet been able to overtake that part of the question relating to the condition of the hemisphere whose winter occurs in perihelion. I have no doubt that when this part of the subject has been fully discussed, Sir Charles Lyell will agree with me: the facts in favour of a warm climate are so numerous and strong.

It is a pity that Sir Charles should have made those remarks on the "secular loss of heat in the solar system," vol. ii. p. 213. He must have done it without due consideration of that point. If there is one thing more than another in physics regarding which we have absolute certainty, it is that the solar system is losing its store of energy. We not only know this fact, but we have a means of determining the actual rate at which it is losing its power. 3,869,000 foot-pounds of energy in the form of heat are radiated off every square foot of the

sun's surface per second. In other words, the quantity of energy thrown off into space by the sun is equal to a 7000 horse-power engine working on every square foot of its surface. And, when we reflect that all this prodigious expenditure has been going on during countless geological ages, we may well ask the question, what is the secret of the sun's great strength. Gravitation only affords up to the present time 20,000 years' heat. There must be some other source in addition to that of gravitation. It is strange that that other possible source did not suggest itself to Sir William Thomson and other physicists when working at this question. It is perfectly obvious that the sun, or rather the matter which composes the sun, might have been in possession of heat prior to condensation. In this case it is difficult to say how old the sun may be, for we do not know what this original store may have amounted to. In my paper I assumed a certain relation between the amount of original heat and that produced by gravitation, namely, 234 to 95 (*Phil. Mag.*, May 1868); but, as I stated, I may be wrong. It may be more than this, or it may be less. This proportion gives 70,000,000 years.

The introduction of this new element changes the entire conditions of the problem, and I have no doubt that the whole matter will have to be re-considered, and it is quite possible that we may yet be able to get considerably more than one hundred millions of years, although very much beyond this we are brought to a limit by other considerations.

As regards determining the age of the earth's crust from the surface cooling of the globe, I am not altogether satisfied with the plan. It would no doubt do if we had proper data to go by, but I don't think we have got these yet. I think that you may quite fairly assume a very long period before the Cambrian formation, even according to Sir William Thomson's theory; for, supposing the earth to have originally been in a molten condition, a solid crust would very rapidly form, and if this

crust would not break up and sink, the globe at the surface would be cool and suitable for life, although a short way down below the surface the heat was intense. This results from the slow rate at which the crust is able to conduct the heat from within.

It is some years since I read Sir William's paper on the secular cooling of the globe, but I think he states the above as his opinion; at all events, I heard him once say, in a lecture on the subject, that, supposing the earth to be in a molten state, in a few thousand years you could walk on its surface and hardly be sensible of the heat from within.

Electricity and magnetism used to be my favourite study, but for the past four years I have been paying little attention to what was going on in that department. A relation between the spots of the sun and the manifestation of electric phenomena on the earth does not *necessarily imply any transmission* of electric force from the one body to the other. One thing is certain, that it is but an infinitesimal quantity of the forces of nature that ever assumes the electric or magnetic form. Electrical phenomena are very imposing, and this is the reason why so much is attributed to them. A thunderstorm is something very striking; but Faraday has shown that more electricity is evolved in the silent decomposition of a few grains of water in the cell of a battery than would be required to produce the most violent flash of lightning.

It is owing to *high tension* that electricity makes such a display in passing from the statical to the dynamical state; but when you estimate the amount of energy thus displayed in foot-pounds it is often very little.

The quantity of energy in the form of electricity coming from the sun, if there be any at all, is certainly trifling compared with what comes in the form of heat. I believe that no physicist will call this in question.

Your suggestion as to the possibility of a cosmical cause for the ups and downs of the crust never occurred

to my mind. I can see no possible way at present how the thing can be, but I shall certainly ponder over it.

I have never heard of Mr. Moseley's papers. My curiosity is very much excited, and perhaps you will be so kind as to let me know when the paper appeared. Edinburgh, with all its books and learning, is miserably behind in scientific literature. Since I came here, I hardly know what is going on in the scientific world around. One can get plenty of good solid books on science, but the current news and literature of the subject are not to be found anywhere. Edinburgh, I fear, is falling behind.

I need hardly say that a present of a copy of the *Origin of Species* from its author will be esteemed worth a dozen copies out of a shop. I trust you will make out to read this rather long affair, written hurriedly to catch the post.—I am, yours very truly,

JAMES CROLL.

P.S.—Keep the MS. sent, it is of no use to me.

DOWN, BROMLEY, KENT, S.C.

6th February 1869.

MY DEAR SIR,—I am very much obliged for your long and to me extremely interesting letter. It is consolatory to me that you are inclined to give a little more age to the world. I read Mr. Moseley's article in *Scientific Opinion* about three or four weeks ago; I have had the house searched, but cannot find the copy. The article was given as extracted from the *Proceedings of the Royal Society*; but I have looked in the two last numbers which I have received, and it is not in them. Hence, I suppose, the author or secretary sent an abstract beforehand, and I suppose it will appear in the next number of the *Proceedings*. The article interested me, though I could not follow all the reasoning, as I hear he is a sound man.

I was reminded of my crude notion that the cause

of elevations, volcanic phenomena, etc., was cosmical, by my son telling me about Captain Clark's paper in *Philosophical Transactions*, which you probably know, on the globe being a little flattened at the equator, that this stands in relation to relative position of continents and oceans. It would be a great gain if some one could show a cause of the many changes of level in the crust of the earth.—With very sincere thanks, believe me, yours very faithfully,

CHARLES DARWIN.

EDINBURGH, 15th February 1869.

Charles Darwin, Esq., M.A., F.R.S.

DEAR SIR,—I am much obliged to you for the copy of *Scientific Opinion* you sent me, containing an abstract of Canon Moseley's admirable paper on the motion of glaciers.

I have a curious incident to tell you regarding the matter. The reading of the abstract thoroughly convinced me that the generally received theory of glacier motion, which I believe is that of Tyndall, must be given up, and that some other explanation must be sought. Strange to say, this conviction had hardly got time to settle down, if I may so express myself, before what I fully believe to be the true cause suggested itself. The cause is so simple, so beautiful, and obvious that I wonder that it should have escaped observation so long. I have drawn up a short account of the matter and have sent it to the *Philosophical Magazine*. I have requested Dr. Francis to send me two copies of proof, and when they come to hand I shall send you one.—I am, yours very truly,

JAMES CROLL.

DOWN, BROMLEY, KENT, S.C.

24th February 1869.

MY DEAR SIR,—I am very much obliged for the proofs, which have interested me greatly. I cannot pretend for a moment to form any judgment, but your view seems to me very ingenious. If accepted, it will be a most

satisfactory and great step in our knowledge of glacier movement.—In haste, yours very sincerely,

CHARLES DARWIN.

EDINBURGH, 23rd June 1869.

Charles Darwin, Esq., M.A., F.R.S.

DEAR SIR,—Please to accept of my warmest thanks for the copy of the new edition of your *Origin of Species*, which you were so kind as to present me with. I trust that you will not consider me vain for saying that I am very much gratified with the complimentary way in which you have referred to my papers. I am particularly interested in the section on alternate glacial periods in north and south. I had no idea of this application of the theory when engaged on the subject, or I might have brought out the point more clearly than I have done, that when the northern hemisphere, for example, is under a Glacial period, the line of highest temperature will not be at the equator, but will lie a very considerable distance to the south of the equator. And again, when the Glacial period is transferred over to the southern hemisphere, the line of greatest heat will move over to as great a distance to the north of the equator. Your idea that the temperate climate plants will move up the mountain side while this line of greatest heat is being transferred from the one hemisphere to the other, is most ingenious, as it is natural.—I am, yours very truly,

JAMES CROLL.

As the result of his outdoor geological investigations Croll was fortunate in discovering two remarkable buried river channels, belonging to a Preglacial and Interglacial age, and some other singular facts bearing on the history of the Glacial period. These researches were continued after he went to Edinburgh with equal success, the particulars of which were given in *Climate and Time*, and in one or two papers contributed at the time.

He embodied the results of these in a paper, written also during the year 1869, on two river channels, between Forth and Clyde, buried under drift belonging to a period when the land stood several hundred feet higher than at present (No. 35), which appeared in the *Transactions of the Geological Society of Edinburgh* during that year. His health engendered a dread of going out in doubtful weather, and prevented him conducting much outdoor investigation on a prolonged scale, but this discovery shows what he might have done had he been possessed of the physical strength necessary for field study.

Retardation," which appeared in *Nature*, August 24, 1871, and in the *English Mechanic* of September 1, 1871.

Dr. Carpenter had advanced a totally different theory of ocean currents from that held by Croll; and, accordingly, he returned to the subject of ocean currents, and wrote a paper on "The Physical Cause of Ocean Currents, containing an Examination of Dr. Carpenter's Theory," which appeared in the *Philosophical Magazine* of October 1871.

The reception which this last paper met with may be gathered from the following letter by Sir Charles Lyell:—

73 HARLEY STREET, LONDON, W.,
28th March 1871.

MY DEAR SIR,—At the end of your paper in the *Philosophical Magazine* for October 1870, you promise a continuation on the effect of Trade Winds on Currents. I ordered all your papers published in the *Philosophical Magazine* to be sent to me as they came out, and I have accordingly received them from the publisher, in addition to the author's copies which you have had the kindness to present to me; but I cannot find that I have received any Part IV. of the "Ocean Currents," and shall be obliged to you to let me know whether this part is in print. At present I continue to believe that the Gulf Stream has far more to do with the conveyance of great bodies of warm water to temperate latitudes of the Atlantic and to the North Polar Seas than Dr. Carpenter allows.—I am, yours very truly, C. LYELL.

The following letter from Mr. Charles Darwin shows in what esteem he held Croll, while Croll's reply is characteristically candid and modest.

DOWN, BECKENHAM, KENT,
19th July 1871.

MY DEAR SIR,—Mr. Youmans of the United States is very anxious to get a series of small monographs

written by the most competent English authors on various subjects, to be published in the United States and I suppose in England. Mr. Youmans is in some way connected with the great firm of Appletons in New York. He has asked me to name some of the most competent men, and I have thought that you would excuse my giving your name, and this note as a kind of introduction. I should add that I do not know on what subject he wishes you to write. I do, however, know that some very good judges think highly of his scheme. Pray excuse the liberty which I am taking, and believe me, yours very faithfully,

CHARLES DARWIN.

P.S.—Many thanks for some interesting papers which you kindly sent me some time ago.

EDINBURGH, 17th August 1871.

Charles Darwin, Esq. F.R.S.

DEAR SIR,—I am much obliged to you for your letter of 19th ult., which I received through Mr. Youmans.

This gentleman wished me to write a small treatise on Geological Time. But I explained to him that, in the present state of the question, nothing satisfactory could be written on it, which would be of any service to general readers. I believe he felt satisfied that the better plan was to let this subject lie over for some time to come.

I have been doing little for some time back, owing to pain in the head, but I expect to have a long paper in the October number of the *Philosophical Magazine* on Dr. Carpenter's theory of Ocean Currents.—I am, yours most sincerely,

JAMES CROLL.

Dr. Croll was always painfully anxious that his speculations should be based on the most accurate data. From the defective nature of his early education, he was not so familiar as he might otherwise have been with the details of mechanics and physics; and consequently, in verifying his data on these subjects, he was from time to

The following letter from Mr. Darwin shows how Croll's work was appreciated :—

DOWN, BECKENHAM, SURREY,
9th August 1877.

MY DEAR SIR,—I am much obliged for your essay, which I have read with the greatest interest. With respect to the geological part, I have long wished to see the evidence collected on the time required for denudation, and you have done it admirably. I wish some one would in a like spirit compare the thickness of sedimentary rocks with the quickest estimated rate of deposition by a large river, and other such evidence. Your main argument with respect to the sun seems to me very striking.

My son George desires me to thank you for his copy, and to say how much he has been interested by it.—I remain, my dear sir, yours very faithfully,

CHARLES DARWIN.

The following letters to his colleague on the Survey, Mr. Horne, lifts the veil as to his daily work and shows the interest he took in the work of his colleagues.

EDINBURGH, 24th April 1877.

MY DEAR HORNE,—I have sent by book-post Sheet 6, 7, 8 (Nairn). Hume has sent to the office a small box for you. Shall I forward it? Peach has gone back to Melrose some time ago. Major-General Cameron, C.B., F.R.S., Director - General, Ordnance Survey, is the man you should apply to for information. You will see by to-day's *Scotsman* what miserable weather we have had for some time past. In your paper on "Shetland" lay special emphasis on the distribution of the stones in the drift and their relation to the underlying rocks, which can only be satisfactorily accounted for by land ice. Nine geologists out of every ten will

forward to you after reading it. If it should not come to hand shortly, write me and I will remind him.—Yours truly,

JAMES CROLL.

Mr. Darwin having sent Dr. Croll a copy of his book, received the following acknowledgment:—

43 CLAREMONT ROAD, ALEXANDRA PARK,
MANCHESTER, 22nd October 1881.

Charles Darwin, Esq., LL.D., F.R.S.

MY DEAR SIR,—I have just received the copy of your most interesting volume which you have so kindly sent me. The subject is new and important.

Please to accept my best thanks for the gift. You will be glad to learn that I am much improved in health of late. Sleep has completely returned. I am at present on my way to winter in some warm and sheltered place in the south of England, where I can live cheaply.—I am, yours sincerely,

JAMES CROLL.

The following letter shows how Dr. Croll even in his illness yearned for scientific news:—

15 STRAND, DAWLISH, DEVON,
21st December 1881.

MY DEAR HORNE,—I want to ask a small favour of you. As I am cut off from all scientific journals and magazines at present, would you let me have a look at your *Athenæum* when you are done with it? I will return it to you next day. I don't read much, but like to look over that journal, more particularly the advertisements part, as it lets one know what is going on in the book world. I have been told that two weeks ago there appeared in *Nature* a lecture by Professor Ball on Mr. G. H. Darwin's remarkable work. I should like to see the copy containing the lecture in question,