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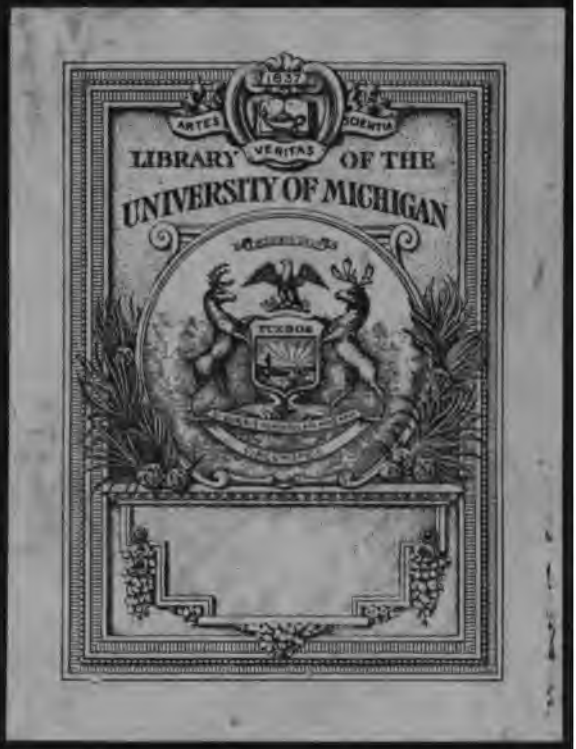
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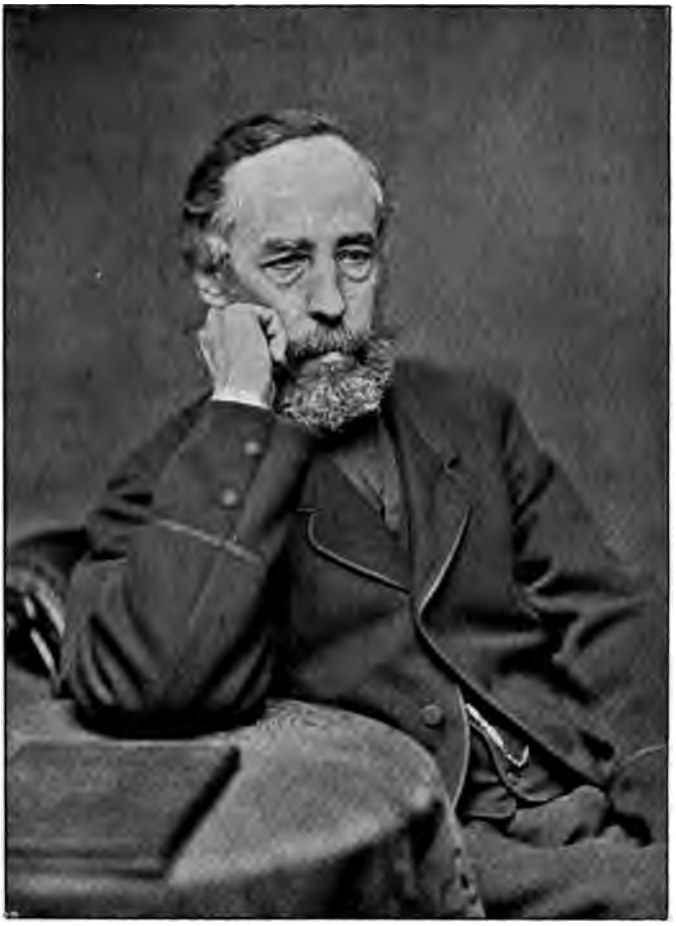

Yours ever truly
James Croll

AUTOBIOGRAPHICAL SKETCH

OF

JAMES CROLL

LL.D., F.R.S., ETC.

WITH

Memoir of his Life and Work

BY

JAMES CAMPBELL IRONS, M.A.

LONDON: EDWARD STANFORD
26 AND 27 COCKSPUR STREET, CHARING CROSS, S.W.
1896

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PREFACE

HAVING been one of the intimate friends who urged Dr. Croll to write an autobiographical account of his remarkable career, it fell to me to arrange the materials which he left for publication. Unfortunately, the autobiographical sketch was never completed, and together with the additional papers which came into my hands, it hardly furnished an adequate account of his life and scientific achievements. Accordingly, an effort was made to obtain further information, and various friends were asked to forward letters illustrating the development of his researches. The cordial response of several leading scientists made my task less difficult, but the time occupied in procuring and examining these materials partly accounts for the delay in the appearance of this volume.

While the work was in progress, kindly advice was given by Professor Masson regarding the Autobiography, which I cordially acknowledge. He wrote that "it is so characteristic, that it would be best to preserve it entire, as it would be a pity to lose anything of the simple and pleasant peculiarities of the autobiographical original." This course has been followed, and a more detailed account of his life and scientific work has been added. Such an arrangement involved a certain amount of overlapping and reduplication, which was hardly avoidable under the circumstances.

To many friends of Dr. Croll who forwarded letters, I beg to express my sincere thanks. In particular to Mr. Francis Darwin, who kindly gave permission to use the correspondence with his father, Mr. Charles Darwin, and

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to Professor G. H. Darwin, who extended a similar courtesy; to Mr. A. R. Wallace, Sir. J. D. Hooker, Professor Foster, Professor Tyndall, Rev. O. Fisher, Mrs. Romanes, Mrs. Tyndall, and many others. I have also to thank Mr. Horne and Mr. Bennie of the Geological Survey for letters, papers, and various suggestions. I beg also to acknowledge my great obligations to Mr. Herbertson, F.R.S.E., Lecturer at the Heriot-Watt College, Edinburgh, for his valuable assistance in making a digest of Dr. Croll's papers; to Mr. Fowler of the Kensington Museum for permission to use his review of the work on *Stellar Evolution*, and to Mr. Whittaker for the republication of his review of *The Philosophical Basis of Evolution*. My special thanks are likewise due to Lord Kelvin and Messrs. Macmillan & Co. for their permission to use the Obituary Notices at the end of the volume.

My brother, the Rev. David E. Irons, B.D., Glasgow, has written part of the biographical account, has aided me generally in the compilation of the work, and has revised the proof-sheets.

This volume has been written with the hope that the life of Dr. Croll, recording the triumph over his early struggles, his scientific researches, which secured him a world-wide reputation as an original thinker, and his earnest belief in the Christian faith, may prove interesting. It may only be added that the entire proceeds of its sale will be handed to his widow.

J. C. I.

10 ROYAL TERRACE, EDINBURGH,
November 1896.

PREFATORY NOTE

I HAVE been frequently urged during the past few years to draw up a statement of the principal incidents of my life. As this is a thing to which I have a strong aversion, I have hitherto declined. Induced mainly by the desire of my wife, I have at last agreed to comply with the wishes of my friends. Mrs. Croll will hurriedly jot down in pencil, to dictation, the facts as they occur to my mind. These jottings will probably never be revised or read over by me. Owing to the state of my head, and the pressure of work of a more important character, I am obliged to adopt this course. Besides, it is a sort of work to which I am naturally ill adapted, being almost devoid of the faculty for descriptive writing.

J. C.

PERTH, *December* 7, 1887.

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LIFE OF JAMES CROLL

AUTOBIOGRAPHICAL SKETCH

MY ancestors, who spelled their name Croil, and some of them, it would seem, Croyl, were inhabitants of the parish of Cargill for at least more than the last two centuries; for I have been able, from the parish baptismal register, to trace my direct parentage back till about the middle of the seventeenth century, that is, as far back as the register extends.

I was born at Little Whitefield, parish of Cargill, on Tuesday, January 2, 1821, at eleven o'clock P.M. It was a cold wintry night, with the snow lying thickly on the ground. I was the second of a family of four sons. My father David Croil, son of Alexander Croil, was born in March 1781, and was consequently forty years of age when I was born. He was a stonemason by trade. Little Whitefield, now a farmhouse, was then a small village of some eight or ten houses. Beside the village were a hundred or a hundred and fifty acres of land, divided principally amongst four of the villagers: namely, William Brewster, James Brown, William Marshall (I think), and my father. The village was demolished, and the land put into one farm, when I was about three years of age.

My forefathers had resided at that village for several generations past. My mother, Janet Ellis, youngest

daughter of James Ellis, was born at Elgin in 1781, and was consequently of the same age as my father. She left Morayshire for Perth during the earlier part of the present century, and was married to my father, some time, I think, about the latter end of 1818. Both my parents possessed a considerable amount of individuality of character. Mentally, however, they were considerably diverse. My mother was firm, shrewd, and observing, and gifted with a considerable amount of what is called in Scotland "common sense." My father was mild, thoughtful, and meditative, and possessed of strong religious and moral sentiments. This amiable disposition and high moral character made him greatly esteemed and respected. But he had the misfortune to possess a most anxious and sensitive mind. I was supposed to possess a considerable share of the peculiarities of both parents, with, no doubt, a predominance of those of my father. I have often thought that, had I possessed some more of my mother's qualities, and less of some of my father's, the battle of life would not have proved so painful to me. When a boy, I was always proud to tell, when asked, who my father was; for the mention of his name generally commanded respect, and procured for me a kindly word, with the remark, "I hope that when you grow up you will be as good a man as your father."

As already stated, I was the second of a family of four sons. Alexander, the eldest, was born on November 29, 1819, and died when about ten years of age. His death was a severe blow to my parents, especially to my father, who never afterwards regained his former vivacity of spirits. My youngest brother, William, born February 25, 1826, died in infancy. David, born April 23, 1822, to whom I shall subsequently have occasion to refer, died on February 28, 1876. He had the misfortune to be deformed. He was very much hunchbacked, which was supposed to have been the result of a fall from a servant's arms, when he was an infant.

When the proprietor of the parish, or, at least, of the greater part of it, the Right Honourable Lord Willoughby, decided to convert the small holdings into large farms, he allotted two pieces of ground for feuing purposes: one piece, about a mile to the south-west of Little Whitefield, now called Wolfhill; the other three miles to the north, now called Burreltown. My father took one or two feus in the former place, on which he erected a dwelling-house, and removed to it about the year 1824, when I was about three years of age.

There is very little in my infant years to which I need refer, except, it may be, to my somewhat early developed power of memory. I have a most distinct remembrance of witnessing the ceremony of my brother David's baptism. My father being a Congregationalist, and a member of the Rev. William Orme's church in Perth, that minister came out and preached an out-door sermon to the villagers on the occasion of the baptism; and I have a vivid remembrance of the scene, although I could not then have been over eighteen months old. I have also a remembrance of a number of little incidents which occurred before I was three years of age, among which was the flitting-day from Little Whitefield to Wolfhill. After this date there are very few long breaks in my remembrance. I suppose I must have inherited this faculty from my father, who told me that he had a distinct remembrance of things which occurred when he was only two years old.

For the first three or four years after removing to Wolfhill nothing of importance occurred worth recording. I was not early sent to school, my parents having been, as I think, judiciously opposed to too early mental work. Another cause was that, about this time, I became afflicted with a rather troublesome pain on the top or about the opening of the head, which prevented me being able, except in the heat of summer, to remain bareheaded; and, as I could not be persuaded to sit in

school with my cap on, my parents had for a considerable time to allow me to remain at home. My first lessons were consequently obtained from my parents, assisted by my eldest brother. The village schoolmaster also now and again gave me private lessons at home. In this way I learned to read and write. The village school to which I refer was a very poor affair indeed. An elderly man, who had seen better days, taught, for a living, a few boys in a private house. A year or two afterwards I went, for a short time, to the parish school, a mile and a half distant from Wolfhill; and subsequently I attended a Voluntary school at Guildtown two miles to the west of the village. The teacher there was rather harsh and tyrannical; and the consequence was that I abhorred school, and, as might be expected, made very little progress. Next year, however, 1834, he was succeeded by a Mr. Keiller, a man of more genial nature. I remained with him some eight or nine months, and then had to leave school finally. The cause of my having to leave so soon was this. My father, having one or two acres of ground, kept a cow, and as he was away from home during the greater part of the year, following his duties as a stonemason, I had to be taken from school to assist my mother at home.

I must say that I was rather a dull scholar, scarcely up to the average of boys of the same age, as far as regards getting up my lessons sharply and correctly. I never succeeded in acquiring an accurate style of reading, and by no amount of labour could I manage to become even a moderately good speller. I unfortunately left school just at the time I was beginning to have a longing desire for a much better education. The circumstances which led to this desire I shall now narrate.

Up to the years of eleven or so I had little or no love for reading; being much fonder of play than of books. This somewhat disappointed my father. One day during the summer of 1832, when at Perth, I

observed in a bookseller's window the first number of the *Penny Magazine*, which had just appeared. Attracted by the illustrations, I went in and purchased it. This incident led to a new epoch in my life. Having read the first number with interest, I then purchased the second, third, and succeeding numbers as they appeared, all of which I read with zest. Shortly after I had commenced to read the *Penny Magazine*, a book on natural philosophy, Dick's *Christian Philosopher*, I think, or some other of his books, came under my notice. Commencing to read it, I was at once struck with the novelty of the ideas. I shortly afterwards procured one or two other books on physical science, among which was Joyce's famous scientific dialogues. At first I became bewildered, but soon the beauty and simplicity of the conceptions filled me with delight and astonishment, and I began then in earnest to study the matter. I may here mention that, even at the very commencement of my studies, it was not the facts and details of the physical sciences which riveted my attention, but the laws or principles which they were intended to illustrate. This necessarily determined me to study the sciences in something like systematic form; for, in order to understand a given law, I was generally obliged to make myself acquainted with the preceding law or conditions on which it depended. I remember well that, before I could make headway in physical astronomy (and this was the only branch of astronomy which I have ever studied), I had to go back and study the laws of motion and the fundamental principles of mechanics. In like manner I studied pneumatics, hydrostatics, light, heat, electricity, and magnetism. I obtained assistance from no one. In fact, there were none of my acquaintances who knew anything whatever about these subjects. Notwithstanding all these disadvantages under which I laboured, I managed in the course of four years or so, or by the time I was between fifteen and sixteen years old, to obtain a pretty tolerable knowledge of all the general principles of those branches

of physical science. The reason of my being enabled at this early age to make so much progress in so short a time was, as I have already stated, that it was the principles or laws to which my attention was mainly, if not exclusively directed. In studying, for example, the electrical machine or the air-pump, I paid little or no attention to the details of their various parts, further than as they illustrated the electrical or the pneumatic laws according to which these machines operated. The consequence was that these multifarious details soon disappeared from the memory, and the laws or principles only remained.

There were two important and, to most people, interesting sciences for which I had no relish, namely, chemistry and geology, more particularly the latter. The reason was that to me they appeared so full of details and so deficient in rational principles, being so much sciences of observation and experiment. Had any one told me then that one day I should be a professional geologist, I would have regarded the statement as incredible. In truth, it was more by accident than by choice that I became a geologist. Geology is almost the only science on which (with the exception of one department of it, to which I shall afterwards have occasion to refer) I have never spent a single day's earnest study. However, the accident of becoming a member of the Geological Survey was of immense advantage to me when I afterwards became engaged in my climatological studies; for it enabled me to become acquainted with geological phenomena necessary for the subject, of which otherwise I would have remained ignorant, and without which my work would have been but very imperfectly accomplished.

No grand physical conceptions acquired in after years ever made such an impression on my mind as those of this early date, except it be those relating to the modern science of energy, which was not then in existence; namely, the transformation and conservation of energy,

and the dynamical theory and mechanical equivalent of heat. My early acquaintance with the general principles of physical science has been of great service to me in after years.

In the summer of 1837, when I was between sixteen and seventeen years of age, the question arose—what occupation or handicraft was I to follow? This was a point somewhat difficult to determine; for I had no liking for any particular occupation, nor was it supposed that I possessed any special fitness for one occupation more than another. The bent of my mind at the time was to obtain a university education, which might enable me to follow out physical science. This, however, was a wish that could not be realised, as my father was by far too poor. After several days' consideration I thought I might try the occupation of a millwright. As I was fond of theoretical mechanics, it occurred to me that this occupation might be the most congenial and the one for which I was best adapted. But this I afterwards found to be a mistake; for, although I was familiar with theoretical mechanics, yet, as a working mechanic, I was scarcely up to the average. The strong natural tendency of my mind toward abstract thinking somehow unsuited me for the practical details of daily work.

After it was decided that I was to be a millwright, I was engaged as an apprentice to a millwright at Collace, a village at the foot of Dunsinane Hill, three miles distant from home. In 1841, at the termination of my engagement, I left Collace, and worked for two years as a journeyman with a firm of the name of Martin & Robertson, Banchory, near Coupar-Angus. The wages received were small, being only eight shillings a week, with food, which was of the poorest description possible. As my employers were in difficulties, small as my wages were, I lost money by them. It was on the whole rather a rough life. A very considerable part of our work consisted in the repairing of corn- saw- and thresh-ing-mills. The consequence was that we were very

frequently away from home, and seldom more than a day or two at one place. During the last two years I occupied, on an average, three different beds in a week; and these were not of the most comfortable character. We millwrights had generally to go to the ploughman's bothy; and when there was no room there, we had to go to the barn or the stable loft above the horses. Frequently we had to bury ourselves under the clothes to secure protection from the rats. Having spent some five or six years as a millwright, I got tired of the trade, and resolved to try something else. I came home to Wolfhill,—being at the time about twenty-two years of age,—and went to St. Martin's parish school for a winter to study algebra. Then, being pretty well up to woodwork, I got employment as a house joiner in the beginning of summer. After a little practice, I soon found this a far more congenial occupation. The first job, in the joiner way, in which I was engaged, was at the Free Church which was being erected at Kinrossie, parish of Collace, for the Rev., now Dr., Andrew A. Bonar. After the church was finished, work became scarce in the neighbourhood, and, in the summer of 1844, I went to Glasgow in search of employment. There I found work at once, and remained for a few months, afterwards removing to Paisley, where I remained for upwards of a year. I liked both the place and my employment. But my stay here was brought to a sudden termination in the spring of 1846, in the twenty-fifth year of my age, by an event which had the effect of changing the whole course of my future life. When a boy of about ten or eleven years of age, I had what is called a boil on my left elbow joint. One day it was accidentally knocked against the corner of a door, and this had the effect of preventing it healing up. It, however, proved of very little inconvenience to me, except that now and again, especially during the spring months, the elbow became somewhat inflamed. But in Paisley, at the time to which I refer, the state of the elbow began to assume a somewhat serious character, and

my medical advisers informed me that it would be absolutely necessary for me to abandon my occupation as a joiner and try some easier pursuit. I accordingly took their advice, left the place, and returned home to think over what should be done.

This I felt to be a great blow to my future prospects. But although, as we shall see, it led to years of poverty and hardship, nevertheless, being freed from the long hours of manual labour, I enjoyed a great deal of leisure for reading and study. Strange are the ways of Providence. Had it not been for a mere accident in early life, I should in all likelihood have remained a working joiner till the end of my days. Here, however, before proceeding further with my narrative, I must turn back a little, in order to refer to another circumstance which exercised a very considerable influence on my future life.

Although brought up under religious influences (my parents being Independents of the good old Congregational type, having in the early part of the century joined that body), I was nevertheless, till over the age of seventeen, indifferent to divine things. During the autumn of 1839 I became deeply impressed, mainly by the reading of Boston's *Fourfold State*; and in this condition I remained for some time, till I was led to see that salvation was entirely of free grace. Simple trust in Christ's vicarious death gave me complete peace of mind and true happiness, a peace which the world can neither give nor take away. The agnostic will smile at my experience. How different would he feel if he experienced this blessed peace himself!

> "The love of Jesus, what it is
> None but His loved ones know."

Shortly afterwards a remarkable religious movement took place in the parish of Collace, where I was then residing, under the ministry of the Rev. Andrew Bonar. I was soon then amongst kindred spirits; and the year that followed was probably the happiest in my life.

Living in a retired country place, after the toils of the day, when the shadows of evening were falling, I generally took a stroll in the fields, or along a quiet road for an hour or two, to meditate and ponder over spiritual things. These hours I enjoyed exceedingly, and I continued the practice for years afterwards, until I went to reside in the city. Nothing in city life did I miss so much as these quiet walks.

> "The calm retreat, the silent shade,
> With prayer and praise agree,
> And seem by Thy sweet bounty made
> For those who follow Thee."

Almost from boyhood I had a love for retired, solitary walks in the country. On these occasions I can enjoy a congenial companion, but I would rather be alone to meditate. It is more sweet, more pleasant. The stillness of nature adds to the charm. I well remember one of the solitary musings of my early days, when I could have been only about twelve or thirteen years of age. It was this. I asked myself, What would there be, if there were no world? The ready answer, of course, was that there would be the sun, moon, and stars. What would there be, if there were no sun, moon, and stars? There would be God. But what would there be if there were no God? There would be nothing but empty space. But if there were no space, what would remain? This question staggered me. I could not in thought get quit of space. Why could not I in thought annihilate space? This was the puzzle; and it remained a puzzle to me till, in after years, I began to study Kant. Kant gave an explanation; but the explanation he gave commits us to such sweeping idealistic consequences that, even to this day, I cannot accept it.

After leaving the spiritual atmosphere of Collace, and going to reside at Banchory, where I had to associate constantly with people in a great measure indifferent to divine things, my spirituality ere long began gradually to decline. Along with this, as a natural consequence,

my former peace and happiness declined, and I often exclaimed, in the language of the poet Cowper—

> "What peaceful hours I once enjoyed!
> How sweet their memory still;
> But they have left an aching void
> The world can never fill."

On leaving Banchory, and returning home, I was again in a more congenial atmosphere.

At this time the controversy between Arminianism and Calvinism was beginning to agitate the country. I took a very keen interest in the controversy, adopting the Arminian side of the question. When I went to the west of Scotland, I naturally associated myself with the Arminians, then called Morisonians, for at this time the Evangelical Union had not been formed.

When I went to Paisley, they were endeavouring to establish a congregation in the town. I took a lively interest in the work. Mr. Landels, now the well-known Rev. Dr. Landels of Edinburgh, then a student of Mr. Morison, Kilmarnock, preached frequently and with much success; he was a most excellent young man, but he soon adopted Baptist principles. After him Mr. A. M. Wilson, now the esteemed pastor of the E.U. Church, Bathgate, came and preached for some time with success; and when the church was formed, he was chosen as the pastor. From a manuscript list of the members, I find that I had been elected as one of the deacons; but I never acted in this capacity, as I left the place just after the church had been organised.

I may here mention, that on coming to Paisley I felt somewhat disappointed at a very marked difference between the Arminians of the West and the Calvinists of Collace, with whom I had formerly associated. The former were more argumentative; but they were not so spiritual, so fond of the social prayer-meeting, or so much inclined to speak to one another of their own personal experiences. This may be accounted for by the fact that at Collace there had been a genuine revival

of religion, while in the west there had been simply a revival of Arminian principles. This difference did not, however, lessen my conviction of the truth of the Arminian principles.

To resume my narrative: After coming home from Paisley, I had to consider what was to be done to enable me to earn a livelihood for the future. I had not received the proper training, or the sort of education which would fit me for becoming a clerk; and, even supposing I had, it was a kind of occupation for which I was naturally unsuited. After some consideration, it was thought that some sort of occupation in the tea trade might suit me. On duly thinking over the matter, I went to Perth to see what could be done. I had come to no conclusion as to whom I should consult, or as to what shop I should enter. Musing over the matter, as I approached the city by the bridge, I observed a man distributing small handbills to the passers-by. All in a moment it struck me that if these bills should relate to the tea trade, I would be guided by this, and would go to the shop to which they referred; at least, before trying any other. What could induce me to come to a conclusion so apparently absurd and incautious, I cannot tell. Strange are the ways of Providence! for had it not been for that decision, in all probability, my future course in life would have been very different from what it actually turned out to be. On coming up to the man, I found the bills related to a tea and coffee warehouse which had recently been opened in the High Street of Perth. Guided by the bill, I went direct to the shop, and found the proprietor to be an agreeable and intelligent person. After talking over various matters, I then told him what I had been thinking about. He agreed with me that I might manage to make a comfortable livelihood by selling tea; and that I might push the sale by going into the country. I accordingly got a small stock and commenced operations. I soon found, however, that the attempt to push a sale in the country

was a rather disagreeable job for me, and I resolved to give it up. The merchant whom I had visited—Mr. David Irons, who afterwards proved to be one of the kindest friends I have ever met with in life—now proposed to me that I might try and open a shop for myself in some suitable town, where I might be likely to succeed. Unfortunately, I had not the means for any such undertaking; but he, in the most kindly manner, offered to assist me. He agreed to give me a stock to commence with, and that I should repay him in regular instalments as it was sold; and that he would in this way keep up my stock. I need hardly say that an offer so generous was readily accepted. As it was now about the end of the harvest season, my friend suggested that I might come to Perth for the winter, and learn the mechanical art of weighing and parcelling up the tea, serving over the counter, and all the usual routine of shop work. I accordingly came in; and, before the winter was over, I became a thoroughly proficient shopkeeper.

On the approach of spring 1847, it was arranged that I should try Elgin, as a likely place. I accordingly went north, secured a suitable shop, and commenced business. As the merchants there were charging large profits, I soon secured a fair amount of trade. I liked the place, my occupation, and the people very much. The shop work suited me well, as it afforded intervals now and again for reading and study. I had not long entered on my new occupation when an incident occurred which I may now mention, as it led to consequences which very much influenced my opinions on theological subjects. Although I had read a good deal on the free will controversy, I had never seen Edwards' famous work on the subject. One day, however, I went out to a bookseller, purchased this treatise, came back to the shop, and commenced reading it. I had not proceeded far before I became much impressed by the singular acuteness, clearness, and force of the reasoning. It excited feelings approaching to astonishment and admiration. I resolved

then to commence at the beginning of the book and study it through, line by line, and page by page, until I should thoroughly master the treatise. This I did with the greatest care, often lingering for a day on a single page, with the view of not only thoroughly mastering the argument, but of examining it under every possible phase. But after I had gone through the book in this manner, I was utterly unable to perceive a flaw in the reasoning, which could in any way vitiate the main conclusions. The whole appeared irresistibly clear and convincing; and yet I could not adopt the theory that man was a necessitated agent. I went over the book a second, a third, a fourth, a fifth, and more times, with the self-same result. In short, for at least a year and a half, every spare hour of the day was devoted to the study of this work. It is probable that no one has ever devoted so much time to the study of the book as I have done. It is the most fascinating book I have ever met with in all my studies.

I had heard a good deal about Professor Tappan, of America, who had published a refutation of Edwards' arguments. I accordingly ordered a copy; and after a delay of several weeks, Tappan's work, consisting of three volumes, came to hand. I eagerly set to work to study Tappan's examination of Edwards' system, with the hope and expectation that I would now be enabled to perceive Edwards' fundamental error; for I still believed that an error somewhere must exist. But I had not gone far into the book, when I began to perceive, to my disappointment, not only that my difficulties were not met, but that Tappan had failed to perceive the real nature of the problem, or the force of Edwards' main argument against a self-determining power in the will. When I had read the second volume on the appeal to consciousness, my disappointment was not diminished. The testimony of consciousness, according to Tappan, seemed to amount simply to this: "I am perfectly conscious when I choose A., for example, of having the conviction that, all things

else being the same, I might have chosen B. instead of A." This no one will deny; but it leaves wholly undetermined the question as to whether the conviction is true or false; and this is the very point in dispute. The impression produced in my mind by the reading of Tappan's work was that the assumed flaw in the reasoning of Edwards had really no existence. Since then I have gone over thirty or forty treatises on the free will controversy, the most of which were opposed to Edwards, and my conviction as to the soundness of Edwards' conclusions remains unchanged. As a natural consequence, all my former objections to the main points of the Calvinistic theology soon disappeared; and I became convinced that some moderate form of Calvinism was nearest the truth, not only of philosophy, but also of Scripture.

I must here refer to a thought which suggested itself to my mind shortly after commencing the study of Edwards' treatise. The idea was this: the determination in reference to the will is merely a special form of a far more general and comprehensive system of determination, in fact, a determination which comprehends universal nature. Every organic form in nature is what it is, in virtue of determination. Whether it is a plant or an animal, or whatever else its specific size, form, genus, and every peculiarity may be—everything is due to the particular determination given to the molecules in its formation. The fundamental question in reference to the production of organic forms is not—What are the forces in action, or on what does their exertion depend? but, What is that which directs or determines these forces, what directs their action? It is not—What moves the molecules in the production of the organic form? but, What determines that motion? It is the particular determination of the force which accounts for the particular phenomenon. The mere exertion of force may be supposed to be the same in all phenomena. And what holds true of the physical world holds equally true of the mental, moral,

and spiritual. In short, the entire universe is a process of determinations, but not of determinations occurring at random. There are a unity, a plan, and a purpose pervading the whole, which imply thought and intelligence. When these considerations suggested themselves to my mind, I was very much impressed; and I resolved that I would go into the examination of this subject, and devote my future time, in so far as it was at my command, to this work. Unfortunately, however, I did not abide by my resolution; for I allowed physical science to divert my mind from the matter for fully a quarter of a century of the best part of my life.

On the 11th September 1848, I was married to my wife, Isabella, second youngest daughter of Mr. John Macdonald, Forres. The union has proved a happy one. She has been the sharer of my joys, sorrows, and trials (and these have not been few) for the past forty years. Her care, economy, and kindly attention to my comfort during the years of comparative hardships through which we have passed, have cheered me on during all my trials and sorrows.

At this time I was addicted to the nasty habit of smoking tobacco. I had only smoked for a few years, but the appetite seems to have got a strong hold of me. The tobacco had a most injurious effect on my stomach and nervous system. I had lost almost all appetite for food, and was altogether in a somewhat shaky condition. I had repeatedly tried to abandon the habit, but without success. At last I determined that, come what might, I would never during my life put a pipe in my mouth, and that, to make this determination more binding on me, I would pledge myself in writing to do so. I mentioned this resolution to one or two friends, who were about as great slaves to the tobacco as myself; and they agreed to follow my example. Accordingly, a pledge was written out, to which we appended our names. From that day, 29th December 1849, till the present hour, I have adhered strictly to my pledge. I believe, however, that I am the

only one of those who signed the pledge who so adhered to it. I had a terrible struggle with the appetite. For two or three months I was in a state of partial stupor, and it was nearly three years before the craving for the tobacco left me.

I may mention that I had been a pledged abstainer from drink for several years before then. In fact, as drink was not used at home by my parents, I may say that I was practically an abstainer from infancy.

A little after the time at which I gave up the use of tobacco, my elbow joint, which had not troubled me since the time I left Paisley, was attacked by inflammation of a very serious character. The effect of the inflammation was to completely destroy the joint, which shortly afterwards ossified, and became immovable. This was a sad blow to me; but, like many more of the strange dispensations of Providence, it proved a blessing in disguise. It completely cured the joint disease, and I afterwards enjoyed better health. By this illness, however, I was so long unfitted for attendance in my shop, that the trade rapidly fell off; and afterwards I could not manage to get the business raised to a paying condition. I struggled on for some time; but at last, in order to avoid losing money and getting into debt, I resolved to close the shop. I accordingly sold off everything, left Elgin, and came south to Perth. This was about the beginning of the summer of 1850. Owing to the weakly state of my arm, it was a considerable time after coming south before I was able to do any manual work. At that time the influence of electricity and galvanism as a curative agent was exciting a good deal of attention. As I was familiar with the construction of the machines which were used, I thought I should try the making of an induction apparatus, which I accordingly did. It was soon purchased; and, as others were required, I continued at the making of them for some time.

For about a year, at this period, I had a great deal

of leisure time for reading and study. My principal reading was on questions relating to liberty and necessity. This led me into theology, and then into metaphysics. I began at this time to study the Scottish school, and read, as I could get access to books, Reid, Stewart, Brown, Hamilton, and others. One of the first books I read was Beattie on Truth, which much delighted me, as it tended to remove a sort of philosophical scepticism and doubt in regard to the foundations of certainty, which made me often feel rather unhappy.

Again, however, the great question to be considered was to what hand I should turn in order to earn a permanent livelihood. Some of my friends suggested that I should try a temperance hotel; and one of them stated that he was about to erect a house at Blairgowrie, and that I could have it for that purpose if I chose. After due consideration I made up my mind to try that course. But here was the difficulty: the house required to be furnished, and this would require a considerable sum, which I had not. It occurred to me, however, that, as it would be some six or eight months before the house could be ready for occupation, and as my arm was now much improved, I might try and make a considerable number of the necessary articles of furniture during that time. I accordingly set to work: made chairs, tables, bedsteads, basin-stands, toilet-tables, and other articles; and, with the small sum in our possession, we managed to open the house in the early part of 1852. As we had no family, my wife, anxious to lessen expenses as much as possible, proposed to dispense with a servant, and do all the work herself. The house was kept in the perfection of cleanliness, and every attention was paid to the comfort of visitors, who generally expressed themselves well pleased. But as there was no railway to Blairgowrie at that time, the visitors were few and far between; and it was only with the most strict economy that we could manage to keep out of debt. Although Mrs. Croll had too much work, I, on the

contrary, unfortunately had too little. I took it into my foolish head to try to learn the Latin language. The reader will smile when I tell him that my main aim in trying to acquire Latin was to enable me to read the discussions of the schoolmen. An assistant teacher in the parish school gave me three lessons a week privately. But it would be difficult to find one with less aptitude for languages than I have. With a great amount of labour and perseverance I managed, in the course of a year, to acquire a knowledge of the rudiments of the language, and was reading in Cæsar. I found, however, that I would require another year's study before my Latin would be sufficient for my purpose; and, as I could not afford to lose so much time, I abandoned the whole affair.

After a year and a half's trial of the hotel, we found that there was little chance of its ever becoming self-supporting; so we gave it up, sold the furniture, and left the place. This was at the May term of 1853. I then went to Glasgow, where I obtained employment in an insurance office. About this time the Safety Life Assurance Company was started. This company was under the directorship of Richard Cobden, John Bright, Henry E. Gurney, Thomas Brassey, and other well-known men. A friend of mine, Mr. Wm. Logan, much esteemed in Glasgow for the interest he took in the temperance and other social reforms, was appointed the agent for the company in Glasgow. Requiring an assistant, he offered me the place, which I accepted. My principal duties were outside the office, pushing for proposals, in which I was pretty successful, particularly among the working classes. During the summer of the following year, cholera broke out very badly in Glasgow; and as the company was desirous that business should be as much suspended as possible till the epidemic should somewhat abate, I left the town for a few weeks and went to Perth. While at Perth, Dr. Robert D. Thomson, the well-known chemist, who was one of the

directors of the Temperance Provident Institution, was there on a visit at the time. The directors of this institution wished an agent in Dundee to devote his whole time to the work, and as something about my qualifications was known, I was offered the appointment. The terms proposed to me were favourable. I accepted the offer, went down to Dundee about the end of August, and commenced operations. I succeeded pretty well in that town. After I had been about nine or ten months in the place, the directors of the Safety Office offered me their Edinburgh agency; and as the situation was much superior, so far as salary was concerned, to that which I then held, I of course accepted the appointment, and at the May term of 1855, left Dundee for Edinburgh.

I found it much more difficult to obtain assurance proposals in Edinburgh than in either Glasgow or Dundee. The Safety was an English company, new and untried; and although the directors were well-known men, it did not offer any advantages superior or even equal to those of some of the old-established Edinburgh offices.

In the spring of 1856 my father died. There is something impressive about the death of one's parents. It brings forcibly to mind the fact that we are here but pilgrims and strangers.

During the time I was in the insurance business, I managed to find a considerable amount of time for reading and study. This was effected by employing every moment of my extra office hours to this purpose. My reading was exclusively in philosophy. It was in Edinburgh that I began the study of Kant. With the exception of Edwards, no writer has made such an impression on my mind as Kant.

At this time I became very much troubled by pain in the eyes, occasioned by looking so much on white paper. When the pain began in the eyes, strange to say, it left the top or opening of the head, the place

where it had been seated almost from infancy. I found that, by placing a small piece of plain coloured glass on the page of the book, I could manage to read without feeling much inconvenience. The pain in the eyes continued for several years.

In the autumn of 1856 I was transferred from Edinburgh to Leicester, where it was expected that more business might be done. When I came to Leicester, I found that there was as much difficulty in obtaining proposals there as in Edinburgh. Although one of the members of Parliament for the town was a director of the company, yet the people of Leicester seemed to think that one of the old-established Scotch offices was fully as safe as the Safety. After being there for about six months, my wife took seriously ill, and the medical advisers stated that it would be necessary that she should leave Leicester when she was able to be removed. She was shortly afterwards removed to Glasgow, to be beside her sisters, and there she lay for upwards of a year in bed.

As I could not return to Leicester, and the company had no opening for me in Scotland, I left their service. I got an engagement again from the Temperance Provident Institution, and went to Paisley, where I remained for six or eight months. I then finally abandoned the insurance business altogether, after spending four and a half years of about the most disagreeable part of my life. To one like me, naturally so fond of retirement and even of solitude, it was painful to be constantly obliged to make up to strangers.

I was now at perfect leisure, and as, for some time, nothing turned up for me to do, I commenced and drew up, somewhat hastily, some thoughts on the metaphysics of theism, a subject over which I had been pondering. These were embodied in the small volume, published anonymously in the latter end of 1857, under the title, *The Philosophy of Theism.* Only five hundred copies were printed, and the most of them were circu-

lated privately. The book, though favourably received by the press, attracted but little general attention. It appeared at a time when metaphysics was at about its lowest ebb.

In the spring of the following year, 1858, I got an engagement in the office of the *Commonwealth*, a Glasgow weekly newspaper, principally devoted to the advocacy of temperance, and social and political reform. On the 29th August of the same year my mother died; and my brother David, who up till this time had been residing at home with her at Wolfhill, came through and took up his abode with us. As I mentioned before, he was deformed, being hunchbacked, in consequence, as it was supposed, of a fall received when an infant.

A few months after my mother's death I unfortunately met with a mishap which has since entailed on me a considerable amount of pain and discomfort, and has disabled me all along for much physical exertion. One day, as I was exerting my whole strength in using a joiner's plane, while dressing a piece of wood, something suddenly appeared to give way about the region of the heart. Medical men have never been able to detect what is wrong. But ever since then, though my health and strength remained unimpaired, I durst not lift anything heavy, or attempt to run, or even walk fast. This mishap, however, has been to me a far less affliction than one which happened seven years later, and to which I shall afterwards have occasion to refer.

After remaining for upwards of a year and a half in the *Commonwealth* office, I learned that a person was required to take charge of Anderson's College and Museum, and applied for the situation. There were about sixty applicants. Fortunately for me, as it afterwards turned out, I received the appointment, and entered on my duties at the end of the autumn of 1859. Taking it all in all, I have never been in any place so congenial to me as that institution proved. After upwards of twenty years of an unsettled life, full of hardships and

difficulties, it was a relief to get settled down in what might be regarded as a permanent home. My salary was small, it is true, little more than sufficient to enable us to subsist; but this was compensated by advantages for me of another kind. It will naturally be asked—why such want of success in life? Why so many changes, trials, and difficulties? There were several causes which conspired to lead to this state of things. The mishap to my elbow joint compelled me to give up the occupation of a joiner when a young man; and the inflammation which destroyed the joint five years afterwards had the effect of blasting my hopes in the way of shopkeeping. The main cause, however, and one of which I had been all along conscious, was that strong and almost irresistible propensity towards study, which prevented me devoting my whole energy to business. Study always came first, business second; and the result was that in this age of competition I was left behind in the race. In this respect, however, my situation in Anderson's College suited me well. Here was the fine scientific library, belonging to the Glasgow Philosophical Society, to which I had access,—a privilege of which I took due advantage. Here also was the library of four or five thousand volumes in connection with the popular evening classes of the institution; and, further, the private library of the founder of the institution, consisting of over two thousand volumes. My duties were regular and steady, requiring little mental labour; and as my brother was staying with me, he gave me a great deal of assistance, which consequently allowed me a good deal of spare time for study. The Museum was open from 11 A.M. till 3 P.M.; but as I had little or nothing to do with the arrangement and classification of the specimens, and there were but few visitors, I had generally a few hours a day of a quiet time for reading and study.

I suspect that the fact of my mind being so evenly balanced between the love of physics and the love of philo-

sophy has been a disadvantage as well as an advantage to me; for when I am engaged in physics, for example, I am continually tempted to turn aside into philosophy; and when in philosophy, the attractions of physics frequently draw me over. In fact, it is only by a strong effort of will that I have managed to keep for years continuously in the same region of inquiry, without passing over into the other.

When I came to Anderson's College, I had been engaged in philosophical and theological studies for a period of fifteen years. Just about the time I entered the institution, I had, in fact, begun to consider in a systematic form the problem which, as already stated, had suggested itself to my mind in 1848, and which had been but slightly touched upon in my little book, *The Philosophy of Theism*, then recently published. I soon found, however, that the attractions offered by the institution for the study of physical science were too strong to allow me to continue my metaphysical studies; and although this problem to which I had set myself was then, as it still is, the one of all others most attractive to me, I nevertheless resolved to lay it aside for a year or two, and begin again the study of physics at the place where I had left it off in former years. At this time, the then modern principle of the transformation and conservation of energy and the dynamical theory of heat attracted my attention. I read also with much interest the researches of Faraday, Joule, Thomson, Tyndall, Rankine, and others on heat, electricity, and magnetism. At this period the question of the cause of the Glacial epoch was being discussed with interest among geologists. In the spring of 1864 I turned my attention to this subject; and, without knowing at the time what Herschel and Lyell had written on the matter, it occurred to me that the change in the eccentricity of the earth's orbit might probably be the real cause. I accordingly drew up a paper on the subject, which was published in the *Philosophical Magazine* for August 1864. The paper

excited a considerable amount of attention, and I was repeatedly advised to go more fully into the subject; and, as the path appeared to me a new and interesting one, I resolved to follow it out. But little did I suspect, at the time when I made this resolution, that it would become a path so entangled that fully twenty years would elapse before I could get out of it.

One evening in July of 1865, after a day's writing, I hurriedly bent down to assist in putting a few tacks into a carpet, when I experienced something like a twitch in a part of the upper and left side of the head. It did not strike me at the time as a matter of much importance; but it afterwards proved to be the severest affliction that has happened to me in life. Had it not been for this mishap to the head, all the private work I have been able to do during the twenty years which followed might have easily been done, and would have been done, in the course of two or three years. The affection in the head did not in any way affect my general health, neither did it in the least degree impair my mental energy. I could think as vigorously as ever, but I dared not "turn on the full steam." After this twitch a dull pain settled in that part of the brain, which increased till it became unbearable, if I persisted in doing mental work for any length of time. I was therefore obliged to do mental labour very quietly and slowly, for a short period at a time, and then take a good long rest. If I attempted to do too much in one day, I was generally disabled for a few days to come. Another consequence was this: before this affliction in my head, I could concentrate my thoughts on a single point, and exert my whole mental energy till the difficulty was overcome; but this I never could attempt afterwards. After struggling so many years against difficulties of every sort, and just at the time I had about overcome them all, and was expecting to be able to do some real work, I felt it very hard to be so disabled for the future. However, under all these difficulties, I managed, during the two and a half years

which elapsed before I left Anderson's College, to write about a dozen of papers of a longer or shorter character.

Amongst the very first to express a favourable opinion of the theory of the cause of the Glacial epoch, which I had recently advanced in the *Philosophical Magazine*, were Professor (now Sir Andrew) Ramsay, then director of the Geological Survey of England and Wales, and Dr. A. Geikie, who, on the reorganisation of the service, had just been appointed to the directorship of the Scottish Survey. At this time a large addition to the staff was required, and I was asked if I would be willing to allow myself to be nominated for the Scottish service. This very kind offer was tempting to me in many respects; but as I was somewhat up in years, and suffering a little from the mishap to my head, and, besides, not satisfied as to my special qualifications for the scientific duties of the office, I did not see my way clear to accept the proposal. As it was necessary that one of the men should be permanently located in Edinburgh, to act as resident surveyor and clerk in the office, I was again asked if I would be willing to accept this appointment. This very materially altered the condition of things; and, after duly thinking over the matter, and consulting some of my friends, I agreed to be nominated. I felt, however, reluctant to undertake duties which required much mental work, as, owing to the state of my head, it might materially interfere with the private work on which my mind was so much set. But, on the other hand, as my salary was so small, and my health at the same time suffering a good deal during the winter months from the cold draughts in the lobbies of the institution, I felt that, taking all things into consideration, I ought not to lose this opportunity of improving my circumstances, and I accordingly agreed to stand for Civil Service examination. I failed, however, in some of the subjects. This is what might have been expected, considering my constitutional nervousness, combined with my age and want of experi-

ence in ordeals of that kind. I, notwithstanding, received the appointment, and got my Civil Service certificate some time afterwards. I accordingly resigned my situation in Glasgow, went through to Edinburgh, and entered the Service on September 2, 1867.

I found, as I had expected, that the duties of the office were not at all laborious, either physically or mentally. They consisted simply in attending to the various details of office work: namely, conducting the correspondence with the men in the field; supplying them with the necessary maps, instruments, and stores; correspondence with the engraver, colourist, and Ordnance Survey; checking the maps; keeping the registers, etc. etc. These various duties kept me busy during office hours, without producing mental exhaustion. The only thing I suffered from was now and again having to write two or three letters in succession. The director, Mr. Geikie, I found to be a most agreeable person. This was all along a great comfort to me. During the thirteen years we were together in the office, never so much as an angry word passed between us. I need hardly add that my duties as resident geologist really did not require much acquaintance with the science of geology. This relieved my mind from having to study a science for which I had no great liking, and thus allowed me to devote my whole leisure hours to those physical questions in which I was engaged. There was, however, one department of geological inquiry with which the physical questions, in which I was then engaged, required that I should be acquainted, namely, surface geology, or drift in its bearings on Glacial and Interglacial periods. I had begun my studies in this department before I left the "Andersonian," and had made frequent excursions into the country in search of glacial phenomena. I was fortunate in discovering two remarkable buried river channels, belonging to pre-Glacial and Interglacial ages, and some other singular facts bearing on the history of the Glacial period. These

researches were continued after I went to Edinburgh with equal success; and their results were given in one or two papers contributed to magazines, and afterwards embodied in *Climate and Time.*

Although the hours of business in my new occupation were not long—being only from 10 A.M. to 4 P.M.—and the duties not laborious, I nevertheless found that in the evening I was not in such a fresh condition to begin my private labours as I was when in the "Andersonian."

This at times made me feel a sort of half regret that I had ever left my former situation. These six hours of mental work, comparatively light as it certainly was, added to my private work, began ere long to tell badly on my poor head; and I had to adopt every means I could devise to husband my energies.

I may here give a short statement of the mode of life which I adopted when I came to Edinburgh, to which I adhered pretty closely during the whole time I was in the Survey. After being in Edinburgh for a short time, I took a small house at Jordan Bank, Morningside, at the southern extremity of the city. After coming home from the office and taking dinner, which was generally about five o'clock in the afternoon, I took a rest for about an hour, and then took a stroll in the country. A few hundred yards beyond Morningside the main road separates into two branches, which, after diverging about a quarter of a mile apart, again unite about a mile and a half beyond the place where they separated. One very frequent walk of mine after dinner was to go out by the one branch and return by the other. This gave me a circuit of about three miles. As I walked very slowly and employed the whole time in study, I was generally out of doors for one hour and a half, or two hours. At other times I would make an excursion to the Braid Hills or to Craiglockhart. As the beauties of nature, especially in its retirement, have such a charm for me, I found these outdoor walks helpful to study. I never took a companion with me in these walks, as not only

would doing so deprive me of the opportunity of getting on with my studies, but the effort and excitement of talking would to a considerable extent unfit my head for mental work on my return.

In these walks I generally carried a pencil in the one hand and a bit of paper in the other, on which I jotted down my ideas as they suggested themselves; or rather, I should say, made jottings to enable me to keep a record of them till my return home, when they would be written out more fully. To save my head, I got a young man to come and read and write an hour each evening for me. This generally would end the labours of the day. But, should I be still in a working condition, I would continue my studies quietly for another hour.

I found that if I attempted to do any head work in the morning, it would completely knock me up for the day. When going to the office in the morning, I was obliged, as much as possible, to avoid talking to people on the way, which sometimes was no easy matter; and this compelled me to take a by-path. Another thing I was obliged to adopt, was not going out to public meetings or to dinner or evening parties. Had I not adopted this mode of life, I could not, in the state of my head, have done private work. During the thirteen years I was in Edinburgh, I remember of being only twice at a scientific meeting, and once at a concert. On the whole, I led a somewhat retired life in Edinburgh. But, in order to get on with my work, I had to adopt another expedient: namely, to confine my reading exclusively to the subjects which I was engaged in studying. So strictly did I adhere to this resolution, that it was but rarely I looked even at a newspaper or a journal of any kind. In fact, my reading was rather a toil to me than a pleasure. It was a means to an end, but a means which could not be avoided. The pleasure lay in the study, and not in the reading; but without the reading there would not have been sufficient materials for study. Had

I been reading simply for pleasure, it would probably not have been either in physics or in metaphysics, but rather in the writings of Wordsworth, Tennyson, and other authors of that *ideal* type.

In all my reading I had to adopt this plan: namely, to mark on the margin of the page, and underline with pencil the passages to which I might have occasion to refer. Had I not adopted this mode, the reading would not have been of great advantage. But, notwithstanding all these precautions, the head grew gradually worse; so much so, that I had frequently to apply for sick leave for a month or six weeks at a time; and in 1873 I was disabled for duty for nearly nine months. For two or three years prior to the publication of *Climate and Time*, it was with the greatest difficulty that I could manage to put together in one day as many sentences as would fill half a page of foolscap. In fact, the appearance of the volume was delayed for two or three years on this account. During all this time the mind was as vigorous as ever; it was pain in the head, and pain alone, which stopped all progress when I attempted mental work. I frequently thought I should be obliged to resign my situation in the Survey; but as I had not completed my tenth year of service, and would therefore not be entitled to superannuation, I was strongly urged to try and struggle on. I was for several years under the medical treatment of Dr. Warburton Begbie, and this experienced physician did everything in his power; but nothing he could prescribe had any effect. I was afterwards under Professor Sanders, and latterly under Professor Grainger Stewart, with no greater success.

In February 1876, my brother, who had been staying with us ever since my mother's death, died suddenly from heart disease. In this same month the University of St. Andrews conferred on me the degree of LL.D., an honour which I felt at the time somewhat doubtful as to the propriety of accepting. A few months afterwards I was elected a Fellow of the Royal Society of London.

I may also mention that in the same year I was chosen an Honorary Member of the New York Academy of Science. I was afterwards chosen an Honorary Member of the Bristol Natural Society, of the Psychological Society of Great Britain, of the Glasgow Geological Society, of the Literary and Antiquarian Society of Perth, and of the Perthshire Society of Natural Science. I had the honour of receiving from the Geological Society of London the balance of the proceeds of the Wollaston Donation Fund in 1872, the Murchison Fund in 1876, and the Barlow-Jamieson Fund in 1884.

When I had finished *Climate and Time*, I resolved to abandon not only my climatological studies, but physics in general, in order to be able to resume those investigations into the philosophy of evolution, which I had laid aside at the time that I entered Anderson's College. I soon found, however, that it was a resolution to which I could not well adhere. After the publication of the volume, I found that, notwithstanding the care I had taken to express my views in the clearest manner I could, these views were on many points very much misapprehended. I therefore found it necessary to endeavour not only to remove those misapprehensions, but to enter at much greater length into some of those difficult points which had perhaps been too briefly discussed in the volume. The consequence was, that, owing to the state of my head obliging me to write so slowly, and also to other circumstances, it was not till 1885, or ten years after the publication of the volume, that I managed to shake myself clear of that perplexing question which had engaged my attention for upwards of twenty years. (Since this was written, I have been obliged to enter again into the consideration of some points.—May 1888.)

One day in the office, during the summer of 1880, while hurriedly endeavouring to remove some maps from a drawer, and standing in an awkward position on a pair of steps, I unfortunately strained something about the region of the heart. The result was, that for months

I was unable to walk about, or make any physical exertion. It became doubtful if I should ever be able for office duties. Just at this time, I had been suffering rather badly from my old complaint in the head, and was at the time under the medical treatment of Professor Grainger Stewart. This gentleman thought that it might be well to try what effect the external application of that powerful drug aconite might have in relieving the pain in the affected part of the head. His instructions were to apply a little of the aconite over the part when I felt the pain badly. I continued to do so for some time, but it had not the desired effect. One evening, after I had applied it once or twice, I all at once found that I had lost my power of speech, or rather, that I could only speak like a paralytic, in an unintelligible form. It is evident that this powerful poison had paralysed some of the nerves or muscles of the tongue or the lips. In course of a week or two I regained my speech, though even yet there are a good many words which I cannot pronounce.

As I was now disabled for duty both by head and heart, and as there was not much prospect that I should ever be fit for office work, it was considered advisable that I should resign. And as I had been admitted into the service at the advanced age of forty-six, it was believed that, in computing the amount of superannuation to be allotted me, my age would be taken into consideration. This I had every reason to believe would be the case, the more so, seeing that there was a clause in the Superannuation Act which applied to my case. I accordingly agreed to withdraw from service. But, to my utter chagrin, no attention was paid to these considerations by the Treasury; and I received superannuation only for the thirteen years I had actually been in the Service. Thus my income was all at once reduced from £350 to £75, 16s. 8d. a year. No effort was made to urge upon the Treasury to allow me a little more; and as this sum was insufficient, seeing that I was a married

man, I had no alternative but to give up housekeeping and go into cheap lodgings. Application was made to Mr. Gladstone, who was then Prime Minister and First Lord of the Treasury, to allow me a small sum from the Civil List; but, after keeping my friends waiting for a year and a half, he stated that he could not recommend a pension for me. A year or two afterwards, when Lord Salisbury came into office, application was again made by my friends for a small grant from the Civil List, but with like unsuccess.

After moving about for five years in this unsettled condition, and having during the time obtained, through the kindness of friends, a little increase to my income, I resolved on taking up house again. I was fortunate in obtaining a lease of a comfortable house in the suburbs of Perth; and, at the end of the summer of 1886, I took up my permanent abode there.

The results of my labours since the appearance of *Climate and Time* in 1875 were, in October of 1885, published in a volume, under the title of *Climate and Cosmology*. This I resolved should terminate my studies, not merely in climatology, but also in physical science in general. This resolution, as was stated before, was made in order to enable me to finish work which had been laid aside for upwards of twenty-five years. But whether my wish in this respect will ever be accomplished, is certainly at present a matter of uncertainty.

CHAPTER I

ANCESTRY, BIRTH, CHILDHOOD, AND SCHOOL DAYS

"AMONG all the provinces in Scotland," writes Sir Walter Scott, "if an intelligent stranger were asked to describe the most varied and the most beautiful, it is probable he would name the county of Perth. A native, also, of any other district of Caledonia, though his partialities might lead him to prefer his native county in the first instance, would certainly class that of Perth in the second, and thus give its inhabitants a fair right to plead that—prejudice apart—Perthshire forms the fairest portion of the northern kingdom. It is long since Lady Mary Wortley Montagu, with that excellent taste which characterises her writings, expressed her opinion that the most interesting district of every country, and that which exhibits the varied beauties of natural scenery in greatest perfection, is that where the mountains sink down upon the champaign, or more level land. The most picturesque, if not the highest hills, are to be found in the county of Perth. The rivers find their way out of the mountainous region by the wildest leaps, and through the most romantic passes connecting the Highlands with the Lowlands. Above, the vegetation of a happier climate and soil is mingled with the magnificent characteristics of mountain scenery; and woods, groves, and thickets in profusion clothe the base of the hills, ascend up the ravines, and mingle with the precipices. It is in such favoured regions that the traveller finds what the poet Gray, or someone else, has termed 'beauty lying in the lap of terror.'

"From the same advantage of situation, this favoured province presents a variety of the most pleasing character. Its lakes, woods, and mountains vie in beauty with any that the Highland tour exhibits; while Perthshire contains amidst this romantic scenery, and in some places in connection with it, many fertile and habitable tracts which may vie with the richness of Merry England herself. The country has also been the scene of many remarkable exploits and events, some of historical importance, others interesting to the poet and romancer, though recorded in popular tradition alone. It was in these vales that the Saxons of the plain and the Gael of the mountains had many a desperate and bloody encounter, in which it was frequently impossible to decide the palm of victory between the mailed chivalry of the low country and the plaided clans whom they opposed."[1]

In the eastern division of this charming county lies the little post-office village of Cargill, which gives its name to a parish in one of the most picturesque districts, known as Strathmore. The village stands on the left bank of the river Tay, about three quarters of a mile west-south-west of the railway station of Cargill on the Caledonian line, being about eleven and a half miles north-north-east of the city of Perth, and four and a quarter miles south-west of the town of Coupar-Angus.

The parish embraces the villages of Burreltown, Woodside, and Wolfhill, and is bounded on the north-east by Coupar-Angus, on the east by Kettins, in Forfarshire, and by a detached portion of Scone, on the south-east by Abernyte and Collace, on the south by St. Martins, on the west by Auchtergaven and Kinclaven, and on the north-west by Caputh. Its greatest length, from east-north-east to west-south-west, is about $6\frac{3}{8}$ miles; its breadth, from north-west to south-east, varies between $3\frac{1}{2}$ furlongs and 5 miles; and its area is $9626\frac{1}{2}$ acres, of which $131\frac{1}{2}$ are water. The noble river Tay,

[1] *Fair Maid of Perth.*

which pours the largest volume of water into the ocean of any river in Great Britain, and was compared by the early Roman invaders with the Tiber, winds its way for four and a half miles along the western boundary of the parish, while the lazy little Isla, gliding for two and a quarter miles down to the Tay, traces the north-western boundary. The land is finely diversified with ascents and declivities, clad with wood and interspersed with water. The western border, to the mean breadth of about a mile, rises gradually from the Tay,—the central tracts forming a low plateau with some unevenness of contour, while the eastern border includes a strip of the Sidlaw Hills. In the extreme south-west the surface sinks to about 100 feet above sea-level, thence rising near Wolfhill to a height of 409 feet, of 414 feet in Gallowhill, of 390 feet at Redstone, of 598 feet near Legertlaw, and of about 1235 in King's Seat on the Abernyte border. Sandstone of excellent quality has been extensively quarried for building purposes, and limestone is to be found in considerable quantity, which might be profitably worked, whilst a reddish rock marl is also plentiful in the district. The soil near the Tay is strongly argillaceous; on the central plateau it is partly loamy and partly moorish; while towards the foot of the Sidlaws it is formed of light, dry gravel. An extensive acreage is under wood, very little is pastoral, and still less is allowed to lie waste. The scenery along the Tay includes the picturesque Linn of Campsie, and ranges from the softly romantic to the grandly magnificent. Tumuli and remains of Caledonian megalithic structures occur in various places, while vestiges of a Roman camp, with *fossæ* perfectly discernible, and with fragments of an aqueduct leading from it to a neighbouring rivulet, are to be seen near the confluence of the Tay and Isla. A Roman road, twenty feet broad, and formed of rough, round stones, passes north-eastward by Burreltown; and a high rock overlooking the Linn of Campsie is crowned by traces of an

ancient monastery, said to have been subordinate to Cupar, whose abbey, being supplied with fuel from Campsie Wood, gives the name of Abbey Road to the track by which it was conveyed. Stobhall House, a prominent feature, belongs to Lady Willoughby de Eresby, who is the largest proprietor in the district.

At Little Whitefield, a small hamlet in this charming county, James Croll was born on Tuesday the 2nd of January 1821, at eleven o'clock at night. The weather was cold and stormy, with snow lying thickly on the ground, as in the case of Burns, who sings—

> "'Twas then a blast of Januar' win'
> Blew hansel in on Robin."

The war of the elements seems to have foretold the troubled and unsettled life which Croll was destined to lead, as well as the comparatively cold reception which his last work was doomed to meet from the unsympathetic world. . . . Most instructive would it be, were we able to tell the history of his ancestors on both sides of the family, as has been so fully done in the case of Ralph Waldo Emerson, and see how nature, working quietly through many years and gathering from many sources, at last produced, by the blending of many different qualifications, the man in whom the family was destined to develop its noblest mental and moral characteristics, and then to pass away. Unfortunately, of his ancestors on the mother's side, we know almost nothing. On the father's side Croll seems to have been the consummate flower of a healthy, vigorous stock of men in lowly life, which flourished for centuries, unknown to the world, in the quiet seclusion of a country parish in the county of Perth. The Croils, or Croyls, for so his forefathers used to spell their name, were inhabitants of the parish of Cargill for a period of at least two centuries. On examining the baptismal register of the parish, James was able to trace his parentage backwards in a direct line to about the middle of the seventeenth century. Here,

unfortunately, the register comes to an end, and he was able to learn no more. Enough, however, remains to indicate that the Croils were a race of patient, plodding men, whose motto might well have been "Steady, aye steady."

For several generations the Croils were inhabitants of the small crofting village of Little Whitefield, in the parish of Cargill. This village contained only some eight or ten houses, whose tenants had about a hundred or a hundred and fifty acres of the surrounding land divided among them as crofts. Here Croll's father, David, son of Alexander Croil, was born in March 1781. Like his forefathers, he clung to his native soil. He learned the trade of a stonemason, and seems to have succeeded in securing for himself a house and croft in his native village only at a somewhat advanced period of his life. At all events, it was not till he had reached the mature age of thirty-seven that he ventured to enter upon married life. His wife, Janet, youngest daughter of James Ellis, of Elgin, was born in that city in 1781. She left Morayshire for Perth during the early part of the present century; and, about the latter end of the year 1818, she was married to David Croil, who was of the same age as herself.

Croll's parents were both distinguished by the possession of great force of character and high intelligence. In some respects, however, they differed widely from one another. His mother was a woman of much native shrewdness, keen powers of observation, and great firmness of purpose. In the ordinary affairs of life she displayed no small amount of that remarkable commodity, usually termed "common sense," which generally indicates a well balanced mind and the power to do well in the battle of life. His father was a man of a deeply religious nature, warmly attached by both sympathy and conviction to the Independent or Congregational Church, whose members enjoyed a high reputation in the beginning of this century for their earnestness of purpose and

purity of character. Endowed with considerable intellectual power, he was more inclined to a quietly meditative than to a busy, pushing life. Strictly conscientious in all things, he was also keenly sensitive by nature, and apt to be too easily troubled by the cares of life. His high moral character and amiable disposition secured for him the friendship and esteem of all who knew him. He was highly respected throughout the district in which he worked, and his children were always proud to tell, when asked, who their father was, as the mention of his name generally procured for them a friendly smile and the encouraging remark, "I hope that, when you grow up, you will be as good a man as your father."

The family of David Croil consisted of four sons, of whom James was the second. Alexander, the eldest, was born on the 29th of November 1819. He seems to have been a boy of much promise, able and willing to assist his younger brothers in the preparation of their lessons; and his death, which took place when he was about ten years old, threw a deep shadow over the family life. The parents felt the blow severely, especially the father, who never afterwards regained his usual vivacity of spirits. David, the third son, was born on the 23rd of April 1822. In infancy he was believed to have fallen from the arms of a girl who was nursing him. This accident brought on a curvature of the spine and general debility, so great that, although he lived to the age of fifty-four, he was never able to leave the shelter of the home circle and fight an independent battle with the world. William, the youngest of the family, was born on the 25th of February 1826 and died in infancy.

James, the second son of David and Janet Croil, was born at Little Whitefield, as already stated, on the 2nd of January 1821. Of his earliest years we know nothing beyond a few characteristic incidents which he has narrated in his autobiographical sketch. Even in infancy a certain delicacy of constitution seems to have become manifest; but, at the same time, some

of his remarkable powers of mind were strikingly displayed. He could hardly have been more than eighteen months old when his brother David was baptized. His father was then a member of the Congregational Church in Perth; and the pastor, the Rev. Wm. Orme (afterwards of Camberwell, London), came out to Little Whitefield to perform the ceremony. Availing himself of the opportunity afforded by his visit, the minister conducted a religious service in the open air for the benefit of the villagers. Open-air services of this kind formed a very special feature in the ecclesiastical life of Scotland at this time. In connection with the Presbyterian Churches, established and non-established, there was usually a great gathering of the people in the open-air at sacramental seasons. Burns has immortalised these in his strikingly forcible satire, "The Holy Fair." During the early part of the century, however, a considerable revival of religion was awakened in the people of Scotland, by the itinerant preaching services of Rowland Hill and the brothers Haldane; and this was largely furthered and developed by the ministers of the Congregational or Independent Church. They regarded it as an important part of their work to make preaching tours throughout the country, to hold out-door services as opportunity was given, and to conduct Sabbath schools for the religious instruction of the young.

The Rev. William Orme was a rather remarkable man among them. It is a curious coincidence that, like Croll (as we shall see), he had served an apprenticeship as a wheelwright in the Grassmarket of Edinburgh. This occupation he only quitted in the month of October 1805, in his nineteenth year, to join Mr. Haldane's Academy, under the tuition of Mr. George Cowie, in preparation for the work of the ministry. Having completed his curriculum there, he was, in March 1807, called to the pastorate of the Congregational Church in Perth, where he laboured for seventeen years, only leaving the city when he accepted a call to a church in Camberwell,

London. Soon after he went to Camberwell he led the English Dissenters in their agitation for the repeal of the odious and obnoxious "Test and Corporation Acts," in which he was successful. He was a man of considerable intellectual ability, much energy and decision of character, while he also possessed the power of ready tongue and pen. He wrote lives of Owen and Baxter, as also a copious and elaborate *Bibliotheca Biblica*, which showed extensive reading, great industry, and high attainments as a writer. He also wrote the life of Kiffin, a Baptist minister in London, *Memoirs of John Urquhart*, an *Argument for the Weekly Observance of the Lord's Supper*, *Discourses on the Work of the Holy Spirit*, a *Catechism on the Constitution and Ordinances of the Kingdom of Christ*, and various articles for the magazines.

A contemporary says that long before his introduction to the metropolis, Mr. Orme had risen to high and distinguished eminence as an able preacher of the New Covenant; and we have learned that in Perth he was considered one of the ablest and most popular ministers of his time. He would therefore, no doubt, on a visit to a small hamlet like Little Whitefield, create considerable excitement in the neighbourhood. His visit would be an event in the place; and people would, doubtless, be gathered together in large numbers from the surrounding district to hear him preach.

Little James Croll was present at the service conducted by Mr. Orme, and the whole scene was so vividly impressed upon his mind that, even to his latest years, he retained a most distinct recollection of the interesting ceremony. Other striking incidents of the first three years of his life were similarly treasured up in memory,—among them the stirring events of the day when the family removed from his earliest home, while, of the years subsequent to these, his mental record was singularly regular and complete, including every important incident in the history of himself and his family. The remarkable power of accurate observation, thus early

displayed and afterwards developed by Croll, he seems to have inherited from his mother. The tenacious memory, from which nothing which interested him ever passed away, came from his father, whose recollections of the past went back to within two years of his own birth.

The first three years of Croll's life passed quietly away in the little hamlet, where his forefathers had lived for several generations. Then there came a change. In the early part of the present century the landed proprietors of Scotland were attacked by the mania for large farms. In many districts crofting villages were swept away to make room for a new style of agriculture; and the old inhabitants were turned adrift to shift for themselves as best they could. Among the old-fashioned hamlets thus abolished, to prepare for the formation of extensive farms, was Little Whitefield. In this case, however, the owner of the ground, Lord Willoughby, evinced a kindly sympathy with his humble tenants, and decided, in their interest, to set apart two pieces of land for feuing purposes: the first, a stretch of waste land, about a mile to the south of Little Whitefield, now known as Wolfhill; the second, a bit of ground lying three miles to the north, now called Burreltown. David Croll took a feu in the former district, erected on it a dwelling-house and other buildings necessary for a crofter, and then, in 1824, removed with his family to their new home. Availing himself of the permission granted to the feuars in the new village, to reclaim a part of the waste ground in the vicinity, he employed himself in this work during the time which he could spare from his labours as a stonemason; and by and by, having succeeded in bringing some four or five acres into fairly good condition, he felt himself restored to his former position.

Having established himself with his wife and children in the new home at Wolfhill, David Croll resumed the even tenor of his humble, industrious life. Himself a highly thoughtful man, he had his own views on the

early education of the young. It seemed to him that the physical system ought to be allowed a considerable period of natural, healthy development, before the work of direct mental training was begun. Accordingly, James was granted three or four more years of careless ease, in which no thought of lessons ever disturbed his dreams. At length, when it seemed high time that he should enter upon the usual routine of school life, an unexpected difficulty presented itself. He had begun to suffer from a somewhat troublesome pain on the top or about the opening of his head. He was thus unable to remain with uncovered head for any length of time, except in the very heat of summer; and as he could not on any account be persuaded to sit beside his uncovered companions with a cap on his head, his parents were reluctantly compelled to keep him at home and give him private lessons. Thus it happened that his first teacher was his father, who was at times assisted by his eldest son, a lad but two years the senior of his pupil. From time to time James also received a private lesson from the master of the little village school; and so, amid many difficulties, he acquired the elements of reading and writing. Even the schoolmaster, who assisted the father and brother in the work of teaching the young philosopher, seems to have been but very imperfectly qualified for his task; he was an elderly man who had once occupied a more lucrative position in the world but had latterly been compelled, through reverse of fortune, to eke out a scanty livelihood by giving lessons to a few boys in the quiet village of Wolfhill. After about two years of somewhat mixed and fragmentary instruction at home, James was sent, for a short time, to the parish school of Cargill, a mile and a half distant from his home; and by and by he was removed to a voluntary school in the village of Guildtown, about two miles west of Wolfhill. The master here, unfortunately, was a rough, pompous, and tyrannical man, who fully succeeded in inspiring his pupils with a thorough detestation of school

and all that was connected with it. It must surely have been of him that Croll, in after years, told the following absurdly ludicrous story:—"One day the 'dominie' was giving the boys their usual Bible lesson, when he suddenly asked the question 'Which of all the prophets, in your opinion, most resembled the Apostle Paul?' Each of the lads, in turn, gave the answer which seemed to him the most appropriate. At last the teacher approached a boy who generally sat at the foot of the class, and never, in any case, was able to rise more than one or two places higher. On this occasion, however, he seemed unusually anxious to be heard; and, when the question was put to him, he at once answered, 'It's yer ainsel, sir.' The astonishment of his comrades may be imagined; but what must have been their feelings when the master calmly replied 'That's a good boy; you go up to the top of the class for that'?" That a man of whom such a story could be related should have been accepted as an instructor of youth in a village but a few miles distant from the city of Perth, seems hardly credible in these days. That he failed to excite any enthusiasm for learning even in the mind of young Croll can excite no surprise. Fortunately for the district, he was soon replaced by Mr. Keiller, a much more kindly and intelligent man, whose work was naturally attended by more satisfactory results. James Croll, however, enjoyed but little of his good offices. He was now (1834) thirteen years of age; and, after some eight or nine months under the new teacher, he left school finally, to enter upon other work.

Croll's school days, taken altogether, extended over a period of about six years; but there were blank spaces in this period, his teachers were too frequently changed, and, as we all know, too many masters spoil the lad. His training seems to have been wholly confined to the three R's; and, even within these narrow limits, the work was but poorly done. The eager, active intellect of the boy was never roused by his early teachers, his interest was

never stirred by anything they presented to his view. Bright and intelligent, full of fun and frolic, he was left to spend his energy on out-of-doors schemes and occupations. In some respects, in view of a certain delicacy of constitution which had already appeared, we may regard this as a bit of good fortune. To the surprise and disappointment of his father, he was (he himself tells us) a rather dull scholar, scarcely up to the average of boys of the same age in the matter of getting up lessons quickly and correctly. He failed to acquire an accurate style of reading; and by no amount of labour could he succeed in learning to spell even moderately well. For him the teacher (so called) was a mere taskmaster, the lessons the labour of a bondsman, like the making of bricks without straw. His real teachers were to be found outside the village schoolroom, the forces which stirred his youthful intellect were to speed from a far distance. In due season they drew near, they acted with quickening power, they presented the wonders of science in all their beauty and charm, they aroused in the boy a passionate desire for the higher education; but, alas! they did so only as the unbending force of circumstances was compelling him to enter upon a lowly lot in active life.

Croll's father was not merely a working stonemason; he was also a crofter, holding some four or five acres of land, the cultivation of which involved a considerable amount of labour. During the greater part of the year he followed the work of his trade, which carried him to many different places at a greater or less distance from home. The lot of a mason in these times in Scotland was rather a hard one. Carlyle, writing of his father and a companion, both of whom were masons, says: "The two 'slung their tools' (mallets and irons hung in two equipoised masses on the shoulders) and crossed the hills into Nithsdale to Auldgarth, where a bridge was building. This was my father's most foreign adventure. He never again, or before, saw anything so new; or, except when he came to Craigenputtock on visits, so distant. He

loved to speak of it. That talking day we had together I made him tell it me all over again from the beginning, as a whole, for the first time. He was a 'hewer,' and had some few pence a day. He could describe with the lucidest distinctness how the whole work went on, and 'headers' and 'closers' solidly massed together made an impregnable pile."[1] "A noble craft it is, that of a mason; a good building will last longer than most books, than one book of a million. I have a dim picture of him (my father) in his little world. In summer season diligently, cheerfully labouring with trowel and hammer, amused by grave talk and grave humour with the doers of the craft. Building, walling, is an operation that beyond most other ones requires incessant consideration,—even new invention." But for all this, as we have been told, they only earned a few pence a day. In the "dear years" (1799 and 1800), says Carlyle, "when the oatmeal was as high as ten shillings a stone, he had noticed the labourers (I have heard him tell) retire each separately to a brook and there drink instead of dining, without complaint, anxious only to hide it."[2]

The mere possession of the croft evinced a strong desire to improve the condition of the stonemason and his family. The little farm, if well worked, was sure to add considerably to the comforts of the household, but it also added greatly to the toils of the hard-working parents. When we consider that those four or five acres had first to be reclaimed from a state of bog or moorland waste, and by years of unceasing hard labour—breaking up, draining, and manuring—brought into a state of cultivation and rendered really productive, we cannot but wonder that, in conjunction with the severe daily toil of a stonemason, Croll's father was able to accomplish such a work at all. Yet, like many other hard-headed and hard-handed Scotchmen, he, along with his wife and family, did so. The wife and children, of course, lent a helping and at times. The work of reclaiming could be carried

[1] *Rem* i. 45, 47, 48 [2] *Ibid.* i. 61.

on at any season of the year; but when the ground was brought into a state of cultivation, the labour of making it productive could be accomplished only at the appropriate seasons of the year. There was not sufficient work to occupy Croll throughout the year on his croft, and it would have been impossible for him in such a way to earn a competent sustenance for himself and his family. He therefore continued, as we have seen, to follow his occupation of stonemason, which often took him from home. The working of the croft was thus left mainly in the hands of his wife, who occasionally obtained from outside helpers such assistance as was indispensable. In ordinary circumstances, however, she required some regular assistance at home. Her eldest son, Alexander, had died some five years before the present date (1834), and James had for some time been engaged at intervals in bits of humble farm work. The position of the affairs of the family now demanded that he should devote himself regularly to this occupation; and, accordingly, with an aching heart, he left school, when not quite fourteen years of age, to enter upon his apprenticeship to the stern work of life.

CHAPTER II

INTELLECTUAL NEW BIRTH AND LIFE-EDUCATION

CROLL'S early teachers did nothing in the way of educating the boy; and the few books in his father's possession were by no means such as to attract his attention or awaken in him any intellectual interest. Thus, up to the age of about eleven and a half years, he evinced no taste for reading, and gave absolutely no promise of his future career. His intellectual new birth had not yet taken place. That event, however, was near at hand. In the beginning of April 1832, when he was on a visit to the city of Perth, he paused one day to look into the window of a bookseller, where some of the latest productions of the press were displayed. His attention was arrested by the illustrations of a little periodical paper, chiefly, perhaps, by the picture of a brown bear walking on his hind legs along a tree which crossed a river, and carrying a dead horse in his fore paws. He entered the shop and purchased the first number of the *Penny Magazine*, which had just been established by "The Society for the Diffusion of Useful Knowledge" for the purpose of acting, like the stage-coach, "as a means of convenience and enjoyment to the people at large." Carrying his treasure home, the country boy found, in the eight large pages of the little magazine, what to him was truly a feast of reason, a feast which in him produced a flow of soul. Here he found a historical article on Charing Cross, London; articles geographical, historical, and social, on Van Diemen's Land and Poland; brief biographical sketches of René Descartes, mathematician

and metaphysician, and of Dr. William Harvey, the discoverer of the circulation of the blood; an outline of the life of the Rev. George Crabbe, with a poetical extract from his *Parish Register* on Isaac Ashford, whom he describes as "a noble peasant, a wise good man, contented to be poor"; accounts of the Wapiti (deer) and the bear in the Zoological Gardens; a quaint sermon on malt, and an article on the antiquity of beer; and, lastly, a few columns of miscellanea. The boy perused with ever deepening interest the pages of the little magazine, in which a new world seemed to be opening up to his view. He purchased the following numbers of the new periodical regularly as they appeared, and became a diligent student of its contents. In this way he became acquainted with the life story of men of many lands and ages who had risen to eminence in all departments of life; he learned something of the great works of art, ancient and modern, in painting, sculpture, and architecture, as well as of historically interesting antiquities of a different order; he gained some insight into many striking features of natural history, and he was at times carried away by romantic narratives of travel and adventure; while, at the same time, he had presented to his mind, in simple and attractive style, the elementary principles of language and numbers, along with well established facts in statistics and economy. The contents of the *Penny Magazine* were, in short, encyclopædic; and, through its volumes, young Croll gained his first notions both of natural science and of philosophy, of which he was afterwards to become so distinguished a student. So highly did he appreciate the benefits he had received from it, that, in later years, he took the trouble of procuring several odd volumes of the work, that he might be the possessor of a complete copy of the magazine from its first number down to its last.

Croll had now entered on a new epoch in his life; his intellectual interest had been aroused; and he had made a beginning in the great work of self-education. There

arose within him a great longing for a better training than he was receiving at the village school; but, alas! the means wherewithal were not to be found. He felt, however, that at any cost he must continue not only to read, but still more to extend his course. In the pages of his first tutor he had perused occasional notices of important works in various departments of literature and science; and as the magazine articles had only whetted his appetite, he determined to lay aside his occasional pence of pocket-money, and devote them to the purchase of books.

Among the first of the volumes which he succeeded in obtaining was *The Christian Philosopher; or, The Connection of Science and Philosophy with Religion*, by Dr. Thomas Dick. This work, although very far from being a "book for boys," proved very valuable to our young student in the way of stimulating his interest in lofty subjects of thought, extending his views of the world in which we live, and developing all his recently awakened powers of mind. The object of the author was to illustrate the harmony which subsists between the system of nature and the system of revelation, and to show that the manifestation of God in the material universe ought to be blended with our views of the facts and doctrines recorded in the volume of inspiration. In his first volume he treats of the natural attributes of the Deity in their relation to religion; he presents his readers with a rich variety of phenomena drawn from many fields of science in illustration of the omnipotence, wisdom, and goodness of God; and he gives a cursory but careful view of natural history, geography, geology, and astronomy in their relation to religion and Christian theology. In the second volume he deals in like fashion with natural philosophy, chemistry, anatomy, physiology, and the inventions of human art; he treats of various scriptural facts and doctrines which may be illustrated from the system of nature; and he urges the advantages to be derived from an enlarged study of science in connection with

philosophy. The effect at first produced on the mind of young Croll by his plunge into this view of science, philosophy, and philosophic facts and reasonings may be more easily imagined than described. He was utterly bewildered by the novelty and grandeur of the conceptions presented to his mind in almost measureless number and variety. With dauntless energy and tenacity of purpose, however, unaided by friend or teacher, he pursued his solitary path through the teeming pages of Dick's work, slowly mastering facts and reasonings, till order, simplicity, and beauty became manifest in what had at first seemed a mere chaos of perplexity and confusion; and now his amazement and bewilderment gave place to admiration and delight. Strange to say, what chiefly charmed the boy, even in his earliest scientific reading, was not the miscellaneous array of striking phenomena in all departments of nature drawn up before him for inspection, but the general laws or principles which underlay these phenomena, and gave them order and beauty. Any novel phenomenon which was presented to his mind suggested irresistibly the question How? and the statement of the general law to which this phenomenon could be referred was usually followed by another How? Croll could find no rest for his mind except in fundamental principles. Thus physical astronomy (the only branch of astronomy which he ever studied), while it deeply interested him, failed to give him any real satisfaction on his first acquaintance with it. He was ignorant of the mathematical and mechanical principles on which it depends.

Croll accordingly determined to set about the study of science in systematic fashion, impelled by the difficulties he had found in his perusal of Dick's *Christian Philosopher*, of which he had probably read only the first volume. He purchased one or two books which seemed likely to prove helpful to him, and among them was Joyce's *Scientific Dialogues*. In this valuable work, which for many years aided greatly in extending a knowledge of

physical science among the young, he found a real treasure. An exposition of the first principles of experimental philosophy prepared for the instruction and entertainment of young people, it was drawn up in the form of conversations between a father and his children, and carefully adapted to the capacities of those of ten or eleven years of age. To the youth who had fearlessly, and with some success, attacked the grim pages of Dick's bulky, close-wrought volumes, those of Joyce's slenderer work promised a speedy and triumphant victory. He positively revelled in the perusal of the book. The romance of science was that which early fascinated his mind and kindled his imagination. Swiftly laying hold of the mass of interesting facts laid out before him, he passed with avidity to the mastery of the general principles which underlay them all and gave them meaning and value in his view. To the details of the construction of philosophical instruments, such as the air-pump or the electrical machine, he paid little or no attention, except in so far as they illustrated the laws of pneumatics or electricity according to which these instruments operated. The details excited his interest only for a moment; as soon as the general laws or principles were firmly grasped, they ceased to charm, and largely passed away from his mind. The thorough mastery of principles to which he set himself, and by means of which he was enabled easily to recall facts and details when necessary, gave the young student an enormous advantage in the prosecution of his work. He was able to advance by leaps and bounds without the assistance of any friend or teacher, where the progress of others who enjoy such assistance is usually very slow. In this way, in the course of some four or five years from the day on which he first opened the *Penny Magazine*, namely, by the time he was about sixteen years old, he had gained a pretty tolerable knowledge of the general principles of mechanics, pneumatics, hydrostatics, heat, light, electricity, and magnetism.

Two very important and, to most people, very interesting branches of science utterly failed to attract young Croll. The fact that they are so largely sciences of observation and experiment, which gives them their interest in the minds of most people, was the means of repelling him. For chemistry and geology, "more particularly the latter," he had no relish. They appeared to him to be so largely made up of mere facts and details, so deficient in rational principles; they seemed to be so lacking in the philosophic method and material which were to him as the very breath of life, that he positively shrank from them. Had any one told him, he says, in his early years, that he would one day become a professional geologist, he would have regarded the statement as incredible. In fact, it was largely by what men call *accident*, more by the constraint of others than by his own personal choice, that in later years he entered the office of the Geological Survey. If we except one department of geology, to which we shall by and by have occasion to refer, it was almost the only science to the study of which he never devoted a single day. He never became, never cared to become, a geologist, in the ordinary sense of the term, even although he became a member of the Geological Survey staff. Nevertheless, his acceptance of a post on that staff proved of immense advantage to him in the pursuit of the climatological studies which made him famous, as it afforded him a comparatively easy means of gaining an acquaintance with geological phenomena, phenomena of which, but for that appointment, he would most probably have remained ignorant; and without the knowledge of which his important work would have been but very imperfectly accomplished.

Thus, in the quiet seclusion of the little village of Wolfhill, passed away the years of Croll's boyhood. They were formative years; by the sacred influences of his parents' pious home, his naturally religious and meditative soul was unconsciously educated; while,

from the pages of the *Penny Magazine*, Dick's *Christian Philosopher*, Joyce's *Scientific Dialogues*, and a few other books to which he was mysteriously guided, he received intellectual impressions and impulses from the influence of which he never escaped. His mental being was stirred to its very depths by the grandeur of the conceptions presented to him in these works; and he received from them an impetus towards the pursuit of truth in natural science and in mental philosophy which remained with him to the end.[1] Indeed, no grand physical conceptions which he ever acquired in later years made such an impression on his mind as those of this early date; excepting, perhaps, those relating to the modern science of energy,—its transformation and its conservation, and to the dynamical theory and the mechanical equivalent of heat. Thus the moral character of the man was determined, his intellectual bent was assigned, and his whole course in life was largely influenced.

For a period of nearly three years, from the age of about fourteen, he was almost wholly engaged during the daytime in the agricultural labour demanded by the culture of his father's croft.

[1] See solitary musing, described on p. 18.

CHAPTER III

CHOICE OF A TRADE

AFTER Croll had been labouring on the croft for two or three years, it became evident to his father and mother, as well as to himself, that some occupation better than that of a crofter must be found for him. What that occupation should be proved a perplexing question to all. Croll himself had, since the "reading fit" came on him, become imbued with an ardent thirst for knowledge and a desire for more education. He had been quietly but perseveringly trying to satisfy the thirst and the desire, and had succeeded only in enormously stimulating them. Accordingly, now, when about to take his first step into the world, he felt the strongest desire to receive the benefit of a university education. The satisfaction of this desire unfortunately proved altogether impossible. His father was too poor to support the lad during the usual four years of a university curriculum, and he had no relations rich enough to render any assistance. In those days the bursaries open to poor but promising students at our Scottish Universities were neither very numerous nor very large; but even had they been more numerous, Croll had not proved himself one of those brilliant boys who, by diligent study at the parish school, have always, in Scotland, been able to enjoy a university education at very small cost to parents or friends. The nearest university was St. Andrews, which was about forty miles distant from Wolfhill; and although several bursaries are annually awarded there to clever lads, these were

altogether beyond the reach of Croll, who had enjoyed no systematic training in even the elements of mathematics or the Latin or Greek languages. His longing desire for a university education had thus to be stifled, and the project given up as impracticable and unattainable. What else, then, could the lad do? He had no special aptitude or desire for any particular trade; but to trade he must go, since his irregular and imperfect education shut him out from the professions. After pondering anxiously over the matter for several days and consulting his friends, he came to the conclusion that, as he had made some study of the theory of mechanics, he might find the work of a millwright one in which his study would be of some service to him. Accordingly, it was resolved that he should be apprenticed to a millwright; and in the village of Collace he entered upon this trade. He himself says little of the experiences of the years of his apprenticeship; and we have not been able to ascertain anything further than what is recorded in the autobiography.

On the completion of his apprenticeship, he left Collace and went to work as a journeyman with the firm of Martin & Robertson, millwrights at Banchory, Coupar-Angus. The business of this firm was chiefly that of making and repairing threshing-mills throughout the "Howe" of Strathmore and the surrounding districts. They usually employed some four or five men at this work, whose wages were by no means large, about eight shillings per week together with food, which was usually of the poorest description possible. The repairs on the corn-saw and threshing-mills had usually to be executed at the different farms where the mills were erected; and this, of course, caused the men to be constantly shifting about from place to place, so that they scarcely ever spent more than a day or two at one place. In those days there were no railways in the Howe of Strathmore; and the men were frequently compelled to walk long distances on foot,—sometimes

of thirty or forty miles a day,—in order to fulfil their engagements. With his usual dry humour Croll says quietly, "It was on the whole rather a rough life." The millwright was looked on as somewhat of the nature of a tramp; and he had generally to rest in the ploughman's "bothy," which, in those days, was a very rough outhouse belonging to the farm, consisting of little more than the four bare walls and containing several beds, a table, and a few chairs or forms. When the bothy was full, the poor millwright had to betake himself to the barn or the stable-loft above the horses, where he had to bury himself under the clothes, generally a few sacks, to protect himself from the rats.

Croll endured these hardships very patiently for a period of some five or six years; but he gradually came to see that this trade was wholly unsuited to the development of either his mind or his body. His experiences during this period, however, did not fail to leave their marks upon his constitution. Long after, in consequence of the excessively long walks he had to perform, his feet were so grown over with corns, that he was obliged to cut holes in his shoes, and allow the corns to grow out without cutting or paring them. During all these years of trial, Croll performed his arduous duties without a grumble, while, according to the testimony of those who knew him then, "his moral character and daily deportment were most exemplary and in every way commendable." The Rev. Mr. Bruce, Free Church minister, Rhynie, writes of him at this period, "I never heard a complaint brought against him by any one. But, on the contrary, everybody spoke well of him, and had the highest respect for him. To me, who was a few years his junior, there always seemed something so modest and unaffected about him which naturally drew my affections towards him and reverence for him."

Croll abandoned the millwright trade when about twenty-two years of age, and returned to the village of

Collace for a season. He had probably saved a little money out of his hard-earned wages, for we find that, the insatiable craving for education having attacked him again, he went to the parish school of St. Martins for a winter to study algebra. The sight of a grown-up, grave-looking working man attending the parish school for "counting," as the schoolboys called it, was a source of wonder and amusement to the lads. Yet cases like this were to be seen in many of the better schools in Scotland, both in town and country, and the younger pupils were rarely rude to such men. During this winter, doubtless, Croll lent a helping hand to his parents in the working of the croft, and made himself generally useful at home. At the opening of the summer, however, he was obliged to resume work, as his little hoard of savings had been almost wholly exhausted, and he had again to earn his livelihood. He accordingly sought and obtained employment as a joiner. His new trade he readily learned so well as to become a very efficient tradesman, and he found it much more suited to his taste, as he did not require to travel from place to place so much, while the life in general was by no means so hard.

The first big job on which Croll was engaged as a joiner was the erection of the Free Church at Kinrossie, in the parish of Collace, of which the Rev. Andrew A. Bonar (afterwards D.D., of Glasgow) was then minister. Collace was some two or three miles distant from Wolfhill; the joiners began work at 6 A.M. and ceased about 6 P.M.; and as it seemed too much for the young man, with already injured feet, to do a hard day's work and walk such a distance both morning and evening, Croll's father thought he should reside at Collace. The kindly old man accordingly called on some of his friends in Collace with this object in view, and one of them agreed "to make way for him among the young folks." This villager is still alive, and writes:

"So James came next morning, and continued with

us all the time the building was going on. In a few days after this, Mr. Andrew Bonar, now Dr. Bonar of Finniston, who was our minister, then called at our house in his ordinary course, and in course of ordinary conversation he says, 'By the bye, haven't you James Croll staying with you.' We said we had. 'Then,' said he, 'how do you get on with James?' I said, 'Remarkably well; I think he is one of the nicest young men I have ever met with.' 'But,' says Mr. Bonar, 'how do you get on with him in your conversations?' I said, 'Remarkably well, so far as I can follow him, but sometimes, when I think the subject of conversation is made clear enough, and no more can be made of it, James is not satisfied; he has some ulterior view—something beyond in his eye that I am not able to follow him.' Mr. Bonar laughed most heartily and said, 'I don't wonder at it, Andrew, I don't wonder at it. Do you know that James has a very striking metaphysical cast of mind?'"

The new Free Church was the church being erected at the time of the Disruption in 1843. It appears that Mr. Bonar, an able and zealous minister, was then in the habit of gathering together a number of grown-up young men in his house on the Sunday evenings, after the services of the day were over, for spiritual instruction. Croll took advantage of this class, and speedily attracted the attention of the minister, who formed a high opinion of him. Writing in 1891, Dr. Bonar says: "It is long since I met James Croll, though in earlier days I knew him well. He lived at that time in the village of Wolfhill, parish of Cargill, not far from where my lot was cast. He was known among us as a young disciple who showed a great inclination to philosophical study, and he was much esteemed."

After the Free Church at Kinrossie was completed, there happened to be a scarcity of building work in the neighbourhood, and in the summer of 1844, Croll removed to Glasgow, where he soon found employment. He remained in Glasgow for only a few months, removing

then to Paisley, where he stayed for about a year. He liked this place and his employment. About this time a controversy arose in theological circles between Calvinists and Arminians. Croll took a deep interest in this controversy, and joined the Arminians, then called Morisonians after Dr. Morison, who was the leader of the new movement. He attached himself to a number of the followers of Dr. Morison, who were endeavouring to found a church in Paisley under the preaching first of Mr., afterwards Dr., Landels, now of Edinburgh, and then of the Rev. Mr. A. M. Wilson, afterwards of Bathgate. He took a warm interest in the formation of this church, and was elected a deacon, but he did not enter upon office, as he was obliged to leave the town before he was called upon to act.

When Croll was working at his trade in Paisley, in the spring of 1846, the elbow joint of his left arm became so seriously inflamed, that he felt compelled to consult his medical adviser, who told him that he must abandon his trade as a joiner, and adopt some easier occupation, which would not necessitate so much physical exertion. It appears that this ailment had its origin in a boil, which appeared on his arm when he was a boy of about ten or eleven. Unfortunately, it was accidentally knocked against the corner of a door, and it proved very troublesome both during the healing up and afterwards, and continued to afflict him more or less every spring for several years. There was nothing for it but to return home and rest for a season, consider his position, and decide what should be done in the future. Accordingly, he left Paisley, and returned to his father's house at Wolfhill. There he remained for some time, reading and studying diligently, till the arm got better; but unfortunately it never recovered sufficiently to allow him to use it freely, as the joint ultimately ossified, and Croll suffered ever afterwards from a stiff elbow.

He was now in a greater dilemma than ever. The joiner business, or, indeed, any kind of trade, was out of

the question. He had neither the education nor the training to fit him for a clerkship, and, moreover, he had no aptitude or inclination for such an occupation. He thought he might get some kind of employment in the tea trade; and accordingly one day walked to the city of Perth, a distance of about eight miles, to make inquiries. The story of his selection and adoption of this trade is so romantic and interesting, that, although it involves repetition, it can be told only in his own words: "Musing over the matter, as I approached the city by the bridge, I observed a man distributing small handbills to the passers-by. All in a moment it struck me that if these bills should relate to the tea trade, I would be guided by this, and would go to the shop to which they referred; at least, before trying any other. What could induce me to come to a conclusion so apparently absurd and incautious, I cannot tell. Strange are the ways of Providence! for had it not been for that decision, in all probability, my future course in life would have been very different from what it actually turned out to be. On coming up to the man, I found the bills related to a tea and coffee warehouse which had recently been opened in the High Street of Perth. Guided by the bill, I went direct to the shop, and found the proprietor to be an agreeable and intelligent person. After talking over various matters, I then told him what I had been thinking about. He agreed with me that I might manage to make a comfortable livelihood by selling tea; and that I might push the sale by going into the country. I accordingly got a small stock and commenced operations. I soon found, however, that the attempt to push a sale in the country was a rather disagreeable job for me, and I resolved to give it up. The merchant whom I had visited—Mr. David Irons, who afterwards proved to be one of the kindest friends I have ever met with in life—now proposed to me that I might try and open a shop for myself in some suitable town, where I might be likely to succeed. Unfortunately I had not the means for any such

undertaking; but he, in the most kindly manner, offered to assist me. He agreed to give me a stock to commence with, and that I should repay him in regular instalments as it was sold; and that he would in this way keep up my stock. I need hardly say that an offer so generous was readily accepted. As it was now about the end of the harvest season, my friend suggested that I might come to Perth for the winter, and learn the mechanical art of weighing and parcelling up the tea, serving over the counter, and all the usual routine of shop work. I accordingly came in, and before the winter was over, I became a thoroughly proficient shopkeeper."

Croll, as "a thoroughly proficient shopkeeper, learned in the mechanical art of weighing and parcelling up the tea, serving over the counter, and all the routine of shop work," is a dream which existed only as a picture in his own imagination, arising largely out of gratitude for a kind act, and thankfulness for a tranquil period of his life. The reality was very different from the picture. The primary qualification for a shopkeeper is an affable, agreeable, and, as some would say, almost obsequious manner. Croll had, to the day of his death, a modest, shy, dry, and almost speechless manner, except on occasions when he was drawn out by congenial conversation among real friends. A second requisite is an active and attractive appearance, alertness and energy of body. Croll was heavy and ungainly in appearance, solid, sound as a rock, true as steel, but somewhat slow and awkward in manner and appearance. A third requisite is adroitness in "serving over the counter." Croll never was very adroit either in mind or body, and could and did serve only with his natural reserve and shyness. A fourth requisite is rapidity and neatness in the mechanical art of weighing and parcelling up the tea, and in the general routine of shop work. Croll never was either rapid or neat in any mechanical work he performed; and with his hands and arms trained only to the hard work of a millwright or joiner, as well

as the awkwardness and inaptitude caused by the weakness and stiffness of his elbow, it can easily be seen that he could not be a proficient by any means in the "mechanical art of weighing and parcelling up the tea." His appearance behind the counter is well described by an eye-witness: "It was something altogether extraordinary to see the man, with his large head, massive forehead, and kindly countenance, with his heavy form of body, hard horny hands and stiff arm, standing behind the counter of a tea-shop. One is accustomed to see rather a small thin man with thin nimble fingers and active arms discharging this duty; and no one, even the most casual observer, could see Croll in the character of shopkeeper at this time without knowing that he was not a shopkeeper to the manner born, and that he was evidently in a new sphere."

During the time he was in Perth, however, he was happy. His friend, Mr. David Irons, was a kind, intelligent, well-read man, who took a deep interest in him, had many conversations with him on religious matters, and encouraged him in all that pertained to his spiritual and temporal welfare. As he had gone to Perth only to learn the tea trade, it now became necessary to fix on a place in which he might begin business. After looking about a while, he thought that there was an opening in Elgin for a tea merchant. Accordingly, that place was fixed upon, and he went north in the spring of 1847, and opened a tea-shop there. For a time he was comparatively successful in business, and liked the place and the people well. Instead, however, of developing the social side of his life, and making friends with the people, he became a hard philosophical student. He got hold of the great work of Jonathan Edwards, on the Freedom of the Will, which he read and re-read, both in season and out of season. Tappan's book he also tackled; but, like many other readers of this work, he was greatly disappointed. He found, in common with most philosophical students, that Tappan missed the point of

Edwards' argument altogether, and failed to grapple with the real difficulty of the problem. He never discovered any satisfactory answer to Edwards' argument, and "became convinced that some moderate form of Calvinism was nearest the truth, not only of philosophy, but also of Scripture."

Here, in Elgin, it was that Croll first formed the opinion which he matured and developed at intervals from that time to his death, namely, that "the entire universe is a process of determinations; but not of determinations occurring at random. There are a unity, a plan, and a purpose pervading the whole which imply thought and intelligence." Again and again he returned to this thought, he brooded over it, and wrote upon it in magazines and pamphlets, till it reached its final form in his last work.

On the 11th of September 1848, he was married to Isabella, second daughter of Mr. John Macdonald, Forres. It is a curious coincidence that Croll's mother belonged to Elgin, and his wife to the immediately adjoining town of Forres. In his own simple, manly way, he writes briefly on his married life, saying only: "The union has proved a happy one. She has been the sharer of my joys, sorrows, and trials (and these have not been few) for the past forty years. Her care, economy, and kindly attention to my comfort during the years of comparative hardships through which we have passed, have cheered me on during all my trials and sorrows." Croll was a man of few words, but a better deserved, a truer tribute of gratitude and respect to the partner of his life, never was penned by any man.

Like many more bachelors in their solitude, Croll had formed a fond liking for the fragrant weed, and, for some time, indulged this taste to such an extent as to produce the inevitable result of excessive smoking—a dyspeptic affection. He tried to give up the habit several times, but failed. At last, getting one or two friends to join him, he made a written pledge, on the

29th of December 1849, to abandon the habit; and from that date up to the end, he adhered strictly to his pledge. This little incident gives the keynote to the character of the man. Nothing could deter him from the accomplishment of what he deliberately resolved upon and undertook. "I had a terrible struggle with the appetite. For two or three months I was in a state of partial stupor; and it was nearly three years before the craving for tobacco left me." But, having once set his hand to the plough, he scorned the very idea of turning back.

In this connection it may be mentioned that he had been a pledged abstainer from intoxicating liquor for several years. His parents had never been in the habit of using intoxicating liquors, which, indeed, were a luxury for which they had no need, no funds, and no inclination. Croll had, therefore, been practically an abstainer from infancy; but when he came to the years of maturity, he gave in his adhesion to total abstinence as a matter of principle. During the time he was in Elgin he acted as secretary of the Temperance Society there.

In the course of his philosophical and religious reading, Croll had become acquainted with the writings of the Rev. James (afterwards Dr.) Morison, of Kilmarnock; and, about the year 1848 he opened up a correspondence with this eminent theologian, which continued at intervals to the time of his death. A letter from Croll to Dr. Morison, dated 24th November 1849, gives a vivid glimpse of the man as he then was.

ELGIN, *24th November* 1849.

MR. MORISON—DEAR SIR,—I duly received your kind letter, and am much obliged to you for its contents, all the more so when I consider how little time you must have for so particular details such as it contains. You express at the end of your note a wish to know something concerning me. This I am most happy to do, though I am sure that, when you know it, it will be of little service to you. I am twenty-

eight years and married, but have no family. My parents dwell near St. Martins, Perthshire. I was a wright to trade, but four or five years ago I was obliged to give up my trade on account of a sore arm, and through the kindness of a few friends, particularly Mr. John Lister, I was enabled to commence business here about three years ago as a grocer. When young, I got a good many years at school, but, I am sorry to say, made little or no progress, having always had a perfect hatred to school. My deficiency in spelling, writing, grammar, has been a great loss to me in after years. When I was about twelve years old, I happened to fall in with a book upon theoretical astronomy which perfectly fascinated me, and in order to get a knowledge of that subject, I commenced the study of mechanics and mathematics, and pursued eagerly this subject for six or seven years, as far as time would permit me, to the neglect of everything else but what I was obliged to do. But being brought under deep religious impressions, I abandoned them altogether, and afterwards studiously avoided them, knowing the danger I was then in of being led away by them if I commenced again. I was brought to the truth under Mr. Bonar, Collace, and about that time your works on the atonement happened to come my way. The reasoning contained in them was so forcible that I could not resist it, and I became an advocate of your views, and eagerly read all the books written on that subject by you and others who were thus cast out. I was in Paisley when the church was formed there, and left that place a little after Mr. Wilson came. Some years ago I fell in with Lord Kames on Liberty and Necessity, and Edwards on the Will. That latter book perfectly astonished me. I studied it over and over again, till I got completely master of it. I saw that Calvinism was a subject that was not so easily got rid of, which set me in earnest to fathom the *mystery* how to get quit of "awful necessity," in order to get a comprehensive view

of the subject. I commenced the study of the philosophy of mind, and read Brown, Reid, and some of D. Stewart on that subject; with Combe, Spurzheim, Smith, and others on the phrenological view of the subject. So, instead of having got satisfaction, I am in no hope that for years to come will I be so clear upon that subject as I would fain wish and expect, though I am perfectly satisfied that liberty is right. There is one comfort, however, that the doctrine of necessity (philosophical) is hid in mist and metaphysics, so that few can see it so as to believe it and act upon the belief. But I must stop, for I have by this time wearied you with what will be of little interest to you.—I am, dear sir, your obedient servant,

JAMES CROLL.

The arm to which Croll refers in this letter began about this time to give him more trouble. The elbow joint was again attacked by inflammation; the effect of which, on this occasion, was to completely destroy the joint and render it stiff and immovable. This was a sore trial to him; but it stopped further inflammation of the elbow or trouble with the arm, and he afterwards enjoyed better general health. This illness unfitted him for some time for attending properly to the business of his shop, in consequence of which the trade fell off, and he was never able to restore the business. He tried hard to regain his lost footing, but in vain; and after a while, finding that he was only losing money, he, in dread of falling into debt, realised his business, paid his debts, closed the shop, and left Elgin forthwith.

CHAPTER IV

RETURN TO PERTH IN 1850

IN the beginning of the summer of 1850, Croll, having left Elgin for good, returned to the city of Perth. He had not, however, quite regained health after his illness at Elgin, and it was a considerable time till he recovered so fully as to be able to do any manual work. At this time the effect of electricity and galvanism for medical purposes had begun to attract attention, and Croll now applied himself to the study of this subject. He had long ago studied electricity in its theoretical aspect, and was familiar with the principles upon which electrical machines were necessarily constructed. It was comparatively easy for him, therefore, to understand upon what principle an electric or galvanic battery must act. It was not so easy, however, for a disabled joiner or tea merchant to construct such a machine. Nothing possible, however, daunted Croll. Dire necessity drove him to do something to earn a livelihood for himself and his wife; and so he applied himself to the construction of induction apparatus. These machines proved thoroughly well made, complete in all respects, and well adapted to the end in view. The writer had one of them in his possession for several years, which he only parted with to Croll himself in later years; and as he subsequently found out, it was given away to a poor person who could not afford to buy one. He continued the making of these machines for some time; but, within the comparatively limited area of Perth and Dundee, the demand for them was very small, and soon became

exhausted. With what he made in connection with these machines he managed, through the economy of his wife, to exist for about a year, during which he succeeded in reading a good many of the writers of the Scottish philosophical school.

The inevitable "bread and butter" problem, however, again presented itself. The demand for electrical energy had been dissipated by Croll's industry, and no force he could exert could conserve or create a demand which had been fairly exhausted by supply. He had to earn a livelihood for himself and wife, and what could he do? With a good deal of Micawber-like philosophy he looked for something to turn up. What did turn up would to any one but a "philosopher" have seemed a most impracticable scheme. We have it in his own words: "Some of my friends suggested that I should try a temperance hotel, and one of them stated that he was about to erect a house at Blairgowrie, and that I could have it for that purpose if I chose. After due consideration I made up my mind to try that course." There is an honest, innocent simplicity about this, which cannot fail to bring a sad smile over a business man's countenance. That a man and wife with no experience of hotel business—and very little experience of business of any kind—should start a hotel, and that a temperance hotel! Was anything more unpractical ever attempted? But that is not all. Where was this temperance hotel to be? In Blairgowrie, a village of some 3500 inhabitants, with already one hotel and fifteen public-houses or small inns in the place. Most people in such circumstances would have seen that his "friend," the builder, was looking rather to get an honest tenant for his new house than to secure a livelihood for Croll and his wife. But the impracticability of the scheme did not end there; for, as Croll says, "here was the difficulty: the house required to be furnished, and this would require a considerable sum, which I had not. It occurred to me, however, that as it would be some six or eight months

before the house would be ready for occupation, and as my arm was now much improved, I might try and make a considerable number of the necessary articles before that time." Accordingly, the brave man set to work, made the most of the furniture necessary for the hotel with his own hands, and succeeded in getting the house opened in the beginning of 1852.

As was to be expected in a small place like Blairgowrie, where there were no railways, the visitors to the temperance hotel were few and far between, and Croll and his wife were unable to make a livelihood out of the concern. He says, "Although Mrs. Croll had too much work, I, on the contrary, unfortunately had too little." With the practical wisdom of a "philosopher," he sought work in the form of endeavouring to learn Latin under the assistant teacher of the parish school. After about a year's hard labour, he acquired a knowledge of the rudiments of the language; but, finding that it would need another year to enable him to read Latin, he abandoned the thing altogether.

The temperance hotel having proved a failure after a year and a half's trial, Croll gave up the business, sold off the furniture, and left the place. He left Blairgowrie at the May term of 1853, and removed to Glasgow, where he got an engagement as an out-door canvasser for the Safety Insurance Co. This company was under the directorship of Richard Cobden, John Bright, Henry E. Gurney, Thomas Brassey, and others, whose names were a great recommendation to the company. For some time Croll was wonderfully successful as an insurance canvasser, particularly amongst the working men; but as cholera found its way to Glasgow about this date, the directors felt that it would be unwise to push the business, and consequently Croll's services were not long required. He accordingly returned again to Perth, where, through the same friend who introduced him to the tea trade, he was introduced to Dr. Robert D. Thomson, chemist, one of the directors of the Temper-

ance Provident Institution, who was then on a visit to the city of Perth. Through him Croll was offered an agency in Dundee, and he was asked to devote his whole time to the work.

He accordingly removed to Dundee in August 1834, and began his canvassing. Here, again, he proved very successful in this uphill work. As we have already seen, Croll was very systematic in his work, and had an indomitable amount of perseverance. His method was something like this. He selected a district and sent out circulars with a prospectus containing table of rates and other information. He then called on the people to whom the circulars had been sent. Frequently he received only a cool reception from one out of thirty or forty whom he circularised and called for. When he did get this, he carefully explained the principles of insurance, the advantages of the company he represented, and tried to press the duty of insuring home to the person. Many times he had to make three or four calls on a man before he got a "proposal"; and when this was got, the sum to be insured was generally of a very limited amount. The writer has been with him on several of these canvassing visits, and could not but admire Croll's patient and persevering plan of pushing the business. His manner was quiet, earnest, and convincing. He was very serious and deliberate in the matter, he had a remarkable knowledge of the advantages of life insurance, and he readily and successfully met objections when stated. The directors of the Safety Society had not lost sight of such a serviceable man; by and by they offered him their agency in Edinburgh on much better terms than he had from the Provident in Dundee, and he at once accepted the offer. So he left Dundee in the month of May 1855, and removed to Edinburgh. He found, however, that this city was the home of insurance, and that it was much more difficult to get "proposals" there than in Dundee. Besides, the "Safety" was an English office, and although

the names of the directors were a source of strength, it was a comparatively new office, and had not any exceptional advantages to offer over those of old established Scottish offices which had their chief places of business, with resident directors and local influence, in the city.

In the spring of 1856, his father, David Croll, died at the paternal home in Wolfhill, at the good old age of seventy-five, having passed the allotted span of threescore years and ten, and done an honest life's hard work.

About this time took place the failure of the Western Bank of Scotland, which threw thousands who were in comparative comfort into penury, and seriously affected commercial enterprise. Nothing so prejudicially affects insurance business as a disaster like this, and Croll as well as all other insurance agents felt this keenly. He could not prosecute insurance business to any extent in the evenings; and, accordingly, after his work during the day was finished, he utilised his spare hours in the prosecution of his favourite study, philosophy. Now he began his study of Kant, perhaps the profoundest and most original thinker of modern times, of whom he says: "With the exception of Edwards, no writer has made such an impression on my mind as Kant." No wonder that this great philosopher should produce such an impression on a purely philosophical mind; the marvel would have been had it been otherwise. The remarkable thing is that he should link with this giant of modern thought the name of Edwards. But it has to be borne in mind that it was Edwards who shone out as a sun on the darkness of his early mind, who appeared as a light at the early dawn of his philosophic life, quickened all his intellectual powers, and cleared to a large extent his pathway for future study and investigation. The persistent study of philosophy in which Croll was now engaged in his evening hours soon began to strain his eyesight seriously, and brought on an ocular affection which was accompanied by considerable pain. Since

infancy he had been more or less subject to a pain at the opening of the head, which seemed now to transfer itself to the eyes. He therefore resorted to a plan of reading with the aid of a piece of plain coloured glass placed on the book; and this had the effect of mitigating the pain in the eyes. It continued, however, to afflict him not a little for several years, but he went on reading with his coloured glass despite the pain.

In 1856 the Directors of the Safety Insurance Company, finding that not much progress was being made by their office in the city of Edinburgh, thought that Croll might be tried in a more industrial centre; and, accordingly, they asked him to go to Leicester, where there was a large working-class population. One of the directors happened to be a member of Parliament for the town at the time, and it was believed that his name and influence would work a charm on intending insurers. Unfortunately, "the people of Leicester seemed to think that one of the old-established Scotch offices was fully as safe as the Safety." Notwithstanding all Croll's patience and perseverance, very few proposals could be got for the Safety; and the months spent in Leicester proved one of the hardest and most trying periods of his life.

After being about six months in Leicester, Mrs. Croll became seriously ill, and the medical men consulted recommended her to leave the place. This she did as soon as she was able to be removed, and Croll returned with her to Glasgow. There she lay ill for about a year, but, under the careful nursing of her sister and the assiduous attention of her husband, she at length recovered.

Of course, Croll could not leave his wife in her delicate state of health to go back to Leicester; and indeed there was not much inducement to return. He accordingly bade that town good-bye, and as the Safety Company had already tried Scotland with too little success to warrant their having an agent in Glasgow, he left their employment. He, however, got an engagement

readily from the Temperance Provident Institution, for which he had formerly done good work. Paisley was the place selected for his operations; but the "Paisley people" have always been rather difficult to deal with, and, as a shrewd observer of human nature once remarked, it is necessary "to keep your eye on Paisley." After six or eight months' trial of insurance canvassing in this old weaving town, Croll was obliged to give it up as a hopeless task; and he "then formally abandoned the insurance business altogether, after spending [in it] four and a half years of about the most disagreeable part of my life." Thus closed Croll's career as an insurance canvasser. Any one who knew the man can only marvel that he continued in it so long and proved comparatively so successful. To the ordinary observer a more unlikely man for an insurance canvasser could hardly be imagined than Croll. As he says, "To one like me, naturally so fond of retirement and even of solitude, it was painful having constantly to make up to strangers." Yet how bravely he struggled on in spite of his dislike of the work, and his constitutional inaptitude for it! His indomitable perseverance and manly independence alone made him a fairly successful agent despite these drawbacks; and it need only be added here that he earned the respect and confidence of the directors of both institutions in which he served in this capacity, leaving the Temperance Institution to the regret of all those connected with its management.

CHAPTER V

LITERARY WORK

HAVING left Paisley, Croll returned to Glasgow in 1857, but for some time failed to obtain any remunerative employment. He could not be idle, so he commenced to bring together some thoughts on the Metaphysics of Theism. On this subject he had been thinking seriously for several years, and the following remarks by an able writer in the *Christian News* are interesting in this connection:—"We met Mr. Croll first about the year 1854 in Glasgow. He was then deeply interested in theology and philosophy. We foregathered in an old book-shop, and had long talks over the doctrine of the Will as explained by Edwards, and other kindred topics which then occupied his attention. At that time he called himself a 'moderate Calvinist,' and under that designation published a pamphlet on Predestination. It was pronounced by the Rev. Dr. Morison, no mean judge, 'an extraordinary production. He also about that time published a pamphlet on the bearings of geology and astronomy on the creation of the world. Dr. Croll took a special interest in the doctrine of the divine existence, and his first volume of any size and pretension was *The Philosophy of Theism.* This is a thoughtful work, and displays not a little philosophical insight and acumen. It was eagerly discussed by a knot of students who used to meet with the author in the old book store. Tappan's works on the Will and Cousin's History of Philosophy were in the hands at that time of not a few who met with Dr. Croll.

The discussions and talk were always of the most friendly character,—though there were few tougher opponents than the author of *The Philosophy of Theism*. We think we see him still, calmly laying down his propositions and pressing home his arguments. If some of the more impetuous youths would break in with a word, he would listen to it, and then with outstretched hand would resume the thread of his argument and go on to the end. In these days he was highly respected, honoured, and loved, felt to be a master of philosophic thought, and the possessor of more than ordinary mental power. He was a devoted student of Hitchcock of the United States, and intended to republish his works in this country. The prospectus of his *Psychology* was issued, but the work never appeared. All the time, Dr. Croll was studying the great scientific problems which engaged the time and thought of savants. He revelled in these studies till he mastered many of them, and advanced in certain directions further than any of his contemporaries."

The Philosophy of Theism did not make its appearance so easily as is indicated by the foregoing sketch. It seems to have been rapidly composed, probably in three or four months, but it was the result of many years' hard reading and earnest thinking. At first, Croll intended merely to write a few columns on the Dependence of Theism on Metaphysics for a local newspaper. He soon found, however, that in the short space allotted for such an article, nothing like justice could be done to the subject. He therefore abandoned his original purpose, and continued writing until he had produced a volume. After it was written, he was at a loss what to do with it. He had not the means to publish it at his own cost, and publishers were chary of a work on such a peculiar subject, which must necessarily have only a limited circulation. He tried several publishers, who, though all satisfied of the merit of the work, were not prepared to run the risk of publishing a work on such a subject by an unknown author.

At last a firm agreed to take the risk of publishing on the system of "half profits" after the expenses were paid. Only five hundred copies were printed, and the book appeared anonymously. It was favourably criticised by the press, though Croll himself says it "attracted but little general attention." The subject was one, however, not calculated to attract much general attention, and it is a very high tribute to the intrinsic merit of the work that an anonymous book on an abstruse subject should have received the notice it did, and that it sold to such an extent. It not only paid all expenses, but left something over to divide as profit. What that was we have not been able to ascertain definitely, but when the publishers remitted to Croll the sum of ten pounds as a first instalment, he felt much gratified.

The aim of the work is stated in the preface as follows, "The direct object of the work is not to prove the existence of God, but to investigate the *method* to be pursued, in order to arrive at a proof of his existence.

"In the first part we have attempted to show that a purely *à priori* or a purely *à posteriori* proof of the existence of God is impossible. We cannot, on the one hand, arrive at a proof by means of *à priori* elements alone without experience; neither can we, on the other, by means of *experience*, without *à priori* elements. The only possible way, then, is by a method which combines both. We have thus two elements in the proof, objects or *facts of experience*, and *à priori principles*. But before we can legitimately use these principles in our proof, in opposition to the atheist, we must first establish their validity. This we cannot do without having recourse to metaphysics. But here a formidable difficulty meets us at the outset; for metaphysics itself is a science, the validity of which few atheists will acknowledge. And, to add to our difficulty, theists themselves have generally misunderstood or underrated this science. We are therefore necessitated to enter into a vindication of metaphysics, which forms Part II. We find that the chief objection

urged against metaphysics, is the fact of its present imperfection when compared with mathematics and the natural sciences. We are then led into an examination of the essential difference between metaphysics and mathematics, in order to show that, from the very nature of metaphysics, it must succeed mathematics, and that its present imperfect state is no proof whatever of any essential defect in its nature. After this, we are prepared to enter into the third, and last, part of the work, a discussion of the method of proof. But before proceeding far, we find that we must have recourse to the principle of causality, and here, again, another difficulty meets us; for this principle is in about as unsettled a state as metaphysics itself, and we are then led into a long discussion, in order to fix precisely its nature and import; after which, all that remains is simply the exposition of the method of proof."

The Rev. Dr. Morison, of Glasgow, writes regarding this volume: "I wrote a critique upon it for the *Evangelical Repository*, December 1857. In his volume Dr. Croll says: 'I affirm that it is an absolute impossibility, a thing which never was, is, or can be, namely, that the will should determine its own acts.'

"I on my part affirmed and still affirm that the will is self-determining. Dr. Croll held by Jonathan Edwards. I on my part held more by Tappan than Edwards. My criticism was severe, but it did not break the friendship that subsisted between us, and which, I am happy to say, continued unabated to the close of his life. The longer I knew Dr. Croll, the more I admired him." The truth of the foregoing observation regarding the severity of the criticism not disturbing their friendly relations is well illustrated by the following letter:—

21 NEW SNEDDON STREET, PAISLEY,
29th October 1857.

REV. JAMES MORISON — DEAR SIR,—Referring to *The Philosophy of Theism*, it would have led to a better

understanding in regard to its arrangement, had I stated that the direct object of the work is the solution of the following problem. Given an organic body, show how it can be *rationally* proved that its cause *must* have been a personality, endowed with intelligence, will, and sensitivity,—that the entire argument is contained in Part III. Sections 1, 2, 3, 9, 10, 11, 12, 13, 14; and that the rest of the book was written, either in anticipation of objections, or to pave the way to the argument. I shall feel obliged if you will, at your leisure, read these sections by themselves in the order I have marked them.

I am, etc., JAMES CROLL.

The following letters from Professor Ferrier and Principal Cairns may also be quoted as indicating the opinions of the merits of the work formed by two distinguished thinkers belonging to different philosophical schools:—

ST. ANDREWS, *15th January* 1859.

DEAR SIR,—I have read attentively the volume entitled *The Philosophy of Theism*, and I am impressed with the acuteness and power of coherent and independent thought which it displays. The style is concise and, for those who are at all conversant with the subjects treated of, perspicuous. The main object of the treatise, to show mainly the distinction between will as producing, and intelligence as determining motion, is well made out. In some form or other this distinction is essential to the Theistic argument; and I am not aware of any quarter where it is more clearly stated than it is in this book. There are many other topics touched upon in the volume in a way which shows a thoughtful and original consideration of the subject: and I believe that all who are interested in philosophical inquiries may read it with profit and satisfaction. Even where they may not agree with it, the book will assist in rendering their own ideas more definite and clear.—Yours faithfully, J. F. FERRIER.

MR. JAMES CROLL.

BERWICK, *12th August* 1858.

GENTLEMEN,—About a year ago you sent me a treatise on *The Philosophy of Theism*, and afterwards wrote requesting my opinion of it. I was obliged altogether to decline a judgment, as my health was very much broken, and I was not equal to my necessary duties. I have since so far recovered as to study a little, and having read the work with some care, though still without critical accuracy, I venture to express my sense of its qualities.

I am much pleased with the advanced state of intelligence displayed by the writer as to the exact shape and pressure of metaphysical questions at the present time. He has evidently read in many schools; and, what is still better, he is a vigorous and independent thinker, who can grasp the essence of a subject, and express it without either vagueness or pedantic nicety in clear and comprehensible English.

His work is a positive contribution to the theistic argument, and I regard him as on the right track, for the argument from causality, however decried by some, is the only valid basis of philosophical theism, and there is both ingenuity and solidity in the way he connects the causal principle with the design argument, so as to reason from the determinations of motion that make up organised bodies. I think, however, that this trunk line of his argument needs to be made a little more prominent, and regret, though his digressions are both able and satisfactory, that they somewhat hide the main thread of discussion. Nor, so far as I remember, admitting as he does that the ordinary argument for the contingency of the universe is invalid, has he supplied any other proof of the non-eternity of matter with its present dispositions and arrangements, so as to make way for the causal principle and its consequences, as he holds it.

I am much interested by the intelligence and vigour of his reduction of all science to metaphysical starting-

points, and also by the vindication of free will as an ontological question from falling under the testimony of consciousness. This is one of the most original things in the book, and betokens a strong thinker. His frequent references to Kant, so far as I remember, are accurate; only I think that Kant and he mean the same thing respecting mathematical reasoning as founded on sensuous imaginations. Kant does not mean that this is *à priori* in the same sense as the categories of the understanding, though he uses the word; and the writer does not mean, by calling it "empirical," that it could ever be based on mere observation, without the antecedent intuitions of space which make observation possible, and which, according to Kant, account for, by necessitating, the harmony between *à priori* or abstract mathematical truths and the actual world of lines and figures.

But I do not go into further details. I regard the work as one of no ordinary promise, far above the great body of similar contributions. The writer, I hope, will produce something still better, and needs only to give himself entirely to this topic or any other to secure distinguished success.

Again thanking you for sending me the work, and apologising for my long delay,—I am, etc.

JOHN CAIRNS.

It is a remarkable testimony to the candour and critical ability of Dr. Cairns that he should, from the perusal of this anonymous volume, predict that the writer needed only to give himself entirely to this topic or any other to achieve distinguished success. This expression of prophetic insight was wonderfully verified in Croll's case, who afterwards achieved such distinguished success, writing on very different topics.

Croll had forwarded a copy of *The Philosophy of Theism* to the late Principal Barclay of Glasgow University, from whom he received the following letter:—

CURRIE, *6th May*, 1858.

SIR,—I beg to apologise for having delayed so long to thank you for presenting me with a copy of *The Philosophy of Theism*. It furnishes very satisfactory evidence that you have thought deeply on what must be admitted to be a deep subject.

I very much sympathise with you in your desire to have the benefit of a university education, and if my influence can avail in obtaining a bursary for you, it shall be most willingly used.—I am, dear sir, yours faithfully,

T. BARCLAY.

JAMES CROLL, ESQ.

This shows how anxious Croll was to have the benefit of a thorough systematic university education, a desire which he was destined never to be able to have satisfied. The book, however, had evidently secured the kindly interest and sympathy of the learned Principal, who, as will be afterwards seen, when opportunity offered, did not hesitate to use his influence in furthering Croll's interests.

The Philosophy of Theism, although it did not attract much general attention, established conclusively the fact that the writer was a man of considerable mental power, who could express his thoughts in clear, forcible style. The reputation thus gained led to his appointment on the *Commonwealth*, a weekly newspaper published in Glasgow, and chiefly devoted to the promotion of the temperance cause. As Croll was an ardent abstainer, this was a sphere in which he could, and did, work with a will.

In the spring of 1858 his mother, Janet Croll, died in the old house at Wolfhill, at the advanced age of seventy-seven. His brother David, who, as already mentioned, was somewhat deformed, having now no one to look after the home, left Wolfhill and went to reside with Croll in Glasgow.

About the middle of the year 1858, Croll met with an accident which caused considerable pain and discomfort,

as well as unfitted him in the future for much physical exertion. He says: "One day, while suddenly exerting my whole strength in using a joiner's plane in dressing a piece of wood, something appeared to give way about the region of the heart. Medical men have never been able to detect what is wrong, But ever since then, though my health and strength remained unimpaired, I dared not lift anything, or attempt to run or even walk fast."

Notwithstanding this, he continued his duties in the *Commonwealth* office. He remained in this situation for about a year and a half. About the end of that period, the directors of the Andersonian College, Glasgow, advertised for a janitor, and Croll applied for the situation. It appears that during his stay in Glasgow Croll had spent some time at Thornliebank, where Walter Crum, Esq., the Chairman of the Andersonian, lived. This gentleman had casually come across Croll there, and learned to respect him. Through his influence, aided, it is said, by that of Professor Ferrier of St. Andrews and Principal Barclay of Glasgow, Croll was appointed to the humble post. He entered on his new duties at the end of the autumn of 1859, and, within the walls of the Andersonian, found a quiet resting-place for several years. "Taking it all in all," he says, "I have not been in any place so congenial to me as that institution proved. After upwards of twenty years of an unsettled life, full of hardships and difficulties, it was a relief to get settled down in what might be regarded as a permanent home. My salary was small, it is true, little more than sufficient to enable us to subsist, but this was compensated by advantages for me of another kind."

His duties in this institution were of a somewhat humble nature; but they did not tax his mental powers, so that in leisure hours he was fresh for such intellectual work as he found congenial. "The Museum was open from 11 A.M. till 3 P.M., and as I had little or nothing to do with the arranging and classification of the specimens,

and there were but few visitors, I had generally a few hours a day of a quiet time for reading and study." His brother David was of great assistance to him in the performance of his duties. In fact, he superintended and performed most of the routine and mechanical work of seeing the rooms cleaned, the fires kept up, and the doors opened and class-rooms aired.

A student of the time writes: "The first time I saw Mr. Croll would be as he stood at the door of the foot of the stair leading to the lecture hall in which the lectures to the evening classes were delivered, as I showed him my ticket as a passport for entrance. I mind that he never officiously demanded a sight of it, but simply gave it a glance when shown to him. Croll was very obliging. When a student wanted to hear any single lecture to which he had not a class ticket, he readily admitted him with a nod. This showed his strong sympathy with any real student thirsting for knowledge. Only to a known student of the institution, however, was the privilege accorded, and that only for 'any single lecture,' so that the teacher really suffered nothing, while the anxious inquirer might profit much."

Croll had evening as well as day duties to perform; but the most irksome and disagreeable duty was that of collecting subscriptions from private gentlemen for the support of the institution. This was odious to Croll, perhaps even more so than insurance canvassing, as he was often met with the remark that if his university could not support itself, it should be given up. Yet not a word of murmur escaped his lips, and the subject is not even referred to in his Autobiography.

During his period of janitorship he had access to the fine scientific library belonging to the Glasgow Philosophical Society, a privilege of which he availed himself. "Here also was the library of four or five thousand volumes in connection with the evening classes of the institution, and, further, the private library of the founder of the institution, consisting of over two thousand volumes."

The attractions of these libraries, containing a large number of the most valuable works on physical science, were so great as to draw off his attention to a considerable extent from his philosophical and theological studies, and for a time he resolved to devote his attention entirely to the prosecution of that department of study. Accordingly, he resumed the study of physics at the place where he had left off in former years.

It will be remembered that when Croll was only about fifteen or sixteen years of age, he studied "the laws of motion and the fundamental principles of mechanics. In like manner I studied pneumatics, hydrostatics, light, heat, electricity, and magnetism," without any assistance, as there was no one near him who could really help him. These were the subjects of which he resumed the investigation now, in the congenial atmosphere of the Andersonian Institution. As in earlier years he had never burdened his memory with the mere details of the physical sciences, but pressed on till he had grasped "the laws or principles which they were intended to illustrate," so he still pursued the same plan. Croll could never keep the results of his study bottled up in his brain for his own selfish satisfaction, but was always, from his earliest down to his latest days, ready and anxious to let others have the benefit of his investigations. So now, no sooner had he made any discovery than he longed to communicate it for the general benefit. He thought very little of the monetary reward he might receive for his scientific work, although he never enjoyed much of this world's wealth. Many important papers were gratuitously sent by him to various magazines, and even when he did receive fees for his articles, they were frequently spent in reprinting separate copies to be sent to scholars at home and abroad. We may truly say that a more unselfish, a more generous man of science, a man more wholly free from the "odium scientificum" than James Croll never breathed.

Dr. Morison writes, regarding Dr. Croll, under date

3rd September 1891: "My acquaintance with Dr. Croll commenced very soon after my removal to Glasgow in 1851. In these early days I had a large and lively theological class. The subjects discussed thrilled into the souls of not a few noble youths; and, indeed, the influence of the class exercises continues, in a subtle form, to the present day, strong and sweet.

"Dr. Croll found his way into this class not exactly as a member, for he put no questions, but as an interested spectator and listener.

"We soon came to know each other, and thenceforward our intercourse grew apace, for I was able to assist him somewhat in literary work. It was not that our thoughts ran in absolute unison. They did not. But somehow we loved one another with pure hearts fervently. Our intimacy increased as time rolled on, and I not infrequently found myself turning into his little room in the 'Andersonian,' that we might exercise a little fencing on some of our favourite battlefields. Dr. Croll about that period revelled among the new books that were laid, as it were, to his hand in the 'University.'

"He had obtained a humble position as bedellus of that vigorous educational institute. He diligently improved his opportunity, and struck on many new veins of ideas which led him far and wide into the interminable fields of science, pure and simple. He had been from early life an omnivorous reader. But the new views of science soon received from him the lion's share of his attention—the place of pre-eminence as regards his most thoughtful thinking on the one hand, and his miscellaneous reading on the other.

"Ere long the consciousness of latent power grew within him, and continued to grow, till an overmastering conviction came upon him, to the effect that he too, as well as others, had a service laid upon him in the way of guiding some of the chief scientific currents of the age. With a view to fulfil this mission, he read and wrote largely on Molecular Physics. It was his first great

effort, the beginning and inauguration of a bright scientific career. Even as regards style of composition he reached maturity by leaps and bounds. The style in which he settled was conspicuous for dignity, manliness, and for translucency."

CHAPTER VI

EARLY SCIENTIFIC WORK

HAVING settled down quietly in the humble sphere of janitor of the Andersonian University in 1859, where the duties, not of a laborious though mostly menial kind, were largely performed by his brother, Croll found the College a congenial home, as he resided on the premises. To most men of mental ability the situation would probably have been monotonous and irksome; but Croll, with his contented disposition and studious habits, found himself planted in a comparatively congenial sphere. For fifteen years previous to his going there, he had been engaged in philosophical and theological studies; and he had already put the result of some of these studies into systematic form. He found the attractions of physical science and the facilities afforded for its study in the Andersonian Library too strong to resist; and, accordingly, he threw himself with characteristic vigour into that department.

From 1859 to 1864 there is really little or nothing to record regarding his personal life, as the due performance of his daily duties at the University, and the equally steady amount of daily scientific study, occupied him from week to week and year to year with little variety. He was fond of long walks in the country, and was a keen observer and admirer of the beauties of nature. His daily walks were most frequently taken alone, so that what he observed might be noted down either on the spot or immediately when he returned home. He was essentially a solitary student and observer of nature, and

did not care for his mental meditations being disturbed by ordinary conversation. It will be remembered that even during his boyhood he had studied physical science with considerable success, and that in those early studies his inclination was more to the mastery of first principles than scientific details. So likewise now, when the opportunity was afforded him of access to the best scientific works in the Andersonian Library, the bent of his mind led him to the study of first principles. Having ascertained these on a given branch of science, his object in all his studies was to advance that science either by the further application of the principles known, or more generally by the investigation and discovery of some new principle. Hence all the results of his observation, investigation, and study, so soon as put into shape, were speedily communicated to the scientific and reading public, both through the medium of scientific societies and the press. With characteristic courtesy and kindly consideration, he was always ready to give any scientific man the benefit of his studies, and to spare no pains in communicating the results thereof, as will be seen later on.

In the subsequent part of this book it is proposed to incorporate the gist of the numerous papers contributed by Dr. Croll to various learned societies and publications. A complete list of these works, with references to the sources where they may be found, is printed in an appendix; and the numbers used in the following account of them correspond to these in that list.

In the early years of his scientific labours, Mr. Croll published several very interesting contributions, which showed that he was busily occupied in thinking out for himself some of the great problems that were being ardently discussed by the leading physicists in the sixties. During the whole of his life he continued to interest himself in his early studies, and occasionally published a paper bearing on physical problems; but for many years he devoted his attention chiefly to the great

problems of theoretic geology, and, in 1864, he began the brilliant series of solutions which make his name one of the most illustrious in the history of this science. In 1865 he discussed, in the now defunct *Reader*, the physical cause of the submergence of the land during the Glacial epoch, and pointed out that the North Sea must have been invaded by land ice during that period.

From that time onwards he produced in quick succession a series of papers of the greatest importance, in which he dealt with the secular variations of climate, more particularly during the Glacial period, and gradually developed his theories of their cause. In ten years' time he had so far elaborated and arranged his results that he could publish *Climate and Time*, a book that was at once recognised as an epoch-making work on theoretic geology. A summary of this work, after four years' further reflection, was published in the *Encyclopædia Britannica* (Article "Geology"), and also in Sir Archibald Geikie's *Text-Book of Geology*. Occasional physical papers had appeared during the ten years devoted to *Climate and Time*; but after its publication they became more frequent. All had some connection with the ideas that had occupied his attention so long and so profitably, and deal with such subjects as the Origin and Age of the Sun, Nebulæ, etc.

Mr. Croll's papers are distinguished by remarkable concentration of thought, joined to a very great lucidity of exposition. They are, therefore, not less interesting and intelligible to the general reader than valuable to the special student.

In order to make the account of Dr. Croll's scientific work as brief and clear as possible, the papers have been considered in six groups, within each of which a chronological order has, as far as possible, been followed :—

1. Early Physical Papers, 1861–1864.
2. Age and Origin of the Sun. Nebulæ.
3. Geological Climate and Chronology.

4. Glacial Epoch and Glaciers.
5. Ocean Currents.
6. Miscellaneous Papers.

The most weighty contributions are those summarised in Nos. 3, 4, and 5.

1. Early Physical Papers, 1861–1864.

Croll was a man who never went into anything without adequate preparation, or adopted theories without making the most thorough investigation possible and applying the most rigid tests that could be devised. Accordingly, before he wrote on any subject, he took all the precautions which his mind could suggest to verify any propositions he might advance. Thus we find that, before he began writing on Physics at all, he went through a course of reading, which to a trained student would be considered tolerably hard work, but which to Croll, with his weak eyesight and other defects, must have been a prolonged mental effort. During the years 1860 and 1861 he appears to have chiefly occupied himself with the study and investigation of the results of the researches of Faraday, Joule, Thomson, Tyndall, Rankine, and others on Heat, Electricity, and Magnetism. The immediate result of these studies was the publication of his paper in 1861 on Ampère's experiment. It appeared in the *Philosophical Magazine* in the month of April of that year, and was entitled "Remarks on Ampère's Experiment on the Repulsion of a Rectilinear Electrical Current on itself" (No. 2).

In May 1862 he returned to the same subject, and wrote another paper entitled "Remarks on Ampère's Experiment on the Repulsion of a Rectilinear Current on itself" (No. 4), which appeared in the *Philosophical Magazine* of that month. In October 1862 he wrote an explanatory note on "Ampèrian Repulsion" (No. 7), accompanied by an illustrated figure, which appeared in the *Philosophical Magazine* of that year.

These communications by Croll throw much light on the extent and depth of his knowledge of electricity. Forty years previously, Ampère had discovered the peculiar action of one current of electricity on another, and also the laws which regulate such action. He found, for example, that if two hoops are hung up beside one another with their planes vertical, and so attached to their supports as to be capable of rotating round a vertical axis, and a current of electricity then passed through each, the two hoops will rotate until they are parallel to one another, the electric current flowing in the same direction in both.

From this Ampère proceeded to examine what action, if any, a current has upon itself. He bent a piece of wire into the shape of an elongated U, and then bent the curved part so as to be in a plane at right angles to that of the straight parallel portions. The wire thus bent was laid upon mercury contained in two separate parallel channels, so that one of the straight parts floated on the mercury in each channel. In this way the straight portions of the wire could move endways with great freedom, while always connected together at one end by the curved part, which formed an arch between them. When the two portions of mercury were connected with the terminals of a battery, a current passed from one to the other by way of the floating wire, and it was found that, under these circumstances, the wire always moved away from the ends of channels with which the battery was connected. This experiment led Ampère to believe that a current was self-repellent; and his experiment was held as establishing this astonishing fact for many years.

In 1861, Principal Forbes, of St. Andrews University, read a paper before the Royal Society of Edinburgh, in which he described an experiment seemingly at variance with the self-repellent theory. His experiment was similar to Ampère's in theory, with the exception that the movable bend of the circuit was detached from the rest; and he found that this movable joint not only was

not repelled, but was held strongly attached to the fixed part of the circuit. He also threw doubt on the experiment of Ampère ever having been successfully performed, or at least verified up till then.

A paper of Croll's appeared in the *Philosophical Magazine* (No. 2) at this time, showing, with considerable subtlety, that, although Ampère's experiment were successful, it would not prove that a current repelled itself, as the experiment could otherwise be easily explained. He showed, in addition, how this theory led to manifest contradictions. He was therefore led to adopt the theory propounded by Principal Forbes, although here again he showed that the attraction of the movable bend could be explained without assuming that the attraction was caused by the moving current. Here the matter rested until Maxwell showed that the experiment gives no proof of what force any one portion of a current exerts upon another.

In March 1862, Croll read a paper before the Chemical Society of Glasgow on "The Relation of Chemical Combination to Specific Heat" (No. 3), in which he showed that by applying heat to solids or liquids, part of it raises the temperature and part works against mechanical cohesion, the relative proportions of each being according to their relative resistance; and that, therefore, the specific heat of bodies increases as the temperature rises. The general principle is, that, other things being equal, the more easily fused a body is, the greater its specific heat.

To the meeting of the British Association in 1862 Mr. Croll communicated a paper on "The Cohesion of Gases, and its Relation to Recent Experiments on the Thermal Effects of Elastic Fluids in Motion" (No. 5).

In this paper, he points out that the deviations from Boyle's law, seen in such easily liquefiable gases as carbonic acid gas, can readily be explained by the cohesion of their particles. He also holds that cohesion explains why, in gases which deviate most from Boyle's law,

the co-efficient of expansion is greatest, the co-efficient of expansion increases with the density, and, when compressed under the same conditions, most work is done, and when expanded by heat, least work is done.

Cohesion and Thomson and Joule's Experiments on the Thermal Effect of Elastic Fluids in Motion.—In these experiments of Thomson and Joule, gases which had been highly compressed in a vessel were allowed to escape through a porous plug, and were then found to have a lower temperature than when they were compressed. As the cooling due to the expansion of the gas is not compensated for by the heat of friction, Croll suggested that part of the heat must have been used in overcoming the cohesion of the gas. He could not suggest an explanation if, as is said, the temperature of the expanded gas is, in some cases, highest.

Cohesion and Carnot's Function.—Dr. Joule suggested this formula for Carnot's function: $\mu = \frac{J\,E}{1 + E\,T}$ where J = Joule's equivalent, E = co-efficient of expansion, and T = temperature in degrees centigrade. Professor Thomson noted that this does not always give the true value. As the temperature of a gas diminishes, the amount of heat consumed by cohesion increases. But with saturated vapours the reverse holds good, cohesion increases as the temperature rises. Only for a perfect gas can the formula hold at all temperatures. For imperfect gases and vapours the function will deviate in opposite directions.

At this time the great question discussed by physicists was the dynamical theory of heat, and Croll made several contributions to the controversy. In his paper, "On the Mechanical Power of Electro-Magnetism," communicated to the British Association in 1862 (No. 6), he contends that, when a current is reduced from A to B, although the heat evolved in the conducting wire is now $x\,B^2$ instead of A^2, the heat in the entire circuit is really B, the missing heat being found in the battery. If mechanical work be done, the heat given off in the

whole circuit, from the same current, will be diminished by the thermal equivalent of the work performed, which he shows to be derived from the electric current. This he explains by supposing the molecules to have resistance, so that when the one at the pole of the battery is set in motion by chemical action, this disturbs the next particle, and so on, giving the series of molecular vibrations which we perceive as heat. In the electro-magnetic machine the current generated by chemical action passing through soft iron makes the molecules magnetic, and equilibrium is regained by the mechanical work done. This will be greater as the resistance producing heat is diminished, and it also depends on the "amount of resistance offered by the magnetic element as an outlet to the electric force"; so that the harder the iron, the less the mechanical work. Hence the amount of molecular resistance determines the amount of molecular work producing heat, or mechanical work produced by electro-magnetism.

Croll next compared *chemical and vital forces and their relations to the potential energies of matter*, in a paper on "The Relation of Chemical Affinity to Vital Force," published in the *Chemical News* on 16th May 1863 (No. 8). Chemical change turns potential into kinetic energy; but heat cannot bring back the former condition, nor can electric currents, for more potential energy is lost in generating the current than is gained in electrolysis. But vital agencies seem to separate atoms of strong affinities, restoring potential energies from actual energy of the sun's rays. Thus, as he shows, "the chemical agent restores the potential energy by consuming actual energy, viz. the sun's rays."

In 1864, Croll wrote three papers dealing with the theory of heat, the first (No. 9) being a short reply to some objections raised by Mr. Gill to the dynamical theory. In the second, on "The Nature of Heat Vibrations" (No. 1), he explained these as molecular and not molar, and concluded that the ultimate atom was

necessarily elastic, the heat-vibrations consisting of alternate expansions and contractions of the atom itself.

The last paper, on "The Cause of the Cooling Effect produced on Solids by Tension (No. 12), may be summarised in his own words: "Previous to the application of tension, the heat existing in the molecules is unable to produce any expansion against the force of cohesion. But when the influence of cohesion is partly counteracted by the tension applied, the heat becomes enabled to perform work of expansion, and a cooling effect is the result."

One of the first distinguished scientists who encouraged Croll in his studies was the late Professor Tyndall. Early in 1863, Croll had written to him regarding his physical investigations, to which the Professor replied: "Your letter was interesting to me as an illustration of power to seize a definite physical image—the molecules acting as hammers was capital. I have no doubt that anything you send me will interest me." Taking advantage of this kind note, Croll sent him the paper on "Supposed Objections to the Dynamical Theory of Heat," regarding which Professor Tyndall writes: "10th Feb. 1864.—Dear Sir,—I have forwarded your paper to Mr. Francis, altering nothing therein." Croll later on sent the Professor a further paper, and received the following reply:—

14th January 1865.

MY DEAR SIR,—It is both amusing and interesting to me to trace the parallelism which has run between your thoughts and mine on the subject of "negative fluorescence" (I have changed this term to *Calorescence*). The very experiment to which you refer, of rendering a body hot by concussion, is the one which most influenced my conviction that it was possible to produce incandescence by invisible rays.

It strikes me you are rather hard on the phrase "breaking up long periods into short ones." In the case of

the hammer there is a conversion of the mechanical motion into molecular motion, and in the case of the hydrogen flame there seems to me to be a conversion of the long periods into short ones. The fact, at all events, is that you hit the mass with waves of slow recurrence, and that you obtain, in return, from the mass, waves of quick recurrence.

Excuse the hurried scrawl.

Do you wish me to send your note to the *Philosophical Magazine*? If so, I would suggest one small and unimportant alteration. Instead of saying "an abuse of language," I would say an incorrect use of language. —Yours very truly, JOHN TYNDALL.

2. AGE AND ORIGIN OF THE SUN. NEBULÆ

In all questions of geological chronology the age of the sun is of prime importance. The heat of the sun cannot be derived from combustion, which is quite inadequate to account for it. If the sun had contracted from a nebulous mass extending far beyond the limits of the present solar system into its present size, Professor von Helmholtz had calculated that sufficient heat would be generated for twenty million years. But Croll said that geologists demanded a longer period than this to account for the earth's development, and that biologists asserted that hundreds of millions of years were needed for the evolution of the present flora and fauna. Croll considered the present rate of denudation, and deduced from data obtained in the Mississippi valley and in Europe, that the oldest sedimentary rocks are probably about 90 million years old. If this be so, some source of heat other than mere contraction of a nebular mass must be found, so that the sun may have given out heat for such a long period. Croll stated that 50 million years' heat might arise from the collision of two bodies, each half the mass of the sun, and moving at a speed of 476 miles per second before the collision. Only 274

miles per second of this speed would be accounted for by the mutual attraction of the two masses, and the rest must be assumed as due to the proper motion of the bodies. Were this original speed to be greater, a greater amount of heat would be generated. How the initial velocity was acquired cannot be explained. Supposing each sun thus formed to last 100 million years, and that all the stars visible to the naked eye were such suns, a star visible in our hemisphere would be formed only once in 15,000 years, if the number of fixed stars be constant; and so the absence of a historical record is not an argument against the theory. The permanent stars, too, are those whose translation motion has been transformed in a large measure into heat. If only a part of this motion were transformed into heat, the probabilities are that only a temporarily visible star would be formed. Supposing that a hundred such temporary stars were formed for one permanent one, and that each on an average was visible for a thousand years, only about six such stars would be visible at present, and it might well be that their greater velocity has not yet been detected.

Croll urged (Paper 71) that the nebulæ could also be explained as a stage in solar evolution. Mr. Lockyer's plausible theory of the evolution of the planets assumes a temperature far too high to be the result of condensation. This temperature cannot have been derived from gravitation, and the collision of two bodies must be the source from which it is derived. After the collision of two bodies moving with a great velocity in space, there would be an enormous mass of incandescent matter spread over a very wide area; and the known irregularity of nebulæ would be accounted for by the chance irregularity of this dispersion. Star clusters would result from this widespread and irregular distribution through space, condensation taking place round subordinate centres. In reply to objectors, Croll urged that it was perfectly legitimate to assume the existence of non-luminous bodies in space, some of which have not yet

received their light and heat, and others which have spent them. The assumed velocity is quite a plausible one; and, if it has not been measured, that can be explained by the fact that on this hypothesis the visible stars are bodies whose motion in space has in great measure been transformed into heat and light. In the case when only part of the motion had been transformed, Croll pointed out that the proper motion of the fixed stars was not accurately known. Collisions, however, must be extremely rare events.

CHAPTER VII

"*CLIMATE AND TIME*"

3. GEOLOGICAL CLIMATE AND CHRONOLOGY

ABOUT 1862–63 considerable commotion was caused among geologists by the publication in 1863 of a paper by Dr. Archibald Geikie, read in Glasgow, regarding the Glacial epoch in Scotland. Doubtless Croll must have read the paper; for, in the spring of 1864, he turned his attention to the subject, and, without knowing at the time what Herschel and Lyell had written on the matter, it occurred to him that the change in the eccentricity of the earth's orbit might probably be the real cause. The subject to which he then turned his attention was the change of climate during the geological epochs; and what he, with marvellous philosophic, independent insight, divined to be the physical cause was the change in the eccentricity of the earth's orbit.

Geological records furnish abundant evidence that very different climates have existed in the same region at different periods of the earth's history. There have been many attempts made, both before Croll's time and since the publication of his works, to give an adequate explanation of the cause of these varieties of climate; but probably no one has done so much to place the theories of these secular variations of climate on a scientific basis as the author of *Climate and Time.*

Before analysing the various papers which Dr. Croll published on this subject, it may be well to summarise very briefly the possible causes of climatic change, and

then show, by an abstract of his works, how he arrived at the results which have made his name so famous.

Possible Causes of Climatic Change.—The whole solar system is moving through space. It may be that different parts of space are at different temperatures, and that the earth's temperature is thereby affected. Should this be so, the globe as a whole would become colder, or hotter, as the case might be. Of this spacial variation of temperature, however, there is no proof.

The great factor in determining climate is the sun. Its influence may be modified in general by changes in the earth's relations to it considered as a whole, or, in particular, by local alterations on the surface of the earth.

The earth itself has some individual heat, which may affect climate; but, unless the crust be assumed to be of unequal thickness, or conductivity, or both, this influence will not vary from place to place, but will show a gradual cooling or heating as time goes on. And this, like the former cause, will generally not have local effects unless there are remarkable variations in the earth's crust. Such considerations may be put aside until it can be shown that no other explanation is possible without taking them into account.

The relations between the earth and the sun are known to be inconstant, and the limits of the variations have been carefully determined. The track of the earth's path round the sun is not a fixed ellipse, but one of varying eccentricity, at some periods approximating to the circle, but at others much more elongated. The mean distance of the earth from the sun is not constant. The inclination of the earth's axis of rotation to the plane of its orbit is not always the same; and the lines of the tropics and those limiting the areas where perpetual day and night occur recede from or advance towards the equator or pole respectively, according as the obliquity of the ecliptic diminishes or increases.

Our northern winter now happens when the earth is

nearest the sun; but times have been and will be again, when the northern winter is in aphelion and the summer in perihelion. The precession of the equinoxes successively brings each season to all parts of the earth's orbit.

The surface features of the earth have not always been identical with those of to-day; and such changes must be considered. The dry land is more readily heated, and parts with its heat more easily, than the ocean; and thus the distribution of land and water influences climate. Even the nature of the surface must be considered, at least in any detailed reconstruction of past climates; for, not only is the elevation of the ground of importance, but also its bareness, or the nature of its covering of herb or tree.

All these factors may modify climate. The question to be settled, then, is not so much whether secular climatic changes are due to this or that cause, but which of all the causes may be considered of primary importance, and which have merely a secondary influence.

No dogmatic assertion can settle this, but carefully considered calculations. It is the great merit of Dr. Croll that he made use of the data at his disposal, and worked out as precisely as they permitted the extremely complex problems involved. In the following paragraphs his methods and results will be briefly summarised, and the significance he assigned to each of the possible causes will be indicated.

Mr. Croll's Paper on Secular Variations of Climate.—The first paper of this remarkable series of investigations into the cause of secular variations of climate was published in 1864 in the *Philosophical Magazine*, and entitled "On the Physical Cause of the Change of Climate during Geological Epochs" (No. 13). In this he summarised the theories already propounded, and declared them to be insufficient to account for the magnitude of the results.

He next discussed the effect of changes in the eccentricity of the earth's orbit, both directly and indirectly, but showed that a lack of data prevented any

very definite conclusions being drawn. Regarding this paper, Mr. Horne of the Geological Survey said, in an obituary notice of Dr. Croll, read to the Scottish Geological Society—

"In connection with this memoir, it is interesting to recall the views then held in this country regarding the phenomena of the Glacial period. Notwithstanding the highly suggestive paper of Agassiz in 1840, in which he showed how the *roches moutonnées*, striations, and glacial deposits indicate the former existence of land ice in Scotland, geologists were slow in accepting his opinions. For years, nearly every geologist in Britain clung resolutely to the theory of the iceberg origin of the drift. At length it was vigorously assailed by Professor Ramsay, Dr. Archibald Geikie, and Mr. Robert Chambers. From a careful examination of the evidences of ice-action in this country, Canada, and the Continent, Ramsay felt convinced that the theory was no longer tenable. In like manner, Dr. Archibald Geikie, who in his early years had accepted the old explanation, was compelled to abandon it in 1861, after an extended series of observations in different parts of Scotland. He prepared an elaborate memoir on the subject, giving a detailed description of the phenomena, and his reasons for attributing them to the action of land ice. While this memoir was in preparation, another eminent Scotch glacialist, Mr. Jamieson of Ellon, arrived at similar conclusions, from his own independent observations, and Sir Charles Lyell also adopted the same explanation.

"Dr. A. Geikie's paper was read in abstract to the Geological Society of Glasgow in 1862, and published as a separate memoir in 1863, appearing subsequently in the first volume of the *Transactions*. When we consider the date of publication of this elaborate memoir, nearly two hundred pages in length, geologists will readily admit that it is of special importance in connection with the history of glacial geology in Britain. There can be little doubt that it paved the way for

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"Dr. A. Geikie's paper was read in abstract to the Geological Society of Glasgow in 1862, and published as a separate memoir in 1863, appearing subsequently in the first volume of the *Transactions*. When we consider the date of publication of this elaborate memoir, nearly two hundred pages in length, geologists will readily admit that it is of special importance in connection with the history of glacial geology in Britain. There can be little doubt that it paved the way for

the final rejection of the iceberg hypothesis in this country.

"The cogent arguments advanced by Dr. A. Geikie in favour of the former existence of land ice in Scotland had doubtless a powerful influence on Croll's philosophic mind. He evidently realised that the iceberg theory was doomed, and that well-nigh twenty years had been lost by geologists in this country, owing to their stubborn refusal to adopt the suggestions of Agassiz. Accepting the land-ice origin of the boulder clay and moraines, Croll proceeded, with characteristic boldness, to grapple with the question of the probable cause of climatic change. To a man of his originality and power, the existence of glacial conditions in temperate latitudes during former geological epochs must have been a problem of absorbing interest. Various theories have been advanced to account for such alternations of climate. Some have suggested that they might be due to a change in the position of the earth's axis of rotation; others, that the earth may have passed through hot and cold regions of space, while Sir Charles Lyell strenuously advocated the doctrine that they may have been caused by changes in the distribution of land and sea, on the assumption that elevation of land about the poles would lower the temperature of the globe, and that elevation round the equator would raise it. Recent researches, however, are rather opposed to the belief in such enormous terrestrial changes, and seem to point to the permanence of continental and oceanic areas from primeval time.

"Owing to an early suggestion of Sir John Herschel the attention of geologists was directed to the probable effect of cosmical causes in producing climatic change. In 1830, he showed that, during a period of high eccentricity, the hemisphere whose winter occurs in aphelion will experience a long and exceptionally cold winter and a hot summer; while the opposite hemisphere will enjoy equable climatic conditions. But he subsequently held that the cold of the Glacial period could

hardly be due to the direct effects of high eccentricity, because each hemisphere must receive precisely the same amount of heat; and, further, the deficiency of heat resulting from the sun's greater distance would be equalised by the excess of heat received during the short but hot summer.

"To Dr. Croll belongs the rare merit of showing that, though glacial cycles may not arise *directly* from cosmical causes, they may do so *indirectly*! As already indicated, his first contribution to the subject was published in 1864, but the development of his theory resulted in a series of brilliant researches, extending over a period of eleven years, to 1875. He was led to investigate the problem of the eccentricity of the earth's orbit and its physical relations to the Glacial period. By means of Leverrier's formulæ, he calculated tables of eccentricity for three million years in the past and one million years in the future, with the view of determining the periods of high eccentricity which, according to his theory, were coincident with cycles of extreme cold."

The publication of this paper attracted very considerable attention among scientific men; and the advanced pioneers of investigation and research early recognised in it evidence of a powerful and original speculator on perplexing problems of the highest order. Among the first to convey any recognition of this was the late Sir Andrew Ramsay, Director of the Geological Survey, who wrote as follows:—

PALL MALL, LONDON, *19th August* 1864.

DEAR SIR,—Though personally unknown to you, I take the liberty of writing to say that I have just read your article in the *Edinburgh Philosophical Journal* on the cause of the cold of the Glacial epoch, and I am very much struck with your views. But I fear I must read it more than once before I thoroughly realise it.

In an article of mine in the *Saturday Review* (12th July, I think) on Frankland's theory, I said that no one

had given anything like an explanation of the cause of the cold. Had your paper been written then, I would not have made so strong a statement.

It does not matter to your argument, but I am not sure that it is yet safe to assume a Cambrian Glacial epoch. The Old Red one is, I believe, true, but it has not been proved to demonstration. I have, however, as near as may be, believed in it for many years. Then there is the Permian cold period, and in the north of Italy there is perfect evidence of glacial erratics during part of the Miocene epoch.

If your theory be true, we may have at length some hope of being able to measure geological time. This occurred to me before I had gone half through your paper, and I was delighted to find the statement so plainly put by you at the end of it.—Yours very truly,

ANDREW RAMSAY.

If you reply to this, it will be best to address me, Geological Survey Office, 28 Jermyn Street, London.

We have not been able to trace Croll's reply, but from this time onwards till his death Sir Andrew kept up a more or less continuous correspondence with Croll.

Following on this, Sir Charles Lyell had noticed the paper and directed the attention of Sir John Herschel to it, whereupon the following interesting correspondence took place.

53 HARLEY STREET, *13th February* 1865.

DEAR SIR,—I asked Sir John Herschel to read your paper (which I found he had not done) which you were so good as to send to me on the causes of change of climate, and I sent Sir John a copy of the *Philosophical Magazine* for August last, that he might look it over.

By a singular coincidence, the copy which you sent to him reached him on the same day as mine by post,

and he has sent me his answer to you as being equally a reply to my queries.

I enclose an extract of a letter which I have written to Sir John after reading his to you.

I am very glad that you have called our attention to Leverrier's calculations, and regret that they seem to afford no clue as to the probable duration of the intervals between such maxima of eccentricity as are possible. If, when these maxima do occur, the extreme climate which they would cause continues for a lapse of years, say 50,000 or more, during which more than one complete precession of the equinoxes would be gone through, you might assume that the extreme of winter cold would alternately visit both Arctic and Antarctic circles within the range of one maximum. As I understand at present the astronomical results, they seem to me by no means so promising as to you. The supposed geological proofs of the recurrence of glacial epochs are contradicted, I think, by palæontological evidence; and the few and exceptional cases of far-transported blocks of large size, even if they require ice to account for them, as in the Miocene, Permian, and in the conglomerates of some older periods, are far from being such as would require an amount of cold making any approach to that of the post-Pliocene Glacial period. The finest case of erratics, the Miocene blocks of the Superga, were contemporary with very warm climate in Europe. And if their transport was due to ice, it must, I should think, have been produced by some very local cause. Mr. Page's diagram and theory referred to by you is not, I think, warranted by facts.

Your assumption that a fifth more solar heat during summer would do so little to keep down the glaciers is contrary to Darwin's notion founded on the glaciers of the Southern Andes, where he attributes their reaching the sea in the latitude of Paris from mountains only 6200 feet high to the effects of an equable climate. Such glaciers are only one of many examples to show the pre-

dominance of geographical conditions in producing ice-action, over astronomical causes. You will see in my new edition of the *Elements*, p. 333, that a rich reptilian fauna existed in Europe in the Chalk period. I know of no cretaceous erratics implying cold. A single piece of granite was found in the chalk near Croydon, which, like the single pebbles mentioned by me at pp. 321, 322, may be explained without the aid of ice. You will see that we now know too much of the Devonian flora and its analogy with that of the Coal period (see p. 453) to suppose such contrast of climate as Mr. Page introduces where he makes the Carboniferous in a warmer and the Old Red in a colder cycle. Again, there is too close an analogy between the Silurian and Cambrian fauna (see pp. 569–573–575) to allow us to infer a great distinctness of climate on palæontological grounds; and I know of no attempt as yet to prove cold in the Cambrian by any reliable signs of glaciation.

You will perhaps have seen in my Bath address, of which I hope you received a copy, that I have alluded to the submergence of the Sahara as one cause of Alpine glaciers. The address was already in print when my attention was called to your interesting paper; and, not being an astronomer, I had not time to get up the subject so as to allude to your paper on Leverrier's calculations.

I still think that my explanation of the predominant influence of the position of land accords best with the want of recurrence of decided Glacial epochs, for, as I have always stated since 1830, it is fair to assume that the present preponderance of land in high latitudes is extremely exceptional, and may be supposed never to have occurred since Cambrian times; whereas the maxima of eccentricity must, I presume, have happened over and over again even since the commencement of the Miocene epoch, unless my notions of geological time and the rate at which species change are altogether wrong, or unless the astronomical instances to which you allude

are much rarer than I suppose you would conjecture them to be.

Your idea of the occurrence of winter when the earth is in the perihelion of its orbit, so that the difference between summer and winter temperature would be almost extinguished, as explaining a Carboniferous flora, is an excellent suggestion; but, independently of the resemblance between the Devonian and Carboniferous flora, the latter alone endured, I think, far too long to be explained by an hypothesis which requires, if I understand you correctly, the coincidence of a certain state of inclination of the earth's axis due to precession, and, secondly, a maximum of eccentricity, and, thirdly, the occurrence of winter when the earth was in its perihelion.

The absence of land from polar regions might well last for many millions of years and give rise to a climate such as is indicated by the Devonian and Carboniferous floras.

We are as yet very ignorant as to the exact coincidence of formations implying similarity of climate of corresponding date in the opposite hemispheres; but the evidence, so far as it goes, appears to me much more in favour of such agreement than of such a discordance as I presume your astronomical explanation would require. Within the epoch of living species both hemispheres seem to me to have been subjected simultaneously to great cold. Geographical causes would seem to me favourable to such a result; for if I could transfer a lofty mountain chain from some tropical region, and place it where there is now a polar sea, it would not only make the glaciers of Greenland and Norway larger, but would make the Antarctic region colder than it is.

If I have misunderstood the bearing of any of your arguments, I should be obliged to you to explain, and I shall take care in the new edition of my *Principles* fully to cite your valuable paper.—Believe me, very truly,

CHARLES LYELL.

Extract from letter of Sir Charles Lyell to Sir John Herschel, 11th February 1865:—

I have been comparing my first and subsequent editions of the *Principles* in which I cite your paper with the last edition. I find that it was in the 3rd edition, 1834, that I first cited your paper of 1830; and all the subsequent editions, down to the last or 9th, are reprinted verbatim, with the exception of Arago's name being introduced in reference to his opinion that "the mean amount of solar radiation can never be materially affected by irregularities in the earth's motion."

I do not see how Mr. Croll could have been misled, as you hinted in your letter to him, by reading my abstract of the results of your paper so as to infer that the possible eccentricity of the earth's orbit might not materially affect the earth's climate; for I have represented you as saying that if the earth's orbit should ever become as eccentric as that of Juno, etc., the winter and summer temperatures would be sometimes mitigated and sometimes exaggerated, etc.

In all editions, from the 1st to the 9th, I allude to the precession of the equinoxes, without any reference to you in the first two editions, because, although you read your paper in December 1830, it did not appear in print till several years afterwards. In my 3rd edition, 1834, p. 163, I cited your paper for the first time; and I did so because I thought it fair to insert what appeared to me to be meant as a check to my reasoning on the excess of eight days; in commenting on which you observe that, whatever be the ellipticity, the two hemispheres must receive annually equal absolute quantities of light and heat. It is true that in all my editions I quoted you for these remarks on the precession of the equinoxes in a different chapter from that in which I cite you for your discussion of the possible effects of variations in the eccentricity of the orbit. The two subjects were treated of, the one in the 7th and the other in the 8th chapter; and if Mr. Croll had been dependent (which I cannot

suppose) on my version of your paper for his information on a subject so important to him, I can hardly imagine that he would have read one of these chapters and not the other. The result of Leverrier's calculations (in which I suppose we may assume there is no error) and the application of it by Mr. Croll are most important, and we must, of course, in the vast lapse of geological times, allow for the frequent operation of the extremes alluded to; but the more I study the palæontological evidence, the more I am inclined to believe that there have not been those fluctuations from hot to cold and from cold to hot which would have occurred if the astronomical causes of change predominated in their influences over the geographical. The Glacial period has, nearly all of it, occurred in what I call the post-Pliocene era; or, if some fluctuations preceded, I have shown in my new or 6th edition of the *Elements* just published, p. 198, that it was only at the close of the Pliocene period, or when a small percentage (only about five per cent) of the shells differed from those now existing, that great cold came on (see also p. 204). I have in all my editions given a reason founded on the present excess of land in high latitudes why the cold may now be greater than for a long succession of preceding geological epochs, where the distribution of the same land was less abnormal. The coincidence of the extreme of cold in the northern and southern hemispheres with the period of the existing species of shells is in favour of the predominant influence of geographical causes over astronomical: not that I wish the least to undervalue the latter, as they have of course been influential; but we are equally certain of the power of the varying disposition of land and sea, and we know that the place of these has changed enormously since the commencement of the Glacial period, whereas it has yet to be shown whether the eccentricity has varied within the same time so as to cause great cold in both hemispheres.

ANDERSONIAN UNIVERSITY, GLASGOW,
16th February 1865.

SIR CHARLES LYELL, BART.—SIR,—I beg to thank you for the long and most interesting letter which I have had the honour of receiving from you.

I shall study its contents most carefully, and in the course of perhaps eight or ten days shall send you any remarks on the subject which may suggest themselves to my mind.

I am also obliged to you for the copy of your letter to Sir John Herschel, and glad to hear that he has sent you a copy of his long and interesting letter to me. As he has slightly misunderstood me on one or two points, I have taken the liberty of enclosing a copy of my letter of explanation to him.

The copy of your address before the British Association, which you kindly sent, came duly to hand.—I am, Sir, yours respectfully,

JAMES CROLL.

COLLINGWOOD, *6th February* 1865.

SIR,—I beg to thank you for the number of the *Philosophical Magazine* you have been so good as to send me, containing your paper on "The Physical Cause of the Change of Climate in Geological Epochs." Sir C. Lyell had also referred me to this paper, and I have been daily expecting to receive from him a copy of it (which I had not before seen), but none has hitherto reached me.

At the time I wrote my paper of 1830 in the *Geological Transactions*, the extent to which the change of eccentricity in the earth's orbit might extend had never been so much as conjectured. Lagrange's theorem indeed proved that the limits could not be *extravagantly* large; for (as I have mentioned elsewhere), "Jupiter and Saturn will always retain the Lion's share" of his eccentricity fund, $\Sigma\, m \sqrt{a}\, e^2$. But such an amount of change as would make it amount to 0·078, though not expressly

contemplated in that paper, is nowhere there assumed to be *impossible*; and indeed the influence of a much larger *conceivably possible* change *is* speculated on, *and that in the very same manner as in your paper*, viz. through its influence, not so much on the mean annual temperature (which would not, however, be quite inconsiderable) as (owing to the equinoxial revolution of the apsides) in alternately exaggerating and mitigating the violence of the summers and winters.

I am quite at a loss to understand how, from anything said in that paper, you can have concluded (as in your p. 129) that, "owing to my not having fully taken into consideration certain conditions which affect climate," I "*seemed to be of opinion* that the general climate cannot be much affected by changes in the eccentricity of the orbit." I could almost suppose, on reading this, that you must have taken your impression of that paper, not from the paper itself, but from what Sir C. Lyell gives in his *Principles* (edition of 1835) as a resumé of it, in which he omits all mention of the *equinoxial revolution of the apsides*, which forms so essential a feature of it. But perhaps I have myself only to blame for this: since, in a subsequent work of my own (*Outlines of Astronomy*, 5th edition, 1858, § 369 *c*) I have myself attributed to Mons. Reynaud, both in my note on that §, and in the preface to that edition, all that part of the speculation and reasoning of my paper. In so doing I have done myself and Sir C. Lyell a most singular and unusual injustice. Referring, in consequence of your letter and paper, to my original paper of 1830, to see what I really *did* say, I find the whole theory there quite distinctly stated, and express reference made to Sir C. Lyell's *Principles* (edition of 1830), *in which* the equinoxial revolution of the apsides *is* adduced as an influential element. All this, at the time of my receiving M. Reynaud's papers, shortly before the publication of my 5th edition, 1858, *I had so completely forgotten* that it was actually with surprise that this morning, on re-read-

ing my original paper, I find the whole of the reasoning (as above stated) there delivered.

From Leverrier's calculations which you cite, it now appears that the possible maximum of the earth's eccentricity is 0·078. *This is ample.* I agree with all your conclusions as to its possible and indeed necessary amount of influence, especially as regards glacial phenomena (which had not been brought on the *tapis* in 1830). Geology has now a fair stand-point and there is no longer now any necessity to have recourse to such suppositions as the passage of our system through hotter or colder regions of space, or to outstanding remains of central heat. *The sun will suffice for all geological requirements.* Of course I have not been able to go *seriatim* into all your details of geological and climatological reasoning, especially as regards the trade winds and oceanic currents. On these, I presume, much will yet remain to be said. But looking at the thing "en masse," *we have* in the secular change of the earth's eccentricity got what we wanted, which, though I always thought possible, I hardly expected ever to have seen proved.—I remain, sir, your obedient servant,

J. F. W. HERSCHEL.

P.S.—This evening's book post brings me a separate copy of your paper, and three more on the tidal wave and on heat. These, of course, are from yourself, and the number of the *Philosophical Magazine* must be from Sir C. Lyell. I shall read the others with all due attention.

ANDERSONIAN UNIVERSITY, GLASGOW,
14th February 1865.

SIR JOHN HERSCHEL,—I beg to thank you for your long and interesting letter of the 6th inst., and am very much gratified indeed to find that you consider the conclusions arrived at in my paper to be correct. I am sorry, however, to observe that you have somewhat misunderstood the main object aimed at in my paper. I

never, for one moment, thought of propounding any new theory regarding the cause of the changes of ancient climate. All that I proposed to do was, simply to show, from the already *received laws* of planetary motion, that the maximum eccentricity of the earth's orbit, as determined by M. Leverrier, is sufficiently great to account for every extreme of climatic change evidenced by geology.

I made use of the fact that eccentricity tends to increase the difference between summer and winter temperature in the one hemisphere, and to diminish the difference in the other hemisphere. But I never entertained the idea of giving this out as an original thought. I was perfectly aware that the fact was well known a century before I was born. I referred also to the fact that the precession of the equinoxes tends to transfer the extremes of temperature from the one hemisphere to the other. But this fact must also have been perfectly familiar for long to the minds of astronomers.

The greater portion of my paper was written before I knew of the existence of your communication to the Geological Society. It was when searching in Sir Charles Lyell's *Principles* for certain facts regarding the climate of the Carboniferous epoch, that I observed for the first time reference made to it. But my remarks regarding your paper were made after I had read it carefully. I regret that I did not give an outline of it. But as I did not consider that I was advancing any *new theory* on the subject, a simple reference to it appeared to me to be all that was necessary.

I regret having used the expression, "Sir John Herschel seems to have been of *opinion* that the general climate of our globe cannot be much affected by the change in the eccentricity of its orbit"; and I beg to apologise for my mistake. I had been led to form this impression regarding your opinion, from a statement made in your *Outlines of Astronomy*, section 368. In that section, when referring to the fact that the

quantity of heat received by the earth between the vernal and autumnal equinoxes is the same in both halves of the year, whatever the eccentricity of the orbit may be, you say, "The greater proximity of the sun in the smaller segment compensates exactly for its more rapid description, and thus an equilibrium of heat is, as it were, maintained. Were it not for this, the eccentricity of the orbit would materially influence the transition of seasons. . . . Now the perihelion of the orbit is situated nearly at the place of the northern winter solstice, so that, were it not for the compensation we have just described, the effect would be to exaggerate the difference of summer and winter in the southern hemisphere, and to moderate it in the northern, thus producing a more violent alternation of climate in the one hemisphere, and an approach to perpetual spring in the other. *As it is, however, no such inequality subsists*, but an equal and impartial distribution of heat and light is accorded to both." In short, you affirm that the change in the velocity of the earth's motion always *compensates* for the change which takes place in the eccentricity of its orbit. This idea, more than any other, has, I fear, tended to produce a general impression that changes of geological climate could not have been owing to changes in the eccentricity of the earth's orbit. It produced this impression on my mind, until I was led to suspect the truth of your conclusion, and I had therefore inferred that it must have produced the same effect on your own.

This compensating principle, I presume, does not hold true in reference to temperature of climate. It simply holds true in reference to the total quantity of heat received from the sun. But temperature depends as much upon the amount of heat radiated into space, as upon the heat received from the sun. Now this compensating principle does not hold good in reference to the cooling of the earth, but the reverse. The southern hemisphere, for example, has not only a colder winter than the northern, in consequence of greater distance

from the sun, but it has also a longer winter. And, on the same principle, our winter in the northern hemisphere is, in consequence of our proximity to the sun, not only warmer than that of the southern, but shorter also. Consequently our hemisphere is not cooled to such an extent as the southern. Thus slowness of motion, as the earth recedes from the sun during the southern winter, goes entirely, in so far as cooling is concerned, to aggravate the rigour of the winter in that hemisphere.

It seems to have dropped out of your recollection for the moment, that, long before your paper appeared in the *Geological Transactions*, Lagrange had undertaken the laborious task of calculating the maximum eccentricity of the orbits of each of the six older planets, and had assigned 0·07641 as the superior limit of the earth's eccentricity.

I hope you will excuse the free manner in which I have expressed my opinion,—I am, etc.

JAMES CROLL.

COLLINGWOOD, *17th February* 1865.

SIR,—I am much obliged by your letter of the 14th. I am always thankful to have it pointed out to me when anything which I ought to have known and referred to when writing on any subject has escaped my notice, particularly if of real importance to the subject, and a knowledge of which would have forwarded the object in view. Such is the fact you mention, that Lagrange had actually extended the numerical computation of the maximum eccentricity of the earth's orbit, and found it to be 0·07641, agreeing very nearly with Leverrier's result. This is new to me. Had I known it in 1830, I should not have stopped short of drawing the same sort of conclusion that you have done from Leverrier's, instead of leaving the conclusion an open question. I should be very glad to be informed in which of Lagrange's numerous writings this important calculation is given or its result mentioned. Neither was I aware that the

effect of eccentricity in exaggerating the contrast between summer and winter in one hemisphere, and mitigating it in another, had been pointed out a century before the commencement of the present generation. It is not unnatural that such a consideration should have occurred to the astronomers and geographers of those days, but that it had done so I was ignorant, or if I may have read it, it had made so little impression as to have been quite forgotten. A citation of the author of this would also be highly welcome, and in what work to be found.

As regards the way in which the equinoxial revolution of the apsides reacts on this last mentioned effect, it is no doubt very true that any astronomer reflecting on the subject *might* have come to that conclusion. Nothing is more common in the history of invention and discovery in every branch, than the fact that two well-known truths, which when viewed in conjunction give rise to an idea not suggested by either of them separately, do often remain a long time uncombined, and so unfruitful of their joint result. There is no doubt that anyone, at any time since Newton, *might* have put these two things together, and drawn the conclusion; but, so far as I am aware, Sir Charles Lyell was the first to do so. This done, there came a fresh item into the combination, viz. the *varying amount* of the eccentricity. Anybody *might*, at *any time* within the last ninety or one hundred years, have put the *three* ideas together, and the result would have been what we now see. But, so far as I am aware, it was not *done* till the year 1830.

I am very far from asserting that this cause (this triple combination of periodicities) is *alone* to be resorted to to solve the great problems of glacial action. I readily admit the great influence of a different distribution of land and sea appealed to by Sir C. Lyell, and it is impossible not to speculate on the possible coincidence in point of time of this fourth great and independent element of alternate *general* heat and cold in the climate of the whole earth with the other three each in its cycle

of periodicity, and with an eternity practically available to work within, as a key to some of the great difficulties of geological revolutions.

I observe in reading your letter over again, I have overlooked to mention your citation of a passage in my *Astronomy*, section 368. The passage in question is from the early editions of my *Outlines*, where it seems to have been copied from the original *Cabinet Cyclopædia* Treatise which was written in a great hurry, *currente calamo*, and has been suppressed in the later editions.—I remain, sir, your obedient and obliged servant,

J. F. W. HERSCHEL.

GLASGOW, *23rd February* 1865.

SIR JOHN HERSCHEL—SIR,—You will find Lagrange's paper on the maximum eccentricity of the orbits of the six older planets in the *Memoirs of the Academy of Berlin* for 1782. M. Leverrier refers to that paper in his paper on "The Secular Variations of the Elements of the Principal Planets," which was published in the *Connaissance des Temps* for 1843, additions. Reference to it will also be found in the *Comptes Rendus*, tome ix. pp. 371, 542, and in Professor Grant's *History of Physical Astronomy*, pp. 116, 117.

It is certainly remarkable that Sir Charles Lyell should have been the first to record the obvious fact that the summers of the southern hemisphere ought to be warmer than those of the northern, and that in course of ten or twelve thousand years hence the conditions of the two hemispheres will be reversed.

I have no doubt whatever that this fact, though perhaps never noticed by any writer, was perfectly familiar to your own mind long before Sir Charles Lyell's *Principles* appeared.

In fact, Sir Charles himself admits that he borrowed the idea from astronomers. In his *Principles*, 3rd edition, vol. i. p. 163, when referring to the pre-

dominance of ice in the southern hemisphere over the northern, he says: "Before the amount of difference between the temperature of the two hemispheres was ascertained, it was referred by *many astronomers* to the acceleration of the earth's motion in its perihelion, in consequence of which the spring and summer of the southern hemisphere are shorter by nearly eight days than those seasons north of the equator."

At all events, you were evidently the first to show how changes of geological climate might be caused by changes in the relative positions of the earth to the sun. —I am, etc.,

JAMES CROLL.

COLLINGWOOD, 30*th April* 1866.

DEAR SIR,—Let me thank you for your papers on the ice-cap and the eccentricities. I cannot help thinking that we are at length brought (always supposing that M. Leverrier's formulæ and your calculations founded on it are right) in possession of a glimpse of light in geology, and that really and truly those notions which were a good deal pooh-poohed when first put forth about geology having anything to do with the perturbations of the planetary system are beginning to bear fruit.

Your speculations about the effect of gravitation on a polar ice-cap are highly worth the most attentive consideration. A change of level of 1000 feet, though not enough for all purposes, will account for much, such as the terraces on the shores of Greenland, etc.—Believe me, dear sir, yours very truly,

J. F. W. HERSCHEL.

CHAPTER VIII

ECCENTRICITY OF EARTH'S ORBIT, ETC

IN 1866 Croll prepared and published in the *Philosophical Magazine* (Paper 16) a table containing "Values of the Eccentricity of the Earth's Orbit for a million years past and a million years to come, at epochs 50,000 years apart"; and in 1867 (Paper 22) he extended the tables between 70,000 and 1,000,000 years back, and from the present time for 250,000 years back, giving the values of eccentricity for every 10,000 years. He also tabulated the relative lengths of summer and winter, and his estimate of the fall in mean winter temperature for these periods.

By 1868 he had studied the eccentricity of the earth's orbit for the past 3,000,000 years; and, in three masterly papers on "Geological Time," published in the *Philosophical Magazine* (Papers 27, 28, 29), he stated his conclusions much more fully and categorically. It will do fuller justice to Dr. Croll's views, if this series of papers is considered together without adhering rigidly to the chronological order in which the results were published.

Effect of Variations of Eccentricity.—First of all, what would be the effect on the climate of our earth when the eccentricity of the earth's orbit was at a high value?

Sir John Herschel pointed out the possible effects of eccentricity on climate; but Arago and Humboldt raised objections to the theory which were considered adequate

by the majority of geologists until Dr. Croll took up the subject. He showed that changes in eccentricity would certainly not affect climate *directly* by increasing or diminishing the mean annual amount of heat received from the sun, for this is inversely proportional to the minor axis of the earth's orbit, and the difference between the maximum and minimum values of this axis are too trifling to produce any sensible modification. But another effect of high eccentricity, and one which is of far greater importance, is the resulting difference in the relative duration of the seasons. For instance, if the northern hemisphere had its winter in aphelion when eccentricity was at a maximum, the sun would be 9,000,000 miles farther from the earth in our winter than it is at present, and the direct heat of the sun would be one-fifth less in winter and one-fifth more in summer than at present; whereas, if the northern winter occurred in perihelion, with the same conditions of eccentricity, the sun would be 14,000,000 miles nearer the earth in winter than in summer, and the difference between winter and summer temperature in the latitude of Scotland would be nearly annihilated. Hence, although both hemispheres received equal amounts of heat per annum, they would have very different climatic conditions. The hemisphere which had its winter in perihelion would enjoy a mild and equable climate; the other, which had its winter in aphelion, would have a long and cold winter. Owing to this increase in the length of winter, a greater amount of heat would be lost by radiation, and thus the severity of winter occurring in aphelion would be still further intensified. At the end of this long, cold winter there would be vast accumulations of ice and snow, which would make the temperature of the ensuing short warm summer much lower by chilling the air. Dr. Croll gives three ways in which this would happen. (1) The air is cooled by radiation from ice and snow more rapidly than

it is heated by the sun. (2) An enormous quantity of heat is consumed in the mechanical work of melting the ice without raising the temperature. (3) Snow and ice chill the air and condense its vapour into fog, which intercepts the sun's rays. A considerable quantity of the winter's snow might therefore lie unmelted all the summer, and the temperature would then rarely rise above freezing point.

Exactly opposite effects would occur on the other hemisphere which had its winter in perihelion. There the shorter and warmer winter would prevent the accumulation of ice and snow, and the lower temperature of the summer would be compensated by its greater length. One hemisphere would thus gradually be cooled, the other would be gradually heated. These effects would be further intensified, since the greater difference of temperature between the poles and the equator would cause the trade winds to be stronger on the cold hemisphere. If this were the northern hemisphere, the median line of the trades would lie far south of the equator, and the warm water of the tropics would be carried across the equator into the Southern Ocean. The strong trades would be compensated by equally strong upper currents from the equator to the pole, which, on reaching the Temperate and Arctic regions of the cold hemisphere, would deposit their moisture as snow, thus increasing the accumulation of snow and ice, and intensifying the causes tending to produce complete glaciation of the cold hemisphere. The deflection of the equatorial oceanic currents would be the most powerful of these causes. Dr. Croll supposed that, at the period of high eccentricity 850,000 years ago, the Gulf Stream could not have been more than half its present volume. At that period the midwinter temperature of Scotland would be 45°·3 F. lower than at present, owing to the greater distance from the sun, while the Gulf Stream would cause a further diminution of 14° F., giving a

total diminution of 59°·3 F. below the present midwinter temperature of 39° F. Thus the midwinter temperature of Scotland 850,000 years ago was — 20°·3 F. These results are due to the *physical* and not to the purely *astronomical* effects of eccentricity. Further, the dense fogs caused by the accumulation of ice and snow during the long and severe winter in aphelion would intercept the sun's rays; while, even if these reached the earth in full intensity, their heat would be consumed in the mechanical work of melting the ice, and so the temperature of the air would remain at freezing point. If the summer heat failed to melt the *whole* of the winter's ice and snow, these would accumulate year by year, so that the mean temperature of summer would ultimately never rise above freezing point. Were the operation of these causes unchecked, the cold hemisphere would in this way gradually pass into a state of complete glaciation. But the maximum is reached when the solstitial point arrives at aphelion, and thereafter a contrary process commences. The original glaciating hemisphere begins to grow warmer, and the warm hemisphere to grow colder. This process too reaches a maximum in 10,000 or 12,000 years, when the precession of the equinoxes brings the solstitial point round to its perihelion. This glaciation of each hemisphere alternately would last as long as the eccentricity remained at a high value. If, then, when the eccentricity of the earth's orbit is great, there is a necessary glaciation if the solstices occur in perihelion and aphelion, it is of great importance to know when these periods of high eccentricity happen, for, by a comparison of the astronomical and geological data, we can now determine more satisfactorily than hitherto the age of the Glacial epochs, and from these deduce the probable age of the various geological formations.

Periods of High Eccentricity.—It may be seen from the table of eccentricity for 3,000,000 years back and

for 1,000,000 years to come, calculated by Dr. Croll from Leverrier's formulæ, that there are three epochs of remarkably high eccentricity during these 4,000,000 years. During the first, 2,600,000 and 2,500,000 years ago, there were two periods of very high eccentricity, separated, preceded, and followed by periods of low eccentricity. The second epoch had three maxima occurring 950,000, 850,000, and 750,000 years ago, with minima between. The third epoch has yet to come, and it will be 800,000, 900,000, and 1,000,000 years before its three periods of very great eccentricity occur.

Besides these epochs of very high maxima, there have been three epochs of subsidiary maxima in the past 3,000,000 years, and another will arrive before another million years have passed. The most recent of these was between 240,000 and 80,000 years ago.

Glacial Epochs and their Dates.—Now there are evidences of three ice epochs since the beginning of the Tertiary times; firstly, in the middle of the Eocene period, secondly, in the Upper Miocene, and, most recently, the Glacial epoch of the Boulder Clay. If glacial conditions occur when the eccentricity is great, then there is a possibility of correlating these ice epochs with definite dates. The first point to notice is that, with two great epochs of maxima and three subsidiary ones, and evidence of only three instead of five Glacial epochs, the coincidence is not complete. Does the Boulder Clay Glacial epoch correspond to the second great epoch of maximum eccentricity between 700,000 and 1,000,000 years ago, or to the most recent of subsidiary maxima 80,000 to 240,000 years ago? If to the latter, can traces of two other Glacial epochs not be found? When the dates of the most recent Glacial epochs are fixed, the approximate age of the other epochs and events of the earth's evolution can

be estimated. The correctness of the results arrived at can be tested by considerations derived from quite different data. A maximum age is fixed for our planet by considerations of the origin and age of the solar system. Dr. Croll studied the origin and age of the sun very carefully, and his conclusions are discussed in a subsequent section. He was disposed to fix the limit at 100,000,000 years. Hence 3,000,000 years must stretch far back into the geological history of the globe.

Of the shells of the Lower Miocene 5 per cent. have changed since the Glacial epoch of the Boulder Clay; and, assuming the conditions of variation of species to be constant, the period of the Lower Miocene may be estimated as twenty times more remote than the last Glacial epoch. Analogous considerations fix the Cambrian period as 240 times more remote than the last Glacial epoch. But if this latter occurred as long as 1,000,000 years ago, the Cambrian period occurred 240,000,000 years ago, a result which is irreconcilable with the age assigned to the earth by the physicist. If, however, the last ice age corresponded with the last period of subsidiary maximum of eccentricity, between 240,000 and 80,000 years ago, the Cambrian period began not more than 60,000,000 years ago.

In order, however, to render this reasoning cogent, it will be necessary first to show that other methods of estimating the earth's age give harmonious results, or else to show to what erroneous assumptions the discrepancies are due; and, secondly, either to produce evidence of three Glacial epochs since the beginning of the Tertiary period, or else to account for the absence of such evidence of glaciation.

Geological Chronology.—Ordinary methods of estimating geological time Dr. Croll regarded as extremely unsatisfactory. No reliable conclusion can be based on the thickness of the stratified rocks, for many circum-

stances affect the rate of deposition.[1] The palæontological method is useless until we know much more of the rate and uniformity of organic change. A far more reliable method, Dr. Croll considered, is the calculation of the rate of sub-aerial denudation. The Mississippi, which is a fairly typical river, has been shown to remove one foot from the surface of the land in 6000 years, and rough estimates show that Europe is being denuded at about the same rate as North America. By this process of denudation continents are cut up into islands, and these into smaller islands, till the whole finally disappears. During the Glacial periods denudation was probably more rapid. At present the limited carrying power of rivers allows disintegrated materials to accumulate as soil, which protects rocks against weathering. But during a Glacial period the presence of a thick soil under the ice would increase the rate of denudation of the rocks over which the ice moved, by greatly increasing the friction between the ice-sheet and the rock. Still, even at the present rate, 1000 feet of rock would be removed in 6,000,000 years, and thus it is evident that stupendous changes can be wrought in a comparatively small period of time.

[1] *Note on Geological Chronology.*—Dr. Croll frequently expressed his distrust of geological chronology and of the current estimates of the probable thickness of the sedimentary rocks. In a paper published in the *Geological Magazine*, 1871 (No. 41), he suggested 100,000,000 years as the maximum age of the stratified rocks, and 7000 feet as their mean thickness over an area equal to that of the ocean, or 5000 feet over the whole area of the globe. This estimate is based on the known rate at which the Mississippi carries sediment into the ocean. Erroneous results must necessarily be obtained from observations of the maximum thickness of sedimentary rocks, unless it be constantly borne in mind that the greater part of the sediment carried down by rivers would be deposited over a very limited area near their mouths, and comparatively little spread over the ocean-bottom. The absence of a particular formation in a given area would thus frequently mean that the area in question had been distant from the coast and the mouths of rivers. In a subsequent letter (Paper No. 42), Dr. Croll remarked that the rate of formation might have been accentuated by such agencies as marine denudation, submarine and other volcanoes, coral reefs, etc., but that none of these could compare in importance with sub-aerial agents of denudation.

With regard to evidence of former Glacial periods, Dr. Croll pointed out that it must necessarily be very imperfect. All the traces of ice-action during the last Glacial epoch which remain on the present land surface will disappear when it is converted into a sea-bottom. The polished and striated stones will be disintegrated, the Arctic shells in low latitudes will have disappeared, and the Boulder Clay will not be deposited as such, but in the form of mud, clay, sand, and gravel. Hence it is not surprising that little or no evidence of former glaciation is found in the stratified rocks, except the existence of erratic blocks in low latitudes. These can only have been transported by the agency of ice. That icebergs do not striate the sea-bottom, and that consequently no striations can be expected in the stratified rocks, was shown by Dr. Croll at great length. He concludes that we have probably as much evidence of former Glacial periods as will ultimately remain of the post-Tertiary Glacial epoch. The paucity of organic remains in the Eocene and Upper Miocene strata is, on the whole, evidence in favour of glaciation.

Indirect evidence, however, is not wanting. A Glacial epoch resulting from a high state of eccentricity would consist of alternate warm and cold periods, each hemisphere alternately enjoying a warm and equable climate while the other was in a state of glaciation. This would occur owing to the precession of the equinoxes, which would cause each hemisphere in turn to have its winter in perihelion. Thus, in order to substantiate the glacial theory, we should require to show that a warm and equable climate had at times prevailed during the period of high eccentricity. Sir C. Lyell described the flora of the Upper Miocene as sub-tropical. The same conditions of ice-transplanted blocks and sub-tropical flora occur in the mid-Eocene periods as well as in the earlier Cretaceous and still earlier Permian periods, and can only be explained by the cosmical theory. There is

evidence of similar alternations during the last Glacial epoch in the commingling of Arctic and sub-tropical fauna in the cave and river deposits. Dr. Croll considered that, had such evidence been lacking, it would have been strong evidence against his theory, which affirms without qualification the alternation of warm and glacial conditions during each Glacial epoch. As it is, all the indirect evidence goes to confirm his view, that a Glacial epoch occurred in the mid-Eocene period, 2,630,000 to 2,460,000 years ago; another in the Upper Miocene, 980,000 to 720,000 years ago; and the most recent, or Glacial epoch of the Boulder Clay, 240,000 to 80,000 years ago, and that the cause was a high state of eccentricity, and that in each Glacial epoch the precession of the equinoxes caused an alternation of warm and glacial conditions.

Influence of Changes in the Obliquity of the Ecliptic on Climate.—Another cosmic phenomenon, the obliquity of the ecliptic, is constantly varying, and Dr. Croll studied the effect of this on the earth's climate and on the level of the sea in two papers, published in the *Philosophical Magazine* for June and August 1867 (Papers 24, 25). He maintained that its influence on climate had been considerably under-estimated by the majority of geologists and physicists. Laplace had calculated that the obliquity would oscillate to the extent of 1° 22′ 34″ on each side of 23° 28′, the obliquity in 1801. Dr. Croll estimated that, when the obliquity was at a maximum, the poles would receive nearly as much heat as is now received in lat. 76°. The polar winter would hardly be affected by the fact of the sun descending 1° 22′ 34″ farther than at present; but the summer would be considerably modified. The mean annual temperature would be raised 14° or 15°, unless the polar regions were covered with ice and snow, in which case the additional heat would be employed, not in raising the temperature, but in melting the ice and snow. The increase in obliquity would therefore considerably

modify the effects of high eccentricity on the glaciated hemisphere which has its winter in aphelion, by diminishing the accumulation of ice, and would intensify the warm climate on the hemisphere which had its winter in perihelion. When obliquity was at its minimum, the poles would receive less heat than at present. The glaciation of the cold hemisphere would be intensified, and the warmth of the other diminished.

Eleven thousand seven hundred years ago the eccentricity of the earth's orbit was somewhat greater than at present, but the obliquity of the ecliptic was near its maximum. Consequently, on the southern hemisphere, which had its winter in perihelion, the ice was rapidly melted, causing a rise of sea-level and submergence of land on the northern hemisphere. Dr. Croll calculated that this rise at the latitude of Scotland would be about twenty-five feet, and that the Carse clays were then formed. Even greater effects may have been produced in remote ages, if Sir John Herschel is right in supposing that, when millions of years are taken into account, the obliquity may oscillate to the extent of 3° or 4° on each side of a mean state. Briefly, it may be affirmed that, when both the eccentricity of the earth's orbit and the obliquity of the ecliptic are near the superior limit, the combined effect of both causes would probably completely remove the ice-cap from the hemisphere which had its winter in perihelion, thus causing a submergence of land to the extent possibly of hundreds of feet on the other hemisphere.

Mild Polar Climates.—In a paper on "The Cause of Mild Polar Climates" (Paper 86), Dr. Croll argued that oscillations of climate would still occur during Tertiary periods,when eccentricity was great, even should there have been no well-marked glacial condition at the time. The very high temperature of which we have evidence in the northern hemisphere during Tertiary times shows that the conditions in the southern hemisphere were

unfavourable for the flow of warm equatorial currents southward; and the same cause which made the relatively cold periods of the Tertiary times less intense increased the warmth of the relatively hot periods. There were evidences of such oscillations in the commingling of tropical palms and trees such as oak and elm. But he believed that glaciation did occur at periods of high eccentricity. Eccentricity, however, brings about glaciation only by means of physical agencies depending on geographical conditions, and the presence or absence of these geographical conditions would determine the presence or absence of glaciation at these periods. The evidence against Tertiary glaciation was far from conclusive, and that in favour of it would still be found. He believed that evidence of glaciation during the Miocene period was afforded by the conglomerates and erratics near Turin, derived from the outer ridge of the Alps on the Italian side. Similar evidence of glaciation in the Eocene period is found in the Flysch of the Alps, which probably corresponds with the high value of eccentricity 2,500,000 years ago; while the Miocene glaciation was probably caused by the high value of the eccentricity 850,000 years ago.

Arctic Interglacial Periods.—Dr. Croll's last paper on geological climate, "On Arctic Interglacial Periods," appeared in the *Philosophical Magazine*, January 1885. He adduced evidence to show that warm Interglacial periods had occurred in Arctic regions. Most of the organic remains from these periods would be destroyed in the severe glaciation which followed. In Siberia, however, which was less severely glaciated, the remains of the mammoth and *Rhinoceros tichorinus* are found. The climate must at that time have been far milder than at present, since these animals lived on long grass and the foliage of trees. The larch and willow were also found in Siberia, far beyond their present northern limit, and the shells found also indicate a warm climate. The

mammoth ranged over Europe during a mild Interglacial period; but, as the cold came on, this creature retired to southern Europe, returning northwards only as the cold subsided. There is strong presumptive evidence that the mammoth did not finally disappear till recent post-Glacial times. Owing to the severe glaciation which followed, there is less decisive evidence of Interglacial periods in the American Arctic regions; but the remains of the mammoth have been found in ice cliffs at Kotzebue Sound, and the remains of full-grown trees in Banks Land, Prince Patrick's Island, and Melville Sound. Professor Nordenskjöld found in Spitzbergen the shells of a mussel still living off the Scandinavian coast, which afford something like indications of an Interglacial period in these regions.

Replies to Critics.—In reply to Mr. Murphy, F.G.S., who had advanced the theory that the glaciated hemisphere would be the one which had its summer in aphelion, Dr. Croll pointed out, in the *Geological Magazine*, that the summer, though colder, would be longer, and that there would be no diminution in the total amount of heat received from the sun. Hence this hemisphere would not be exposed to the influences which produce glaciation through an accumulation of ice and snow, too great to be melted during the summer, —a state of things which could occur only in the hemisphere which had its winter in aphelion.

In 1881, Mr. Alfred R. Wallace, in his work on *Island Life*, took very much the same views of geological climate as Dr. Croll, although he differed from him in some details. Dr. Croll was prevented from considering these modifications at the time; but, in February and May 1887, he published a paper in the *Philosophical Magazine*, entitled "An Examination of Mr. Alfred Wallace's Modification of the Physical Theory of Secular Changes of Climate." There Dr. Croll restated his own view, and dealt with a variety of current misconceptions

of its real nature. He showed that the only real modification introduced by Mr. Wallace was his suggestion that, during the height of the Glacial epoch, the snow and ice would not disappear where precession brought the winter solstice round to perihelion, and that warm Interglacial periods could not occur either in temperate or polar regions, except during the commencement or towards the close of a Glacial epoch. Dr. Croll showed that glaciation resulted from three sets of causes, astronomical, physical, and geographical. The latter he regarded rather as necessary conditions than as causes, although, had geographical conditions been such as to prevent glaciation, obviously no glaciation would have occurred. Dr. Croll showed at some length that, when the winter solstice is in aphelion, it sets in operation many physical causes the tendency of which is to produce an accumulation of ice and snow; and that, when the winter solstice is in perihelion, the tendency of these causes is reversed, and they produce a melting of the ice and snow which they had before produced. Mr. Wallace's modification of the theory assumed that the astronomical conditions might be reversed without a reversal of the physical, which would thus still tend to produce and maintain a glacial state. It overlooked the fact that the heating of one hemisphere is largely affected by a transference of heat from the other, which is thereby cooled. Hence, no change in geographical conditions is necessary for the disappearance of the ice on the hemisphere with the winter in perihelion. Mr. Wallace further suggested that glacial conditions would be unaffected by any change in the astronomical conditions, if the ocean currents remained unchanged. Dr. Croll replied that they must necessarily be modified by the modification of the other physical agents. He showed by geological evidence that climatic conditions had undoubtedly oscillated precisely as the theory required during the last Glacial epoch. Below the Carse clays of a Glacial period lies a buried

forest of a warm Interglacial period. There was a fall of sea-level, due probably to the preponderance of ice on the southern hemisphere. The strata below the buried forest bear witness of another rise of sea-level, which Dr. Croll considered a proof of more or less severe glaciation. He adduced a variety of other facts in support of his view. Palæolithic man hunted the reindeer in southern France; and, at another period, the hippopotamus and elephant ranged as far north as England. The commingling of sub-tropical and Arctic flora and fauna proves that strongly contrasted climatic conditions prevailed at different periods of the Glacial epoch. All the evidence, as shown by Dr. Croll, proved that the last warm Interglacial period occurred between two Glacial periods of extreme severity. If these Glacial periods were due to eccentricity, then the intervening Interglacial period must also have occurred during a period of high eccentricity; and Mr. Wallace's view, that the ice would persist throughout a period of high eccentricity, would be disproved. Evidence is gradually accumulating in favour of the existence of more than one warm Interglacial period, in spite of the difficulty of detecting traces of the more remote of them and the comparatively slight attention given to the search. The Glacial periods, though not astronomically longer than the Interglacial, would be lengthened by the physical effects of the gradually diminishing accumulation of ice and snow. The last inch of ice at the end of one period and the first inch at the commencement of another would effectually chill climate. But the time required to melt the ice would only shorten and not prevent an Interglacial period.

Mr. W. G. M'Gee having expressed a doubt whether the disappearance of ice on the warm hemisphere during the period of high eccentricity could be accounted for by the greater length of the summer, Dr. Croll replied that the melting of the ice was chiefly due to the heat

carried from equatorial regions by ocean currents which were deflected into the warm hemisphere.

Mr. Hill maintained that fogs retain nearly as much heat as they exclude, and do not tend to lower the mean annual temperature; and many objectors assumed that the quantity of snow melted must be proportional to the sun's heat. Dr. Croll replied that this is not so, for Greenland receives from the sun every year more than fifty times the heat necessary to melt the ice which covers it. Glaciated South Georgia, in the latitude of England, is another example, and the snowy peaks of the Himalayas receive nearly all the sun's heat unintercepted.

Professor Newcomb had reviewed *Climate and Time*; but Dr. Croll asserted that his objections were based on misapprehensions of his views, and that the view that the quantity of snow and ice vary as the heat received from the sun, is erroneous. Dr. Croll, it will be seen, utterly repudiates all cataclysmic theories of climatic change. He expressed his views on this subject in a paper on "Cataclysmic Theories of Geological Climate," in the *Geological Magazine*, September 1878, originally read before the Geological Society of London. Sir Charles Lyell accounted for climatic variation by changes in the relative distribution of land. Others suppose that some violent change in the obliquity of the ecliptic had brought the Arctic Circle down to the latitude of England. When this theory was disproved, changes in the earth's axis of rotation were assumed to account for the phenomena. This in turn had been given up. Gradually it was becoming recognised that the agents of change had been ordinary ones, like ocean currents, winds, clouds, and aqueous vapour. Of these the most important was the influence of ocean currents, and nothing had misled geologists so much as the utterly inadequate conception of the magnitude of their work. When this was properly recognised, it would be admitted that their

deflection into one hemisphere would alone almost suffice to produce the glaciation of the other. The cause of their deflection has been already shown, and sufficient proof given of the alternation of the warm and cold periods, which may be considered as the great secular summers and winters of our globe.

Final Paper on Misconceptions of his Theory.—On January 23, 1889, a paper by Dr. Croll was read to the Geological Society, on "Prevailing Misconceptions of the Evidence we ought to expect of former Glacial Periods," which was subsequently published in the *Quarterly Journal* of the Society. All evidence of former glaciation is necessarily imperfect; for the old land surfaces have perished, and, with them, almost every trace of the action of ice. The entire stratified rocks of the globe, with the exception of the coal beds and under clays, are old sea-bottoms. Hence, the geologist cannot expect to find boulder clay which is broken up into soft mud and clay, or polished or striated stones which are carried into the sea and there broken up and deposited as gravel, sand, and clay. Even the absence of erratic blocks would not prove that glaciation had not occurred, for in a period of extreme glaciation the whole country is under the ice-sheet, and there is no source from which erratic blocks could be carried on the surface. The Antarctic ice-sheet appears to carry no boulders into the sea; and when the sea-bottom surrounding the Antarctic continent is elevated, no geological evidence of glaciation will be found. Hence, in a region like Spitzbergen, where a glacial condition is normal, the absence of boulders would probably mean that the whole country was buried under a sheet of continental ice. In low latitudes erratic blocks would occur, having been transported by icebergs and dropped into the sea. How unconvincing the mere absence of evidence is, Dr. Croll showed by pointing out that, in a million years, the

geologists of that remote future era will find little or no evidence of the last Glacial epoch, of which we have such overwhelming proof. The striated stones will be nearly all disintegrated, the till washed away and deposited as sand and clay on the sea-bottom, and only a few erratic blocks will reveal the truth. Dr. Croll urged that, assuming a Glacial period occurred every time the earth's orbit reached a high state of eccentricity, we have as much positive evidence of the glaciation as could be expected.

CHAPTER IX

PERSONAL HISTORY—ACCIDENT IN 1865

CROLL was essentially a domestic man. At this time his household in the humble home at the Andersonian University consisted of himself, his wife, and his brother. They managed their domestic affairs entirely within and by themselves, and lived to a very large extent for, and on, each other. Croll, having been bred a joiner, was frequently called on, in connection with domestic matters, for the exercise of his craft in some shape or other; and he was always ready and willing to lend a helping hand, even though sometimes that might be asked at a time when he was puzzling his brains with some abstruse physical problem. On one occasion, when performing one of those minor domestic duties, he unfortunately met with an accident.

One evening in July of 1865, after a day's writing, he hurriedly bent down to put a few tacks into a carpet, when he experienced a twitch in a part of the upper and left side of the head. At the time it did not seem to be a matter of much importance; but it afterwards proved to be the severest affliction that happened to him during his life. Had it not been for this misfortune, all the private work he was able to do during the twenty years which followed it, would have been done in the course of two or three years. The affliction in the head did not in any way affect his general health, or impair his mental energy. He could think as vigorously as ever, but "dared not turn on the full steam." A dull pain settled in that part of the brain, which increased till it

became unbearable if he persisted in doing mental work for any length of time. He was, therefore, obliged to work mentally very quietly and slowly, for a short period at a time, and then take a good long rest. If he attempted to do too much in one day, he was generally disabled for a few days thereafter. Before this accident he could concentrate his thoughts on a single point and exert his whole mental energy till the difficulty was overcome; but after this he never could attempt to do so.

Notwithstanding the very serious interdict which was thus laid on the performance of prolonged mental work, Croll not only wrote (as he says with characteristic modesty) "about a dozen of papers of a longer or shorter character within the following two and a half years," but also opened up a subject entirely new to himself, and threw light upon it even to the most advanced scientists. During the very year, 1865, in which the foregoing accident befel him, he wrote and published three papers on "The Physical Cause of the Submergence of the Land during the Glacial Epoch." One of these "dozen of papers" he submitted to Professor Tyndall, from whom he received the following letter:—

9th May 1865.

DEAR SIR,—I detained your manuscript for two days, in order to make myself acquainted in a general way with its contents. It was, however, posted to you yesterday.

The philosophic tone of the manuscript pleases me, and there are numerous remarks in it to which I would heartily subscribe. But I must frankly state my opinion that what you say of gravity needs reconsideration.

Gravity is in precisely the same category as all the other forces. We cannot compare gravity with *heat*, but we *can* compare it with the molecular attractions and repulsions which produce the motion of heat, and every difficulty urged against gravity may be urged against them.

If I had time, I would write to you at greater length, but I would now simply say that in my opinion there is nothing inconsistent whatever with the principle of the conservation of energy in our present ideas of gravity.—Yours very truly, JOHN TYNDALL.

Croll had for many years been making frequent excursions into the country in search of glacial phenomena, and had thus acquired an extensive and accurate knowledge of surface geology, or drift in its bearings on Glacial and Interglacial periods,—the results of which were partially embodied in these papers.

These papers were followed in 1866 by other four papers on the subject of Glacial Submergence, which appeared in the *Reader*, the *Philosophical Magazine*, and the *Transactions of the Geological Society of Glasgow*. But while thus occupied investigating, and writing on so abstruse a subject as Glacial Geology, Croll also found time, notwithstanding his impaired health, during the same year to conduct a series of investigations, and write three papers on "The Astronomical Aspect of Geological Science," on "The Eccentricity of the Earth's Orbit," and on "The Influence of the Tidal Wave on the Motion of the Moon." These appeared in the *Philosophical Magazine* of January, August, and November 1866.

One of these papers was sent to Professor Tyndall, who wrote as follows:—

19th May 1866.

MY DEAR SIR,—Almost *all* the luminous heat, and a vast flux of non-luminous heat, manages to get from the sun to the earth, so that a screen which prevents this from falling on a properly absorbent thermometer must, I think, make some difference whether close to the earth or at great elevations. I do not in the least understand Mr. Glaisher's observation, but I was rather astonished to see the conclusions which he drew from it,

and which were put in a more definite form by Mr. Wilson. I have the greatest respect for facts, but this alleged fact is, as you say, so opposed to all our other knowledge that it would require far more evidence than now exists to cause my mind even to entertain the possibility of it. Probably at some future day we may hear more about it from Mr. Glaisher, but until then all attempts to account for it would in all probability be mere waste of time.—Yours very truly,

JOHN TYNDALL.

P.S.—Instead of the shaded thermometer, one with a silvered bulb might be compared with the black bulb. The difference would assuredly make itself felt, though here we should have some loss by radiation from the black bulb.

During the course of preparation of these papers he had occasion to get books and information which were not readily accessible to him in Glasgow, and he therefore wrote to his friend Professor Foster, on which the following correspondence ensued :—

UNIVERSITY COLLEGE, LONDON, W.C.,
23rd April 1866.

DEAR MR. CROLL,—I was very glad to get a letter from you this morning. I have not read Delaunay's or Dufour's papers at all carefully, but I will try to do so in a few days and send you the result. I will also try to get you a copy of the Astronomer-Royal's paper, which I think I can very likely do through De La Rue, whom I often see at the Chemical Society.

I have been intending to write to you for some time, to thank you for your last paper from the *Philosophical Magazine*, and also to ask whether you have still one or two copies to spare of your first paper on "The Causes of the Climate of the Glacial Epoch." I was talking of it a little while ago with a friend of mine, Whitaker of the Geological Survey, who is anxious to see your papers.

GLASGOW, *26th April* 1866.

Professor Foster.

DEAR SIR,—Many thanks for your kind offer to procure for me a copy of the Astronomer-Royal's paper, and also to give me an outline of Delaunay and Dufour's papers.

I am sorry that I cannot send you a single copy of my first papers on Climate. They are all gone. In order to make sure that there would be no scarcity in regard to the last paper, I ordered a hundred copies; but I find that these also are all gone but two or three copies.

Under another cover I have sent you a copy for Mr. Whitaker. Mr. Grant is still with us, and in good health. Your old friends and students are now and again inquiring after your welfare.

For as well as I like the Andersonian, I fear I shall be under the necessity of leaving it. So much exposure to cold draughts during winter is making rather serious inroads on my constitution. If any quiet indoor situation, with easy duties and short hours that would not interfere much with my studies, cast up, I shall assuredly leave.

If anything suitable should come under your notice, perhaps you will let me know.—I am, dear sir, yours truly, JAMES CROLL.

LONDON, 57 ALBERT STREET, N.W.,
28th April 1866.

MY DEAR MR. CROLL,—I was very glad to have your paper on the Glacial Submergence and Emergence to send to Mr. Whitaker, as I am sure he will value it.

I send by this post an abstract of all I have found in the *Comptes Rendus* about the retardation of the earth's rotation by the tides. I hope you will be able to make out what is intended, but if any of it is unintelligible, or if you would like fuller reports on any points, you must let me know. I send also a copy of De La Rue's recent address on presenting the Gold Medal of the

Astronomical Society to Adams, which he gave me a few weeks ago. It contains a good deal bearing upon the question. I have not yet seen him since I heard from you, nor anyone else who was likely to be able to get me a copy of Airy's late paper.

You may rely upon it that, if I hear of any opening which I think likely to suit you, I will let you know of it. I hope to be able to send you in a day or two a copy of my edition of Lardner's *Electricity*. I venture to think I have improved the book, but it is a long way yet from being what an elementary treatise on electricity ought to be nowadays.—Believe me, very sincerely yours,

G. C. FOSTER.

GLASGOW, *May 3rd*, 1866.

Professor Foster.

DEAR SIR,—I am exceedingly much obliged to you for your clear and elaborate account of all the papers which have lately appeared in France on the question of the acceleration of the moon's motion. I am really sorry to think that I should have put you to so much trouble. I am also much obliged to you for Dr. De La Rue's most interesting paper on this subject. I shall return it in a few weeks.

None of the writers seems to take notice of the fact that the solar tide tends to diminish the moon's mean distance, and thus produce a *real* acceleration. There is one passage, however, where something of this sort seems to be mentioned: viz. Delaunay's reply to Bertrand, 29th January 1866.

Delaunay admits with Bertrand that whatever retarding effect the moon exerts on the earth, the moon itself must be accelerated to the same extent, so that $\frac{1}{2}$ of the apparent acceleration of the moon's mean motion due to the action of the moon on the waters of the ocean is a *real* acceleration.

I cannot see how any real acceleration can result from the principle adopted by those physicists. I send

you a table containing the results of some additional calculations; and as you are on intimate terms with De La Rue, I enclose under another cover a copy of my last two papers for him, which you may hand to him if you think that he would care anything about them.

I had a letter from Sir John Herschel. He considers that the submergence theory is correct.

I am delighted to hear that your new edition of Lardner is nearly ready. I have no doubt it will prove to be the best text-book we have on the subject of electricity.—I am, yours very truly, JAMES CROLL.

UNIVERSITY COLLEGE, LONDON,
26th October 1866.

DEAR MR. CROLL,—I found yesterday at the Royal Society the passage from Arago which you wanted. It is in a paper of his in which he discusses whether the climate of the earth can have been modified by any recognisable astronomical causes. I daresay you would find it in Arago's collected works if you are able to refer to them. The translation is made in a hurry, and therefore often not elegant, but I think you will find the sense correct.

I am sorry you have had to wait so long for the quotation, but yesterday was the first day I was able to go to the Royal Society.

I was much interested in seeing the announcement of the Andersonian courses for this winter which you were kind enough to send me.—In haste, yours very truly, G. C. FOSTER.

During the year 1867 he pursued the subject of the eccentricity of the earth's orbit and the influence of the tidal wave on the motion of the moon with characteristic vigour, and wrote five papers connected therewith, which appeared in the *Philosophical Magazine*. The earlier papers he submitted to Professor Tyndall, who wrote in regard thereto as follows:—

15th February 1867.

MY DEAR SIR,—Your reasoning seems to me to be quite fair. But I confess I should feel a greater interest in the practical question if I were sure of the observations.

Is there any necessity for saying that the surface of the moon exposed to the sun's radiation is *colder than the earth*? Is it necessary for your argument to assume that the planetary envelopes are more pervious to the radiation from the planet than to the radiation from the sun? If not necessary, I would not, if I were you, bring these points so definitely forward. Your general principle is perfectly secure, that an atmosphere forms a local dam which elevates the temperature of the planet. I have said a few words on this subject in paragraph 546, page 437 of the 2nd edition of my book on Heat. Also in the tract which I now send you, at pp. 45 and 47.—Yours very truly, JOHN TYNDALL.

19th February 1867.

DEAR SIR,—I certainly think that the view you have expressed is more in harmony with existing physics than that expressed by Mr. Wilson.—Yours very truly,

JOHN TYNDALL.

While doing so, however, he was neither neglecting nor forgetting his glacial investigations. On the contrary, he was pursuing these with the intensest earnestness and characteristic thoroughness. He visited and inspected all the glens, river banks, and seashores in the neighbourhood of Glasgow, making careful notes of what he observed bearing on the subject he was pursuing. Very soon, however, he came to the conclusion that the short sections found in glen, or river banks, or the seashore, were of little use in giving an idea of the time represented by the surface deposits,—were, in fact, too short for any estimate to be founded on them, and that the entire thickness could only be found out by the study of bores, or pit shafts, which went to the rock-head.

To realise this idea he went to all the coal and iron-masters he knew or heard of, as well as to professional borers; and he got from them journals of the bores which had been made ere they gained the rock-head. Most of them gave the information readily, as he was careful to explain that it was only the surface he cared about, and did not want the portions that referred to the minerals. Early in these investigations he found a series of rock surfaces running in a line to the north-west of Glasgow, where a deep yet narrow channel, or trough, as he called it, existed in the rocks, and was filled in by the surface deposits. To this he makes frequent reference in a series of letters to Mr. Bennie of the Geological Survey of Scotland.

While collecting these bores, he gave copies of them to Mr. Bennie, who sometimes went with him, or by himself, to verify the exact places where they were put down, or to ascertain the character of the deposits passed through, as often the descriptions in the journals were not definite enough for his purpose.

Early in 1868 he requested Mr. Bennie to make what he could of the bores; and, accordingly, he wrote the paper referred to in the letters of that year. In that paper Mr. Bennie could only refer to the trough as a deep channel, probably of the sea, running across Scotland in the line of the bores. Afterwards, however, Mr. Croll himself took up the subject of the trough; and, in a paper read to the Geological Society of Edinburgh, he proved the trough to be the channel of two rivers which, starting from near the middle of Scotland, ran the one into the Firth of Forth and the other into the Firth of Clyde.

Croll's idea of ascertaining the thickness of the surface deposits by the methods he thus took was as original as it was ingenious; and it shows how thoroughly he made his investigations down to the minutest details, thus laying a solid foundation of facts upon which to base his subsequent scientific inductions and generalisa-

tions. His discovery of the deep trough by means of these bores, excursions, and investigations shows that nothing he came across escaped his observation. He was not a practical field-geologist (indeed this is almost the only bit of physical geology he did), and yet he showed himself a master in that comparatively strange department, subsequent investigation only proving how well the results he arrived at justified his early insight into geological phenomena.

The only other bit of physical geology he actually performed was his excursion to the top of the Pentlands for evidences of the passage of the ice-sheet over them. These will be found in his paper on "The Boulder Clay of Caithness," in his own terse and vigorous words. Mr. Bennie also wrote a description of the same excursion, in a paper to the Physical Society of Edinburgh "On the Glaciated Summit of one of the Pentlands."

Mr. Bennie made notes of some of the excursions he had with Mr. Croll while pursuing his geological investigations; and he has kindly placed them at the writer's disposal.

The following is an account of an excursion with Mr. Croll to Blairdardie Clayfields, 20th April 1867:—"Mr. Croll having expressed a wish to see this place, we went there to-day. The day was fair, but not bright, the sky being full of loose watery clouds which threatened rain perpetually. However, the rain did not come,—the clouds being, I suppose, rarefied by the sun and made retentive of the moisture they were charged with. As Mr. Croll had a horror of rain, and would not go out in it, I was frightened throughout the day by every dull blink that occurred. We went by 'bus to Whiteinch, and thence by our natural powers of locomotion.

"When we emerged from the houses, the country burst upon us pleasantly and charmed us with its beauty. The fields were sweet and green, the delicate dawn of the revived light of the year. The thorn bushes

were everywhere half budded, and the sprightly yellowish green of the young leaves was fine in every sense and to all degrees. The trees were putting forth the 'tender leaves of hope,' and the delicate green light which Alexander Smith loved so much to see was flickering among the branches.

"I found Mr. Croll's 'crack' good, quiet, undemonstrative, but full of pith and power. I cannot remember half of what he said, but the impression that remained was that he was a close observer and deep thinker on those objects he had seen or those subjects he had thought upon, but he was not cosmopolitan in the extent of objects nor encyclopædic in the range of subjects, as, indeed, in this age, when knowledge fills the earth as the waters fill the sea, nobody can be. One thing I remember well: he told me of a small river about twice the breadth of Kelvin which had cut a gorge in the rough conglomerate rocks of the Old Red Sandstone somewhere in Strathmore, about 36 yards wide, and 200 feet deep, so clean and sharp cut that he measured the depth by hanging a string and stone over the precipice and sounding it as the sailor fathoms the sea. The gulf, he imagined, might have been made in some geological period long past, when the river was larger and more powerful than now; for the present river was engaged in cutting a smaller channel inside the larger one, and of course in the bottom of it, a rather curious illustration of a geological sequence of before and after.

"Coming to Blawart Hill, we ascended to have a look at the country around, and realise its carse-like appearance better than we could have by walking through it. The look-out was very good,—the carse lying at our feet, parti-coloured as a tartan plaid, while the lealands, the stubble fields, and the ploughed soil, intermingled in beautiful confusion and admirable disorder; while the softly greyish-blue hills in the west, south, and north ranged in the area fitly and well, and the soft bluish-black clouds overhanging all, diversified with the sunshine

breaking through in parts, made the sky as piquant as the earth.

"We examined Blawart to see if it was in any way a made hill. We found only that it might have been helped a little that way by the Romans or the canoe-men to make it a better outlook station; but it was essentially natural, the last part of a terrace which rose above the lower ground to the west. We recognised the edge of this terrace as an old coast line as we went Blairdardiewards, and we got proof of it in a drain cut in the rise of Blairdardie.

"Blairdardie had been much talked about and visited by the Glasgow naturalists, owing to an idea that had been suggested that the stem and roots of *Equisetæ* found in the clay possessed a latent vitality which caused them to bud and give off shoots when exposed to the diggings; and the work we set ourselves to was to discover some proof of this supposed latent vitality. We found some stems apparently imbedded in the clay, but we could not determine whether they had been deposited with the clay or not.

"We now walked Whiteinchwards, quietly talking all the way. Having Mr. Croll with us, our talk naturally drifted in his direction; and we discoursed of the eccentricity of the earth's orbit and its climatic effects as intelligently as we could, not knowing it so thoroughly as we could have wished.

"The only new thing that was elicited related to Mr. Croll's share in bringing out the periods when the eccentricity was great, and so, humanly speaking, dating the last Glacial epoch.

"Mr. Mahony quoted some London review or paper in which Mr. Stone was alone spoken of as having calculated the eccentricities, when Mr. Croll said in the quietest tone possible that it was an error, and that instead of Stone the paper should have said Croll, meaning thereby that it was himself that had done what Mr. Stone was getting credit for. The cool, quiet way

in which it was done struck me, and I could not help respecting Mr. Croll for it more than ever.

"No passion was evinced, no jealousy was disclosed, no chagrin even seemed felt by him at his honour being given to another. But, as if he was simply correcting an error in arithmetic or a *lapsus linguæ*, he substituted his own name for that of the other, and it was done. He afterwards said that all that Mr. Stone had done was that, at the request of Sir Charles Lyell, he had calculated the eccentricity of the earth backwards, and found that 210,000 years ago it was so great that, according to Mr. Croll's theory of the effects of such an eccentricity, a Glacial period was probable then; and that, after having done so, Mr. Stone stopped calculating, and read the riddle no further. Mr. Croll spoke of his having waited eight months, to see if Mr. Stone would count more periods, during which time he was idle, as it were at a standstill for want of raw material of facts to manufacture theory therefrom. At last he wrote to Sir Charles Lyell, asking if Mr. Stone was working at the problem, and received answer back that he had done nothing more, as the work was difficult and the results to him unexciting. Mr. Croll then commenced, became his own calculating machine, and brought out one period, 850,000 years ago, during which the eccentricity was at the greatest, which he conceived was the date of the last great ice age. He had afterwards calculated further and found out many other smaller periods, which I cannot here catalogue. He had also made forecasts of the future in the same way, and found that it would be 800,000 years ere another eccentricity would occur sufficient to glacialise the earth as before. The only other thing elicited was that, according to Mr. Croll, Leverrier had appropriated without acknowledgment the discovery of a comet-like orbit for the meteoric bodies which have a periodic term of 33 years, giving it forth as his own entirely, whereas it had been discovered or elaborated by a Roman astronomer some time before.

"We found our conveyance waiting for us at Whiteinch, and were speedily hurled home to Glasgow by it."

"*Excursion with Mr. Croll to Paisley Clayfields.*—We went to Paisley to-day to let Mr. Croll see the glacial shells *in situ*, he having a desire to verify their existence by eyesight. We left by the 4.35 train.

"We lost no time in making for our hunting ground. As we passed Glen Street, Mr. Croll, with a wistful look, said he once lived two years in that street, and liked it better than Glasgow. I asked him, however, if he remembered if the folk of that time, twenty-three years ago, 1843, spoke about the shells of the clayfields, which, doubtless, were then as now disturbed in working the clay. He said 'No, he remembered nothing,' so that they were not a world's talk then as now.

"While examining some heaps of glacial shells we found, I had to name and explain every shell to Mr. Croll as I gave them to him, which was rather a pleasure, as he was apt and appreciative.

"We now made headway for Westmarch Claywork, but were arrested on our way by seeing a pit in course of shanking, close by the wayside. We stepped aside instinctively, as we felt the full significance of such a sight too well to pass it by unimproved. We soon saw that they had passed through the brick clay bed and were fairly in grips with the boulder clay, unmistakable proofs of which were vouchsafed to us promptly. I soon realised its character by the striated pebbles amongst it, and pouched a few as mementoes, and induced Mr. Croll to do likewise. In no respect could I see any dissimilarity between this drift and that of the old 'hills' in Glasgow; only in Glasgow there are no rich shell beds above it, and to me that made a difference, as I felt that the Paisley drift was thereby glorified.

"We lamented that the working shankers were not about, as from them we could have got definite depths

for all the beds. That, however, Mr. Croll tried to mend by lifting a plank, looking into the abyss, measuring it with his eye, and making the depth from the surface to the water 18 feet,—a short enough section, I thought, and barely enough to demonstrate the long time which must have elapsed betweenwhiles; but then, crowded as it is with events, on second thoughts we realised the time better that way than a mere mass of dead clay devoid of events would have done. At Westmarch we found several large *Littorinas*, etc., to bag, which was a pleasure. I gave them all to Mr. Croll, as I wanted to interest him thoroughly in the glacial shells. He answered completely to my hopes, and became geological mad in mind and spirit, as a scoffer would say, and shouted a jubilant 'Eureka' over every new shell. It was on this occasion that Mr. Mahony, who came by a later train, told us of the death of Mr. Walter Crum,—a piece of news which deeply affected Mr. Croll."

"*Excursion with Mr. Croll to Boghall and Daldewie, to verify a story of a great bed of shells, which a borer told Mr. Croll occurred.*—There we found that some mistake must have been made, as we saw none, and the farmer to whom Mr. Croll appealed knew nothing of them. The only thing interesting is a note describing our return and our crack by the way. 'We now sought our straightest way home down London Road and its continuations. We discoursed of many things, and I profited much by Mr. Croll's crack,—he being of a higher grade of mind than myself, and of a higher range of study than mine. I realised in my experience the maxim, "He that walks with the wise shall be wiser"; and I got a wrinkle in geology which I shall be the better of in future. It related to a measurement of geological time by denudation rather than by deposition, because while the latter must necessarily be irregular, the former was probably constant, as constant as the rainfall was equal in quantity year by year. He instanced the Mississippi: the quantity

of sediment it carried down had been measured and found to amount to a foot in 1300 years from the area of drainage. Now, if we suppose the whole mountains of the earth levelled and spread over the surface, they would make it 1000 feet high, and denudation would at the Mississippi rate wear down this 1000 feet in 1,300,000 years, a mere fraction of time compared with the illimitable spaces of astronomy.

The following is an account of an excursion with Mr. Croll to the Allander Swallow Bank, written by Mr. Bennie, September 1867:—"Mr. Croll had suggested a trip to this bank, to see if we could determine whether the stony clay that tops this hillock was boulder drift or not, because, if so, it would give a glacial character to the great series of deposits we know to be associated with it. Accordingly we went there to-day. Our route was by 'bus to Canniesburn, thence by parish road to Allander and home by Possil. Coming to Canniesburn, we went along the 'Milngavie' road half a mile or so, and thence took up the road to Allander, which led us eastward. We saw that the country was very hillocky, large mounds rising on either hand, some of them very prettily crowned with trees, others with wheat or corn, over which the swallows sweep merrily like spirits at play. I eyed them stealthily, as I did not like to break in upon our talk with allusions to them.

We soon saw from our map that we were on the track of the Roman wall, and went over a stile to look at the remains of it. All we saw was a hollow trench 8 or 10 feet wide running along the ridge of a height that overlooks the Allander. Very emphatic was the notch in the ground, and one which was not easily reducible, unless purposely, by the same agent that made it—man—filling it up with the same instrument of denudation—the spade. I remarked that simple monuments were longest lived; ditches, or mound holes, or hummocks, beating obelisks, towers, or temples in resisting obliteration, and in preserving the remembrance of what they seek

to commemorate. For a millennium and a half this simple ditch had testified of man's handiwork, while how many elaborate buildings had in that time disappeared 'like the baseless fabric of a vision, and left not a wrack behind!' But I was soon absorbed in the great monuments that lay before and around us. The haugh of the Allander lay at the foot of this height, some hundred feet and more down. It was broad and flat; it was a ditch dug by nature, as clearly as the Roman wall was dug by man; and the hillocks were the wall she built in some former period, when it suited her purposes to gather together what she had aforetime scattered.

"We now passed the deepest bore in Mr. Croll's list, that made by the Summerlee folk in the farm of Millikhen, some 355 feet deep, the particulars of which we have sure, but as we cannot interpret the terms into geological language, the information is not so satisfactory as we could wish it to be. It is situated in a hollow between two high hillocks, which rise above it some 50 or 60 feet, giving a probable depth of surface to this locality which seems astounding and almost unbelievable, were it not confirmed by other bores of proximate magnitudes. One of these hillocks is planted with trees, rises very abruptly, almost like a wall, and looks very picturesque in the landscape.

"Nothing further remarkable was noticed till we came to the goal of our walk, the Swallow Bank at Allander Toll. This I had seen many years ago; but I had never inspected it geologically, and so missed a good opportunity of discovering something great in geology. Mr. Croll had visited it some few weeks ago, and was struck by the hardness of the red clay at the top, which he thought might be boulder clay from its hardness. He said he blistered his hands trying to extract a few pebbles. He brought some of it home: I washed it, but could not think it boulder clay, as I had found no striated pebbles in it. The sand was red, and unlike that which I had been accustomed to wash out of

Glasgow boulder clay. I therefore said I was afraid it was simply a gravelly hillock.

"The fine sand I got from him at the same time needed no washing, so pure was it from dirt; and I riddled it as I got it. On inspection I found no life remains in it, and saw it was as like the fine white sand of Windmill Croft as it could be. The matter rested not with these negative conclusions, as Mr. Croll said he saw plenty of striated pebbles; and this remaining in his mind, resulted in our visit of to-night to make sure.

"The Swallow Bank consists of the end or side of a great hillock which overhangs the haugh of the Allander, and is composed of a great bed of a fine white sand with a great bar of red clay atop. The Allander has eaten into the side of it for many years, carrying away the sand; and, of course, the clay topples, and a sand cliff is formed thereby. The river makes a half circle nearly at the base of the bank, and is instrumental in making it as steep as possible, and plain to the most uninitiated mind. The swallows have found that the white sand is dry and warm, and they have holed it with nests till parts of it are like a riddle.

"We first tried this white sand, and found it to be distinctly and emphatically laminated, the laminations, however, not being straight, but undulating as a ripple mark, *e.g.*~~~~~~~~~~. The sand was fine and soft and agreeable to touch. We saw no pebbles or grit in it, only the laminations darker and brighter. We next attacked the clay atop, and found it softer than when examined by 'one of us,' as we could cut into it easily and detach pebbles readily. We critically examined every pebble as we took it out, but failed at first to find striæ upon them, though we had little doubt they were of glacial origin; for all were more or less humpbacked, and that so emphatically that I was astonished at the persistency in which this form turned up. Our hope of certainty revived, and we thought we would be able to settle our problem to-night. I now bethought me that I might

settle the question quicker if I turned to the pebbles that had weathered out and were still lying scattered about. I therefore stopped excavating and commenced gleaning; and I speedily had enough of well-marked pebbles in my hand to settle the question. Some of them were as decidedly striated as they could be, others were very faintly marked, but still sufficiently so to indicate the cause. We therefore had no hesitation in coming to a decision that it was boulder clay; although, from the absence of limestones and other scratchable rocks, the proofs were not so emphatic as our Glasgow boulder drift gives to us. We tried to measure the thickness of the stratum of boulder clay, and roughly guessed it to be 12 feet. In some parts lower down, a soft red clay like brick clay mingled with the stones. Other places were visited; but I shall not describe them.

"We had a long walk home in the dark, but it was not dreary, for Mr. Croll enlightened it with short expositions of astronomical and other phenomena as they occurred to him or me, and the time passed delightfully by. I will not attempt a record of our crack, as I feel unequal to it; but one theory I may note which we worked out in our crack. It was that the Glacial period was not one simple and indivisible time, as we had been in the habit of regarding it; but that it was composed of several periods, with great breaks between; these breaks being mementoed by the great beds of sand, laminated, ripple-marked, or amorphous, as may be the order of their disposition, giving evidence thereby that they were formed during an unfrozen water period, as decidedly as the boulder clay gives evidence that it was formed during a period of ice. We brought home a number of pebbles that we could not wash at the bank and washed them at home. About half of them are striated and the other half moulded by ice.

"This would be the last excursion I had with Mr. Croll ere he left Glasgow for Edinburgh to join the Survey."

CHAPTER X

APPOINTMENT ON GEOLOGICAL SURVEY

IN order to give a connected account of the production of the paper on "The Physical Cause of the Change of Climate during the Geological Epochs," which appeared in the *Philosophical Magazine* in August 1864, and of the brilliant series of original investigations and researches embodied in various papers on the same subject which followed on that, we have been obliged to depart to some extent from the chronological order in the personal narrative. We now, however, resume the biographical sketch.

Croll's papers on "Glacial Submergence and Climatic Changes during the Geological Epochs," as well as his other able and interesting papers on equally abstruse subjects, had naturally attracted the attention of distinguished scientists both at home and abroad. It is a trite saying that a prophet has no honour in his own country; but Croll's case belies that saying to a certain extent. Early in February 1867, the Geological Society of Glasgow elected him as an Honorary Associate of the Society. But more eminent men were beginning to recognise the value and importance of his work.

Among the first to express a favourable opinion of the theory of the cause of the Glacial epoch, which he had advanced, were Professor (afterwards Sir Andrew) Ramsay, who was then Director of the Geological Survey of England and Wales, and Dr. (now Sir Archibald) Geikie, who had then been appointed to the Directorship of the Scottish Survey on the reorganisation of the service. At that time a large addition to the staff of that Survey

was required; and Croll was asked if he would be willing to allow himself to be nominated for the Scottish service. In the estimates to be laid before Parliament for the year 1867–1868, it was proposed to make a large increase to the grant for the Geological Survey. The staff in Scotland was to be augmented and made a separate branch of the service. If these arrangements were approved by the House of Commons, a number of new men would be required for the service.

The new men, it was understood, were not to be in the Civil Service, and therefore would not have any Civil Service examination to pass, besides which, no limitation as to age was to be specified.

Although the new men were not to be taken, at least at first, into the permanent staff, there was no doubt that in actual practice they would be as permanent as the others, more especially if they proved good men. The proposal made was that Croll was to take field work first, was not to be on the permanent staff, and was not to be subjected to any examination. This offer was in many respects a tempting one; but as Croll was then somewhat up in years, and suffered a little from the mishap to his head, he did not see his way clear to accept the proposal. Moreover, he felt that his health was not adequate to field work, and, with characteristic modesty, he did not feel satisfied that his qualifications were equal to the scientific duties which would be required for the proper performance of the duties of the office, and he therefore reluctantly felt obliged to decline this proposal. Sir Archibald Geikie, however, knew the value of the man, and not only was anxious to secure so able and conscientious a scientist for the Survey, but also was desirous, after he had learned the nature of Croll's situation at the Andersonian, to improve his position, add to his comfort, and get him near to a good library, where he could carry on his original work.

It appears that Government intended to authorise the new arrangements to begin with the new financial

year, that is, with the 1st of April 1867; and it was necessary, therefore, to lose no time in making the necessary arrangements.

Out of the increased staff proposed to be given in Scotland, it was expected that it would be necessary to have one man stationed in Edinburgh to act as Secretary and Accountant, and as the official representative of the Survey at the Scottish headquarters. His pay would begin at 7s. a day and rise to 12s. (including Sundays and a month of holidays), with the prospect of rising ultimately to £350 a year. The work, it was expected, would be very easy, and at first would consist in forwarding letters to the geologists in the field, ordering maps from Southampton as they were written for by the field surveyors, keeping the accounts, and seeing that each man's accounts went in properly at the end of the quarter, and generally looking after the Survey in Edinburgh and the wants of the men in the field. Dr. Geikie, then, with characteristic kindness, wrote and asked Croll if he would be willing to accept that appointment. This very materially altered the condition of things. Croll, however, felt reluctant to undertake duties which required much mental work, as, owing to the state of his head, it might materially interfere with the private work on which his mind was so much set. But, on the other hand, as his salary at the Andersonian was so small, and his health at the same time suffered a good deal during the winter months from the cold draughts in the lobbies of the Institution, he felt that, taking all things into consideration, he ought not to lose the opportunity of improving his circumstances. Contrary to original expectation, however, the new men, or at least a number of them, were to be considered Civil Service men, and the office for which Croll was intended was one to be included in the Civil List. It was therefore necessary that the entrance examination for the Civil Service should be passed. This was a serious consideration for Croll, and he was very reluctant to

attempt it; but "my Lords" were not inclined to make any exception, at least on the score of exceptional ability, fitness, or distinction in science. Croll was therefore in the dilemma that he must either attempt an elementary examination, in which he knew he was almost certain to fail, or relinquish the chance of improving his position by joining the Survey. He elected to try the Civil Service examination. As he himself expected, and as every sensible man, knowing his constitutional nervousness, his early education, or rather want of education, his utter want of experience in examination ordeals, and his advanced age, could have foretold, he failed in some of the subjects. No shame to him that he should fail. The stigma was rather that he should have been asked to undergo a boy's examination. It makes a Scotchman's blood boil to read that a man who had made such a reputation for himself as to be practically pressed to accept a post on the Geological Survey, and who was undoubtedly recognised to be a man of genius, should be made to undergo the humiliating farce of an elementary entrance examination, only to be rejected for not being able to do at his time of life what modern schoolboys could. As it was, however, Dr. (now Sir Archibald) Geikie, greatly to his credit, cast the rigid rules of Civil Service to the winds, and insisted on Croll's appointment to the Survey. In this he was most cordially and ably assisted by Sir Roderick Murchison, who had also come to recognise Croll's genius, and was anxious to secure his services for the Survey. "My Lords," having taken into account the special recommendations made in Croll's favour, as Lord Kelvin pithily puts it, "accepted his great calculations regarding the eccentricity of the earth's orbit, and the precession of the equinoxes during the last 10,000,000 years as sufficient evidence of his arithmetical capacity, his book on *The Philosophy of Theism* and numerous papers published in the scientific journals, as proof of his ability to write good English," and

magnanimously authorised his appointment at once as a member of the Geological Survey of Scotland without any further examination, his salary to begin from the day on which he joined the Survey. The result was, that shortly afterwards he received his appointment, accompanied by a Civil Service Certificate with the privilege for his advanced age.

Croll thereupon removed to Edinburgh, and entered on his duties on the Geological Survey of Scotland on 2nd September 1867. At first he and his wife resided in lodgings, till they could look about for a house adapted to their requirements in a suitable locality. As already explained, Croll's appointment to the Survey arose out of a reorganisation of the Service which took place in 1867, when the Geological Survey was disjoined from that of England and formed into a separate branch. The staff consisted of Director, District Surveyor, two senior Geologists and six assistant Geologists, with an office in Edinburgh. Mr. Croll was selected to be office-keeper, with the rank of assistant Geologist. A vacancy occurring in 1869, he was promoted to the rank of Geologist.

The office work was—(1) Ordering copies of the six-inch Ordnance maps; dividing and preparing these for use in the field; keeping a register of their allotment to the several surveyors; superintending their transference from one to another, and their return to the office when the ground was surveyed. (2) When the survey of any area was completed, and the map ordered for publication, dry proofs were obtained from Southampton and given to the surveyors to have their work transferred to them, one inch or six inches as the case might be, and sent to Southampton to be engraved. When proofs came back from the engraver, they were sent to the various surveyors, whose work was on them, to be checked (sometimes three or four proofs were necessary ere they could be passed as correct), all which Mr. Croll conducted. (3) Colouring of maps. The maps, when

engraved, were coloured by the surveyors, one copy as original pattern copy, and one copy as colourist's pattern. But as the colouring was done by hand, every copy as it came back from the colourist had to be checked; and this work, which was done by Mr. Croll, was constant to keep the stock ready for sale. Horizontal sections which are coloured passed through the same processes as the maps; but vertical sections, not being coloured, only passed through the processes of engraving and printing. (4) Sale of maps. The stock of maps for sale was kept in the office, under Mr. Croll's charge, and given out to the agents on demand; a record was kept in the ledgers, and reports were made to the Stationery Office at the end of each year. (5) Accounts of the Survey. The expense incurred by the surveyors in travelling was repaid by the Treasury each quarter, and certified accounts duly checked and sent. Incidental expenses—tradesmen's bills, etc.—also had to be checked and sent each quarter. (6) Stores. All the instruments and material necessary for field work were supplied by the office, and a record kept. (7) Colouring of MS. maps. All maps not ordered for publicatio.. were copied and kept for reference in the office. The colouring was generally done by the surveyor who mapped the ground, but often done by others who had time in the winter. Mr. Croll also copied some, at slack times in the office work.

With his usual characteristic modesty and brevity he says regarding these duties: "I found, as I had expected, that the duties of the office were not at all laborious, either physically or mentally. They consisted simply in attending to the various details of office work, namely, conducting the correspondence with the men on the field, the supplying them with the necessary maps, instruments, and stores, correspondence with the engraver, colourist, and Ordnance Survey, the checking of maps, the keeping of the various registers, etc. etc. These various duties kept me busy during office hours, without

producing mental exhaustion. The only thing I suffered from was now and again having to write two or three letters in succession. The Director, Mr. Geikie, I found to be a most agreeable person. This was all along a great comfort to me. During the thirteen years we were together in the office, never as much as an angry word passed between us." His duties as Resident Geologist did not really require much acquaintance with the science of geology, and this relieved his mind from having to study a science for which he had no great liking, and allowed him to devote his whole leisure to those physical questions in which he was engaged. There was, however, one department of geological inquiry the physical questions of which he was engaged studying at this time, and with which it was necessary that he should be acquainted, namely, Surface Geology, or Drift in its bearings on Glacial and Interglacial periods. He had begun his studies in that department before he left the Andersonian, and had made frequent excursions into the country in search of glacial phenomena. These studies and excursions, with the results thereof, will be dealt with in the next chapter.

Although the hours of business in his new occupation were not long,—being only from 10 A.M. to 4 P.M., and the work not laborious, he found that in the evening he was not in such a fresh condition to begin his private studies as he was when in the Andersonian. This at times made him feel some small regret that he had left that institution. These six hours of mental work, comparatively light though it was, added to his private studies, began ere long to tell prejudicially on his head, and he had to adopt every means he could devise to husband his energies. He was thus obliged to adopt a mode of life which practically shut him off from all social intercourse.

Shortly after he went to reside in Edinburgh, he formulated a scheme for the regulation of his life, to which he adhered pretty closely during the whole time

he was in the Survey. After being in lodgings in Edinburgh for a short time, he took a small house at Jordan Bank, Morningside, at the southern extremity of the city. When he came home from the office, which was generally about five o'clock in the afternoon, he took dinner and rested thereafter for about an hour. He then took a stroll into the country. A few hundred yards beyond Morningside, the main road separates into two branches, which branches, after diverging for about a quarter of a mile apart, again unite about a mile and a half beyond the place where they separated. His most frequent walk was to go out by the one branch and return by the other, which gave him a circuit of about three miles. As he walked very slowly, and employed the whole time in observation and study, he was generally out of doors for one hour and a half, or two hours. At other times he would make an excursion to the Braid Hills or to Craiglockhart. As the beauties of nature, especially in quiet retirement, had a special charm for him, he found the outdoor walks helpful to study. He rarely took a companion with him in these walks, as doing so not only deprived him of the opportunity of getting on with his studies, but the effort and excitement of talking, to a considerable extent, unfitted his head for mental work on his return.

In these walks he generally carried a pencil in the one hand and a bit of paper in the other, on which he jotted down the ideas as they suggested themselves; or otherwise made such jottings as enabled him either to keep a record, or such a note as would recall them to his memory on his return home. He then wrote out and expanded these ideas more fully, and, to save his head, he obtained the services of a young man for an hour each evening, who either wrote for or read to him, as occasion required. This generally ended the labours of his day. If, however, he felt that he was still in a working condition, he would continue his studies quietly for another hour.

His niece, who stayed with him for some time about

this date, says: "He was very fond of long walks, and often went alone; but generally I accompanied him, as my aunt was not able to walk any distance, and during our walks, should any thought occur to him on any of the subjects he was writing or thinking about, I had to write it down there and then, and sometimes I would grumble, as it was of no interest to me. He would add, 'Well, if you don't, it may never occur to me again.' This has even happened sometimes in a *tram-car*. He always carried paper and pencil when we went away any distance or for any time. He could not be idle. While I was chattering about general topics, he was busy all the while at his own theories. Yet I never seemed to disturb him, so intent was he. He was kind and generous to a fault, and I found him a very agreeable companion. He was an early riser. Early to bed, early to rise, was his maxim. He had a most wonderful memory. He seemed to me as if he never forgot anything. He was orderly and very correct. He could go to his library at any time for any book, paper, or pamphlet, as the case might be, and without any trouble whatever lay his hand upon it. He used to say he had a place for everything, and everything had to be in its place. Never any confusion. If we were going a journey on a holiday, all had to be ready the previous night, so that there would be nothing left for the morning. He had a great admiration for trees, and, indeed, nature in every form.

"He was very much interested in Sir Thomas Dick Lauder's wonderful account of the Moray floods. When on a visit to Forres, we took him up the banks of the Findhorn to see some of the places mentioned on the Divie and the Norloch. He gave special attention to Randolph's Leap, where the Findhorn comes into such narrow space. He was in raptures with the scenery all down, and several times expressed his great astonishment at how nature had so obliterated almost every trace of the terrible destruction of flood.

"He was a great admirer of Sir Walter Scott, and

was particularly fond of *The Heart of Midlothian.* Although he had read this work previous to the time I was there, he bought me the book and listened to my reading it over again; and, in order to interest me and make me understand it, he took me to all the various places in Edinburgh in connection with it, such as Jeanie Deans' cottage, and several old houses in the High Street. He was very fond of visiting places of any historical interest which it was in his power to reach."

CHAPTER XI

INVESTIGATIONS INTO SURFACE GEOLOGY— CORRESPONDENCE WITH MR. BENNIE, 1867–1868

AS we have already seen, Croll was engaged in making a series of investigations into Surface Geology, or Drift in its bearings on Glacial or Interglacial periods; and that, for this purpose, he had made many excursions round about Glasgow, to river-beds, glens, and the sea-shore, personally investigating the phenomena to be there seen bearing on the subject. As he could not conveniently continue these, so far as Glasgow and neighbourhood were concerned, he got Mr. Bennie to follow them up and report results. Croll himself, however, carried on the same line of investigation round about Edinburgh; and on this subject the following interesting letters were written to Mr. Bennie.

EDINBURGH, *11th November* 1867.

DEAR MR. BENNIE,—Many thanks for the papers and circular with which you are so good as to favour me. I have to thank you also for your letter of 2nd inst.; and I shall try to get the information about the position of the shell referred to by Hugh Miller, but it may take some little time, for I have as yet made no acquaintance in Edinburgh, neither have I done anything out of doors. The days at present are so short, that by the time I get dinner over, darkness is on. When the days become somewhat longer, which will not, however, be for some months, I purpose to begin outdoor work on the surface deposit around Edinburgh. Did Messrs.

Young and Armstrong observe the direction of the striation on the rock at Beith, and the quarter from which the ice appears to come?

I shall be glad to hear now and again how you are getting on in the field.—Yours truly, JAMES CROLL.

EDINBURGH, *2nd December* 1867.

DEAR MR. BENNIE,—I have again to thank you for a copy of the *Herald* and a notice of the Society's next meeting. Mr. Geikie's lecture appears to be a very interesting one. Things with me here are moving on in their usual quiet way. I was down at Leith the other day. A dock something like the one at Windmill Croft, Glasgow, is being formed there at present; about 15 or 20 feet of sand has been dug out, and the workmen are digging into the boulder clay which underlies the sand. The sand is full of shells down to the till. I collected a dozen or two of them. When I get a little spare time to examine the contents of my parcel, if anything of interest casts up, I shall send it through.—I am, yours truly, JAMES CROLL.

EDINBURGH, *17th December* 1867.

DEAR MR. BENNIE,—I am much obliged to you for your long and interesting letter. I have taken a note of what you have observed about the till over the gravel at Langbank, and also of the useful information about how to wash the clay and look for the shells. I have been at Leith Docks to-day, and have collected a few things. Shells are very abundant amongst the sand and gravel. There is somewhere about 20 feet or so of fine sand at the top, containing two or three thin beds of gravel. Below the sand is a bed of gravel a few feet in thickness. At the bottom of this bed, directly on the boulder clay, shells are found in abundance. In a small box, which I shall forward you at the shop to-morrow, you will find a sample of the gravel and the shells it contains. The shells are so abundant that I suspect that they are not

of much interest. But, however, you can look over them; and if you should happen to meet with anything which is of value in a geological point of view, you will perhaps let me know.—Yours truly,

JAMES CROLL.

EDINBURGH, *7th January* 1868.

DEAR MR. BENNIE,—I am much obliged for your long letter with all the particulars of the nature of the deposits in the Leith dock. The result that you have come to is what I half expected. The shells appear to be far too numerous to be of ancient date. It is not all lost labour. It will afford you an idea of the general character of the shells of the coast. Mr. Archibald Geikie says that he visited the Portobello clay beds along with Hugh Miller. He says that the shells were in the same bed as the vegetable drift. The *Scrobicularia* were found in the position in which they had died. The place, he says, seems to have been an estuary in which the tide advanced and receded. And the vegetable remains had evidently been carried down by a burn or water running into the estuary. If there is anything further you would like to learn regarding the matter, I shall ask Mr. Geikie. I shall be glad to hear from you at your leisure.—I am, yours truly,

JAMES CROLL.

EDINBURGH, *14th January* 1868.

DEAR MR. BENNIE,—Many, many thanks for your long and most interesting letter. I shall certainly write to Mr. Dennistoun, and request him to send me all his surface bores. I got a peep into the Bore Book the other night, and have extracted a number of interesting surface bores. There are some of the localities strange to me, but I shall try and find their whereabouts and let you know. I should like exceedingly well if you would draw up a short paper on the subject of these deposits which we have examined with so much interest of late.

The facts are so remarkable and striking, and, besides, they have such an important bearing on the history of the Glacial epoch, that they ought to be made generally known amongst our geological friends. A short paper, or a long one if you can find time, on the subject might be the means of interesting some of the able and energetic members of the Glasgow Geological Society to direct their attention more exclusively to the surface geology of the neighbourhood. Certainly there is not a spot in the kingdom where the surface deposit may be studied to better advantage than around Glasgow. I intended to have a short paper myself on the subject this winter; but I have again got so immersed in the question of climate that the thing is impossible, and, besides, you know the subject far better than I do. I am glad to learn that my good friends in Glasgow have not forgotten me.—Yours truly,

JAMES CROLL.

EDINBURGH, *17th January* 1868.

DEAR MR. BENNIE,—Mr. Armstrong has kindly sent me a very interesting letter which he had received from Mr. Craig, Beith, giving an account of hazel nuts and stems which were found in the Boulder Clay. If you have not seen the letter, by all means get a look at it. I showed it to Mr. James Geikie. He believes that the clay overlying the bed containing the hazel remains is *true till.* He has often found *beds* of *peat* in the Boulder Clay in Ayrshire, and he thinks that there is perhaps not a valley in that county where evidence of an old land surface in the shape of peat beds in the true Boulder Clay may not be found, if it is properly examined.—I am, yours very truly,

JAMES CROLL.

EDINBURGH, *28th January* 1868.

DEAR MR. BENNIE,—I am delighted to hear that we are to have a paper from you on the surface deposit around

Glasgow. You need not trouble sending through the paper to me, unless you like. Only let me know before it is read, and I shall write to the editor of the *Herald* and request him to print it in full. Do not make any allusion to me in your paper, further than merely to say that I had been at a good deal of trouble in collecting particulars as to the bores which led to the discovery of the deep trough filled with sand and gravel and capped with till, lying to the north-west of Glasgow. Beyond this I have no merit in the matter. Everything else about the subject I learned from yourself and Mr. John Young. If you can manage to write out the paper without referring to my name at all, so much the better. Express in the paper our thanks to iron and coal masters, mining engineers, managers of pits, borers, and others who have so kindly allowed us to copy their journal of bores. You may wax eloquent on this point if you choose. We have just got in a cargo of six-inch maps. All the Dumbarton sheets are in this cargo, I think. I shall look if I can find the height of Garscadden House above the sea level, and also that of the high ridge on which the Roman wall is seen beside the deep surface bore at Millikhen farm. If the surface deposit were removed all around Glasgow, those two spots, so far as we yet know, would be the deepest part of your sea loch which would then be formed. Please to give us some of your beautiful poetic musings on the banks of the ancient loch. Let us have a panoramic view of Ben Lomond in the distance, covered with perpetual snow, and long glaciers crawling down the valley of Strathblane. I often in imagination visit that strange spot; but, unfortunately, I cannot describe what I see in that poetic style, in which you so often indulge. I can feel as a poet, but cannot write as a poet. I read with much interest your musings at Craigielea. Near that spot I have spent many a pleasant hour in retirement. There is a strange episode in my life connected with that place. That was in my metaphysical days, when I did not

know what a glacial shell meant. Mr. Neil Robson has neither answered my letter nor sent me the bores. I have written to Mr. Dennistoun.—I am, dear sir, yours truly, JAMES CROLL.

EDINBURGH, *29th January* 1868.

DEAR MR. BENNIE,—The surface of the bore at Drumry farm is 68 feet above the sea-level. The surface is 298 feet in thickness: consequently, were the surface deposit all removed, the sea would stand here 230 feet in depth. The knowe on which Garscadden House is built rises 78 feet above the bore. It is, therefore, probable that the surface under the house is 376 feet in depth. The highest part of the road beside the bore at Millikhen is 214 feet above the sea-level. The bore itself is, however, in Stirlingshire; and, as the maps for that country are not to be seen at present, I do not know the level of the bore. Say that it is 80 feet below the level of the road: this would make the surface of the bore 134 feet above the sea-level, and the bore 355 feet in depth of surface. This gives 221 feet as the depth of the sea at this place, and 435 feet as the probable depth of the surface under the Roman wall. I find that I have got in all 190 bores. I have a few that you have not got, but they are not of much interest. I shall send them along with some others that I expect in a week or two. Please to remember me to our friends Mr. M'Cartney and Mr. Mahony, when you happen to meet them.—I am, yours truly, JAMES CROLL.

One of the bores at Walkinshaw has 159 feet of surface. The surface there is 17 feet above the sea-level. This would give 142 feet of water there.

EDINBURGH, *15th February* 1868.

DEAR MR. BENNIE,—Never apologise for sending Me a long letter; and as for the penmanship, I shall let

you know when I cannot decipher it. I am glad to learn that Dr. Young did me the honour to criticise my notions about the method of measuring geological time from denudation. I may here state that my calculations were based on observations made on the amount of mud carried down by the Mississippi. The observations were made about the year 1847 or 1848. The estimate I adopted of the quantity of the mud has been found to be too great. Since then, most elaborate investigation and observation have been made by Humphreys and Abbot for the American Government; and the result of their researches is embodied in a large folio volume which Mr. Geikie showed me the other day. Calculating according to the data thus given, about the thickness of a sheet of paper is being removed off the face of the American continent every two years. Surely this estimate will satisfy you. It takes upwards of four thousand years to remove 1 foot. But in 40,000 years 10 feet would be removed. In 400,000 years 100 feet would be taken off, and in 4,000,000, 1000 feet, or the entire continent, would be carried into the sea. Now, if the general level of the country is being gradually lowered at the rate of 1 foot in 4000 years, at what rate are the valleys being deepened? Surely a river will cut its channel at a rate at least ten times greater than that of the gentle rain which lowers the general level of the country. If so, then our (the American at least) valleys must be deepened at the rate of 10 feet in 4000 years; 100 feet in 40,000 years; 1000 feet in 400,000 years. A valley might be cut down from the top of Ben Nevis to the sea-level in less than 2,000,000 years. I am unable to perceive any flaw in the mode of reasoning. The rate at which our rivers are carrying down the land into the sea is the rate at which the country is being denuded by sub-aerial agency. Ascertain the rate, and you have not only a measure of the rate of denudation, but also a measure of geological time. Of course, it is assumed that rain and rivers existed the same in past

ages as now. I pointed out this to Professor Ramsay several years ago, and he appeared to concur in my opinion. I am happy to say that this view of the subject has been adopted by our Director; and he has been already using the argument with effect against those who, like the Duke, despise the denuding power of "gentle rain from heaven." The great objection to sub-aerialism is that it demands too long time to do its work. I am no prophet, but I shall venture to predict that, before a twelvemonth is past, the complaint will be that it does the job too hurriedly to allow monkeys to turn into men. We have just ordered the Stirlingshire maps; and I shall be able in the course of a few days to give you the height of Langbank above the sea-level. Pardon me for having neglected to ask after the whereabouts of Sandy Croft. I shall try and find out on Monday whether he be a countryman of ours or not. Mr. M'Lelland, the mechanic at the Andersonian, has my small book out of the library. He should have returned it long ago. Please to tell the librarian, Mr. Ferrie, to look after it. Please to remember me kindly to Mr. Ferrie. I am glad to hear of Mr. Mahony's discovery, and also to hear that Mr. M'Cartney has taken unto himself a wife. I am much obliged to you for directing my attention to where I can find Professor Heer's paper. Hoping to have another long letter from you soon.—I remain, yours truly, JAMES CROLL.

P.S.—I am truly sorry to hear that Dr. Young is so poorly. Mr. Geikie's lectures at the Museum were the best lectures I have yet heard on physical geology.

EDINBURGH, *3rd March* 1868.

DEAR MR. BENNIE,—The Stirlingshire six-inch maps are at hand this morning; and, from them, I observe that the height of the road at Langbank is, west of farm, 123 feet; east of do., 131 feet. The 200-feet contour

goes round the hill in an oval form. Height on top of hill 246·3 feet. Don't worry your life out about the paper, if you are limited for time and busily engaged otherwise; but if you can find time to draw up something on the subject, there is none that can do it better than yourself. As soon as the days are an hour or two longer, I hope to see a little of the country around this place. By the bye, I hear that Professor Sir William Thomson was giving you a lecture on the physical causes that limit geological time. Perhaps you will oblige me with a copy of the *Herald* containing an account of the lecture, as soon as it appears. I am working at this very subject just now, and have got some curious results. Thanks for the prospectus of the Society.—I am, yours very truly, JAMES CROLL.

EDINBURGH, *5th March* 1868.

DEAR MR. BENNIE,—I am sorry to have to inform you that our friend, Sandy Croft, who is in possession of such a store of bricks, is a Welshman. His address is Queensferry, Flintshire, Wales.—Yours truly,

JAMES CROLL.

EDINBURGH, *5th March* 1868.

DEAR MR. BENNIE,—Thanks for your letter of yesterday. So far as I can learn, Professor Thomson did not advance anything new in his lecture. He is in no way indebted to me for the idea of determining the temperature of bores. When I heard that he was engaged at this work, I sent him the journal of the bore at Fairfield. The pamphlet is intended for yourself. Mr. James Geikie got a few copies from the author to present to geological friends. I am glad to hear that you are to go into the subject of the ground deposits. I sent a copy of Mr. Craig's letter to Sir Charles Lyell, and also a copy of one or two of our curious bores. I enclose his letter in reply. It will show how much he is interested in the

matter. You may return the letter along with the communication with which you have promised to favour me. I need hardly say that I feel almost absolutely certain that Professor Thomson's view in regard to the age of the globe is much nearer to the truth than those of geologists in general. I hope that Mr. Geikie's lecture will satisfy you that *denudation* gives no warrant to the ordinary notion of extravagantly long time. If geologists would just condescend to calculate the rate at which the country is at present being denuded, they would feel somewhat surprised at the amount of change that must be effected in any such small period as a million of years. In this respect, physical geology and physical science agree. But perhaps your arguments, when I receive them, will cure me of this geological heresy.—I am, yours truly, JAMES CROLL.

EDINBURGH, *28th March* 1868.

DEAR MR. BENNIE,—I had a talk with Sir William Thomson this afternoon about the bores which he is busily engaged with at present. I told him that you could give him a good deal of information concerning bores around the city, as you were so perfectly familiar with the geology of the place. He stated that he was anxious to get some one to assist in taking the measurements of temperature. I said that perhaps your engagement would prevent you being able to do that, but that, perhaps, during the summer evenings, you might be able to do something in the close neighbourhood of the city. If it was to add a good few shillings a week to your income, perhaps you might think about it; but, of course, you will know best yourself whether such a thing would be suitable; only I thought there would be no harm in him becoming acquainted with you. He is a very kind-hearted man. The *heart* is not so hard as the *head*. Somehow or other I love that sort of people. By the way, how did you relish our Director's lecture? He was telling me how splendid a meeting he had, and

the still more splendid discussion at the close. I trust that he satisfactorily met your objections to Mississippi cutting its banks. Many thanks for your long and deeply interesting letter. I should have written you, but I have been so very busy at home of late that you must excuse me. I have not got time to read the small book you were so kind as to forward; but I shall do so shortly, and write you regarding the matter of its contents. I have received the second volume from Lyell, but I have got little read as yet. I have read Professor Heer's remarkable paper on the Miocene flora of the Polar regions. I observe that he gives Mr. Stone the credit of some of my work; but this is of little importance. I can easily see, from the nature of his remarks, that he has not read any of my papers, at least not the last one. I am working away just now on the probable date of the Miocene period. Things are turning up better than I anticipated they could possibly do. We have had a most delightful winter here, plenty of wind, as you saw by the *Scotsman*, and I can vouch that the *Scotsman's* account was not extravagant. It was something fearful: but we have had but little rain and a clear sky overhead. You people in the West, who consider that you have a vested right in all that comes from the Atlantic, rob every cloud as it passes by, and won't let a single drop come our way; but, for my part, I do not care. When is your paper to be ready? Mr. Geikie stated that Mr. Dougall had found that the Clyde, at Rutherglen Bridge, has, during the flood, $\frac{1}{800}$ of sediment in suspension. Is it $\frac{1}{800}$ in weight? Is it our friend Mr. M'Dougall of the Geological Society? If so, get the particulars from him.—I am, yours truly,

JAMES CROLL.

EDINBURGH, *1st April* 1868.

DEAR MR. BENNIE,—I have this morning sent you, in a parcel by rail, about a dozen of striated stones, which Mr. Young and I dug out of the Boulder Clay overlying

the sand at the Swallow Bank at Allander Toll. They are addressed to you at the Andersonian. They might be of some use to you on Thursday; if not, it is of no consequence. Much obliged to you for your letter. I shall write you again regarding your argument. The thickness of the deposits, I am working at that subject just now. I shall write to the *Herald* to give you a good report.—I am, yours truly, JAMES CROLL.

EDINBURGH, *7th April* 1868.

DEAR MR. BENNIE,—Your interesting manuscript came to hand yesterday. I have not as yet had time to read it carefully; but, from the glance I gave it, I had no difficulty in seeing that it is a most admirable paper, and that, when published, it will excite a great deal of interest. I wrote to Mr. M'Allister to try to get the *Herald* to publish it entire. I also wrote to the editor; but I fear it will be too long for them. The longer the better for geology, however. If the *Herald* does not want the MS., and you can spare it for a few days, I shall have an opportunity of studying it carefully. I shall be very busy till the end of the week.—I am, yours truly,

JAMES CROLL.

P.S.—You need not return the stone which I have sent.

EDINBURGH, *9th April* 1868.

DEAR MR. BENNIE,—I am glad to learn from Mr. M'Allister that your most interesting paper is to be published in the *Transactions*, "On the Surface Geology of the District around Glasgow, as indicated by the Journals of certain Bores," by J. Bennie (*Trans. Glas. Geol. Soc.*, vol. iii. p. 133), and he requested me to send the MS. to him when I have read it. I have had a long and most interesting letter from our friend Mr. Dougall, regarding the sediment of the Clyde. I shall see if Mr. Geikie knows anything about the shell beds.—I am, yours truly, JAMES CROLL.

EDINBURGH, 13*th April* 1868.

DEAR MR. BENNIE.—Many thanks for sending me the *Heralds*. Being on the outlook, I had to go to the book-stall at the railway station and purchase a half-dozen copies on Saturday morning. So, with your supply, I am enabled to send my geological friends a copy. I trust that you will allow me to pay for the copies you sent. The abstract is admirably written, and the editor deserves thanks for allowing so much space. If Mr. Armstrong sends a report to the *Geological Magazine*, would you please to tell him to make the slight addition which I have indicated in the enclosed slip from the *Herald*.—I am, yours truly, JAMES CROLL.

EDINBURGH, 17*th April* 1868.

DEAR MR. BENNIE,—In the hurry of sending off your MS., I forgot to delete the sentence, somewhere near the beginning, where you speak in a disparaging sort of way of your own labour in some such way as this: "all I have got to do is simply to state," etc. This is not correct, and please to erase it. I asked Mr. Geikie about Castlecary. He said that he had never been at the place, and never heard anything about it.—Yours truly,

JAMES CROLL.

EDINBURGH, 17*th April* 1868.

DEAR MR. BENNIE,—By all means send copy to Mr. Jamieson. Many thanks for your information regarding Mr. Wunsch's experiment. I herewith, under another cover, return the MS. About two years ago, Mr. James Russell gave me details of three rather important pit sections about Peep-o'-Day. You might allude to these. They would tend to strengthen your position, seeing that they were examined, while the shafts were being sunk, by one who knows so much about geology as Mr. Russell does. I enclose the

particulars in a slip, which you can arrange as you think best.—I am, yours truly, JAMES CROLL.

I shall send a slip to Sir William Thomson.

EDINBURGH, *25th April* 1868.

DEAR BENNIE,—Thanks for your two interesting bores. I had no idea that the surface was so thick at the place where they were made. In regard to the Sumnerton bore, I am not altogether sure about its being 150 feet. You will remember that I got its depth from the farmer. I called on Mr. Morton, the manager of the pits. He said that no journal of the bore was made, as the whole affair was so suddenly brought to an end by the rush of water up the bore. It is probable that the farmer's statement is correct, but perhaps it will be as well not to state the depths positively in your paper. The particulars regarding the bore at Skinflats I got from one Robertson, a borer at Lambhill Bridge. I intend, however, to go out some afternoon to Skinflats, and try to get some more information on the subject. Should I happen to be too long in looking after that matter, drop me a note when your paper is ready for the press, and I shall try to start off without delay. I am obliged to you for the two nicely mounted copies which you sent to me. It is a great pity that D. Baird did not give us some more bores. He promised that he would give me a great many bores, extending from Garscadden all along by Bearsden Station, *and not one of them under forty fathoms.* Those, so far as I can remember, are the very words he used. The first part of a long paper on "Geological Time" will appear in the next number of the *Philosophical Magazine.* The purport of the paper is to show that geology and physical science bear the same testimony as to time.—I am, yours truly, JAMES CROLL.

Please to remember us to Mr. Nasmyth when you meet him, and to old friends.

EDINBURGH, 15*th May* 1868.

DEAR BENNIE,—I was quite delighted with your long and interesting letter. It was a real treat to follow you in your various journeys. I am glad that you visited Duntocher and ascertained all about the eruptions of sand in the pit, and also that you paid another visit to Blairdardie. It was a pity that you did not see Jolly. When you go back again, if you do not see Mr. Jolly, try to see young Mr. Robertson, the son of the proprietor. He is a very nice young man. The granite is not to be seen in the wall of Mr. Jolly's house, which is situated near the row of huts down at Blairdardie beside the canal, but, I believe, if I can remember right, in some houses built at the very mouth of the pit out of which the boulder was taken. I met Mr. Jolly and young Mr. Robertson at the canal side, and had not time to go back and examine the wall. I hope that, when you again visit the place, you will be successful in seeing a piece of the stone, and ascertain, if possible, whether it is granite or not. I had a visit of Mr. James Russell, Chapelhall, yesterday. He stated that 52 fathoms was the depth at which the miner tapped the trough at Duntocher. I was very sorry to learn from him that Dugald Baird has got into bad health, and has been obliged to leave the superintendence of the boring. This may explain why we have not got any more journals of bores from him. Perhaps, in a quiet way, you might be able to learn who is his successor. Stimulated by your example, I made an exploring expedition to Portobello clay fields a few days ago. Don't expect, however, to get a graphic description of my journey, such as you can give with such fine effect. Although I have the feelings of a poet strongly developed, yet I am totally devoid of that poetic genius which you evidently possess. However, I saw little in my journey that could positively stimulate the pen of the poet. The country around Edinburgh is very fertile, good ground for farming; but, so far as I have yet seen

it is flat, tame, and uninteresting to one fond of woody glens, dens, and deep-cut gorges. My first work, when I visited the pits, was to look out for shells; but I could see none. The workmen told me that they were sometimes found. One of them showed me the spot where they say that the bones of a wolf were found, that had been drowned during the flood, and buried amongst the clay. They told me to call on John Ness, the foreman, and he would tell me all about it. After some trouble I found John Ness, a worthy man, and was glad to learn from him that the story was perfectly true. It was found, he said, among a tough blue clay, that they use for making tiles of, about 30 feet below the surface. It lay, he said, just in the very spot where it was overtaken by the flood. As I did not want to betray my ignorance, I made no remarks. On asking if they had preserved any of the bones, I was glad to learn that they had been all sent up to the Edinburgh Museum. When I came to the Museum next morning, I found that Mr. Davis had transformed the wolf into a common seal. The clay from top to bottom is evidently quite a modern deposit. I wrote to Mr. Geikie to ascertain the nature of the remains found in it: and his reply is as follows:—

"The shells in the clay at Portobello are not glacial: they are chiefly *Scrobiculurii piperata* or estuary shells, still living in the Forth at below low water. Fragments of thorn, hazel, birch, oak, etc., also occur in the clay." On leaving the clay pits, I visited some of the coal pits to the east of Portobello, where the coal seams are nearly perpendicular. I expect in the course of a week or ten days to visit Skinflats district. I shall then report progress. I would like to go out more than I do; but I have again got so involved in that interminable climate question, that almost every moment of my after hours is spent at home.—I am, yours truly, J. CROLL.

I am glad to find that your paper has excited Mr. Jamieson's attention.

EDINBURGH, *24th July* 1868.

DEAR BENNIE,—I have just received your long and, I doubt not, most interesting letter, which I shall reserve the pleasure of reading till I go home in the afternoon. In one of my evening strolls, which have been rather few of late, I met in with a most interesting example of stratified bed of sand and mud in the Boulder Clay. It covers an area of some acres, and the sand is being at present dug out for building purposes. A covering of till, varying from two to eight feet in thickness, full of ice-worn and striated stones and large boulders, is removed; and then they come upon the stratified sand, and underneath the sand lies the till again, resting on the rock. On carefully examining one of the pits, I observed pieces of wood, which at first I took for drift-wood, making its appearance in one of the beds of sandy mud. To make sure that it was not the roots of a tree that had grown on the surface, and had pierced through the till above into the bed below, I carefully examined the till above, but found it perfectly solid and continuous. The ends of the pieces of wood, which were nearly all vertical, or inclined to an angle of say 70 or 80 degrees, all terminated at the top of the stratified bed. I soon convinced myself that those pieces of wood must have been there before the till at the surface was laid down. To make sure, I got old Mr. Peach out to examine the matter; and he said that there can be no doubt whatever but the wood is older than the upper till. There is a boulder in the till, above the pieces of wood, more than a ton in weight. Mr. Peach declares that the wood is not driftwood, but the roots of a tree which grew there before the upper bed of till was laid down. He thinks that the Boulder Clay is not the old land-ice till, but a water formation, and that the boulder had been carried probably by floating ice. I have a suspicion that he is wrong; but I will not pass an opinion until I see Mr. Geikie, who is to be home on Monday. The rock-head upon

which the till lies is polished and striated, and that in hollows where an iceberg could not reach.—Yours,

J. CROLL.

I shall send the pieces of wood through to you next week.

CUMBERNAULD, *7th August* 1868.

DEAR BENNIE,—I expected to have seen you when in Glasgow, on my way back; but, unfortunately, I had to leave by the 5 P.M. train, and therefore could not call on you. I was at Crofthead, and saw the spot where the head of the ox was got, the head itself, and also the other phenomena of the place which you describe so admirably. James Geikie had visited the spot, and he has sent a long account of it to the *Geological Magazine*. He says that the silt in which the head was found is probably that of an old fresh-water loch, as you have concluded. He says that the till above is the same in character as that below, *the real old land-ice till*. I examined the rock to which you alluded; and it certainly looks as if the ice had moved northwards, but he (Mr. Geikie) says that the ice in the whole of that district moved southwards down the valley, which, however, is almost a dead level, for the 400-feet contour line passing from Crofthead to the loch along the side of the road.

I left at the shop a few specimens of the wood found at Edinburgh in the till. Professor Balfour has kindly agreed to examine the wood. Try what you can make of it, and we will see how the observations will agree. I sent you some of the sand in which the remains were found. Mr. Geikie visited the spot. When I see you, I shall give you all the particulars. I have tried every possible test; and I am in the *meantime* unable to see how the upper clay could have been older than the roots, or whatever else they may turn out to be. I have just had one day in the field here; but in that short time I

have seen enough to excite my curiosity. By all means get out your paper on the Crofthead district.

I really cannot say whether James Geikie has gone over the ground or not; but that is of no consequence. —Yours truly, JAMES CROLL.

CUMBERNAULD, *13th August* 1868.

DEAR MR. BENNIE,—Thanks for your letter. I am glad to see you are getting light on the history of the deposit in which the ox's head was found. I have gone over the ground from Kilsyth to Castlecary, and I find it very interesting. The district is covered with sand and gravel knolls of the Garscadden type; and the mounds are almost covered in some places with huge blocks of trap rocks. The quantity of boulders lying on the face of the country here is something remarkable. The following are the dimensions of one measured yesterday:—Length, 13 feet; breadth, 12 feet; height, 9 feet. I have measured, I suppose, nearly 100 with the tape line; 30 feet in circumference, and from 6 to 8 feet in diameter, is quite a common size. You will be very pleased to hear that our deep trough is not cut off by the track at Kilsyth, but passes right through, maintaining a depth of from 15 to 20 fathoms below the level of the canal. Hurrah for the trough! After passing Kilsyth, it goes onwards and enters the Forth at a great depth about Skinflats. If the weather is good on Saturday afternoon, might you not take a run out? You might leave by the 5 P.M. train, and come off at Castlecary station, where I would be waiting for you. We would then walk back along the ground to Kilsyth, and reach Croy station in time for you to see everything, and catch the train from Perth, which is due about 9.36 P.M. My time-table is two months old; but you could call at the station at Queen Street, and make sure about the trains, and then drop me a note. If you could not come on Saturday first, or if the weather

should turn out bad, you might perhaps come on the Saturday following.—I am, yours truly,

JAMES CROLL.

CUMBERNAULD, *20th August* 1868.

DEAR MR. BENNIE,—On my return from Edinburgh, where I have been for a few days, I found your letter. I have been at Skinflats, and got the journal of bores. I saw the pit to which you refer, and have traced out the line of the trough from Kilsyth to Grangemouth. Had I been at home, I would have sent you the particulars ere this, but as I shall see you on Saturday, I need not go into details.—Yours truly,

JAMES CROLL.

Forty-five fathoms 3 feet is the deepest surface found.

CUMBERNAULD, *22nd August* 1868.

DEAR BENNIE,—Lest your enthusiastic love for geological science should have overcome your regard for the laws of health, I went down to Castlecary at 5.43 P.M., but, disappointed as I was, I must say that I was happy that you did not venture out, for, in such weather, we could not have managed to have gone over the ground before total darkness would have overtaken us. I shall, however, be here for two or three days longer. Could you not manage to get out some day in the beginning of the week by the 5 P.M. train? If not, it can't be helped. Should you think of coming, drop me a note in time.—Yours truly,

JAMES CROLL.

Croll was not a mere reader of books or speculator upon other people's outdoor labours. Whenever he was engaged in anything which required personal inspection outside, or verification by minute investigation of natural phenomena, capable of being seen, he required to see it

personally. Whenever he had a holiday, it was invariably employed in visiting the scene of some geological formation. Thus, in August 1868, he spent his first Survey holidays at Cumbernauld with Mr. Macdonald, his wife's brother, and employed much of his leisure time in tracing the trough in the neighbourhood, and in hunting up erratic boulders in the fields thereabouts. He found many, and had great pleasure in his observations of them. Mr. Bennie writes regarding this holiday: "He invited me to come and see them likewise, and on Tuesday, 25th August 1868, I was conducted by him over the ground, and here is a description of what I saw in a letter to my friend Mr. Mahony.

"I had a long walk with Mr. Croll on Tuesday last, from Castlecary to Croy, and was introduced by him to several hundreds, if not thousands, of new acquaintances, since he came into this part of the country to spend his holidays. Though holiday friends, yet they have been long known in imagination at least to him, as they are the natural offspring of the ice which he has been so intimate with in mind for so many years, and certainly they are worthy of their origin, being of the race of Anakim in every sense. They consist of great boulders of trap, 10, 12, or more feet in length or breadth, but their numbers were even more remarkable than their size. In some parts they actually covered the surface, and made it useless for any purpose of agriculture; and it was laid down in grass or planted with trees, and often more of the surface consisted of stone than either. At others they occurred singly, in picturesque situations, though some were not romantic, especially one which stood on end 10 feet in a bank of gravel which overhung a midden, and to see it we had to look over a great bing of 'coo shairn,' etc. The country is very bleak and barren from Castlecary to Croy, and looked bare, especially the fields where the trap blocks were most numerous. I noticed that the modest eyebright was very modest here, not above one and a half inches from root to flower.

"Mr. Croll has been very successful in proving that the deep trough of his discovery goes straight through from sea to sea, and is not cut off by the trap at Kilsyth, as was feared. It is, however, very narrow there, not wider than 500 or 600 yards, yet the depth is nearly about 140 feet."

Croll persevered in his investigations as to the bores till he had probed the matter to the bottom, as will be seen from the following letters to Mr. Bennie:—

EDINBURGH, *11th September* 1868.

DEAR MR. BENNIE,—I wrote to Mr. Stirling, manager of Grangemouth pit, requesting him to preserve specimens of all the beds beneath those we have already got; and he has answered my note stating that he will do so. I also wrote to Mr. Mackay, the proprietor, requesting some more bores; but I have not got any as yet. I am inclined to believe that our deep trough is an old *pre-glacial* river-bed, scooped out of the rock before the time of the Boulder Clay. The watershed would be at Kilsyth: to the east, the water would run eastwards along the trough to the Forth at Grangemouth; and, west of Kilsyth, the water would run to the Clyde. The trough enters the sea at Grangemouth, at a depth of 260 feet below the present sea level. Consequently, if it was hollowed out by running water, it must have been at a time when the land stood at least 260 feet higher in relation to the sea than at present. The thing which convinces me that it was scooped out by running water is this: Deep as is the trough at Kilsyth, the sea would not flow through if the land stood any higher than at present; for the bottom of the trough at Kilsyth is hardly at sea-level. You will observe that, in the flat carse land at Grangemouth, the trough is quite narrow. The average depth of surface of the whole of the flat country along the side of the firth and extending up in the direction of Carron is about 20 or 25 fathoms. Out

of this flat, rocky surface a narrow trough has been scooped out to a depth of more than 20 fathoms, away inland for miles, in fact, right across the island. The following would represent a cross section of the trough, as it entered the Firth at Grangemouth, before the surface deposits were laid upon the rock, or, in other words, before the Glacial epoch.

Now it is apparently absurd to suppose that a sea extending for miles on either side should take it into its head to cut out a narrow trough like this and leave the rest of the rock untouched. The form of trough that the sea would make would be this—

I have always felt a little puzzled why the trough should be so narrow all the way through if it was cut by the sea when it occupied the Midland Valley. But if it had been an old river, all is plain. Undoubtedly the trough must have been used as a strait by the sea when the sea-level was higher than the bottom of the trough at Kilsyth. But on thinking over the matter for a week past, I feel inclined to believe that the trough was hollowed out of the rock by running water at a time when the land stood higher than at present. Might not this have been at the time of the forest of Cromer? There ought, according to the area of drainage, to be a far greater quantity of water running through the Midland Valley to east than there actually is. The Bonny water, which has a large area of drainage, would hardly drive a good water-wheel. The water must find its way to the sea through the deep surface of sand and gravel which

covers the country. But probably before these sands and gravels were deposited, a pretty large quantity of water would be running down the trough. Think over the matter, and let me hear your opinion.—I am, yours truly, JAMES CROLL.

EDINBURGH, 19*th September* 1868.

DEAR BENNIE,—I send you, by book post, the office copy of the *Geological Magazine*, containing the account of the bores. When you have done with it, you can return it. I am glad to find that you are busy at the paper for the Geological Society. Do not allow yourself to be influenced in any way by any opinion of mine regarding any of the various points connected with the subject. Never mind how much we may differ on any particular point. Come out with your own views in the strongest form. I am glad that Dr. Young is going to interest himself in the matter; and I am also glad that you promised to let him have a copy of the bores at your leisure. I shall write to Mr. Stirling, and request him to send some more of 5 and 7, and also to collect samples from different parts. . . . The red clay, No. 7, and the sand containing the shells were taken from a number of places. These two I collected myself; but the others were collected by Mr. Stirling and a borer who happened to be there at the time. I suppose that they were all of one piece. It is a pity that we cannot manage to find out whether the trough actually runs into the Clyde or not; I suppose that it does. The quantity of drift lying in the bottom of the Firth of Forth must be very great. I have not been able to get the part of my paper ready for October number.—I am, yours truly,

JAMES CROLL.

EDINBURGH, 10*th October* 1868.

DEAR BENNIE,—I send you the *Geological Magazine* for the present month, as it contains an absurd letter by

Mr. Craig "On the Bores of Crofthead." Mr. J. Geikie is going to reply, I suppose. Mr. Craig evidently thinks the upper clay not to be till, because it is not tough and blue.—I am, yours, JAMES CROLL.

Method of Determining Sub-aerial Denudation.—Correspondence with Mr. Charles Darwin.—In addition to the luminous memoirs bearing on the theory explaining the occurrence of an Arctic climate in temperate latitudes in former geological epochs, Mr. Croll pursued other lines of research, the results of which were published in various periodicals, and subsequently incorporated in his volume on *Climate and Time.* In the *Philosophical Magazine* for 1850, Mr. Alfred Tylor published a paper in which he estimated the amount of sediment brought into the ocean by denuding agents. He inferred that one foot removed off the general surface of the land during that period would raise the sea-level three inches. At a later date Croll approached this question, and pointed out that the rate at which the materials are carried off the land is measured by the rate at which sediment is carried down by our river systems. Hence, in order to determine the present rate of sub-aerial denudation, we have only to ascertain the quantity of sediment annually carried down by the river systems. From the estimates of the materials discharged by the Mississippi, furnished by Humphreys and Abbot, he inferred, in 1868, that the rate of denudation is about one foot in six thousand years. Taking the mean elevation of the land, given by Humboldt at 1000 feet, he contended that the whole would be carried down into the ocean by our river systems in about six million years, if no elevation of the land took place. He further showed the value of this method as a measure of geological time. These views were embodied in several papers written in 1868, entitled "Paper on Geological Time," Part I.; "Method of Determining the Rate of Sub-aerial Denudation," which appeared in the *Philosophical Magazine* of

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?

CHAPTER XII

4. PAPERS ON THE GLACIAL EPOCH AND GLACIERS

THE Glacial Epoch in Europe.—Some of Dr. Croll's most interesting work is occupied with a consideration of the path of the ice-sheet in North-Western Europe during the last Glacial epoch. In his series of papers on "Glacial Submergence," published in the *Reader* for 1865, Dr. Croll first suggested that, during the Glacial epoch, the North Sea was probably invaded by continental ice.

In 1869 he considered the special case of two buried river channels which he had discovered through boring operations made for mining purposes (Paper No. 34). One extends from the Clyde above Bowling, across the country by Kilsyth, along the valley of the Forth and Clyde Canal to the Firth of Forth at Grangemouth. It is filled with immense deposits of mud, sand, gravel, and boulder clay. On the surface there is no trace of its existence. All along the line of this trough the surface of the country is covered with enormous beds of sand, obviously of marine origin, indicating that at a recent period the sea occupied this valley. The trough, however, is certainly excavated, not by a sea filling the valley, but by running water.

This river channel enters the Forth, a few hundred yards to the north of Grangemouth Harbour, 260 feet below the present sea-level, so that at that time the land stood some 300 feet higher than at present. This river, which apparently belonged to the early part of the Glacial epoch, may have corresponded to the Carron.

Grangemouth was probably not the mouth of the river, but the place where it joined the river Forth of that period, when the North Sea was in all probability dry land, and the Forth, Tay, Tyne, and other British rivers tributaries of the Rhine, then a huge river passing down the bed of the North Sea and entering the Atlantic to the west of the Orkneys. Further borings might reveal the old beds of the Forth and the Rhine, and enable us to estimate accurately the height at which the land stood at that remote period. But this buried channel shows that the sea-level was some hundreds of feet lower than now, and that our island must have then formed part of Continental Europe.

The western part of this great hollow, from the water-shed at Kilsyth to the Clyde, is probably also an old river channel, possibly the ancient bed of the Kelvin. If so, the ancient river probably entered the Clyde 200 feet below the present sea-level, and the rocky bed of the ancient Clyde must be buried 200 feet below the surface. There are other examples of buried channels both in Scotland and England. Probably all our British rivers now flow into the sea over their old buried channels, except where they have changed their courses since the beginning of the Glacial epoch. Some of these channels would be glacial, others pre-glacial in origin. It is certain that many of them were used as water-courses during the warm periods of the Glacial epoch, for they have been filled with boulder clay, re-excavated, and finally filled up again with clay.

In 1870 Mr. Croll published an extremely important and ingenious paper in the *Geological Magazine* for May and June, entitled "On the Path of the Ice-Sheet in North-Western Europe and its relation to the Boulder Clay of Caithness." In this he attempted to trace the path of the ice-sheet as indicated by the geological evidence of the boulder clay of Caithness. The glacial drift of Caithness consists of a boulder clay with ice-scratched débris, together with sea-shells scratched and

ice-worn, which were evidently broken, striated, and pushed along by the ice as the clay was formed. The striæ show that the ice came from the sea and not from the interior, and their direction indicates that it was either from Moray Firth to the south-east, or from the Atlantic to the south-west. Dr. Croll accumulated evidence of former glaciation to show that Scotland was covered with an ice-sheet which buried the Ochil, Pentland, and other ranges, whose flattened and rounded tops still bear witness to the denuding power of ice. The North Sea, unless it was then ten times deeper than at present, was too shallow to permit an ice-sheet of this thickness to float, and consequently the ice did not break off into icebergs, but moved along the bottom of the sea as one unbroken mass. The Scandinavian ice also entered the North Sea. The deep trough south of Norway was soon filled, and the ice was prevented from moving northwards by the vast mass of polar ice moving southward. Had Scotland been a low, flat country, the Scandinavian ice would have passed over it; but the pressure of the vast Scottish ice-sheet was sufficient to compel the Scandinavian ice-sheet to move round by the Orkney islands. The median line of the two sheets was near the Scottish coast. Where there was a low shore and little pressure of ice from the interior, as in some parts of the English coast, the Scandinavian ice crossed the country. Another contingent of the Scandinavian ice also went into the Gulf of Bothnia and filled the Baltic Sea. A part of this moved southwards over North Germany; but the greater part kept to the bed of the Baltic, turned to the right round the south end of Gothland, crossed Denmark, and entered the North Sea somewhere to the north of the Elbe. This is conclusively shown by the direction of the striæ and the débris of Scandinavian rocks. This immense glacier would have to find an outlet to the Atlantic through the English Channel, or press in between the shores of Scotland and the great glacier from Gothland and North-

west Europe. Consequently, the Scottish ice-sheet, after entering the North Sea, was obliged to turn abruptly north and cross Caithness, which lay in its path, depositing there the shells, oolitic fossils, and chalk flints from the North Sea which are now found in its Boulder Clay. That it was the Scottish and not the Scandinavian ice which crossed Caithness is shown by the total absence of all Scandinavian débris. The Scottish ice deflected northwards by the Baltic glacier, and the Scandinavian ice deflected northwards by the Scottish ice-sheet, moved on round by the Orkney and Shetland Islands into the Atlantic. Scandinavian débris appear in these islands, and striæ pointing in the direction of Scandinavia. Thus Scotland, Scandinavia, and the North Sea formed one immense tableland of ice, 1000 to 2000 feet above the sea-level, which terminated in the deep waters of the Atlantic by a perpendicular wall of ice, extending probably from the west of Ireland away in the direction of Iceland. From this barrier icebergs rivalling in size those now met with in Arctic seas would constantly break off and float away.

Mr. Croll published a paper in the *Geological Magazine* for January 1871, on the "Transport of the Wastdale Granite Blocks." He explained the transport of these blocks across the Pennine Range to the east by supposing that the ice-sheet of Scotland overlapped the high grounds of the North of England. The Wastdale boulders, which are neither ice-marked nor rounded, and which are found on the surface of the Boulder Clay, and not imbedded in it, must have been carried on the top of the ice-sheet. There is, moreover, evidence—as, for example, the presence of hard red chalk from the Yorkshire and Lincolnshire Wolds in the Cotswold Hills, and of chalk débris from the Yorkshire Wolds in the Midland counties of England—that a great ice-sheet extended south-west from Yorkshire across the centre of England. In his paper on the "Boulder Clay," Dr. Croll supposed that the ice which entered the North Sea from

southern hemisphere. This, as Dr. Croll pointed could not happen if both hemispheres were glaciated he same time, and he claimed the submergence of l as a strong argument in favour of his theory that iation prevailed on each hemisphere alternately, and irred at periods of maximum eccentricity of the earth's t. As already shown, both hemispheres could not in case be glaciated at the same time, and the conse- ice would be a submergence of land during the cold ods, and an emergence of land during the warm nterglacial periods.

Mr. Croll's views provoked considerable comment, the objection was brought forward that the period of imum glaciation could not have been the period aximum submergence, since the Boulder Clay shows the rocky surface of the land must have been ed by a gigantic sheet of ice. Mr. Croll replied, in *Reader*, 14th October 1865, that the land with its ing ice-sheet might have lain under the rising sea, f the ice were of sufficient thickness, its pressure on urface of the land would be nearly thirteen times er than the pressure of the water to get under- it. He suggested independently that the land ice d gradually encroach upon the sea, and that a ow sea like the North Sea was probably filled by ice from Scandinavia and the British Isles. Another objected that the conversion of the ocean into ice d lower instead of raising the sea-level. Mr. Croll ght that this would be balanced by the melting of ice he warm hemisphere, and that the accumulation of n the cold hemisphere would also tend to raise the vel by confining the ocean within smaller bounds. But ttached considerable weight to this objection, with- withdrawing his opinion that, however caused, there a submergence of land during the cold periods of Glacial epoch and an emergence during the warm ds. He pointed out, however, that if, as is most able, the centre of the earth is in a molten condition,

the East Coast of England and Scotland all passed round the North of Scotland; but in this paper he admitted the probability that, on meeting the Scandinavian ice, part of the North British glacier was deflected southwards, and crossed England. The ice which passed over Wastdale Crag moved in the direction E.N.E., and did not cross the chalk of the Yorkshire Wolds, while the ice which bent round to the south by the Wold came from the district lying to the south of Wastdale Crag, and did not carry any Wastdale granite with it.

Mr. Croll summarised his views on the ice-sheet of Northern Europe and America in a short paper on the "South of England Ice-Sheet," published in the *Geological Magazine*, June 1874. The ice of Greenland was too thick to float in the surrounding seas, and moved over the North American continent in one continued mass. The North Sea, as already shown, was filled with land ice from Scandinavia and Britain, which passed round the Orkneys and Shetlands into the deep trough of the Atlantic. The North of England must have been covered by an ice-sheet, and ice-markings would probably be disclosed by an examination of the Pennine range under the turf. Dr. Croll suggested that the ice which crossed England from the north-east to south-west was a portion of the great Baltic glacier, one or two thousand feet in thickness, which entered the Atlantic in the direction of the Bristol Channel.

Glacial Submergence.—In 1865 and 1866 Dr. Croll published an important series of papers in the *Reader* on the "Physical Cause of the Submergence and Emergence of the Land during the Glacial Epoch." He suggested that the submergence of land on the glaciated hemisphere was due to a rise of the sea-level caused by the influence of the weight of the ice-cap on the earth's centre of gravity. If the northern hemisphere was undergoing glaciation, the centre of gravity would be shifted somewhere to the north, causing a rise of the sea-level on the northern hemisphere and a sinking of the sea-level on

the southern hemisphere. This, as Dr. Croll pointed out, could not happen if both hemispheres were glaciated at the same time, and he claimed the submergence of land as a strong argument in favour of his theory that glaciation prevailed on each hemisphere alternately, and occurred at periods of maximum eccentricity of the earth's orbit. As already shown, both hemispheres could not in that case be glaciated at the same time, and the consequence would be a submergence of land during the cold periods, and an emergence of land during the warm Interglacial periods.

Mr. Croll's views provoked considerable comment, and the objection was brought forward that the period of maximum glaciation could not have been the period of maximum submergence, since the Boulder Clay shows that the rocky surface of the land must have been covered by a gigantic sheet of ice. Mr. Croll replied, in the *Reader*, 14th October 1865, that the land with its covering ice-sheet might have lain under the rising sea, for, if the ice were of sufficient thickness, its pressure on the surface of the land would be nearly thirteen times greater than the pressure of the water to get underneath it. He suggested independently that the land ice would gradually encroach upon the sea, and that a shallow sea like the North Sea was probably filled by land ice from Scandinavia and the British Isles. Another critic objected that the conversion of the ocean into ice would lower instead of raising the sea-level. Mr. Croll thought that this would be balanced by the melting of ice on the warm hemisphere, and that the accumulation of ice on the cold hemisphere would also tend to raise the sea-level by confining the ocean within smaller bounds. But he attached considerable weight to this objection, without withdrawing his opinion that, however caused, there was a submergence of land during the cold periods of the Glacial epoch and an emergence during the warm periods. He pointed out, however, that if, as is most probable, the centre of the earth is in a molten condition,

this would considerably affect the changes in the earth's centre of gravity and increase the total submergence. Even after allowing for the fall of sea-level due to the conversion of the sea into ice, there would remain a considerable rise of sea-level during the cold periods.

In April 1866 Mr. Croll published a short paper (Paper 19) on the same subject and under the same title in the *Philosophical Magazine*, to which Sir William Thomson added a short note on the mathematical aspects of the theory. In it Dr. Croll attempted to calculate the effect, on the centre of gravity of the earth, of the transference of an ice-cap, of given magnitude, from one hemisphere to the other, and the consequent effect on the sea-level, which, he showed, would rise on the glaciated hemisphere.

In 1874 Mr. Croll restated his theory of oscillations of sea-level during the Glacial epoch (Paper No. 57). He took his data from the condition of things at present existing at the Southern Pole, and showed the effects which would follow, if, owing to a high eccentricity of the earth's orbit, all the ice were transferred to one hemisphere. Continental ice moves in consequence of pressure acting from the interior, and the larger the surface, the greater the thickness of ice needed to produce the requisite pressure. Little is known of the Antarctic Continent, but in Greenland the ice rises steadily towards the interior, and there is good reason to suppose that the northern ice-cap must be at least nearly two miles thick. The Antarctic Continent is about twelve times the area of Greenland, and hence the thickness of Antarctic ice must be far greater than that of the Arctic ice-cap. Antarctic icebergs have been found rising from 700 to 1000 feet out of the sea, showing that the edge of the cap must be over a mile thick. The submergence of land during the Glacial epoch can be readily accounted for, if it is admitted that continental ice may accumulate to a mean thickness of two miles, which is very probably the case on the Antarctic Continent. During the Glacial

epoch, however, all the ice was accumulated on a single hemisphere. The centre of gravity was consequently shifted towards the heavier or cold hemisphere, causing a rise of the sea-level on that hemisphere. But a still greater rise might occur either before or after the period of maximum glaciation. If the ice melted on the warm hemisphere more rapidly than it formed on the cold, the rise of sea-level on the glaciating hemisphere would be due to two distinct causes: first, to the displacement of the centre of gravity; and, secondly, to the increase of the waters of the ocean by the melting of the ice on the warm continent. The displacement of the centre of gravity towards the ice-cap would be far greater if the centre of the earth is in a molten state. Dr. Croll suggested that if the extent of the general submergence of the Glacial epoch was known, and also the present amount of ice on the Antarctic Continent, it would be possible to determine whether the interior of the earth is in a fluid condition or not. In the same paper Dr. Croll pointed out that oscillations of the sea-level would help to account for the geographical distribution of plants and animals. For instance, Asia and America might be connected, so that plants and animals could pass from one continent to the other.

Antarctic Ice and Glacial Epoch.—In 1879 Dr. Croll published a paper on the "Thickness of the Antarctic Ice and its Relations to that of the Glacial Epoch" (No. 73). Sir Wyville Thomson, director of the scientific staff of the *Challenger*, had expressed an opinion that the Antarctic ice could not exceed 1400 feet in thickness. This, if true, would of course disprove Dr. Croll's theory. Sir Wyville Thomson maintained that when the ice is 1400 feet thick the pressure on the lowest layer is a quarter of a ton to the square inch. Hence there results a constant system of melting and regelation, the water passing down from layer to layer till it reaches the floor of the ice-sheet and works out channels for itself between the water and the land.

Dr. Croll pointed out that numerous instances of ice-bergs nearly a mile in thickness, and much geological evidence, would disprove Sir Wyville Thomson's statement, that ice at the temperature at which it is in contact with the earth's crust in the Antarctic regions cannot support a column of itself more than 1400 feet high without melting. Mr. Croll showed that this assumed the temperature of the column to be 32° F. The temperature of the great mass of the ice-sheet is more probably 10° or 12° below freezing point, the amount of heat derived from all possible sources being practically infinitesimal. No experiments have been made as to the thickness of ice which could support its own weight without melting at this temperature, and it is therefore impossible to say what amount of pressure would be necessary to lower the melting point to any assigned temperature. With regard to the effect of pressure in melting ice by compression, Dr. Croll showed that if the bulk of the ice were only 2° below freezing point, the total amount of heat generated by compression during 10,000 years would not raise the temperature of the ice to melting point. Hence Dr. Croll's theory remained unassailed. The rest of the paper is occupied in a demonstration that the ice at the South Pole may well be twenty-four miles thick, although Dr. Croll is content to accept only a fraction of this estimate. Inadequate conceptions of the magnitude of continental ice had led to the misunderstanding of geological evidence, which otherwise would clearly show that, during the last Glacial epoch, Northern Europe was buried under an ice-sheet of enormous thickness, which filled the shallow seas, and buried hills 2000 feet or more in height. Hence the thickness at the centre of dispersion must have been many times as great, and the total weight of the ice accumulated on a single hemisphere quite sufficient to account for the submergence of land in the manner already described.

Physical Cause of Motion of Glaciers.—In April 1869 Mr. Croll contributed a short paper on "The Physical

Cause of the Motion of Glaciers" to the *Philosophical Magazine*, and a second paper on the same subject to the *Philosophical Magazine*, September 1870. The ice of a glacier, though hard and solid, moves with a differential motion, the particles of the ice are displaced over one another, or, in other words, the ice *shears* as it descends. It had been generally assumed that the mere weight of the glacier was enough to *shear* the ice. Canon Moseley, F.R.S., after careful investigation and experiment, showed that for the ice to shear as it was supposed to do, a force thirty or forty times the weight of the glacier would be required.

Mr. Croll's own view was that the glacier descended molecule by molecule, each molecule losing for an instant its shearing force, and descending by its weight alone. Heat applied to a molecule of ice at 32° does not raise the temperature of the molecule, but is consumed in work against the cohesive forces binding the atom into crystalline form. The energy exists in the dissolved crystalline molecule as a tendency to reassume the crystalline form; and when the molecule is allowed to re-crystallise, energy is given out again in the dynamical form of heat. This heat is employed in melting the next adjoining molecule. It must be observed that the crystalline molecule is melted for an instant only. When a molecule B takes energy from A in the form of heat, A instantly reassumes the crystalline form. B is now melted, but reassumes the solid state immediately the next molecule, C, takes heat from B. This process goes on from molecule to molecule till the energy is transmitted through to the opposite side, and the ice is left in its original solid state.

This being established, every difficulty about the descent of a glacier disappears; for a molecule in the fluid state loses its shearing force, and descends by its weight alone. All it requires is space to advance in, and this it has, for, in passing from the solid to the liquid form, its volume is diminished by about one-tenth. Nor

does it return to its original place on resolidifying; for the molecules in front of it have been similarly affected, and so each molecule resolidifies a little below its original position. Each molecule of the glacier consequently descends step by step, as it melts and resolidifies; and hence the glacier, considered as a mass, is in a state of constant descent downwards.

This explains the formation of crevasses. Suppose a change of inclination from 4° to 8° in the bed of a glacier. The molecules on the slope of 8° will descend more rapidly than those above on the slope of 4°. There will therefore be a state of tension at the point where the change of inclination occurs. The ice on the slope of 8° will tend to pull after it the mass of the glacier moving more slowly on the slope above. The pull being continued, the glacier will snap asunder as soon as the cohesion of the ice is overcome. The greater the change of inclination, the more readily a crevasse is formed.

As the supply of heat is greater in summer than in winter, the molecules will pass oftener into the liquid state in summer, and also the glacier will descend more quickly during the day than during the night.

This theory of glacial descent also explains why a glacier can move off a slope almost horizontal, or off the face of a continent perfectly level.

A short paper on the same subject in the *Geological Magazine*, August 1876, deals with a misunderstanding of Mr. Croll's explanation.

In the debilitated state of body in which Croll was during the year 1869, he yet managed, by his marvellous conservation and concentration of energy, as well as by his unique saving of time, to do a considerable amount of independent investigation over and above his daily work in the office. During the year 1869 he wrote papers on "The Physical Cause of the Motions of Glaciers," which appeared in the *Philosophical Magazine* of March 1869, and in *Scientific Opinion* on April 14, 1869 (No.

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.

CHAPTER XIII

5. PAPERS ON OCEAN CURRENTS

OCEAN Currents, Gulf Stream.—Nearly twenty of Mr. Croll's scientific papers are devoted to the examination of the influence of ocean currents on climate. In the *Geological Magazine* for April 1869 he published a paper (No. 31) "On the Influence of the Gulf Stream." He adopted as the basis of his calculations the data given by Mr. A. G. Finlay, who estimated the breadth of the stream at 50 miles, its depth 1000 feet, its velocity 4 miles an hour, and its temperature on leaving the Gulf 65°. From these figures Mr. Croll calculated that 133,816,320,000,000 cubic feet of water were conveyed from the Gulf daily, and that the total quantity of heat transferred from equatorial regions by this current is equal to all the heat received from the sun by 3,121,870 square miles at the equator, or by 6,873,000 square miles of the Arctic regions. The frigid zone contains 8,130,000 square miles. Therefore the quantity of heat transferred from tropical regions by the Gulf Stream is nearly equal to the amount of heat received from the sun by the entire Arctic regions. The Gulf Stream alone probably conveys more heat to the Temperate and Arctic regions than all the aerial currents blowing from the equator. Aerial upper currents, in fact, tend to dissipate rather than to transfer the heat they receive from the earth. Even at the equator they are nowhere below the snow-line, and are cooled to the temperature of freezing point. Their heat is radiated off into stellar space and lost. On reaching temperate regions they descend, become warmed by contact with

the Gulf Stream, and blow over our country as warm south-westerly winds. From these considerations Dr. Croll concluded that to raise the mean temperature of the whole earth, water, and not, as was generally supposed, land, should be placed along the equator. Land could only effect heat transference by aerial currents which, as already shown, obtain all the heat they convey to Temperate and Arctic regions from warm ocean currents which would be absent if land were substituted for water at the equator. Aerial currents are an effectual means of dissipating the earth's heat into space. Air in contact with water is less rapidly heated than air in contact with land, and thus the presence of water at the equator also tends to raise the mean temperature by checking the dissipation of heat by aerial currents and permitting it to be transferred to higher latitudes by ocean currents.

In the years 1870 and 1871, Dr. Croll published a remarkable series of papers in the *Philosophical Magazine* "On Ocean Currents" (Papers 3, 5, 36, 39, 44). Only the general conclusions reached can be stated, and it is impossible to give any idea of the brilliant handling, the wealth of illustration, and the amount of knowledge which they display. The more important portions, in a somewhat condensed form, will be found in the section of *Climate and Time* which deals with ocean currents.

Dr. Croll began by examining the influence of the Gulf Stream, a typical ocean current. It has already been shown how vast is the quantity of heat transferred by this current from the equatorial regions. Our climate is largely modified by warm south-westerly winds, which derive their heat from the warm waters of the Atlantic. The Atlantic, if deprived of warm currents, would be unable to supply the necessary heat to the south-west winds. Assuming the heat conveyed by the Gulf Stream to be only half the amount already calculated, it is still equal to all the heat received from the sun by 2,062,960

square miles of the temperate regions. The total area of the Atlantic within the temperate zone is about 8,500,000 square miles, and consequently nearly one fifth of the entire heat of the Atlantic is derived from the Gulf Stream. The mean annual temperature of the Atlantic is 56°, or 295° above the temperature of space, which is usually estimated at —239°. Hence, if the Gulf Stream were withdrawn, the temperature of the Atlantic would be lowered one fifth of 295°, or 59°, giving —3° as its temperature.

Influence of Ocean Currents on Climate.—Dr. Croll proceeded to show the magnitude of the work done by ocean currents, by a variety of illustrations. Under existing circumstances the difference in temperature between the equator and the poles is 80°. Assuming that the proportionate quantity absorbed by the atmosphere is the same in both cases, the quantity of heat received direct from the sun at the equator is to the quantity received at the poles about as 12 to 5. Were all heat-transferring agencies withdrawn, the temperature of the equator would be 374°, and that of the poles 156°, above the temperature of space. That is, the temperature of the equator would be 135° F., and that of the poles —83°. The influence of ocean currents is thus to reduce the difference of temperature from 218° to 80°. In the latitude of London the temperature as determined by the sun's heat would be 249° above the temperature of space, or 10°. The actual mean normal temperature in the latitude of London is 40°, and the actual mean temperature of London is 10° above the normal. Thus the actual rise of temperature at London, over and above all lowering effects resulting from Arctic currents, is 40°.[1]

[1] The glacial shells of the Clyde bed show that the climate of Scotland has undergone far more change since the last Glacial epoch than the climate of Canada. The glacial cold of the two countries was probably nearly the same, and the subsequent disproportionate rise of temperature in Scotland is due to the return of the Gulf Stream, which was deflected from our

The influence of ocean currents on temperature may also be shown in the following way. A variety of considerations suggest that the mean annual temperature of the ocean ought to be greater than that of the land in equatorial as well as in temperate and polar regions. Actual observation shows that in tropical regions the mean annual temperature of the ocean is below, that of the land above, the normal. The explanation is, that in tropical regions the ocean is deprived of heat by warm currents, and is further cooled by the compensating polar currents. From the consideration that the temperature of the sea should be higher than that of the land, it follows that the southern hemisphere, which contains a larger proportion of sea, should be warmer than the northern hemisphere. But the actual mean temperature of the southern hemisphere is lower than that of the northern, a circumstance undoubtedly to be explained by the fact that the warm currents of the southern hemisphere set northwards, and that these are compensated by cold under-currents from the northern hemisphere. There is thus a constant transference of heat from the southern to the northern hemisphere, quite sufficient to account for the lower mean temperature of the southern hemisphere.

The influence of currents on climate is obvious. Owing to the spherical form of the earth, too little heat is received at the poles, and too much at the equator. The function of the two great oceans is, by means of currents, to render both habitable by transferring the excess of equatorial heat to temperate and polar regions. This work they accomplish not directly, by radiation, but indirectly, by heating the aerial currents which blow as warm breezes over the land.

shores during the Glacial epoch. Canadian winters are not modified in the same way by this current, and are cooled by the Davis Strait Polar Stream. The mean temperature of the Gulf of St. Lawrence is the same as that of Iceland, and consequently glacial shells are not extinct.—Dr. J. Croll, *Transactions of the Geological Society of Glasgow*, 1866.

Ocean Currents in the Glacial Epoch.—If all the ocean currents were deflected into one hemisphere, that hemisphere would have a very high mean temperature, while that of the other would approach freezing point. Dr. Croll proceeded to consider the part played by ocean currents during the Glacial epoch. He gave a full and lucid statement of his own theory, that the Glacial epoch resulted from the physical effects of a high state of eccentricity of the earth's orbit (see sec. 3, *Geological Climate*). The trade winds from the cold hemisphere would be far stronger than those of the warm hemisphere, since there would be far greater difference of temperature between the equator and the pole. Hence the median line of the trades would be in the warm hemisphere beyond the equator, and these strong trades would impel the warm waters of the equator into the warm hemisphere, thus further cooling the cold hemisphere, until it reached a state of more or less complete glaciation. The cause of secular changes of climate is the deflection of ocean currents, owing to the physical consequences of a high degree of eccentricity of the earth's orbit.

Theories of Cause of Ocean Currents.—Before explaining his own views as to the cause of ocean currents, Dr. Croll proceeded to examine various other theories. Of these, the most important are those which attribute movement of the ocean to the influence of gravity resulting from difference of density. Lieutenant Maury and Dr. Carpenter are the best known advocates of this view, the former holding that difference of specific gravity would produce currents, the latter that no currents would be produced, but a general movement of the ocean from the equator to the poles.

Mr. Croll first examined the views of Maury, who attributed the difference in specific gravity to two causes: first, the difference in temperature, the equatorial water being expanded by heat, and consequently lighter, and

the polar waters contracted by cold, and consequently heavier; secondly, to difference of salinity, the waters of the equator being salter and consequently heavier. Mr. Croll pointed out that these two causes would obviously tend to neutralise each other, and to prevent currents. Lieutenant Maury's theory could not in fact claim any scientific value.

Dr. Carpenter was a more important antagonist, and the controversy between the two opponents lasted for some years, and was conducted with singular power on both sides. Dr. Carpenter, in the course of dredging expeditions in the North Atlantic, found great masses of warm water which he referred, not to the Gulf Stream, but to a general movement of the ocean polewards, of which the Gulf Stream was a peculiar case modified by local conditions. Dr. Croll first attempted to calculate the effects on the temperature of the ocean, if any such general oceanic movements existed. The amount of heat conveyed by a general movement of the ocean could hardly be less than that conveyed by a single current like the Gulf Stream. Taking the lowest probable estimate of the volume of the Gulf Stream, the heat conveyed by it is equal to all the heat received from the sun by 1,600,960 square miles of the Atlantic in the torrid zone. If the general oceanic movement conveys only the same quantity, the total amount of heat conveyed into the Atlantic in temperate regions is equal to all the heat received from the sun by 3,201,920 square miles of the Atlantic in the torrid zone, or $\frac{33}{77}$ of the heat received from the sun in that area. Now the quantity of heat received in the torrid zone is to the heat received at the equator as 975 to 1000; and, if $\frac{33}{77}$ of these 975 parts of heat, or 405 parts, are removed, there remains to the ocean in the torrid zone 570 parts of heat. The Gulf Stream, as already shown, conveys into the Atlantic about $\frac{1}{4}$, or, more exactly, $\frac{100}{412}$ of the amount of heat the Atlantic receives from the sun. Allowing for the general oceanic movement, the amount of heat conveyed into the

Atlantic in Temperate regions would be twice this, or $\frac{100}{208}$ of the heat received from the sun. The Temperate zone receives proportionately 757 parts of heat from the sun, and to this must be added the quantity transferred from equatorial regions, $757 \times \frac{100}{208}$, or 367 parts, giving a total of 1124 parts. That is to say, the Atlantic in Temperate regions would possess 1124 parts of heat, and would be warmer than the ocean in the torrid zone; or, if only half the heat transferred went to raise the temperature in Temperate regions, the rest going to Arctic regions, the Atlantic would still have 940·5 parts of heat in Temperate regions, against 570 in equatorial. Similarly, it might be proved that the Arctic Ocean would possess 766 parts of heat, and would consequently be warmer than the Atlantic in tropical regions. From this it is obvious that there can be no such movement as supposed by Dr. Carpenter, since there is not enough heat in equatorial regions to supply such a current. Even the Gulf Stream is not supplied only from the northern hemisphere, and could not be so supplied without rendering the Atlantic in the torrid zone as cold as the Atlantic from the Tropic of Cancer to the North Pole. Yet, obviously, in any general oceanic movement, it may be assumed that the hot water moving towards either pole is derived from the equatorial regions of that hemisphere only.

Mr. Croll next criticised the supposed motive force, granting for argument's sake that such a general oceanic movement did actually exist. The waters of the ocean at the equator were expanded by heat, and the height of a column of equatorial water would be higher than a similar column of water at the pole. That is, the surface of the ocean would slope from the equator to the pole. This state of things, in which a longer column of lighter water of the equator balanced a shorter column of heavier water at the pole, would constitute static equilibrium. At the same time, however, the molecular equilibrium of the ocean would be disturbed, and con-

sequently the molecules of water would tend to roll down the slope under the influence of gravity. This again disturbs the static equilibrium, and there is therefore a constant tendency to restore this. Neither complete static equilibrium nor complete molecular equilibrium would ever be attained, but there would be a constant endeavour to attain both. The result would be a double circulation, a movement of warm surface water polewards, and a corresponding under-movement of cold water equatorwards. Dr. Croll argued at great length, from mathematical and physical laws, that the maximum possible slope would be insufficient to produce any appreciable movement, so that, if such a movement really existed, the motive force must be something more than the mere force of gravity, which, as he attempted to show, would be quite inadequate. No theory based on difference of gravity could explain, for example, such a current as the Gibraltar current, where the difference of level between the Atlantic and Mediterranean could not be more than 1·2 feet. Still less could the current into the Baltic, the entrance to which is in some places only fifty to sixty feet deep, be thus explained.

In 1872, Mr. Croll wrote several letters to *Nature* in support of his theories, in reply to various objections raised in the course of the correspondence on the question. They dealt chiefly with the physical difficulties of any theory of oceanic circulation based on specific gravity.

In 1874, he continued the subject in a paper on "Ocean Currents," published in the *Philosophical Magazine* for February (Paper 52). Both he and Dr. Carpenter agreed that ice-cold water is found at the bottom of the ocean in intertropical regions, and that it must have come as an underflow from the pole. Mr. Croll, however, regarded this as due to compensating *currents* from the pole, and due to the influence of wind; Dr. Carpenter, as part of a general underflow of the ocean from the poles to the equator, and due to gravi-

tation. Mr. Croll pointed out that there are many cold *surface* currents from the pole, which would consequently, *ex hypothesi*, be flowing uphill against the influence of gravity. Of these he gave numerous examples, which he affirmed were true currents and not part of a general oceanic movement, which would take place, if it existed at all, in the opposite direction. Such surface cold currents were easily explained by the wind theory, since the current would always take the path of least resistance, remaining on the surface so long as winds were favourable, and descending when they became antagonistic. The rest of the paper is devoted to an examination of the mechanics of Dr. Carpenter's theory.

In the *Philosophical Magazine* for March 1874, Mr. Croll explained his own views in an article entitled "Ocean Currents. The Wind Theory of Oceanic Circulation" (Paper 53). The currents of the ocean are caused by winds, not, however, as often incorrectly stated, by trade winds only, but by the total system of prevailing winds. All the seas and oceans, excepting a few small inland sheets, are really members of one great system, and any great change in one would modify the condition of all the others. Similarly, all the prevailing winds form one great system. The direction of the currents produced depends, first, on the general system of the prevailing winds, and, secondly, on the conformation of sea and land. A chart compiled from the standard authorities shows that without exception the direction of the main currents of the globe agrees exactly with the direction of the prevailing winds. So complete is the agreement, that, given any two of the following,—1. The conformation of sea and land; 2. The general system of prevailing winds; 3. The general system of ocean currents; the third might be deduced approximately, *à priori*. Currents, as already stated, take the path of least resistance. The Gulf Stream, for example, passes under the polar stream on the west coast of Spitzbergen, and this polar current in its turn passes under the Gulf

Stream near Bear Island. The current in each case continued a surface current as long as the prevailing winds permitted. Under-currents are usually compensatory, as, for example, the under-currents of the Gulf Stream compensating for the water impelled northwards by that current. Dr. Croll alluded briefly to the light thrown on the cause of the Glacial epoch by the true understanding of the relation between winds and currents, and showed the part played in secular changes of climate by the modification of the trade winds, owing to the physical effects of a high state of eccentricity, and the consequent deflection of the equatorial currents.

Mr. Croll next published a short article in *Nature*, May 21, 1874 (Paper 54). Dr. Carpenter had pointed to a polar under-current in the North Atlantic, 3600 miles in breadth, and 1500 fathoms thick, which, he maintained, was too vast to be merely the reflux of the Gulf Stream, a comparison of sectional areas showing it to be about 900 times greater. Mr. Croll in reply did not admit that it was a current, since there is no evidence that it is in motion, and the sea-depths, below the stratum heated by the sun, are ice-cold. But, even were it a current towards the equator, its volume would depend not only on its sectional area, but also on its velocity, so that it might still be the reflux of the Gulf Stream. But, as return under-currents as well as surface currents are due to winds, it is not correct to talk of this mass of water being set in motion by the Gulf Stream.

A paper on the "Physical Cause of Ocean Currents," published in the *Philosophical Magazine*, June 1874, only requires to be noticed (Paper 56). It pointed out a number of exceptional cases, for which the gravitation theory offered no explanation.

In 1875, Mr. Croll contributed to the *Philosophical Magazine* for September an important paper entitled "The *Challenger's* Crucial Test of the Wind and Gravitation Theories of Oceanic Circulation." A slope

from the equator to the poles is essential to any gravitation theory. The results obtained by the *Challenger* show that the surface of the Atlantic is lower at the equator, and rises by a gentle slope to about the latitude of England. Therefore, it cannot be gravitation which impels the surface water of the North Atlantic from the equator to the pole. This, Dr. Croll showed by calculating from the *Challenger's* data the height of three columns of sea water, two in the mid-Atlantic and one at the equator, the result being that the equatorial column was the shortest of the three.

In the *Philosophical Magazine* for the following month, October 1875, Dr. Croll published a paper on the same subject (Paper 60). The supporters of the gravitation theory had objected that Dr. Croll failed to account for the presence of ice-cold water in the ocean depths, which cannot be due to the comparatively insignificant polar under-currents. He replied that, since the sun's heat can only warm a stratum of the ocean, and the heat from the earth's crust is practically unappreciable, an extremely small current of polar water would keep the ocean depths ice-cold.

Dr. Croll discussed two other trifling technical points in two letters in *Nature*, October 7 and November 25, and concluded his examination of the theory of ocean currents by a paper in the *Philosophical Magazine*, December 1875 (Paper 63). Dr. Carpenter had disputed Dr. Croll's estimates of the columns of Atlantic water, published in the *Philosophical Magazine* for September 1875, and urged that the results would be neutralised by the inferior salinity of the equatorial column. The extra salinity of the North Atlantic had been shown by the *Challenger* to extend to no great depth, and Dr. Croll consequently met the objection by pointing out that the *Challenger* had found scarcely any difference in specific gravity between the polar and North Atlantic columns. Hence Mr. Croll maintained that, using Professor

Hubbard's tables for the expansion of sea water, the surface of the Atlantic in lat. 38°, to be in static equilibrium, must be 3 feet 3 inches above that of the equator, and 2 feet 3 inches in lat. 23°. Hence there was no alternative but to admit that gravitation could not be the cause of a surface current to the pole, and that the true cause must be the influence of prevailing winds.

CHAPTER XIV

6. MISCELLANEOUS PAPERS

IN 1864, Dr. Croll published a paper on "The Influence of the Tidal Wave on the Earth's Rotation," and on "The Acceleration of the Moon's Mean Motion" (No. 9). In 1866 he again took up this subject, and wrote a paper on "The Influence of the Tidal Wave on the Motion of the Moon" (No. 20). In these he concluded that diurnal rotation of the earth will some day cease. He also argued that the moon and earth are approaching each other, and some day will fall together, which is contrary to current ideas.

The following letter to Dr. Croll from Sir John Herschel, dealing with this matter, will be of interest:—

COLLINGWOOD, *14th August* 1866.

DEAR SIR,—I beg leave to thank you for your paper from the *Philosophical Magazine* for this month, on "The Influence of the Tidal Wave on the Moon's Motion." At the same time, I hope you will excuse me when I say that I am obliged to demur to most of your reasoning, and to some of your conclusions. I will waive, for the present, the question as to the validity of the assumption that the heat generated by the friction of the waves is at the expense of the *vis viva* of the earth's rotation. Granting this, for argument's sake, I am unable to see upon what sound dynamical principle you conclude that such an expense of *vis viva* will in any way affect the orbital revolution of the moon and earth about their common centre of gravity. I cannot under-

stand how, or in what sense, it is the *vis viva* of rotation (meaning rotation of the earth on its axis) that keeps the two orbs separate from each other. I always thought that it was not the motion of rotation of one or both bodies about their own axis, but the motion of bodily translation through space, under the mutual control of their joint attractive forces, which caused them to describe ellipses round each other and so not to come together. I have always considered, for instance, that, if the earth revolved on its axis a hundred times as fast as it does, or did not revolve on its axis at all, the length of the year would be unaltered, and so of the sun. So also of the earth and moon in their monthly revolution. I believe, too, that every one who has considered the subject of orbital motion (including yourself on more mature consideration) will admit this to be the case.

No change in the time of rotation of a sphere arising (if possible) from internal causes, or if from external, from the application of any forces whose resultant is not in a direction opposed to the motion of translation through space, *can* affect the *momentum* of the sphere, however it may alter its total *vis viva*. The direction and velocity of its *motion of translation* must therefore remain unaltered, however its total *vis viva*, which is the sum of the *vis vivas* of translation, rotation, and intermolecular movements of every other kind, may vary, and upon this, and this only, its orbital motion depends.

Permit me to remark on your illustration by two balls connected by a rod and revolving. If you retard the motion of the ball representing the earth, as for instance, by applying your finger against it, no doubt the effect you speak of will take place. But to make the cases parallel, you ought to suppose your two balls revolving in gimbals at the two ends of the rod, and your force should be applied so as only to *retard* the rotation of one of them in its gimbal, *without*, at the same time, acting on the rod, as, for instance, by applying cold and freezing the oil that lubricates the axes in the gimbals.

As before remarked, the total *vis viva* (?) of the earth is the sum of three things—(1) the *vis viva* of bodily translation through space; (2) the *vis viva* of its bodily rotation round its axis; and (3) that of all the molecular movements going on within it, independently of its motion or rest as a mass. Its *total momentum* is that of bodily translation only, that of the other two species of movement being, each of them, *nil*. Now, according to all dynamical theories, both $\int mv^2$ and $\int mv$ are invariable, except by the action of external force; and (for the reason last stated) no change in $\int mv$ *can* arise indirectly through the application of forces which go to change only the two last portions of $\int mv^2$.

Granting, lastly, that the frictionally evolved heat *is* solely evolved at the expense of the total *vis viva* of the earth (an assumption in my opinion very open to question), and knowing *à priori* that it cannot be at the expense of the portion due to translation, there are still *two other* sources from which it may be drawn, the rotatory source and the intermolecular. Now, considering the sort of movements in which we fancy we have reason to think heat consists, I for one should feel much more inclined to derive frictional heat from the latter than from the former of these two stores. However, if any one choose to assume the contrary, I see at present no distinct and clear grounds for denying its *possibility*.

But, after all, the primary question still returns. *Is* the heat generated by the friction of the "tide waves" produced at the expense of the earth's *total vis viva?* When water is heated by being shaken up in a bottle, does the heat come *out of the water*, *i.e.* is it produced at the expense of the inherent *vis viva* of that water, or does it come out of the agitating mechanism, the external source of power? There is the bottle of hot water, quietly standing on the table, where it stood before cold. Its movement of translation through space, and

of rotation on its axis as part and parcel of the earth, are the same. But it is hot now, and was cold. It has gained, not lost *vis viva*, and that has come to it from without.

Now is it certain that that may not be the case when heat results (if it does result) from lunar attraction? And, again, is it certain that any whatever increase in the total heat of the earth *is* produced by the friction of the tide wave?

Hoping you will excuse these observations,—I remain, yours truly, J. F. W. HERSCHEL.

In considering the climate of our globe past and present, an essential factor is that of the relation between the heat received by the earth, to that radiated out from it into stellar space. The temperature of space is of great importance, and various attempts have been made to estimate it. M. Pouillet assigned a temperature of -224° F. and Sir John Herschel one of -239° F. or 222° above the absolute zero, -461° F. If the heat of the stars be as feeble as their light, it is extremely unlikely that the absolute temperature of space can be so high as 222°, and it is very probably little above absolute zero. Dr. Croll argued (Paper 78) that the rate of radiation at the outer confines of the earth's atmosphere would be nearly proportional to the absolute temperature, and would diminish with the deepening of the atmosphere, and the increase in its density and volume, and the amount of water vapour. In high latitudes, as well as at high altitudes, ice and snow can more readily accumulate, as most of the heat received from the sun is rapidly radiated off into space. But in high latitudes excessive humidity may have the same effect, the clouds and fogs cutting off so much of the sun's heat, that the winter's snow and ice are not melted in summer (Paper 79).

Mr. Croll examined M. Adhemar's theory (Paper 33) that the radiation during the seven or eight days' excess of the southern over the northern winter was

sufficient to cause the accumulation of Antarctic ice. But radiation from land is more rapid than radiation from water, and hence the great preponderance of ocean in the southern hemisphere renders the rate of radiation much slower than in the Northern, where land is accumulated. In this way the influence of the slightly longer winter is more than counterbalanced. The true cause of the accumulation of Antarctic ice Dr. Croll found in the deflection of the more important warm ocean currents northwards across the equator, by which means the southern hemisphere is deprived of a vast quantity of heat.

Ocean and aerial currents have also a very great effect on the temperature of the air at the equator. If temperature were determined solely by the intensity of the sun's heat, the maximum temperature at the equator would occur in January, when the earth is in perihelion. Owing, however, to the distribution of sea and land, the northern hemisphere is more cooled in winter and more heated in summer than the southern; and the mean temperature of the air over the surface of the earth is higher in July than in January. The trades of July, especially those of the northern hemisphere, are consequently warmer than those of January, and, therefore, the mean temperature of the equator is also higher in July than in January (Paper 76).

Mr. Croll was always greatly interested in metaphysics; and this is shown in several of his papers, but more particularly in those on gravitation and molecular motion. His discussion of the latter (Paper 49) is a vigorous attack on purely materialistic explanations. Recent progress in physical science, he considered, was concerned almost exclusively with quantitative relationships of energy; and here, not less than in the natural sciences, little or no attention had been concentrated on the real problem, the investigation of "*the objective idea in nature*" as distinct from the forces which are working it out.

Another ingenious investigation relates to the dis-

placement of the earth's centre of gravity by a polar ice-cap, resulting in submergence. This suggestion was first advanced by M. Adhemar, in his work *Revolutions de la Mer*, in 1842, but when Croll published his views on the subject in the *Reader*, he was unaware of Adhemar's conclusions. In connection with this question Croll estimated the probable thickness of the Antarctic ice-cap, and computed the rise in the level of the ocean, resulting from the transfer of an ice-cap two miles thick from the southern to the northern hemisphere. According to the method which postulates the rise at the Pole to be equal to the extent of the displacement of the earth's centre of gravity, he inferred that the rise at the North Pole would be about 380 feet, and the rise in the latitude of Edinburgh would be 312 feet. By this means he endeavoured to account for the submergence during the Glacial period, instead of ascribing it to a subsidence of the land.

Following up the idea of the existence of Continental ice-sheets during the Glacial period, he suggested that the Scandinavian and Scotch ice-sheets coalesced on the floor of the North Sea, moving westwards towards the Atlantic, thereby accounting for the marine shells and boulders of Secondary rocks in the Caithness Boulder Clay.

In connection with the movement of Continental ice-sheets and glaciers, he was led to investigate that perplexing question in physics, viz. the physical cause of glacier motion. He reviewed the various theories which had been advanced to explain this phenomenon, indicating various objections to them. He ultimately advanced an ingenious explanation of his own, which may here be briefly summarised from his statement of the theory in *Climate and Time.*

Ice is not absolutely solid throughout. It is composed of crystalline particles which are not packed so closely together as to exclude interstices. They are united to one another at special points determined by their polarity, and on this account they require more

space. It will be obvious, then, that when a crystalline molecule melts, it will not merely descend by gravitation, but capillary attraction will cause it to flow into the interstices between the adjoining molecules. The moment that it parts with the heat received, it will of course resolidify, but it will not solidify so as to fit the cavity which it occupied in the fluid state. For the liquid molecule in solidifying assumes the crystalline form, and of course there will be a definite proportion between the length, breadth, and thickness of the crystal, and consequently it will always happen that the interstice in which it solidifies will be too narrow to contain it. The result will be that the fluid molecule in passing into the crystalline form will press the two adjoining molecules aside in order to make sufficient room for itself between them. The crystal will not form to suit the cavity, the cavity must be made to contain the crystal. And what holds true of one molecule holds true of every molecule which melts and resolidifies. This process is therefore going on incessantly in every part of the glacier, and in proportion to the amount of heat which the glacier is receiving. This internal molecular pressure, resulting from the solidifying of the fluid molecules in the interstices of the ice, acts in the mass of the ice as an expansive force tending to cause the glacier to widen out in all directions.

The lateral expansion of the ice from internal molecular pressure, according to Dr. Croll, explains how rock basins may be excavated by means of land ice. It also removes the difficulties experienced in accounting for the movement of ice up a steep slope. Nay, further, he called attention to the fact that the ice which passed over Strathmore must have been over 2000 feet in thickness. An ice-sheet 2000 feet thick exerts a pressure on the rocky floor of upwards of 51 tons per square foot. When we reflect, he contended, that ice under such enormous pressure, with grinding materials lying underneath, was forced by irresistible molecular energy up an incline of one in seven, it is not at all

surprising that the hard lava would be ground down and striated. It also helps us to realise how the softer portions of the rocky surface over which the ice moved should have been excavated into hollow basins.

The papers on the movement of glaciers Croll had sent to his friend Professor Foster, who wrote the following letters thereanent:—

16 KING HENRY'S ROAD, REGENT'S PARK ROAD, N.W.,
21st February 1869.

MY DEAR CROLL,—You must think me a very inattentive correspondent, for I believe I have had for a long time two letters of yours unanswered, and it is very long-suffering of you to write to me again. But the truth is, I have never yet found time to consider your last paper carefully enough to enable me to make any criticism upon it worth your having. I was, however, very much interested in it, and, so far as I was competent to form an opinion of your reasoning, it seemed to me all sound.

I am not sure that I clearly see what your idea in the paper you sent me the other day is. So far as I make it out, you attribute the movement of glaciers to what one might call waves of fusion going through the whole mass, each part being in its turn melted and solidified again. This no doubt would produce movement, but I do not feel to have a clear conception of how you suppose it to take place. Is not your notion something like James Thomson's (*Proc. Roy. Soc.* vol. viii., 1857), only that he attributes the fusion to pressure, and you to heat propagated through the mass?

In the first part of the paper, when you speak of the transmission of heat through ice without melting it, you seem to me to refer to *radiation*, but the kind of mechanism by which you suppose the process to take place would correspond better with *conduction*. The two phenomena, whatever may be the mechanism of them, are certainly distinct, and as a rule the bodies which conduct best (*e.g.* metals are those which allow the least

radiation to take place through them, and *vice versa*) *e.g.* glass, rock-salt, transparent things generally.

I hope you like your work in Edinburgh and find your present position satisfactory.—Believe me, very sincerely yours, G. C. FOSTER.

UNIVERSITY COLLEGE, LONDON,
16th November 1865.

DEAR MR. CROLL,—I have read your paper, and return it by this post. It seems all right and good; the only points that occur to me are as follows, p. 7: "As the ice continued to accumulate . . . the melting power of the summer sun would consequently diminish. . . ."

Is not this, to some extent at least, putting cause for effect? does not the ice accumulate *because* the melting power of the sun is diminished?

Same page, lines 4 and 5 from bottom. Is not "by lengthening the ice-accumulating period" already included in effect under 1. "By allowing the ground to cool . . . etc."? I would say "2. By shortening the ice-melting period," leaving out the first clauses. Near the end of the paper there seems perhaps to be a little repetition from about page 12; and the calculations about the summer and winter temperature depending on change of eccentricity, etc., from page 9, though quite clear enough for me to be able to follow them, would require more care for their understanding than most readers would give. No doubt the whole matter necessarily requires thought on the part of the reader, but I think it is worth while to try to put it a little more easily. For instance, it would help if, when numbers are mentioned, they were accompanied by statement of what they mean, thus, bottom of p. 11 and top of p. 12, a reader would easily miss the meaning of the numbers 222 and 230.—Yours very truly,

G. C. FOSTER.

UNIVERSITY COLLEGE, LONDON,
12th November 1867.

DEAR MR. CROLL,—I have been much interested in reading your remarks on the theory of gravitation, but fear I may have caused you inconvenience by keeping the papers so long.

Does not the difference between your view and that involved in the way in which the theory is commonly stated turn, to a considerable extent, on the sense attached to the word "force," and on what it is to which we take the law of *Conservation* as applicable? Do you not use "force" in two senses when you talk of the *force* or dead pull of gravity and the *force* = *vis viva* of a falling stone? The distinction has of course often been pointed out,—see specially J. R. Mayer in *Phil. Mag.* xxv. "Mech. Equiv. of Heat," 1863,—as you are quite well aware. *Vis viva* is never generated by dead pull or dead pressure unless the pull or pressure does work, *i.e.* causes displacement of its point of application. And I think it cannot be said that there is such a thing as Conservation of Force, if we understand by Force mere dead pull or pressure. What the law of Conservation does apply to is "energy," which we may define nearly enough as "capacity for producing physical change." The *energy* of a raised weight is thus the product of the gravitation pull upon it × distance through which this pull can act; in other words, *energy* = a *force* × a *length*. Hence force is only a factor of energy and is not of itself included under the law of conservation. And I think all kinds of potential energy are expressible in a similar way, as products in which one factor is a *force*, dead pull or pressure. *E.g.* energy of fuel = force of affinity for oxygen × mass of fuel.

I don't write this because I suppose I am telling you anything new, but I do not see that in the paper you sent me you have given these considerations sufficient weight; in fact, I think there is a little confusion between *force* and *energy*.

I am afraid what I have said will not be of much use to you, but I am just now unable to write more. If you think I can help you at all by further discussion, I hope I shall hear from you again about the matter, and of course about anything else where I can be of any assistance.—Believe me, always very truly yours,

G. C. FOSTER.

In 1870 he seems to have been in fairly good health, and mentally he was very busy. He wrote and published the results of two speculations on "Ocean Currents," Part I., which dealt with ocean currents in relation to the distribution of heat over the globe, and appeared in the *Philosophical Magazine* of February 1870. This paper was closely followed by Part II., which dealt with "Ocean Currents in Relation to the Physical Theory of Secular Changes of Climate," and appeared in the *Philosophical Magazine* of March 1870; and Part III., which was on "The Physical Cause of Ocean Currents," and included an examination of Lieutenant Maury's theory, appeared in the *Philosophical Magazine* of October 1870.

Though speculating and writing so much on the abstruse subject of ocean currents, he still devoted a share of his leisure time to geology; and wrote two papers on "The Path of the Ice-sheet in North-western Europe, and its Relations to the Boulder Clay of Caithness," which appeared in the *Geological Magazine* of May and June 1870, and were afterwards reprinted, with a slight rearrangement of paragraphs, in *Climate and Time*, chap. xxvii. He also continued his investigations regarding glaciers, and wrote two papers on "The Cause of the Motion of Glaciers," which appeared in the *Philosophical Magazine* of September 1870. (A short abstract of the same, by the Editor, appeared in the *Geological Magazine*, December 1870.)

Being so much engaged with the investigations necessary for and the writing and publishing of these papers during the year 1870, he had not much time for

correspondence; but that these papers were attracting the attention of the most distinguished scientific men there can be no doubt. Accordingly, Croll received numerous letters on the subjects treated of by him; and the following letters from Mr. Alfred R. Wallace and others will show the interest taken in Croll's speculations at this time. Unfortunately Croll's replies have not been preserved.

9 ST. MARK'S CRES., REGENT'S PARK, N.W.,
14th March 1870.

James Croll, Esq.

MY DEAR SIR,—I must apologise for not having written before to thank you for having kindly sent me copies of some of your papers. The last one on "The Old River Channels" was exceedingly interesting.

You will see that I have made use of your tables of eccentricity in a paper in *Nature*; and if you have still any copies of the papers containing those tables, I should much like to have them, for though I have a very scanty practical acquaintance with geology, I am exceedingly interested in all the wider problems with which it deals, and one group of which you have done so much to elucidate.—I remain, dear sir, yours very faithfully,

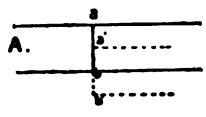
ALFRED R. WALLACE.

HOLLY HOUSE, BARKING,
25th September 1870.

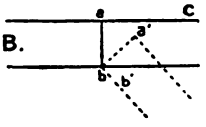
James Croll, Esq.

DEAR SIR,—Thanks for your paper on "The Cause of Motion in Glaciers," which I have read with much interest, though I cannot say I am quite satisfied your explanation is the correct one. The fact that glaciers do move in winter when thickly covered with snow, seems entirely to upset your theory of molecular changes caused by heat, as it does that of Canon Moseley. It seemed to me when I read Canon Moseley's paper that his first error was in considering that the fractures of ice in a glacier are

shearing at all, in his sense of the word. If I take a piece of ice A, and by contrary and parallel pressure at



a and *b* cause it to fracture all along the line *a b* simultaneously, so that one half of it is moved into the position a^1 b^1, I exert the shearing force as understood by Canon Moseley; but if I take the same or a similar piece of ice B, and by pressure at *c* with *b* as a fulcrum cause a fracture on the line *a b*, bringing



the outer half into the position a^1 b^1, will it not require a very much less than the shearing force? Now the uneven bottom and sides of a glacier must supply a number of fulcra, and the constant motion of the glacier in an uneven bed must cause unequal tensions and compressions, producing numerous points and lines of least resistance, and to cause a fracture at some point a line of least resistance extending over a few inches or feet, a mass of ice many hundreds of feet thick may act by its weight. Thus a number of *small fractures* and dislocations may be produced in *succession* at the ever-varying lines of least resistance by a weight of ice which would be quite sufficient to fracture by *shearing* or in any other way the whole mass at once. If *molecular changes* caused the motion of glaciers, then why should there be so many fractures as there are? If the weight of the glacier is insufficient to produce fracture, whence come crevasses? If it is sufficient under favourable conditions to produce the great fissures which sometimes run for a mile and widen into crevasses, surely it can produce the smaller cracks and fissures, which, continually occurring at every changing line of least resistance, and being soon obliterated by regelation, will suffice for the slow and viscous-like motion of a glacier. It would be very important to watch the interior of a plank of ice while bending as in Mr. Matthews' experiment, and see whether this occurred by minute cracks or by molecular change. This could be done by means of

a beam of light passed through it and condensed on a screen.

The cause of the *greater* motion by day than night, and in summer than in winter, seems simple enough, the action of running water *beneath* the glacier and in its fissures forming a buoyant cushion for it to move on, and keeping its parts to some extent free.

Can it be proved mathematically that the force of gravity is not sufficient for the continual readjustment of the equilibrium of the parts of a glacier by repeated *small fractures* along lines of least resistance, the weight of large portions of the glacier acting *successively* to effect these fractures and readjustments of small portions of it? The parallel and simultaneous fracture of a mass of ice termed *shearing* can never, I think, occur in nature, but *angular fracture*, produced by unequal pressures on two sides of a fixed point, must be continually occurring. The fact that glaciers are most *crevassed and fissured* where the greatest change of form occurs, whether by passing through a narrow gorge or down a steep incline, plainly indicates that it is by means of fractures, and not by molecular changes, that the viscous-like flow of a glacier is produced.

I was sorry you were not at the British Association meeting. I exhibited a large diagram drawn from your tables of eccentricity, and explained its bearing on climate and the rate of organic change. It excited some interest and caused a pretty good discussion, but a Russian mathematician maintained that all calculations of eccentricity and precession for more than a few 10,000 years were so uncertain as to be of *no value whatever*, and I wanted you to answer him, as I am not sufficiently acquainted with the subject.—Believe me, yours very faithfully, ALFRED R. WALLACE.

About this time Croll was invited to lecture at the Royal Institution, as appears from the following letter. Unfortunately, the state of his health, the pressure of

other duties, and his shrinking modesty led him to decline this flattering request.

84 BROOK STREET, *20th November* 1870.

SIR,—At the wish of the managers of the Royal Institution, I write to ask whether it would suit your convenience next summer to give a short course of three lectures on any geological question, or on ocean currents, or any other subject. The 23rd and 30th of May and the 6th of June at three o'clock would be the lecture time,

HENRY HOME JONES.

Pursuing his investigations into glacial geology, he made an excursion to Allermuir, one of the Pentland range, which is thus described by Mr. James Bennie, of the Geological Survey of Scotland, in a paper read to the Physical Society of Edinburgh, on 18th April 1883:—

"ON THE GLACIATED SUMMIT OF ALLERMUIR, PENTLANDS.

"Some time during the spring of 1870, Dr. Croll told me that he intended going to some of the higher slopes of the Pentlands, to see how far up the ice-markings of the Glacial period extended, and asked me to help him. I readily agreed, and one bright morning we started for that purpose, duly equipped with compass, hammer to take chips, spade to dig for clay, and bag to carry the samples of each collected.

"We went through the fields to Swanston, going round the farmhouse, up a defile, passing by the old village of Swanston, and so on to the flanks of Caerketton. The way was pleasant, and the suggestive talk of my companion made the walk delightful and piquant. Among other things we discussed the why and wherefore of our mission.

"Dr. Croll said he had been trying lately to account for and explain the condition of the Boulder Clay of Caithness, on the theory that it was, like all the other boulder clays of Scotland, the product of land ice.

This theory he had applied to every circumstance and characteristic of the Caithness Boulder Clay, and found that it accounted for them more satisfactorily than any other;—nay, that the very exceptional one—the occurrence of fragments of marine shells throughout its entire mass—was not against its theory, but in favour of it—indeed, a main proof of its truth.

"Caithness, he said, being a low, flat country—little more than thirty feet above sea-level—could not produce land ice of its own to glaciate it; but that very circumstance made it possible to be invaded and glaciated by land ice from other regions. That ice he supposed to be the ice which flowed outwards from the eastern slope of Scotland into the North Sea, or rather, the hollow now known as the German Ocean, but which at that time could scarcely be called a sea, but rather a *mer de glace*, the ice having poured into it from the highlands on its eastern and western sides. That ice would be certainly many hundreds of feet, perhaps some thousands of feet in thickness, and would have no difficulty in filling to overflowing a basin which we know from the present depth of the German Ocean to have been only 200 or 300 feet in depth. The ice-streams coming from opposite directions would meet and press against each other with a force proportional to the impulse each had. The ice from the eastern side—that is, from the highlands of Sweden and Norway—would be far greater in amount and power than that which flowed in from the western side, from the highlands of Scotland, just as the area and height of the Scandinavian mountains surpass those of Scotland. In consequence of the superior volume and force of the Scandinavian ice, the ice which streamed out from Scotland would be pushed back upon itself, and forced to heap itself up against the land, and, if the land was low, to override and pass over it, carrying with it all the *débris* of mud or stones it had originally, or had picked up by the way. This theory was confirmed by the stones found in the Boulder Clay of Caithness. Many

of these were known, by the researches of Mr. Dick of Thurso and Mr. Charles Peach, to consist of chalk flints and fragments of Oolitic limestone, which had come from the southern shores of the Moray Firth, and which could only be carried to Caithness by ice flowing from thence, and crossing over the basin of the Moray Firth, from which also it could easily get the shells so abundantly scattered through the Caithness Boulder Clay. This was the theory which Dr. Croll applied to the solution of the problem, and it fitted into all the circumstances of the case so completely, that he had no doubt of its truth.

"But it was with only one consequence of this state of things that we are concerned at present. This was, that the Scottish stream of ice, having been met by one greater in volume and force than itself, would be dammed back, or heaped up, as it were, till it rose to a height almost equal to that opposed to it; and, of course, as the Scandinavian ice was many hundreds of feet in thickness, the Scottish ice would be almost as thick, and would be piled up upon the land till it overtopped the highest hills on the eastern slope of Scotland. Now, if such a result had taken place in the Glacial period, evidence of it might yet exist on the sides or slopes of, say, the Pentlands, and it was to search for such that our excursion was projected. This idea, given with all the force of language and aptness of illustration of which Dr. Croll was capable, imparted a zest to it which inspirited me to brave the stey braes we had to surmount to verify it. My companion proposed that we should go first of all to the very top of the hills, and if we found them glaciated, that would prove at once that the ice had reached that height at least. We therefore set ourselves to climb the two highest peaks of the east end of the Pentlands, called Caerketton and Allermuir, respectively 1600 and 1618 feet above the level of the sea. We ascended Caerketton first, but found the top a mass of stony *débris*, recently made by the weathering of the rock of which the top consists, a volcanic breccia which frosts could easily split and

divide. We looked for glacial striæ on some of these fragments, but found none.

"We then descended into the hollow between the two hills, and climbed up to the top of Allermuir. We found the top to be only a few square yards in extent, partly in grass and partly bare rock. We turned to the rock first, and saw that it was a hard red porphyrite, quite capable of receiving and retaining striæ, and we eagerly looked over the exposed surfaces. At first we found nothing, the surface was too fresh, having been recently formed by fracture from weathering. We then examined the fractured pieces lying loosely about, and very soon found some which had unmistakable striæ upon one surface. These, we concluded, had formed the original surface of the hill-top very recently. Looking round, we saw signs that the hill-top had been purposely bared, the turf having been taken up to be built round a signal pole for triangulation. Encouraged by this success, we then sought the original surface *in situ*, and putting the spade into requisition, we soon uncovered a portion, and found it smoothed and as finely striated as it could possibly be. On the very top of the hill was a small space, perhaps a square yard or so, which showed not rock, but earth. Into this I struck the spade, and dug till I came at the depth of a foot or so to the rock. The earth I turned up was full of small pebbles, and might be considered a kind of boulder or stony clay. A portion was immediately bagged for washing and examination afterwards. The rock surface beneath the earth was, as I have said, finely smoothed and striated, and having cleared and dusted two or three square feet, the direction of the striæ was taken by compass, and made out to be a few degrees north of west. From the mouldings of the surface we concluded that the ice which had polished and scratched it had come from the west and gone to the east. On examining the rock we had just bared, we found it cracked in various directions, doubtless by recent weathering since the turf had been removed, and with

little difficulty we loosened a small portion of it, and lifted it up and bagged it. This was afterwards set in Portland cement, and is now on the table for the inspection of the members. [It may be seen in the Edinburgh Museum of Science and Art.]

"Having thus successfully accomplished our mission, I shovelled the earth back into the hollow again, my heart humming Eureka, not loud but deep, befitting the occasion. As a fierce north wind was blowing strongly over the hill-top, we slipped down a few yards on the south side of the hill, out of the wind and into the sun, which was shining brightly and pleasantly warm, and discussed our lunch and the facts which our discovery proved. The summit of Allermuir, my companion at once concluded, could have been glaciated only by ice that had overridden it. That fact there could be no gainsaying,—ice in motion being the only agent in nature capable of polishing and striating rock surfaces such as we had just uncovered. If ice, then in what form? Was it sea ice—in floe or berg—or coast ice? The answer was, No, decidedly no;—the reasons for which, as Dr. Croll had studied the question thoroughly, he readily recited to me, and which may be found set forth at length in the twenty-seventh chapter of his work on *Climate and Time.* Land ice was then suggested—not, however, as glaciers formed on hill slopes and flowing down valleys as in the Alps, but land ice in the form of a sheet, covering the whole face of the country,—hill and valley alike, as is seen in the case of Greenland,—and flowing like it from a centre of dispersion outwards to the sea on all sides of Scotland. The centre of dispersion in Scotland would, of course, be the highest part of it—the group of its highest hills, the Grampians, and the high land surrounding them. There the snowfall then, as the rainfall now, would be greatest; and as the snow accumulated and became, as its habit is under pressure, glacier-ice, it would flow outwards on every side towards the lowest levels it could find. On the western side, the

slope being short and steep, it would speedily reach the sea, but on this, the eastern side, the slope being low and long, and the country wide, the greatest outspread of ice would necessarily occur. At first, in the beginning of the Glacial period, the ice-sheet would not be very thick, but as the glacial conditions became more intense, the greater quantity of snow that fell in the longer winters would have less heat to melt it, as well as less time to be melted in the shorter summers, and the ice would grow thicker and thicker, and flow farther and farther outwards, year by year, till it not only spread over all the low grounds, but even invaded the North Sea, till its advance was checked by the stream of Scandinavian ice coming from the other side, as already explained. The obvious result of such a check should be to cause the ice streaming out from Scotland to be banked upon it, till it swallowed up and over-rode, first all the lower, and finally all the higher hills on the eastern slope.

"Such seemed to us the only method by which we could suppose the top of Allermuir to have been glaciated, and the stony clay to have been deposited in the little hollow on its highest point."

CHAPTER XV

PERSONAL HISTORY—CORRESPONDENCE WITH SIR C. LYELL, PROF. FOSTER, AND MR. A. R. WALLACE

IN 1871 Croll was very unwell; and, at the urgent suggestion of several friends, he consulted Dr. Warburton Begbie, the eminent Edinburgh physician. He had been suffering long from a mysterious pain in the head, which always came on when he used any mental exertion. During this year the pain was much more frequent and aggravated, but Croll was inclined to bear it patiently and work away for short intervals when he was free from it. He had occasionally consulted a family physician, who treated the matter more as a stomach derangement. Croll's friends, however, saw that that was mere trifling with the matter; and, after much persuasion, he agreed to see Dr. Begbie. The genial doctor received Croll after his usual kindly manner and prescribed for him. The treatment afforded some relief, for which Croll was truly grateful; and he ever after spoke of Dr. Begbie in the highest terms. This enabled him to go through a good deal of work this year. He wrote a paper on "The Transport of the Wastdale Granite Blocks," which appeared in the *Geological Magazine* of January 1871. He also wrote a paper on "The Method of Determining the Mean Thickness of the Sedimentary Rocks of the Globe," which appeared in the *Geological Magazine* of March 1871, and another on "The Mean Thickness of the Sedimentary Rocks," which appeared in the *Geological Magazine* of June 1871.

He opened up a new subject, and wrote two papers on "The Age of the Earth as determined from Tidal

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

time obliged to ask the revision of them by experts on these subjects. In this and many other respects no one ever proved to be a reader more interested or a friend more faithful than Professor Foster, F.R.S., of University College, London. From 1862, when Professor Foster was lecturing at the Andersonian University, Glasgow, where he had then met Croll, down to the date of Croll's death, he had been a constant correspondent, continually assisting him in checking his data and fortifying him with references to the authorities on which Croll was writing. From the kindly way in which Professor Foster always responded to Croll's requests on these matters, the latter felt that he had always a friend to whom he could apply for assistance in the matters under discussion, and who would readily respond without considering it a trouble to do so. It is not too much to say that without the assistance which Croll received from Professor Foster, he would not have been able to overtake nearly so much work as he did, or to have done nearly so much as he did, and with such accurate results. The following correspondence is only one of many illustrations of the kind of service which Professor Foster rendered to Dr. Croll.

EDINBURGH, *1st September* 1871.

Professor Foster, F.R.S.

MY DEAR SIR,—I have received to-day the proof-sheets of the next part of my paper on "Ocean Currents." It is chiefly devoted to an examination of Dr. Carpenter's theory.

I should like exceedingly much, could you do me the favour to glance over the sheets and point out any errors in my Mechanics which you may happen to observe.

If Dr. Carpenter be right in this matter, certainly my mechanical notions are far wrong. Of course any remarks which you may happen to make I shall regard as strictly confidential and a special favour from an old friend.

In case you may happen to be on the Continent on

your holidays at present, I shall not send you the proof till I hear from you. Excuse the trouble I put you to. —I am, yours sincerely, JAMES CROLL.

16 KING HENRY'S ROAD, REGENT'S PARK ROAD, N.W.,
27th September 1871.

MY DEAR CROLL,—I went to University College yesterday for the first time during eight weeks (having only returned here from Lancashire the day before), and found your letter of the 1st September awaiting for me, nothing having been forwarded to me since Grant left for his holiday just before your letter arrived.

If I had received your letter in time, I need scarcely say that I should have been extremely glad to try to give you any assistance in my power, and I was very much pleased by receiving your letter, as it shows that you believe as much.

I was greatly disappointed not to see you while I was in Edinburgh last month at the British Association. I spent a good while one afternoon trying to find your office, but I knew so little of the town that the answers I got when asking directions did not do me much good. I fancy now, however, that I must have passed the door twice. I felt sure I should be able to make another attempt, but the last day or two I was so much engaged that I had to come away without doing so.

After leaving Edinburgh, I was in Arran for a fortnight with my wife and two babies; since then we have been at my father's in Lancashire. With kind regards to Mrs. Croll,—Believe me always, very sincerely yours, G. C. FOSTER.

EDINBURGH, *27th November* 1871.

Professor Foster, F.R.S.

MY DEAR SIR,—I should long ere this have acknowledged the receipt of your kind letter.

I am sorry I did not see you when you were in Edinburgh. I have been off duty for a month or two previous, owing to pain in the head which prevented me

from doing any mental work, and had just returned to the office the week previous to the B. A. meetings. I could not on this account, with a clear conscience, ask for leave of absence to attend.

I was glad to learn from your letter that your family and yourself are well, and I am happy to add that Mrs. C. and myself are enjoying the same great blessing at present. I observe from your note that Grant is still with you. Please to remember me to him, and I will be glad to see him when he may happen to be in Edinburgh.

When you find leisure to read my reply to Dr. Carpenter, I need hardly say that I shall feel glad to get your opinion on the subject in a few lines. You may rest assured that I shall never follow Dr. C.'s example, and blab out to the world what my scientific friend may think of my views. If they cannot stand without such subterfuge, let them go to the four winds of heaven.—I am, yours most truly, JAMES CROLL.

The Rev. Mr. Fisher having written for a reference to some of his papers, Croll wrote the following letter in reply :—

EDINBURGH, *28th August* 1871.

Rev. Osmond Fisher, M.A.

MY DEAR SIR,—Parts I. and II. are stitched under one cover. The first of Part III. you have, and the next portion of it will be published in the *Philosophical Magazine* for October next, but when I shall get to the end I know not. I am getting tired of the subject.

I am glad the newspapers which I sent were of interest to you. I intended to have been in the country in August, but my illness in spring disarranged my plans.

I did not, however, attend any of the meetings or excursions of the British Association. There are several reasons for that. One is, I had exhausted my holiday time, and could not well leave the office. My chief reason, however, was that I dislike all such public displays. The truth is, I have very little sympathy with

the leading idea of the British Association, viz., that science is the all-important thing. I don't believe anything of the kind. There are more noble and ennobling studies than science. You can hardly expect one who has devoted twenty years of the best part of his life to the study of mental, moral, and metaphysical philosophy to have much sympathy with the narrow-mindedness of the British Association.

Philosophy is just as real a part of human knowledge as science; and the time is, I trust, not far distant when this will be universally recognised.

Had my house been a little more commodious, I should have been delighted to have seen you in Edinburgh.—I am, yours truly, JAMES CROLL.

HARLTON, CAMBRIDGE,
1st September 1871.

MY DEAR SIR,—It was careless in me to give you the trouble of telling me what I ought to have remembered, that the two first numbers of your treatise are bound together. But I am not sorry that my carelessness was the means of eliciting a letter from you.

I am much pleased to find that you think much as I do about the self-assertion now so much in fashion in the scientific world. I think, however, this is more noticeable among the pure naturalists than among the physicists. I think Darwin has upset some not over strong minds, and I cannot but fear that the moral effect of his doctrines will be injurious. I noticed your letter in *Nature* about the retardation of the earth's rotation by the tides. The Astronomer-Royal (Airy) once explained to me his opinion that the tides can have little effect in the open ocean on account of the great meridional divisions caused by the large continents, and that it is only that portion of them which can get round such capes as the Horn which can directly affect the rotation.

It strikes me that, in channels where the scour of the tides is great, the friction must act equally in opposite directions at the flux and reflux, but that portion of the

motion which is converted into heat must cause a loss of energy somewhere, and no doubt part of it must come out of the rotation. I must confess that I do not understand your letter in *Nature*. Possibly that arises from my not having seen the discussions to which you refer as having taken place at the B. A.

I have always understood that the internal arrangement of the materials of the earth can be proved, by the law of the variation of gravity upon its surface, to be intimately connected with its external form. If, therefore, it solidified when it was of a different form from what it has now, the present form being due to denudation, a discrepancy between the law of internal arrangement and the external form would be betrayed by observations on the pendulum. But if there be this difficulty, would the idea I suggested in the paragraph at the bottom of page two of my tract "On the Elevation of Mountains" be of any help? That is, would change of velocity in rotation so far alter the pressure as to allow of redistribution of the surfaces of equal pressure and density?

But this introduces a difficulty on the other side. For if the degree of pressure due to changes in the velocity or direction of rotation were competent to induce liquefaction and consequent changes in the distribution of the materials, would not the attraction of the moon and sun also be capable of destroying the rigidity? In other words, does not the rigidity of the earth, according to Thomson's view, prove that its rigidity is not due to pressure, and therefore that it is not throughout, as he thinks, at or near the melting temperature for the pressure at each depth? If you have time to think of what I have said, give me your opinion, and also as to whether you think the question worth propounding in a letter to *Nature* or elsewhere. But I fear *Nature* soars above my level, and would not put in a communication from me. They would, however, if you enclosed it for me.

Should you wish to see what Archdeacon Pratt says about the internal arrangement of the strata of equal

density and pressure, I will send you the book.—Believe me, very sincerely yours,

O. FISHER.

EDINBURGH, *9th September* 1871.

Rev. Osmond Fisher, M.A.

MY DEAR SIR,—No doubt what you say regarding the internal arrangement of the strata of equal density is all correct. And probably an argument from this could be made out to show that the earth must have solidified when rotating at its present rate. But all this has nothing to do with Sir William Thomson's argument. You will find on examining his original paper read to the Glasgow Geological Society, and also his reply to Huxley, that he does not make the least allusion to the internal arrangement of strata. Your idea does not appear to have entered his mind. His entire argument is of an entirely different character. It is neither more nor less than this: If the earth solidified some thousands of millions of years ago, it must have solidified at a time when it was rotating at a far greater rate than at present, and consequently at a time when its *equatorial diameter* must have been much greater than it is now; and if so, the land at the equator must have now been standing several miles above the sea. Or, in other words, the present form of the earth ought to be exactly what it was at the time of solidification. This is his argument. Now, if it can be shown, as I have endeavoured to do in my letter in *Nature*, that denudation would lower the equatorial land as fast as the sea would sink in consequence of decrease of rotation, Sir William's conclusion that the equatorial lands ought to be sticking out of the sea goes for nothing.

By all means draw up an argument on the subject, and send it to *Nature*, or *Geological Magazine*, or some suitable source. But you will require to take care that your readers do not confound your argument with Sir William's.

In my letter in *Nature* I had nothing to do with the

question whether the earth is actually losing rotation.—I am, dear sir, yours very truly,

JAMES CROLL.

In 1872 the pain in the head returned on the slightest mental exertion, and Croll was obliged again to consult Dr. Warburton Begbie, under whose treatment he remained during the greater part of the year with some benefit. Temporary relief, however, was all that could be afforded by the use of palliative measures. Still, Croll was able to attend to his office duties and go through a considerable amount of independent investigation and literary work after his business day was over. He returned to the subject of Ocean Currents, and wrote a paper which appeared in *Nature* on 11th January 1872. He replied to some criticisms by Mr. Ferrel on his theory of Ocean Currents in *Nature*, 21st March 1872, and followed this by proof that ocean currents are not due to gravity, in *Nature*, 25th April 1872. He again replied to Mr. Ferrel's objections in *Nature*, 25th July 1872. He returned to one of his former physical studies and wrote a paper on Kinetic Energy, which appeared in *Nature*, 15th August 1872. He likewise wrote a reply to Professor Everett and Mr. Wallace on Oceanic Circulation, which appeared in *Nature* on 3rd October 1872.

In addition to these physical papers, notwithstanding his impaired state of health, Croll returned to his favourite study, metaphysics, and wrote a paper entitled "What determines Molecular Motion, the Fundamental Problem of Nature," which appeared in the *Philosophical Magazine* of July 1872.

Professor Ramsay of the Geological Survey, who, as Croll stated, was almost, if not the very first distinguished man of science to recognise Croll, and lend him a helping hand, remained a faithful friend to the time of his death. He was a constant correspondent, and, as was characteristic of his kindly nature, never lost any opportunity of doing Croll a good turn. In January 1872 he proposed

Croll as a fit and proper man to receive the proceeds of the Wollaston Fund for that year. Some objection to this proposal was made on the ground that Croll's researches did not involve any personal outlay. The following letter, thoroughly characteristic of Sir Andrew's best manner, shows the energy with which he laboured to bring his proposal to the favourable issue which it happily reached.

LONDON, *11th January* 1872.

MY DEAR GEIKIE,—Yesterday, in the Council of the Geological Society, I proposed Croll as a proper man to receive the Wollaston Fund for the year. . . . The President and others hailed my proposition. One objection raised was that Croll's researches involved no personal expense. Prestwich and I thought that of no importance; but, nevertheless, if you can tell me anything on that score, I shall be doubly armed—

> "And on the top of opportunity,
> Quell the base scullion rogues, whose envy dull
> Would squash the light of Genius, and instead
> Display a dirty, spluttering farthing dip,
> And swear that 'tis the sun."

So look alive, my pigeon, and help in this good cause. . . . I can do nothing till my third edition of *Physical Geology and Geography* is in the press. I am now at the last lecture of it. I will turn it into chapters. It will be nearly twice as long as it was, and so much modified (I hope improved) that it may almost be said to be a new book.—Ever sincerely, A. C. RAMSAY.[1]

At the anniversary meeting of the Geological Society of England on 6th February 1872, the President, Professor Prestwich, in handing the balance of the proceeds of the Wollaston Donation Fund to Professor Ramsay for transmission to Dr. Croll, said—

"The Wollaston Fund has been awarded to Mr. James

[1] Sir Archibald Geikie's *Life of Sir A. Ramsay.*

Croll, of Edinburgh, for his many valuable researches on the Glacial phenomena of Scotland, and to aid in the prosecution of the same. Mr. Croll is also well known to all of us by his investigation of oceanic currents and their bearings on geological questions, and of many questions of great theoretical interest connected with some of the large problems in geology. Will you, Professor Ramsay, in handing to Mr. Croll this token of the interest with which we follow his researches, inform him of the additional value his labours have in our estimation, from the difficulties under which they have been pursued, and the limited time and opportunities he has had at his command."

Professor Ramsay thanked the President and Council in the name of Mr. Croll for the honour bestowed on him. He remarked that Mr. Croll's merits as an original thinker were of a very high kind, that he was all the more deserving of this honour from the circumstance that he had risen to have a well-recognised place among men of science without any of the advantages of early scientific training, and that the position which he now occupied had been won by his own unassisted exertions.

Professor Ramsay communicated the gratifying intelligence to Croll in the following simple, kindly letter:—

LONDON, *17th February* 1872.

MY DEAR CROLL,—I have the pleasure of sending you a cheque for the proceeds of the Wollaston Fund, which I received for you yesterday at the annual meeting from the President, and for which I returned thanks in your name. I wish it was more, but it is all they had. It is customary for the person to whom this honour has been awarded to acknowledge the honour in a letter addressed to the President and Council. I write in haste. —Believe me, yours very sincerely,

ANDREW RAMSAY.

Croll's reply has not been preserved, but it would no doubt be characteristically modest and grateful.

He was busy with his investigations on ocean currents, and wrote to Professor Foster for some information, from whom he received the following reply:—

16 KING HENRY'S ROAD, LONDON, N.W.,
13th April 1872.

MY DEAR CROLL,—I am exceedingly sorry to have been so long in answering your letter of the 26th March and returning your MS. It arrived just at the beginning of our Easter holidays, when I went away from home for more than a week. I intended to take your letter and paper with me, and to write as soon as I got away, but unfortunately overlooked them at the moment of starting, and left them behind.

If I rightly understand your argument, it is that, were water frictionless, it would, when transferred from the equator, supposing it to have been at rest there relatively to the land, to a place of given latitude, possess an eastward velocity equal to the difference of velocity due to the earth's rotation at that latitude and at the equator respectively, and that the difference between actual eastward velocity of the water at any latitude, and the eastward velocity due to this cause, may be taken as affording a measure of the resistance encountered by the water in passing from the equator to that latitude.

This seems to me unquestionable, but in arguing from it to the force needful to produce the observed velocity towards the poles, is it quite right to regard the resistances to motion as equal in all directions? Of course this is so, so far as fluid friction is concerned, but would not the trade winds oppose motion eastwards more than motion polewards? On the other hand, centrifugal force would oppose poleward motion, but not eastward motion. How far this is important I don't know, but the centrifugal force per pound of matter due to the earth's rotation is at the equator = 0·1113 pound-foot-second units. A pound of water transferred to latitude 60°, and keeping the same eastward velocity as at the equator, would have twice this centrifugal force,

and the component of the centrifugal force at 60° which acts tangentially to the meridian towards the equator is half the whole amount, or, in the case supposed, it would be $\frac{\sqrt{3}}{2} \times 2 \times 0{\cdot}1113 = 0{\cdot}1928$ unit per pound of matter or $\frac{1}{187}$th of its weight, nearly. Of course the effect of centrifugal force in opposing motion polewards is very small near the equator, but increases more and more rapidly as the latitude increases.

It might be said that the water which arrives at say latitude 60° does not come solely from the equator, but partially from latitudes between 0° and 60°. This would of course diminish the eastward velocity due to change of latitude, but if it came from anywhere *near* the equator, it is plain that the velocity would be nearly the same as if it came from the equator itself.

I have ventured to suggest in folio 2 two very slight verbal changes, and have marked a few words in folio 3 which seem not quite clear. Near the top of folio 2 you allude to the relative propelling and deflecting energies, but I do not see any previous allusion to the *distances* traversed parallel to meridian and equator.

I hope my long delay will not have caused you great inconvenience. I regret it very much.—Yours very sincerely, G. C. FOSTER.

Several of Croll's discoveries were ascribed occasionally to other people, and Mr. Carrick Moore, having unwittingly ascribed a computation made by Croll to Sir Charles Lyell, wrote the following honourable letter:—

113 EATON SQUARE, *6th May* 1872.

DEAR SIR,—I am quite sensible that I have done you injustice in speaking of the computation as having been made by Sir C. Lyell, instead of by you. If there is any opportunity given me, I will rectify it. On the main subject of my letter to *Nature* Sir Henry James writes to me that I am unquestionably right, and, strange as it sounds, the accurate Humboldt and the accurate

Herschel had each made an important oversight.—I am, yours truly, JOHN CARRICK MOORE.

The following letter sufficiently explains itself:—

1 SAVILE ROW, BURLINGTON GARDENS, LONDON, W.,
28th May 1872.

James Croll, Esq., Geological Survey.

DEAR SIR,—As the time for another meeting of the British Association approaches, we are mustering promises of papers for the Geographical section.

Mr. Galton, who will preside over the section, hopes to have some papers on the subject of Oceanic Circulation and Currents.

Dr. Carpenter will, doubtless, take a leading part in the discussion of the subjects; but, as it is not desirable that he should carry everything his own way, I trust you will be able to attend the meeting, to give your views, which have now become standard. I need not say that I shall be delighted to learn from you that you will contribute a paper on any subject which may properly come under the Geographical section.—Believe me, very truly yours, KEITH JOHNSTON.

Croll having written that his health would not permit him to attend the meeting, Mr. Galton wrote urging him to attend.

RUTLAND GATE, S.W.,
12th June 1872.

James Croll, Esq.

DEAR SIR,—Mr. Keith Johnston, whom I asked to write to you about a paper on Currents, in Brighton, has shown me your reply. I write still with some hope, that you might be induced to come for this reason. It is probable that Dr. Carpenter will allude at some length to Ocean Currents in his address. He tells me he is not quite sure yet whether or no that will be one of the subjects in it, but I can hardly doubt that in the end it

will. He assures me that he will willingly engage in any discussion on the subject in Section E. I have also asked Sir William Thomson, who will also be prepared to speak. It seems to me, and, I think, speaking on general grounds, you will concur, that if the discussion is to take place, it ought to be done as well as possible, and I should therefore much regret if so noted and able an expositor of the theory of Ocean Currents as yourself were absent. If, on second thoughts, you were inclined to come, I would suggest that a recapitulation in a few words of your chief arguments in a short paper would bring in the discussion in a very effective manner.—I am, faithfully yours, FRANCIS GALTON.

THE DELL, GRAYS, ESSEX,
4th August 1872.

James Croll, Esq.

DEAR SIR,—Your note sent to Dorking has been re-directed here from London. The paper you are so kind as to send me may not arrive, so perhaps you will send me another. Mr. Herbert Spencer will be found at the Athenæum Club, Pall Mall, and Mr. George Mivart at 7 North Bank, Regent's Park, London, N.W.

I shall read your paper with *very* great interest.—Believe me, yours very faithfully,

ALFRED R. WALLACE.

4 N. MANSIONHOUSE ROAD,
21st November 1872.

James Croll, Esq.

DEAR SIR, — Let me thank you for "What determines Molecular Motion." I have read your papers with great pleasure. The present is fully equal to your former memoirs. The outstanding thoroughness and scientific honesty of your discussions, and the great ability you have shown in illustrating them, won my admiration long ago. Believe me, very sincerely yours,

J. JUKES.

CHAPTER XVI

PERSONAL HISTORY—GENERAL CORRESPONDENCE

CROLL had evidently very much overworked himself during the year 1872, and that to such an extent that he was quite incapacitated for anything beyond his office duties during the whole of 1873. During the latter year the pain in his head was so constant and so aggravated that mental work was quite impossible. He struggled manfully through his daily duties, but was glad when the hour arrived to liberate him even from these. He again had recourse to Dr. Warburton Begbie, who received him with his usual kindliness, and tried a different course of treatment, with some beneficial effect. That eminent physician was, however, himself in comparatively poor health. He told Croll when he consulted him that he was then suffering from a somewhat similar ailment, and ere many months elapsed he had passed away. He died on 25th Feb. 1876, and Croll felt, as many did, that he had lost a friend as well as a physician. The pain still continuing, and rendering, on many occasions, sleep almost impossible, he consulted Professor Sanders in the end of the year. The Professor directed his attention chiefly to the securing of sleep for Croll; and by medicinal treatment, dietary, daily walking in the open air, and total abstinence from mental work, sleep was restored towards the end of the year. From the want of sleep and stomachic derangement he was, however, very weak, and unable to do any private work all this year. Regarding all his reading, and during this period particularly, from the short hours he could read, he had to adopt the plan of marking on the margin of the page and underlining with pencil the portions to which

he might have occasion to refer. Had he not adopted this mode, his reading would not have been of much advantage. Notwithstanding these precautions, however, the head grew gradually worse, so much so that he had frequently to apply for sick leave for a month or six weeks at a time; and in 1873 he was disabled for duty for nearly nine months. For two or three years prior to the publication of *Climate and Time*, it was with the greatest difficulty that he could manage to put together in one day as many sentences as would fill half a page of foolscap. In fact, the appearance of the volume was delayed for two or three years on that account. During all this time the mind was as vigorous as ever. It was pain in the head, and pain alone, which stopped all progress when he attempted mental work. He frequently thought he would be obliged to resign his situation in the Survey; but as he had not completed his tenth year of service, and would therefore not have been entitled to any superannuation, he was strongly urged by his friends to try and struggle on.

Croll, having sent his papers on "Ocean Currents" to Mr. Clements Markham, received the following acknowledgment:—

21 ECCLESTON SQUARE, S.W.,
1st July 1873.

James Croll, Esq.

MY DEAR SIR,—I am very much obliged to you for sending me your papers on "Ocean Currents," which I am reading with great interest. I should be extremely obliged, also, if you could send me a short memorandum on the importance of investigating the currents and temperatures of the sea at different depths, in the seas within the hitherto unvisited area round the North Pole. We are preparing a statement of the various results in different branches of science, to be obtained by exploring the unknown area. An investigation of the currents will be one of those results, and your views on the

importance of such researches, and on the question of practical consequence which would be solved by them, would be very valuable to us. Yours very truly,

CLEMENTS MARKHAM.

He was asked to read a paper at the Victorian Institute, on "What determines Molecular Motion," in the following letter:—

VICTORIAN INSTITUTE, *28th June* 1873.

James Croll, Esq.

DEAR SIR,—At a recent meeting, the Council considered the questions to be taken up next session, and a strong wish was expressed to have some purely scientific papers; and amongst those they were desirous of asking to kindly give papers, your own name was specially noted. The question taken up in your much-admired paper, "What determines Molecular Motion," it is felt, would, should you kindly consent, be a most valuable one to be discussed in a paper.

I may add that, should you not be able to be present, the paper would be read by one of the Council or any one named by yourself; you would have a report of the discussion thereon, that you might make a reply if necessary. Authors of papers get twenty-five copies, and as many more as they wish, at the cost of printing, generally twopence a copy. I send a list of some of our members and our annual address. Several F.R.S.'s have recently joined, and sixty-six have been added to our number during this first half of 1873.—I remain, yours respectfully. J. PETRIE.

October or December will be time enough for the paper.

The state of Croll's health compelled him to decline this invitation.

In 1873 he was invited to assist in the Geological Department of the *Encyclopædia Britannica* in the following letter:—

Encyclopædia Britannica, 6 NORTH BRIDGE, EDINBURGH,
27th August 1873.

DEAR SIR, — May I take the liberty of asking whether you have any leisure to assist in the Geological Department of the above work. If you have time, and are willing to help, may I ask you to favour me with a call here any day between eleven and three o'clock.— Yours truly, THOS. S. BAYNES.

Croll's health here again, and the demand on his time otherwise, precluded him from contributing articles as desired, but in the article "Geology" his theory of Climate and Time is embodied and acknowledged by Sir Archibald Geikie.

The period of rest from extra mental exertion beyond his ordinary office duties during the year 1873, restored Croll's health considerably, and enabled him to enter with renewed vigour on his independent scientific work early in 1874. He returned to the subject of Ocean Currents, and wrote a paper entitled "A Further Examination of the Gravitation Theory of Ocean Currents," which appeared in the *Philosophical Magazine* of February 1874. This was followed by a paper dealing with the "Wind Theory of Oceanic Circulation," which appeared in the *Philosophical Magazine* of March 1874. He also wrote a reply to Dr. Carpenter on Ocean Currents, which appeared in *Nature*, May 21, 1874. Two further papers on "The Physical Cause of Ocean Currents" appeared in the *Philosophical Magazine* of June 1874, and in the *American Journal of Science and Arts* of September 1874. These were followed by two papers on "The Physical Cause of the Submergence and Emergence of the Land during the Glacial Epoch," which appeared in the *Geological Magazine* of July and August 1874. He also wrote a paper on "The South of England Ice-Sheet," which appeared in the *Geological Magazine* of June 1874.

Dr. Croll's papers on "Ocean Currents" had attracted

the attention of Professor Haughton of Dublin, who wrote him the following letter. Unfortunately, Dr. Croll's reply has not been preserved, but the papers asked for were sent almost by return of post.

TRINITY COLLEGE, DUBLIN,
11th March 1874.

DEAR SIR,—I have read from time to time your papers on "Ocean Currents" in the *Philosophical Magazine*, and entirely concur with you in rejecting Dr. Carpenter's absurd theory of a general oceanic circulation caused by the relative heat of the equator and poles. His theory will not receive the support of any person acquainted with the elements of physics and mechanics. In the *Philosophical Magazine* (March, p. 172) you remark *obiter* that "the greater part of the water evaporated in inter-tropical regions does actually fall as rain in those regions."

I doubt greatly if this be true on the *ocean surface*. Our rainfalls are taken on land, often in the neighbourhood of lofty mountains, and so we come to exaggerate the tropical rainfall.

At St. Helena mid-ocean observations made at my request, 1861–1864, showed

Annual evaporation from ocean surface	82·98 ins.
„ rainfall „ „	18·85 „
Difference . .	64·13 „

I forward by post a copy of my book on *Animal Mechanics*, of which I beg your acceptance. If you could send me a copy of your papers on Eccentricity and Ocean Currents, you would oblige,—Yours very faithfully, SAMUEL HAUGHTON.

Dr. Croll, knowing the interest that Captain Nares, the Arctic explorer, took in ocean currents, wrote him the following letter:—

EDINBURGH, *23rd April* 1874.

DEAR SIR,—I have much pleasure in forwarding you the volume, which, I trust, may both interest and please you for an hour now and again, during your long winter nights. In chapter xvi. I have given some details of a class of facts connected with the Arctic regions which have, unfortunately, been too much overlooked, viz., evidence of a warm condition of climate in Greenland during very recent post-Tertiary times. According to theory, we might expect that during the warm periods of the Glacial epoch, Greenland was comparatively free of snow and ice; and the probability is that the ancient forests, found by M'Clure, Warham, and others, flourished during that period. Might I take the liberty of suggesting to you, when in those regions, to be on the outlook for any evidence bearing on that point, such, for example, as remains of trees, plants, or shells which could not now live in such a climate. In chapter xviii. I have given all the facts which I could find relating to warm conditions of climate in Greenland during earlier geological epochs. Yours truly,

JAMES CROLL.

Dr. Croll was not a mathematician. He made his marvellous and intricate calculations by a process of mental logic of his own. He always wished, however, to have these verified by mathematical calculation. On one occasion he had made a calculation as to the light or heat received in a year from the sun, and asked Professor Tait to test it. The Professor wrote in reply, giving the geometrical formulæ, and added, "which is what you make it. At your leisure and mine I should like to know how you got this result."

The year 1875 was a period of exceeding activity with Croll. In that year appeared his great work, entitled *Climate and Time in their Geological Relations: a Theory of Secular Changes of the Earth's Climate*, published by Daldy, Isbister & Co., London. This work,

which is fully noticed elsewhere, created a profound impression in scientific circles both at home and abroad. No scientific work of equal importance or interest had appeared anywhere for many years. Whether his views were accepted or rejected, friends and foes alike bore testimony to the striking originality, the wide philosophic grasp, and the marvellous faculty of inductive generalisation displayed in the book. His first paper on "The Physical Cause of the Change of Climate during Geological Epochs," which appeared in August 1864, followed by two other papers on the subject, during the next ten years, effected an entire revolution in the method of interpreting glacial phenomena. As already explained, Croll's ill health greatly retarded the publication of *Climate and Time*. His theories regarding glacial questions were, however, well known to scientific men from his published papers, as also from *The Great Ice Age* of Professor James Geikie (also of the Geological Survey of Scotland), who adopted and expounded them with his characteristic clearness and accuracy. The views so ably expounded by these authors, regarding the former extension of land ice in the northern hemisphere during the Glacial period, have been generally accepted, and now form part of the common stock of geological knowledge all over the world.

But the theory of the physical causes of secular changes of climate developed by Dr. Croll, and the researches on which that theory was based, gave rise to prolonged controversy. Though one of the most modest of men, Croll was a keen controversialist. Numerous replies to his antagonists appeared in the pages of *Nature* and the *Philosophical Magazine*, as well as elsewhere. The first, entitled "Climate and Time," appeared as a letter in *Nature* on August 26, 1875. This was followed by a paper entitled "The *Challenger's* Crucial Test of the Wind and Gravitation Theories of Oceanic Circulation," which appeared in the *Philosophical Magazine*, September 1875, in the *American Journal*

of Science and Arts, September 1875, p. 222, and in the *British Association Reports*, 1875.

Another paper on "The Wind Theory of Oceanic Circulation," in which objections to the theory were examined, appeared in the *Philosophical Magazine* of October 1875, and in the *American Journal of Science and Arts*, January 1876, p. 58.

Another paper on "Oceanic Circulation," being a reply to Dr. Carpenter, appeared in *Nature*, October 7, 1875, and was followed by a further paper, entitled "Oceanic Circulation, Expansion of Sea Water as determined by Professor Thorpe," which appeared in *Nature*, November 25, 1875.

These papers were followed by further remarks on the Crucial Test argument, which appeared in the *Philosophical Magazine* of December 1875.

He sent copies of his work on *Climate and Time* to Dr. Petermann and Mr. Crookes,—Croll's letter to the former being as follows :—

EDINBURGH, 21*st April* 1875.

Dr. Petermann.

DEAR SIR,—I have, with much pleasure, sent you by to-day's book-post a copy of my new book on *Climate and Time*, which I hope you will accept.

At pp. 220–225 I have detailed some curious results, which you might glance at. I need hardly say that a notice in your valuable journal will be esteemed a special favour. I may mention that an American edition appears simultaneously with the English. I am much obliged to your kindness for the valuable papers you send me now and again, which interest me and my friends here very much.—Yours sincerely, JAMES CROLL.

Quart. Journ. of Science, LONDON,
23*rd April* 1875.

James Croll, Esq.,

MY DEAR SIR,—Thank you very much for your book on *Climate and Time*. Although I have not yet

had time to read it through, I have read sufficient to convince me of its intrinsic value, and I shall make it the basis of an article for the next number of the *Quarterly*.

I should be very glad if you could write a paper on "The North Pole," for the next number. Now that your book is complete, you will, I hope, have sufficient time at your disposal, and a paper from your pen on a subject of such present interest is sure to be received with pleasure by the readers of my journal.—I am, dear sir, very sincerely yours,

WILLIAM CROOKES.

Croll had rather a fastidious taste in the matter of binding and general appearance of books, as is shown by the following letter to his publishers :—

EDINBURGH, *24th April* 1875.

Daldy, Isbister & Co.

GENTLEMEN,—I am glad to find that the somewhat defective appearance of the volume has been accidental. I have little doubt that the alteration which you propose making will improve its appearance very much.

I am also glad to learn that you intend to use every means possible to bring the book prominently before the public, and I trust it may have a steady if not a rapid sale.

I have already distributed all my copies. A number of them I gave to influential men connected with the press, who would likely on my account give it a notice. If, however, you could favour me with, say three copies with the improved cover for my own library, I shall feel obliged.—Yours truly,

JAMES CROLL.

Croll had sent a copy of *Climate and Time* to his fast friend Sir Andrew Ramsay, from whom he received the following letters :—

GEOLOGICAL SURVEY OF ENGLAND AND WALES,
14th May 1875.

MY DEAR CROLL, — I have written to one of the *Times* reviewers, asking him to come and see me, which he will do, but what may come of it, of course I do not know. I also sent an extract from one of your notes to Mr. Tylor, who may, perhaps, see that he has come a day behind the fair.

In p. 267 of your book, I see you state that I give the total thickness of the British stratified rocks at about 14 miles, and in p. 361 at 72,000 feet. I presume that this thickness is quoted from Darwin's first edition of the *Origin of Species*, to whom I gave the thicknesses of all the English formations, and this statement has been often quoted. Many of the quoters (and I think Darwin himself) are under the idea that that is the thickness of these formations at some place or places. But I never said that, for there are many unconformities, and much of the country was now and then dry land when later formations were being deposited.

At p. 267, in treating of land surfaces, you say that hardly one is to be detected in all these formations. There were, however, plenty of land surfaces in Britain and elsewhere, at various times. For example, when the Old Red Sandstone, the coal measures, as you state, the Permian and the New Red series, were being deposited; also during the Oolitic, Purbeck, Eocene, and Miocene times. I have seen what I considered to be *roches moutonnees* under the Old Red; but, as a general rule, the old land surfaces are so old that all traces of the effect of the ancient glacial action has been removed. It exists, however, in Africa under the Permian boulder beds, and the underlying rocks are *moutonneed* and grooved, and, I believe, the same is the case in Central India. I also think it is not quite right to say that the entire stratified rocks of the globe, with the exception of the coal beds and underclays, consist almost entirely of a series of *old sea-bottoms*. I feel sure that there are far

more formations than is generally supposed that were deposited in the interior of Continental areas, *e.g.* great part of the Cambrian, all the true Old Red, Permian, and New Red (with a Muschelkalk episode), and almost all of the Miocene, and, I believe, much of the Eocene formations.

I see that they are finding Miocene boulder beds in Bavaria and elsewhere, with glacial scratches on the stones.

I hear that my Rhine paper, which was translated into German, has been read over all Germany, at least so Dr. Hoffman told me. I have never had time to write out my pre-Miocene Alps, and the amount of denudation they have subsequently undergone, but I lectured on it at the Royal Institution.—Yours very sincerely, ANDREW RAMSAY.

GEOLOGICAL SURVEY OF ENGLAND AND WALES,
15th May 1875.

MY DEAR CROLL,—I am surprised that none of your critics, that I have read, mention that in the Antarctic regions there is a very respectable Glacial epoch going on now, and that the reason thereof is explained by your theory in all its details. In one of my last lectures of the course just finished, I specially dwelt on this point, and, indeed, I have done so for some years. The argument in your favour, derived from that point alone, is very strong.—Yours truly, ANDREW RAMSAY.

Having received a copy of a Memoir from the Rev. O. Fisher, M.A., Croll acknowledged it in the following terms :—

EDINBURGH, *17th June* 1875.

MY DEAR SIR,—Many, many thanks for the copy of your valuable and interesting memoir which you have kindly sent. You go too deep for me. It must have cost you much thought and labour. I have got my book

off hands; and, as I am getting tired of geological subjects, I think I shall turn to my old favourite study of philosophy after I get a long rest.

I am still almost wholly disabled for work by my old complaint in the head.

I trust you are keeping well.—With kind regards, I am, yours very truly,

JAMES CROLL.

The following correspondence relative to his controversy with Dr. Carpenter is interesting:—

EDINBURGH, *15th September* 1875.

Professor Foster, F.R.S.

MY DEAR SIR,—I send you a copy of my paper read before the British Association. I should like exceedingly well to have your opinion as to the correctness of my result. Of course I shall make no public use of what you may say, it is simply for my own satisfaction. The point I wish more particularly to have your opinion on is this:—Assuming that my computation from Muncke's table is correct, and that column A is longer than column C for equal weights, must not the North Atlantic be above the level of the equator? And if it is so, how can there possibly be a surface flow like what is assumed in the gravitation theory?

I intend applying for admission into the Royal Society. If you can see your way clear to give me a little aid, it will be esteemed a very special favour. Apologising for troubling you so often, I am, yours most truly,

JAMES CROLL.

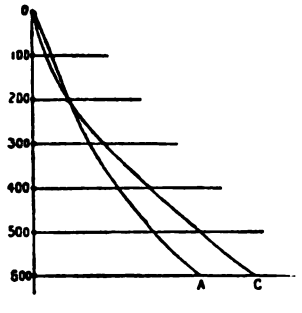
12 HILLDROP ROAD, LONDON, N., *1st October* 1875.

James Croll, Esq. Edinburgh.

MY DEAR CROLL,—I have been away from home, and so did not get your letter and enclosed paper (for which

many thanks) till Wednesday. I have since read the paper and Dr. Carpenter's reply (*Nature*, September 25).

As far as I can understand the matter, the question raised in your paper is rather complex, and the data do not seem sufficient for a decisive answer. I do not see any proof of your assumption, bottom of p. 4 and top of p. 5, that the pressure at bottom of column C is the same as the pressure at the same level in column A, which is what I understood you to take as the foundation of your calculations of the height of the columns. Of course, if the water were at rest, the pressure must be the same at all equal levels, but then, from some cause or other it is in motion: why, therefore, assume equal pressure at this particular level rather than at any other? I suppose there is no question about a general poleward flow near the surface and a general equatorward flow lower down. At some intermediate depth therefore there must be no flow either way. If the depths of the stationary strata in the two columns respectively can be determined, I should say we ought to assume equality of pressure at these depths. It would, I should think, be instructive to form a diagram of pressures at the various depths for different latitudes, as those of columns A and C. I mean diagrams where ordinates should show depth and abscissas pressures. If the curves for different latitudes are superposed, as in the sketch, they would show at a glance whether there is a force urging water from one column into the other or not. Thus, assuming equality of surface level for illustration, both curves have the same starting-point 0, denoting atmospheric pressure. To begin with, the pressure increases fastest in A (being colder for the first 100 fathoms) but afterwards faster in C (colder after first 100 fathoms about). The pressure



would of course be *from* the column whose curve lies *outside* to the other. The points of equal pressure (intersection of pressure curves) cannot, that I see, be determined from the data. Your assumption (see above) is equivalent to making the curves intersect at level of bottom of column C. The temperature soundings, with determinations of expansions, determine the shape of the curves, but not their absolute position.

Though I thus think your conclusions not fully established, I by no means see much force in Carpenter's objections. His "trough experiment" that he thinks so much of is simply silly. Let him try how much circulation he can get with a difference of temperature at the rate of $\frac{1}{100}$ degree F. per mile of his trough, which I suppose would not be *very far* from the average variation in the ocean. Nobody doubts that water can be made to circulate by heating one end and cooling the other: the only question is whether the heating and cooling which occur in the Atlantic are sufficient to cause circulation to the extent that actually exists. For my own part, I don't believe it. No doubt this is a co-operating cause, but I have also very little doubt that the wind is a much more active cause. It also seems to me very silly to appeal to Froude's experiments as bearing perceptibly on the question. Relatively to the forces brought into play in them, the viscosity of water may well be negligible, and at the same time it may be considerable enough in comparison with the forces producible by differences of temperature.

I shall be greatly pleased to support your candidature at the Royal Society in any way in my power.—Yours always very truly,

G. C. FOSTER.

HARLTON RECTORY, CAMBRIDGE,
24th September 1875.

MY DEAR SIR,—The question in dispute between yourself and Dr. Carpenter appears to me far from

simple. When I heard Dr. Carpenter lecture at Cambridge,—it must be at least two years ago if not longer,—I thought that he held that the existence of a cold stratum at the bottom of the Atlantic and its approach towards the surface near the equator proved his theory to the exclusion of yours. I saw that it was a consequence of yours as well as of his, so I came to the conclusion that he had not proved his theory as against yours, and I think so still. But whether his gravitation theory apart from a wind theory would account for a circulation of water is another question. For my own part, I do not believe it would account for *the existing* phenomena, though I am not prepared to go the full length with you, and deny that it would cause any circulation. This I think at any rate is clear, that if there was not a cause in operation besides temperature, the strata of water ought to be bounded by plane surfaces between the regions of descent and ascent. Your diagram in the paper you have sent me shows this is not so. There must therefore be some disturbing cause at work, which cannot well be other than wind.

I do not think, as far as I can see,—and this occurred to me before reading Carpenter's letter in *Nature*,—that you are justified in assuming that the surface of the ocean would be absolutely level between the sections A, B, and C, were the temperatures the same at the like depths, so that you cannot conclude for certain that the levels are in fact such as the difference of temperature would make them, as if they would be truly horizontal, barring those differences. Hence, as a matter of fact, B may still be lower than C and A lower than B.

On the whole, I suspect you are right in considering the wind the chief cause of the currents of the ocean, but that Carpenter's theory may still be so far true, that if there was no wind a slow circulation would still go on.

I have given you my notions, but they are merely

crude ones. The question depends too much upon niceties for me to be at all positive. The argument from the viscosity of water is one not easily settled.—Believe me, sincerely yours,

OSMOND FISHER.

Did you happen to notice Mr. W. Green's letter in *Nature* this week? According to him, the stones at the bottom of a building ought to be hotter than those at the top.

September 27.—I open my letter written on Saturday night to answer yours received this morning. I know nothing of Mr. Henderson's whereabouts, but I should think that a parcel addressed to him either at S. John's Coll., Cambridge, or at the County School, Guildford, would be sure to find him. But I will inquire at the College the next time I go into the town, which will probably be on Wednesday, and see whether the porter knows his address. But I feel pretty sure I shall only get the address I sent you before.

I am very sorry that I cannot feel as sure as you do about the level of the Atlantic. The fact is, I have very little knowledge of the laws of fluid *motion*. If the ocean was in equilibrium, its surface must be horizontal and the strata of equal density and temperature horizontal also. It is evident the latter is not the case, and therefore I suppose the other is not so either. But I do not know if this influences *motion*. I can conceive a tidal wave advancing up a river and the water flowing right over its crest all the while, in which case it may be said to flow up hill on one side of the wave and down hill on the other. In fact, this actually occurs in every ripple caused in the surface of a stream by an unevenness of the bottom.

The following letter shows to some extent how Croll stood pecuniarily about this time:—

EDINBURGH, *8th November* 1875.

SIR,—In reply to Circular No. 910 from the Science and Art Department, October 13 ult., I have to inform you that with the exception of my regular salary, I have not received a single farthing from any source whatever, either public or private, since I entered Her Majesty's Service.

Croll's labours had now for a considerable time been recognised by all the leading men of science, and among those anxious to recognise these was the late Professor Heddle of St. Andrews. He had brought Croll's name forward in 1873 for the Degree of LL.D., but had to delay his request in deference to his colleagues considering Croll too young for the honour. Croll was then fifty-two years, an age not usually considered juvenile. The following letters speak for themselves:—

ST. LEONARDS, ST. ANDREWS,
23rd December 1875.

MY DEAR SIR,—I have to acknowledge receipt of many of your deeply interesting and powerfully reasoned papers.

I have for some past years considered your work so deserving of public recognition that two years ago I recommended you to our Faculty of Arts for promotion to the degree of LL.D. in our University. Your claims were even then admitted, but I was requested to defer my motion on the grounds of your juvenility. I again this year brought you forward, and our faculty has unanimously and cordially agreed to recommend you to the Senate for that degree, so that the matter may be considered as settled, unless you yourself decline. I have to mention that there is, however, a diploma fee of £10, 10s., which I fear we cannot remit. For myself, I have had more than usual freedom in bringing you forward, seeing that I, unfortunately for myself, have not

the pleasure of your personal acquaintance, and that I do not quite agree with you in all your views,—which perhaps enables me all the more to admire the originality of your mind, the courage of your argument, and your independence of scientific red-tapeism.—Truly yours,

W. FORSTER HEDDLE.

Croll's reply to Professor Heddle.

24th December 1875.

The announcement that your University proposed to confer on me an honour the highest to which any scientific man can aspire, certainly takes me by surprise. I hope you will not deem it affectation when I say that I do not consider that I have done anything deserving of such an honour, and that I must look upon it more as a reward to a self-taught man for a long and persevering struggle against difficulties, than for any positive results which he has as yet been able to achieve.

Allow me to offer to you personally my warmest thanks for the kindly interest you have taken in this matter.

CHAPTER XVII

CORRESPONDENCE WITH MR. JANSEN

WHEN Croll had finished *Climate and Time*, he resolved to abandon not only his climatological studies, but physics in general, in order to be able to resume those investigations into the philosophy of evolution, which he had laid aside at the time that he entered Anderson's College. He found, however, that this was a resolution to which he could not adhere. Notwithstanding the care he had taken to express his views in the clearest manner he could, these views on many points were very much misapprehended. He therefore found it necessary to endeavour not only to remove those misapprehensions, but to enter at much greater length into some of those difficult points which had perhaps been too briefly discussed in the volume. The consequence was, that, owing to the state of his head obliging him to write so slowly, and other circumstances, it was not till 1885, or ten years after the publication of the volume, that he managed to shake himself clear of this great question which thus engaged his attention for upwards of twenty years.

In 1876, Croll wrote a paper entitled "Remarks on Mr. Burns's Paper on the Mechanics of Glaciers," which appeared in the *Geological Magazine* of August 1876. This was followed by a paper on "The Transformation of Gravity," which appeared in the *Philosophical Magazine* of October 1876. A short abstract also appeared in the *British Association Report* of 1876. He also wrote a paper on the "Tidal Retardation Argument for the Age of the Earth," which appeared in the *British Association Report* of 1876, in *Nature* of September 28, 1876,

and in the *American Journal of Science and Arts*, December 1876, p. 457.

Croll having communicated his paper on "Oceanic Circulation" to Mr. Jansen of the Hague, the following interesting correspondence took place between them:—

THE HAGUE, *17th January* 1876.

James Croll, Esq., of H.M. Geological
Survey, Edinburgh.

DEAR SIR,—I am exceedingly obliged for your kindness in sending me copies of your papers, "The *Challenger's* Crucial Test of the Wind and Gravitation Theories of Oceanic Circulation," for which please accept my sincere thanks. I have read them with great interest, but I did not find a test of both theories. The wind theory was not tested at all, otherwise I think you would have found that there is as much difficulty to prove that the winds are the cause of oceanic circulation as any other force.

If the computations of different expansions in three columns of sea water, observed by serial temperature soundings in the *Challenger*, give right to suppose that the North Atlantic, to be in equilibrium, must stand at a higher level than at the equator, it appears to me that such a higher level would make it more difficult for the winds to drive the Gulf Stream and its much larger so-called offshoot, not only against the Mexican, northern, and partly against the N.E. trade winds through narrow straits and channels, but also up hill against an incline, and all this display of force derived only from the propelling power of the excess of the S.E. over the N.E. trades. If the propelling power is so great, how much greater must then the propelling powers be of the westerly winds, and how tremendous the currents created by them, and where are they? I do not know. But I can point to the Mozambique current, which is all the year round in the same locality, and flowing in the same general direction, notwithstanding the N.E. trades are pushed back by the S.E. trades, and these converted into

the S.W. monsoon in our summer months. According to the wind theory, this warm river ought to shift its position with the monsoon in the Indian Ocean, which it does not.

The Japan current is, according to the same theory, caused by the propelling powers of the excess of S.E. over N.E. trades. But here the S.E. trades are during one half of the year driven back by the N.E. trades, which are converted into a N.W. monsoon in southern latitudes. How is it possible that the Black Japan current, flowing constantly in a N.E. and E. direction against the N.E. trade and N.W. monsoon, can have been created by the S.E. trade in that season of the year?

I do not think that the long narrow trough of the Atlantic is a fit place to investigate the causes of oceanic circulation, and certainly not for drawing general conclusions from a few observations made in it.

The researches into the phenomena of the sea are of very recent date, and our empirical collection of facts is as yet too small to investigate their antecedent causes with any hope of success.

Theories, at present, can have no other aim than to direct and stimulate observations in every ocean, all over the world, from which we may hope to find at last the different causes by which this wonderful machine, the ocean, is enabled to perform its manifold duties.

Then, no doubt, it will be found that the winds are among those causes, but perhaps more as distributors and mixers of surface water, as carriers of vapour, dispensers of rain, and propellers of ice, than as creators and propellers of currents, which they are undoubtedly in some measure, but that measure, be it great or small, has never been approximately ascertained by the advocates of the wind theory, and this ought to be done to enable them to draw their conclusions from the systematic discussion of facts.

With renewed thanks for your very valuable and interesting papers, I remain, with the kindest regards, yours faithfully,

JANSEN.

THE HAGUE, *6th February* 1876.

James Croll, Esq., Geological Survey, Edinburgh.

MY DEAR SIR,—For your kind letter of January 21, and for the reprints wherewith you have obliged me, please to accept my sincere thanks. I am in possession of your admirable work *Climate and Time*, which I have read with the greatest interest and pleasure, but in which, I am sorry to say, I did not find any test of the wind theory of oceanic circulation. Your kindness in sending me some reprints of articles embodied in your above-named splendid work, encourages me to give you the reasons why I have not found in your work a proof that the winds are the cause of oceanic circulation. I can do this with great impartiality, being not a partisan of any theory of oceanic circulation, which I think of not much value, being based more upon scientific speculations than upon facts; and not having the required empirical facts, we are unable to test them.

My experience and studies combine to make me believe, suppose, or guess, that the ocean is as much necessary for the winds to help them through the circuits of their circulation, as the winds are necessary to help the ocean in performing its circulation, but I do not think that we have collected a sufficient number of different observations upon which to establish, or by which to test, a theory which shall explain all the phenomena in the circulation of sea and air.

I have not yet seen any theory of oceanic circulation to which I adhere, and I hardly expect that I shall live long enough to see a satisfactory solution of this "question *tres épineuse*," as it was called by the great thinker Laplace. The only opinion with which I agree is that of Maury, viz., that a current like the Gulf Stream could not be impelled and maintained by a force due to the impulse of the trade winds. You think (p. 211) "this is a somewhat weak objection. It seems to be based upon a misconception of the magnitude of the force in operation. It does not take any account that this force acts on nearly the whole area of the ocean in intertropical regions."

I have been surprised to read this opinion from an advocate of a wind theory, wherein no account at all is taken of the force of the wind, because you say (p. 212): "If the winds be the impelling cause of currents, the direction of the currents will depend upon two circumstances, viz. (1) the direction of the prevailing winds, including of course under this term the prevailing winds proper and the trade winds, and (2) the conformation of land and sea." According to this view, it does not matter what the force of the different prevailing winds may be, we have only to look towards their direction, and to the conformation of land and sea, to determine the directions of the currents, and doing this, I think it would lead to conclusions against, and not in favour of, the wind theory. It seems to me that the force of the wind has not been taken into account in your *exposé* of the wind theory as much as was done by Maury, when he rejected it.

Never have the force and direction of the trade winds in the Atlantic been more thoroughly investigated than was done by Maury, and by this investigation he was led to the conclusion that the Gulf Stream could not be due to the trade winds, and that the winds in general were inadequate to be the cause of the circulation of the ocean. After coming to this conclusion, he searched for other causes, which he brought to light to direct and stimulate research, and which you have submitted to a scientific test. By this test you were led to the conclusion that the gravitation theorists are wrong. But if your conclusion is accepted, it does not follow from it as a natural consequence that the wind theory is right, as your argumentation seems to imply. It ought to be proved, and that you have not done.

Although you call it a weak objection, you agree with Maury that the impulse of the trade winds is inadequate to impel and maintain a current like the Gulf Stream, but you start a new wind theory, according to which the oceanic circulation is due to the impulse of all the winds of the globe acting upon the surface of the ocean within and without the tropics all over the world.

This new wind theory is based upon the fact that (page 213) "in every case, without exception, the direction of the main currents of the globe agree exactly with the direction of the prevailing winds. There could not possibly be a more convincing proof than this general agreement of the two systems as indicated by the chart."

After reading your criticism of the theories of other savants in your excellent work *Climate and Time*, and your logical deductions from facts, I do not think that such a proof would have been convincing to you, if this wind theory had not been your own.

If we make a proper distinction between the general drift of the ocean and certain definite currents in the ocean, then I should think that an agreement between the directions of those currents and the directions of the prevailing winds over them, is no proof at all that they have been caused by those winds, as I hope to show further on in discussing the wind theory.

If both the systems of circulation of the atmosphere and of the ocean are due to the same general cause, the differences in temperature at the poles and under the equator, it would of course be natural that there should be some analogy between the direction of aerial and oceanic currents; but if that could be proved, it would certainly be wrong to deduce from that analogy that the winds are the cause of the currents, or that those are the cause of the winds. But even if your conclusion is accepted, that only the winds and not the ocean currents are due to those differences of temperature, then still I cannot see in the circumstance that there are some ocean currents having the same direction as the prevailing winds over them, not even if all the ocean currents had the same direction as all the prevailing winds over them, that this should be a convincing proof that the whole oceanic circulation is caused by the winds. It may be a sufficient inducement to collect facts for a test to get a convincing proof that the whole oceanic circulation is caused by the winds, but as long as those facts are wanted, we may suppose, on more or less plausible

grounds, that the winds are the cause of oceanic circulation, but it will never come up to a semblance of a proof.

To get the materials for such a proof, we ought to look into the great workshop of nature, to observe how the winds are making currents, and to do this, to please you, I will think myself for a moment a wind theorist, and will try to explain your theory with the light of my experience.

We know that the winds act mechanically upon the surface of the ocean by raising a sea of more or less height according to the force of the wind. Those billows foam and are blown down, throwing a great deal of water from the crest into the trough of the wave. If there is no wind to raise a sea, the existing sea becomes a swell, and by the height of this swell and its rate of translation or progression, a certain amount of work derived from the impelling force of the wind is carried "far as the breeze can bear the billow's foam."

That the sea, or the swell, let us say the waves, are indeed the vehicle of the impelling force of the winds upon the ocean may be seen in the Arctic regions. When the edge of an ice-pack is reached, and a swell coming from the ice is met with, it is a sure indication that behind the pack there is open water upon which northerly winds have been playing, raising seas and leaving a swell carrying the stored-up impelling force of the wind towards the ice, which prevents the progress of the undulations, but, by concussion, the stored-up force is propagated underneath the ice, and as soon as it reaches open water, the swell is formed again, and it continues its undulations in the direction it has received from the winds. No force applied by the wind upon the ocean in any part of it is lost; it is stored up in the height and rate of progress of the undulations of the waves, which are the vehicles in which the impelling force of the wind is immediately carried away to any distance in no time from pole to pole, if not impeded by land or any other interruption in its progress.

It is a remarkable fact that, although we see every day before our eyes how the waves, with their height and

rate of undulation derived from the impelling forces of the winds, throw their water, here high up on a sandy beach, there fifty or a hundred feet upon the slope of a rocky coast, and, in other places, breaking holes and caves in softer rocks, we have never availed ourselves of all these opportunities to measure the impelling force of the wind carried by the waves in the direction in which they move.

This direction, the height and rate of progress of the undulation of the waves, the rotation of its molecules, and the general drift in the layer of water which undulates, are altogether the results of all the forces of the winds which have contributed to their formation. Other winds may raise a sea upon a swell, they may promote or retard the progress of its undulations or divert it from its former direction, but the remaining waves will be again the resultant of all the forces of the winds which have contributed to its formation. It may be that by the changeable nature of powerful winds in some localities, for instance, between latitudes 40° and 60° south, different swells from S.W., W., and N.W. will constantly remain and never disappear, of which each in turn will predominate with its foaming crest according to the prevailing gale, but finally there will remain certain waves carrying the stored-up energy from all those gales in the resultant directions to other regions, leaving, in its trackless quick undulations, nothing perceptible behind, but the general drift of the ocean following the undulation of the waves in the same direction, but at an incomparably slower speed. Perhaps this general drift is caused by the breakers blown down into the trough of the waves and by their fall moving the water in the direction of the wind and of the wave. All this I leave to the physicists to explain. I deal only with the facts which I have observed in the great workshop of nature, where I have seen the winds and the waves at work, and then I come to the conclusion that the impelling force of the wind given to the ocean is nowhere to be found except in the height and rate of progress of the undulation of the waves and creating a general drift by the tumbling down of their crests.

We know that in a gale or hurricane, a sea may be raised 40 feet high, 20 feet above, and 20 feet below the level of the ocean, but after the gale there will remain a swell of no greater height than about 10 feet above and below the level; and in regions where there is never, or very seldom, a gale of wind, it will not be of half that size.

Let us for argument's sake suppose that the average height of all the waves on the surface of the ocean is 30 feet above and below the level, which is a most extravagant figure, and undoubtedly more than twice too high, but speaking now as a wind theorist I must be on the safe side. Then the consequence will be that the surface of the ocean is not deeper disturbed by the wind than to a depth of 30 feet, and although I admit that the impelling force of the wind is carried by the waves all over the world, still, my conscience as a would-be wind theorist cannot swallow the idea that this very thin layer of the surface of the ocean sets the whole ocean in motion, and directs and regulates its circulation.

It is true what you say, that, comparing the depth of the ocean, from two to three miles, with its immense surface, it must appear to be a shallow pool, but even in this shallow pool a layer of 30 feet is so infinitesimally small that the undulations of the waves cannot even be compared with the ripples which the wind makes on the surface of our shallow ponds, the water of which, according to the wind theory, which makes no difference between fresh and salt water, must also be put in circulation by the agency of those ripples. This, of course, can be verified very easily, but I fear that this test will not give a satisfactory result for the wind theory. Knowing that a wind theorist is not so easily thrown from his hobby, I will set this objection aside, to go a little deeper in this investigation. Therefore I admit that the waves are the vehicle of the impelling force of all the winds on the globe, which have contributed to their formation, and that they disturb only a very thin layer of the surface of the ocean, *but all over its immense surface.* I also admit that the winds on the high sea give their impelling force to the waves, which

by their undulation run immediately away with that force, stored in their height and rate of translation in a resultant direction, and causing only a very slight movement in the sea water, called general drift. I admit that this is all what the winds do. They make no currents on the high sea far from the land, and the objection that such a very thin layer, to which the translation of the waves is confined, cannot cause the circulation of the whole ocean is apparently well founded. But what becomes of the waves and the general drift, when they encounter land in their way? To give an answer to this question, we ought to look again into the workshop of nature. When the general drift finds a resisting body in its way, it presses against it, and if it will not move, the drift takes the direction of the coast line, and by the pressure against it, a current is formed along the coast of small dimensions, having a little more speed than the slow general drift, the water of which current acquires some definite properties which give it a greater cohesion, by which it is kept together and prevented from being dispersed. Consequently, it will continue its course along the coast, and the current will increase in size as long as it is pressed by the general drift against the land, but from this cause it is not probable that any current will acquire great dimensions; it depends upon the angle between the direction of the general drift and the coast line. If those directions are parallel, there will not be a current, but it will be formed and increased according to the obliquity in which those directions meet each other.

In the same manner the waves strike the shore, if their direction is more or less oblique to the coast line. By so doing, a part of their stored-up force is spent, another part rebounds by concussion into the sea, and by the continual action and reaction six and more times every minute, all along the coast, the current made by the general drift is considerably increased, and a definite current will appear, which, moving along the coast, receives continually new support as long as the waves break upon that coast. If this is a correct view of the

way in which currents are made, then although the winds are the prime cause of them, their immediate cause is the land. Without land there would be no currents, and where they are made by the land, the waves, which have so largely contributed to make them, must have lost in the current a great amount of their stored-up impelling force derived from the winds, and cannot remain so high as they were, nor their rate of undulation unaltered, both having contributed to form the current.

If this is true, then the waves must be highest, and have the greatest rate of undulation, where there is no land to make currents. Where there is much land and the longest coast line, there we must find the greatest and most numerous currents in the ocean, and consequently waves less high and with slower rate of progress, because it will be more difficult to raise a sea in a current than in the general drift, or where there is no drift at all. This may be the reason why the most irregular, nasty seas may be observed in a gale on the limits of those definite currents. Where there is the greatest surface of water with no land in it, there we must find the highest waves, all other influential circumstances being alike, with the quickest rate of undulations and few or no definite currents, which are made, not by the wind, but by the land. Herewith we set the argument aside, that where the strongest winds are met with, as in the southern anti-trades, we ought to find the largest currents. We can only expect to find them where there is a long prolongation of coast line, and where, by an unceasing accumulation of the stored-up impelling force of the wind, a current is formed and maintained, which received its directions from the conformation of the land.

Consequently we ought to find more currents produced by the land in the northern than in the southern half of our globe, and this ought to be the case for all the other causes from which sea-water may derive its motion, because the land makes currents, and because there is more land on this than on the other side of the equator.

Where there is land south of the equator, we find on every west coast a definite current, and always a cold current, and besides that, we find all along those coasts a greater surf by the breaking of the waves, than on the opposite east coast. It shows plainly that the direction of the waves, namely, the resultant directions of the impelling force of all the winds which have contributed to their formation, is oblique to the coast line, and being so, a current is produced which would not be there if the general drift and the waves were not oblique to the coast line. Now if, according to this explanation of the wind theory, the impelling force of the winds in high southern latitudes is carried by the waves and the general drift to the west coast of the southern continents, by which that stored-up force is taken from the waves, and put in a definite current along the coast in a south and north direction, it must be evident that the S.E. trade winds, which blow over that current nearly at right angles to the direction of the swell from the anti-trades, cannot be a convincing proof that those S.E. trade winds are the cause of those currents.

Even if there was an agreement between the direction of the currents and that of the winds over them, it would not be a proof that the winds which blow over a current have anything to do with the cause of it. But on the west coasts of southern continents the prevailing winds blow from the land, and the prevailing waves drift and currents press on the land. There is between them no agreement at all, and we have to go to the east coast to search for the effects of the sea created by the S.E. trade winds over the swell from the anti-trades which runs to the west coast, let us say in a S.W. and N.E. direction or thereabout. If we consult the wind charts, we find that the prevailing winds in the southern anti-trades are from N.W.; how is it then, that the resultant direction of the impelling force of all the winds as indicated by the swell along the west coasts of southern continents is from S.W. to N.E. or thereabout? The answer must be because in high southern latitudes the S.W. or southerly

gales are more powerful, and store a greater amount of impelling force into the waves than all the other prevailing winds together. This may be, and probably is so, but we don't know it exactly by observation; but whereas the waves, with few exceptions, are made only by the winds, we have good reason to infer from the fact that there is a swell in some direction, that this is the resultant direction of all the forces of the winds which have contributed to its formation. If, then, we find in the South Atlantic a swell running from S.W. to N.E. or thereabout, towards the west coast of South Africa, making, in conjunction with the general drift, a current along the coast, that swell and the general drift have been made by the winds in high southern latitudes, and these winds are the prime cause of the current along the coast of Africa, and not the winds which blow over it, and the convincing proof which you have found in "the agreement of the two systems as indicated by the chart," is not only no proof at all, but is a strong argument against your own wind theory.

The undulation in a thin layer of the surface water is the vehicle by which the impelling force of the winds is transmitted, but except the general drift, no water is transported by the waves, and whereas the general drift is very slow, and at the surface of the ocean receiving constantly the influence of the sun's heat, how can the circumstance that all the currents along the west coasts of southern continents are cold currents be explained by the wind theory? Is all the cold water which abounds in the South Atlantic the result of the impelling force of the wind transmitted in a very thin layer of the surface of the ocean? Here, at the fountain-head of the Gulf Stream, the wind theory must be tested, and if you are able to give a convincing proof that the cold current along the west coast of Africa is due to the winds in high southern latitudes, and that it receives a constant supply of impelling force from the waves in its long course directed by the coast line, then you ought to explain how it is possible that such a current, impelled

by a force which does not disturb the surface of the ocean to a greater depth than thirty feet, is not only able to drive it across the Atlantic through the Carribean Sea and the Gulf of Mexico, but to make its exit through several narrow straits in the North Atlantic, and there becoming a powerful current extending over a thousand miles, with a depth of 200 fathoms.

I have tried, by looking into the workshop of nature, to give an idea how a wind theorist may think ocean currents are made by the winds and by the land, so as to draw your attention to all the facts which are required for a convincing proof that the winds are the cause of oceanic circulation. This letter has grown much longer than I intended to make it, so that I only hope you will read these last few lines in which I ask your pardon for troubling you with my views on this subject, which deserve a better treatment and more time than at present I can bestow upon it. Again thanking you for all the papers you have sent me,—I am, yours most faithfully,

JANSEN.

Croll's reply to Mr. Jansen.

14th February 1876.

MY DEAR SIR,—I am much obliged to you for your long and interesting letter, and its many suggestions, which I have perused with much profit and pleasure. I quite agree with what you say in reference to the imperfection of our knowledge of the phenomena of *oceanic* circulation. I believe, however, that we know quite enough to enable us to conclude that oceanic circulation must be due either to gravitation or to the winds, or to both combined. With the exception of that derived from the earth's rotation, all other forces are infinitesimal when compared with these two. But it is well known that rotation cannot be the *impelling* cause of oceanic circulation. Rotation is simply a deflecting cause, that is to say, if any other cause produce motion of the water, no matter in what direction, rotation, as has

been demonstrated by Mr. Ferrel, will deflect the water to the right on the northern and to the left on the southern hemisphere. As there can be only these two causes — gravitation and the winds — I think it is perfectly obvious that if it can be proved that gravitation is insufficient, then it necessarily follows that the wind must be the cause.

I also quite agree with you that the first and immediate effect of the wind acting on the ocean is to produce undulations or waves; but if the wind is continuous, the horizontal component of the wind's force will generate a forward or progressive motion of the water. This is a result which necessarily follows from dynamical principles, but the point which is so much disputed is this: Will this surface flow extend to any considerable depth? The prevailing opinion is that it will not, and that the wind can only generate a mere surface drift. I, however, wholly differ from this opinion. An important element pointed out in *Climate and Time*, pp. 135, 136, seems to have been overlooked. The same may be said in reference to undercurrents, in Chap. xiii., and in the small paper which I sent you on "Objections to the Wind Theory." I hope I have given physical reasons why the wind can produce deep under-currents, and this will account for the cold polar water found under the equator. Again thanking you for your interesting letter,—Yours sincerely, J. CROLL.

Mr. Jansen's reply to Dr. Croll.

THE HAGUE, *17th February* 1876.

MY DEAR SIR,—I would not have troubled you again with my views upon your wind theory if I had not received your letter of the 14th, wherewith you have obliged me, and wherein I read, "If the wind is continuous, the horizontal component of the wind's force will generate a forward or progressive motion of the water. This is a result which necessarily follows from dynamical principles."

I do not think that the wind can get hold on the water to drag it forward, and if it does give a progressive motion, it must be very, very slight, and not to be compared with the progressive motion derived from the breakers tumbling down in the trough of the wave. Both combined have never exceeded in the strongest lasting gale 2 knots, and in general in southern latitudes, far from land, it will be on an average only 1 knot.

From Java to Natal we find on an average a difference between the dead reckoning and observation of true position of 8 degrees in longitude. The passage is made between twenty and thirty days, giving $\frac{8}{20}$ or $\frac{8}{30}$ of a degree in those latitudes as the daily average of surface currents produced by the continuous action of the trade winds, if not by other causes. With these facts before you, please make a Gulf Stream with them. I have not overlooked pp. 135, 136, nor the under-currents in *Climate and Time*, but as under-currents can't be made direct by the wind, you may not use them in your argumentations before you have proved that the surface currents of which they are the consequence, are caused by the winds.

You think that it is perfectly obvious that if it can be proved that gravitation is insufficient, then it necessarily follows that the wind must be the cause. Your work *Climate and Time* is written to show that, although the eccentricity of the earth's orbit cannot produce a direct effect, it must have an indirect one. I think so too in regard to the causes of oceanic circulation, which may be more indirect than direct, and in the present state of our knowledge about the currents, which I see with great regret in *Climate and Time* to be very imperfect, no one has a right to advocate any theory, than to stimulate research.—Yours most truly,

JANSEN.

CHAPTER XVIII

PERSONAL HISTORY

IN 1876 Croll was awarded the proceeds of the Murchison Fund, by the Geological Society of London, in recognition of his distinguished services to science.

On 18th February 1876, the retiring President, Sir John Evans, F.R.S., in presenting the balance of the Murchison Geological Fund to Professor Ramsay, for transmission to Dr. Croll, addressed him as follows:—

"Will you convey to Mr. Croll the balance of the proceeds of the Murchison Fund, and at the same time express the hope of the Council of this Society, that it may prove of service to him in the prosecution of those studies with which his name has been so long and so honourably associated.

"His researches on Ocean Currents, on Glacial Phenomena, on the bearing of the latter on Geological Time, and of both upon Climate, were generally known and appreciated, even before the appearance last year of his work on *Climate and Time*, in which the results of his studies are so carefully and ably expounded.

"The author of that book would be the last to regard the subjects of which it treats, as being all now definitely settled, and requiring no further investigation; and it is in the hope that his inquiries into the phenomena of glaciation, and into the physical causes conducing to extreme modifications of climate may be still further prosecuted, as well as in recognition of the valuable past labours of Mr. Croll, that the fund which I place in your charge has been awarded to him."

Professor Ramsay in reply said: "Mr. President,—In returning thanks on behalf of Mr. Croll, I have no need to enlarge on the merits of a man so well known to geologists by his numerous memoirs, and now especially by his remarkable work, *Climate and Time*; and though, on a range of subjects so wide, it is not to be expected that there should be no opponents to some of his views, there can be no doubt that the ability which he has displayed commands the universal respect of men of science and the adherence of not a few."

The gratifying intelligence was communicated to Croll in the following terms by the Secretary:—

GEOLOGICAL SOCIETY, BURLINGTON HOUSE, W.,
5th February 1876.

James Croll, Esq.

MY DEAR SIR,—I have much pleasure in informing you that the balance of the proceeds of the Murchison Geological Fund was awarded to you by the Council of this Society at their last meeting. The anniversary meeting of this Society will be held on Friday the 18th inst., when it is hoped that you will be able to be present.—Believe me, yours faithfully,

W. L. DALLAS.

Croll was unable to be present to receive this honour in person, partly from the state of his own health and partly from the death of his brother. He wrote a characteristically modest and grateful letter thanking the Society. He thus writes in his Autobiographical Sketch: "In February 1876, my brother, who had been staying with us ever since my mother's death, died suddenly from heart disease. In this same month the University of St. Andrews conferred on me the degree of LL.D., an honour which I felt at the time somewhat doubtful as to the propriety of accepting. A few months afterwards I was elected a Fellow of the Royal Society of London. I may also mention that in the same year I was chosen

an Honorary Member of the New York Academy of Science. I was afterwards chosen an Honorary Member of the Bristol Natural Society, the Psychological Society of Great Britain, the Glasgow Geological Society, of the Literary and Antiquarian Society of Perth, and of the Perthshire Society of Natural Science. I had the honour of receiving from the Geological Society of London the balance of the proceeds of the Wollaston Donation Fund in 1872, the Murchison Fund in 1876, and the Barlow-Jamieson Fund in 1884."

He was apparently anxious that the little bit of family graveyard at Cargill should be made as decent as possible, and that an appropriate stone should be put up, as appears from the following letter.

CARGILL, *25th April* 1876.

Mr. James Croll.

SIR,—I got the inscription that you sent me to put on the tombstone, and I send it to you as you wished to know it for the putting on of your brother's name, and I saw Mr. Farqueson about the border stones. He has no objection if you don't encroach on other ground. So I have got the stones forward and dressed and ready to put up. If you would send me the inscription you want put on the stone as soon as convenient, I will try and get it done as soon as I possibly can, and in so doing you will oblige,—Yours truly, JOHN FENWICK.

He sent a copy of his book, *Climate and Time*, to Sir Wyville Thomson, who wrote acknowledging, as follows:—

Challenger, ASCENSION, *2nd April* 1876.

MY DEAR SIR,—I am greatly obliged to you for your most interesting book, which did not catch us up quite so soon as it ought to have done, on account of a slight change in our route. At all events, whatever conclusion we may come to ultimately, we have brought

together a vast quantity of material. I will get it put into shape and published as soon as I possibly can. In the meantime, the reports published by the Admiralty give nearly all that is required. I have just sent to the Royal Society a notice of the great cold indraught into the Atlantic, which is between the coast of South America and the middle rise. The section of the cold stream under 1° 5′ is 800 square miles. The effective part of the Gulf Stream is 6 square miles in section. The debateable part of this affair must wait till we get home now—sufficient for every day is its work. I have no doubt whatever that the cause of this great indraught from the Antarctic Sea is an excess of evaporation in the North Atlantic. Everything seems to support that view. Down to 500 fathoms, all the difference of temperature depends upon the direct or indirect influence of wind currents, below that you have the temperature of the water of the Northern Sea. Such is my present rough notion. I hope to send you shortly the first volume of my rough journal, which I mean to publish with tables, etc., at once.—Believe me, very truly yours, W. THOMSON.

P.S.—We shall be home about the 20th of May.

In 1876 he was elected a Fellow of the Royal Society of London, one of the highest honours attainable by a man of science, and which was highly appreciated This was intimated to him in the following terms:—

HYDROGRAPHIC DEPARTMENT, ADMIRALTY,
11th April 1876.

James Croll, Esq.

MY DEAR SIR,—I have just left a Council meeting of the Royal Society, and am exceedingly pleased to acquaint you of your selection as one of the fifteen candidates for election by the general body. Accept my best wishes on the occasion.—Yours faithfully, F. EVANS.

Croll was always ready to give any assistance in his power to any scientific man, and the following application at once received his sanction:—

56 LUDGATE HILL, LONDON,
28th June 1876.

James Croll, Esq.

DEAR SIR,—We are just putting into the printer's hands a new edition of *The Great Ice Age.* In it Mr. Geikie wishes to use the chart of "Ocean Currents," which appears in *Climate and Time.* We presume you will have no objection to this.—Yours sincerely,

DALDY, ISBISTER & CO.

Croll's work was getting to be known all over the civilised world. The following letter from an Italian correspondent is interesting:—

ROME, *6th August* 1876.

MY DEAR SIR,—I have just this moment received your kind letter, accompanied by your manuscript upon the "Outline of the Theory of Secular Changes of Climate," and I have no words to thank you for so great and important a work. I do not doubt that the note made by you and inserted in my translation of Lyell's *Principles of Geology* will give a very great importance to the esteemed work of the English geologist, and the Italian readers will be grateful to you for the great trouble you have had the kindness to take.

At this moment I am making my preparations in order to go for some months to the country at Eocea (Piedmont) with my family, and I regret very much not to have time to read your interesting "Outline" because I leave Rome this evening. In passing by Turin I shall not fail to pay a visit to my publisher and insist upon his immediately commencing the printing of my work. As soon as he shall have decided to print it, I will inform you.

I feel myself greatly obliged to you also for the

translation that you have had made of my small paper, and for the interest that you take in it. My intention in writing this small paper was only to make known the effects that the discharges of artillery could produce in the atmosphere, and the meteorological changes that might result. Time only will prove if my observations are true, and I feel very much obliged to all those who are so good as to take some interest in that physical question.

I thank you also very much for the interest that you take in my health, and I am happy to tell you that I am perfectly recovered. I am obliged to go to Piedmont in order to arrange my affairs in consequence of the death of my father; and if I can oblige you in any way, I beg you to write to me at Eocea till the end of October. Meantime, I am, with very great respect, yours very truly, L. GATTY.

The following letter from Mr. Herbert Spencer is very interesting from a philosophical standpoint. Unfortunately Mr. Spencer has not preserved Croll's reply.

38 QUEEN'S GARDENS, BAYSWATER,
13th October 1876.

James Croll, Esq.

DEAR SIR,—I was much gratified the other day on reading your article in the *Philosophical Magazine*. I was the more gratified because I found in it verification of a view for which I have myself contended. When preparing the last edition of *First Principles*, I had a prolonged discussion with my friends Tyndall and Hurst, who wished me to modify certain views I had expressed,—views which, in consequence of our discussion, I elaborated more fully. They failed to convince me that I was wrong, and I failed to show them that I was right. Now I am glad to find that I do not stand alone in the view I took, that the conception of potential energy, as scientific men at present hold it, is not a legitimate conception, and that in fact a force cannot be transformed into a relation of positions in space.

I should like to send you a copy of the last edition of *First Principles*, in which I have contended for this view, but I don't like to forward it until I am sure of your address. Would you kindly let me know to what place I may safely send it?—Faithfully yours,

HERBERT SPENCER.

The meeting of the British Association was now about to be held in Glasgow, and Dr. Morison, having invited Croll to stay with him, received the following reply:—

EDINBURGH, *17th August* 1876.

DEAR DR. MORISON,—Many, many thanks for your very kind offer. Had I been coming through to the meetings, I should have been so delighted to stay with you.

I shall have a paper in Section A. but will not manage to be present. The older I grow, the more disinclined I feel to go out to public meetings of any sort.

I have not been at a scientific meeting for upwards of half a dozen of years. The real truth is, there is a cold materialistic atmosphere around scientific men in general, that I don't like. I mix but little with them. There is, however, indication of a reaction beginning to take place towards something more spiritual in science; and the day, it is to be hoped, is not far distant when religion, philosophy, and science will go hand in hand.

Before the cold weather sets in, I shall be delighted to come through to Glasgow and spend an afternoon with you.—With kind regards, I am, yours ever truly,

JAMES CROLL.

In 1876 Croll paid a flying visit to London, and wrote the following letter to his colleague, Mr. Horne of the Geological Survey, who had returned from spending his holidays in Shetland, where he had obtained evidence proving the westerly movement of the ice during the Glacial period.

EDINBURGH, *28th November* 1876.

MY DEAR HORNE,—I have got back from London after an agreeable journey. The *British Quarterly Review* has come to hand. Have you sent off your letter to *Nature* yet?—"The Glaciation of the Shetland Isles" (*Nature*, vol. xv. p. 139). Skertchley's discovery is making some little noise in London, I believe, and of course a number are of opinion that it cannot be Interglacial.—Yours truly, JAMES CROLL.

Professor M'Farland of America having written a paper on the "Curve of Eccentricity of the Earth's Orbit," and communicated a copy to Dr. Croll, the following interesting correspondence followed between them :—

EDINBURGH, *8th August* 1876.

Professor R. W. M'Farland.

DEAR SIR,—I have read with much interest your paper in the *American Journal of Science* on "Curve of Eccentricity of the Earth's Orbit." Please to accept my best thanks for the very great amount of labour you have bestowed on my table. I am delighted as well as surprised to find that it contains so few errors. You refer to one case where the error exceeded ·001. Would you kindly inform where the error is? If it be the value for the period 900,000 years ago, you will find from the enclosed letter in *Nature* that it is a typographical error. Does your calculation extend from the present date back 1,100,000 years?

I am much pleased to observe that Mr. Stockwell's curve agrees so closely with that of Leverrier. Apologising for troubling you, I am, yours most truly,

JAMES CROLL.

COLUMBUS, STATE OF OHIO, U.S.,
24th August 1876.

Mr. James Croll, of the Geological Survey of Scotland.

SIR,—Your very kind note of the 8th inst. was received three or four days ago. At the earliest oppor-

tunity I reply, answering your queries and explaining pretty fully the causes which have led to this correspondence.

I send herewith a catalogue of our college, but since that may miscarry, I put in the letter a leaf containing a list of the Faculty. Our President, you see, is Mr. Orton, who in this country has a very good reputation as a skilful geologist. If you have seen the *Ohio Geological Report*, his name is already familiar to you. Late last fall he procured your work on *Climate and Time*, and one day in speaking of it, he said it was very interesting, and that when he had finished reading it, he would hand it to me. Stepping into a book-store a few days afterwards, I saw a copy of your book, and, looking over the synopsis of the chapters, I was so struck with it that I did not care to bide the time of President Orton, but purchased at once, and read it with ever-increasing interest and delight. In a few days, then, the book was again the subject of conversation, and our President inquired whether I thought the astronomical calculations could be depended on, and if so, he thought you had struck the path which would finally lead to a solution of the Glacial problem. As I write now, tons on tons of worn, rounded, polished, striated boulders of all sizes, from a few pounds weight to that of a ton or two, are lying by the side of the street before my door,—the street is being lowered—granite, syenite, every kind of old rock, whose native bed is hundreds of miles to the north or north-west, far beyond Lake Erie. The Glacial age is thrust in one's face every day, and we meet with it whether we will or not. It is, then, but natural that the matter of which your book treats should frequently recur in conversation. But to return to the question. I remarked to President Orton that I had no doubt the figures were right, but that if he wished, I would re-compute some of the values, and see. About Christmas, then, I began it, spending a couple of hours a day—as my duties would permit. Becoming much interested in it, I concluded to go over the ground pretty thoroughly,

and computed backwards 350,000 years, — forwards 750,000,—always dividing your longer intervals, in many cases computing for intervals of 5000 years, especially near the maximum and minimum points,—and in one or two instances for intervals of 1000 years. This occupied me till the beginning of April. Then came Mr. Newcomb's critique on your book in the April number of the *American Journal of Science.* I did not like the criticism, it seemed too much in the style of one who sees no beauty nor brightness nor glory in the sun—sees only spots. The impression left on the general reader would be, I think, that little confidence should be placed on the mathematical part. He in part neutralises this, but the larger portion of the impression would perhaps remain as above indicated. Mr. Stockwell, a very careful and laborious mathematician, worked out the perturbations of the planets, eccentricity, etc. The work is in vol. xviii., 1873, of *Smithsonian Contributions to Knowledge.* You probably have access to it. Mr. Newcomb commended that work. It contains part of the results printed in 1867, in a small volume on the moon's mean motion. The small work contains the value of the eccentricity for 1,000,000 years.

It occurred to me that if Stockwell's work was good,—and he certainly displays great labour and skill in his masterly treatment,—it could do no harm to put yours side by side with the part on the eccentricity, and to tabulate the extreme variations in solar distances as drawn from his treatise and from yours . . . in order that whoever should see light in Stockwell's curve, might also see more than moonshine in yours. I therefore set the curves side by side from his initial point (175,000 years back) forward to the date 150,000, in advance, and gave in round numbers the table of variation in solar distances. In short, whoever upholds Stockwell must be careful how he criticises you. My article in the *Journal* was made as brief as possible.

Further, I have reduced the work to the same scale, and compared the curves onward to 825,000 years.

I think it will be conceded that Stockwell's formulæ are more trustworthy than Leverrier's, yet see how the curves run on, almost identical in form, and differing chiefly in the value of ordinates at particular times. I send the two from 150,000 to 825,000.

Since April I have extended my computation 500,000 years ahead of the date then reached (750,000 in advance), stopping at 1,250,000. I purpose to go back in the other direction as opportunity shall be afforded. I have in all computed for 1,600,000 years. Now as to differences. The one which I deemed in error by over ·001 is + 150,000. Your figures are ·0353, mine ·0335. I have looked over my calculation several times and can find no error in it; and it occurred to me that the figures might easily have changed places. From 35 to 53 is not a difficult process.

Two numbers, 170,000 and 220,000 marked S in your table differ by more than ·001, but as those numbers are from Mr. Stone, I had no reference to them. For + 300,000 you have ·0158,—my figure barely reached ·0068, and as you sometimes find considerable fractions over, this too was omitted. The only point since my communication was sent to the *Journal* (April, although it did not appear till June) is your last number, 1,000,000; you make it ·0528, I find ·0333. For 950,000 you have ·0086, I have ·0088; and for 975,000 I have ·0217. I have computed many points along this place, and I think your figure wrong. As a specimen of your chart and my work I give the following:—

	Chart	M'F.
50,000	·0173	·0173
100,000	·0191	·0191
150,000	·0353	·0335
200,000	·0246	·0247
250,000	·0286	·0287
300,000	·0158	·0168
350,000	·0098	·0098
400,000	·0429	·0429
450,000	·0232	·0231
500,000	·0534	·0525
550,000	·0259	·0268
600,000	·0395	·0401
&c.	&c.	&c.

This is about the usual way all through.

Thanking you for your kindness, allow me in conclusion to express sympathy on your behalf—the envelope seemed to say that while we write and think and speak of years by the million, few and short are the days which fall to us here.—I am very truly and with great respect, yours, R. W. M'FARLAND.

GEOLOGICAL SURVEY, EDINBURGH,
8th November 1876.

R. W. M'Farland, Esq.

MY DEAR SIR,—I am exceedingly obliged to you for your long and interesting letter of the 24th August, which I should have acknowledged ere this. I have, however, been almost knocked up for some time past by pain in the head, a complaint I have suffered from, more or less, for several years back.

I am delighted to observe that you have extended your calculations much beyond what you published in the *American Journal*; please to accept my many thanks for the particulars of these calculations which you have sent. I am also gratified to find that your figures agree so closely with my own. I have preserved all the MS. of my calculations, and when my head gets a little stronger, it will be interesting to go over those referring to the period in which you found I had been slightly in error. When you finish your computations, might it not be a good thing to publish your table of values in the *American Journal of Science*? Your short paper in that journal has been referred to by several of our leading scientific journals here. The computation must have cost you a very great amount of labour indeed.

I was gratified to find that you were so well pleased with my book. The theory, I am glad to say, is at present exciting a very considerable amount of attention in this country, and is being gradually adopted by our leading geologists.

I am much obliged to you for the prospectus of your

college, which you sent, and which I have read with much interest.

After you have finished your computation, I shall be glad to be favoured at your convenience with a copy of your results.

I feel grateful for the words of sympathy which closed your letter. I mourn the death of a brother suddenly removed from among us.—I am, yours most truly,

JAMES CROLL.

OHIO AGRICULTURAL AND MECHANICAL COLLEGE,
COLUMBUS, O., *1st January* 1877.

Mr. James Croll, Geological Survey, Edinburgh.

DEAR SIR, — Your note of November 8th was received in due time, and should have been answered sooner, but my time has been much taken up with my regular duties,—so much so that I have not yet recommenced computations. Mr. Stockwell's formula (which is identical with Laplace's, as given in the translation by Bowditch, *Mec. Cel.* vol. ii. book 7, section 56, with elements computed for 1850) contains 28 terms, and the computation is very laborious. I propose to use subsidiary formulæ, so that the work shall be executed more rapidly. When the computation has been extended backwards in this way to say 350,000 years, I will send you the curve on the same scale with yours.

Mr. Dana, chief editor of the *American Journal of Science*, has requested me to furnish him with whatever I may think of general interest in this line, or others; and when I have thoroughly reviewed the work already done, and extended the computations backwards, so as to include those old Glacial epochs, I propose to offer the table for publication.

Did you ever see a little work published by Mr. Stockwell about ten years ago, in which he examined Mr. Adams's work on the acceleration of the moon's motion? I copy the points made by Mr. Stockwell, p. 12.

"And we propose to show—

"*First.* That Mr. Adams's solution of the problem is incorrect;

"*Second.* That if his solution were correct, the results are not applicable to the problem;

"*Third.* That his integral, depending on the tangential force, is equivalent to the correct integral increased by a constant quantity;

"*Fourth.* That the tangential forces produce neither non-periodic terms, nor periodic terms with variable co-efficients; and

"*Fifth.* That the principles enunciated by Mr. Adams as the basis of his solution, when correctly developed, lead to precisely the same result as the more simple and apparently less general method of Laplace."

It is entirely possible that when the fourth figure differs from yours,—I mean my fourth decimal,—the correction may have to be applied to my table, not to yours. I used six decimal logarithms.—Wishing that you have recovered from the indisposition of which you spoke in your last note, and that you may yet live many years, to the advantage of science and of your fellow-men, I am, with very great respect, your obedient servant,

R. W. M'FARLAND.

In 1877, Dr. Croll opened up a new field of inquiry, and, betaking himself to astronomical problems, he wrote a paper on the "Probable Origin and Age of the Sun," which appeared in the *Quarterly Journal of Science* for July 1877.

The speculations of physicists regarding the limit of geological time prompted him to investigate the question of the probable age and origin of the sun. Accepting gravitation as the only conceivable source of the sun's heat, he reviewed the two forms in which this theory had been presented,—first, the meteoric theory advocated by Meyer, and, second, the contraction theory, expounded by Helmholtz. Even if we postulate 100 millions of

years as the limit of geological time, he maintained that gravitation will not account for the supply of the sun's heat during so long a period. According to the foregoing theories, it is assumed that the matter composing the sun, when it existed in space as a nebulous mass, was not originally possessed of temperature, but that the temperature was developed as the mass condensed under the force of gravitation. He suggested that the nebulous mass might have been possessed of an original store of heat previous to condensation. Proceeding to consider how the sun's mass could have become possessed of energy in the form of heat previous to condensation, he argued that it was readily explained by means of the dynamical theory of heat. "Two bodies, each one-half the mass of the sun, moving directly towards each other with a velocity of 476 miles per second, would by their concussion generate in a single moment 50,000,000 years' heat." He further contended that we are led from physical considerations regarding the age of the sun's heat to the conclusion, that the geological history of our globe must be limited to 100 millions of years.

His papers attracted the attention of the Astronomer-Royal for Scotland, who wrote the following letter to him :—

15 ROYAL TERRACE, EDINBURGH,
2nd February 1877.

DEAR MR. CROLL,—Will you kindly allow me to ask if, in your researches touching the eccentricity of the earth's orbit, which have become classical long since, you had occasion to compute the *obliquity of the ecliptic*, and can you in that case supply me now, without trouble to yourself, with a note of what you made it for the epoch of 4000 years, or 4050 years before the present time? I shall be happy to explain afterwards what it is wanted for.—I remain, yours very truly,

P. SMYTH.

JORDANBURN, *3rd February* 1877.

MY DEAR SIR,—Mr. Stockwell, in his *Memoir on the Secular Variation of the Elements of the Orbits of the Planets*, published about four years ago, discusses the question of the obliquity of the ecliptic, and gives a table of its amount for 8000 years past and future. According to this table, p. 196, the obliquity 4000 years before epoch 1850 was 23° 57′ 42·6″.

As Mr. Stockwell's Memoir may interest you, if you have not already seen it, I have sent you my copy. I am in no hurry for it back.

I believe Mr. Stockwell's table is the best thing we have on the subject.—I am, yours most truly,

JAMES CROLL.

15 ROYAL TERRACE, EDINBURGH,
8th February 1877.

DEAR MR. CROLL,—I beg to apologise for not having yet thanked you for your obliging note and effective answer, touching the obliquity of the ecliptic. I had hoped to have written on returning the quarterly pamphlet ere this, but though I have been prevented hitherto, I still hope soon to do so, and remain, yours very truly, P. SMYTH.

Croll wrote to Mr. Herbert Spencer for a reference to a passage in one of his works, but Mr. Spencer has not preserved his letter. Mr. Spencer's reply, however, gives a key to Croll's inquiry, and is as follows :—

37 QUEEN'S GARDENS, BAYSWATER,
24th February 1877.

MY DEAR SIR,—The passage you refer to, you will find in *First Principles*, section 182, and extends over pages 532–5. The conclusions drawn, however, though like the one which you name in respect of the genesis of enormous heat by the collision of celestial

bodies meeting and destroying one another's great velocities, has in view another issue. Instead of the formation of bodies thus raised to high temperature, and continuing thereafter to radiate heat for long periods as suns, the argument is rather to the effect that the heat evolved by such collisions, taking place with the enormous velocities eventually acquired by stars gravitating into clusters and coming into collision, will have the effect of dissipating the matter they are formed of into the gaseous state, and eventually into a nebulous form.—Truly yours,

HERBERT SPENCER.

The following correspondence with his friend, Mr. Osmond Fisher, is interesting:—

HARLTON, CAMBRIDGE,
3rd March 1877.

MY DEAR SIR,—No doubt you have observed the article in this month's *Geological Magazine* by Mr. Carpenter, in which he seems to have inserted your name with Mr. Murphy's. I cannot quite make out his argument, but there seems to be something to be got out of it. Shall you reply to it? I have an old note about Phillip's paper on "Mass," taken from the *Reader*, 18th February 1865. This was a notice of a paper at the Royal Society, 26th January 1865.

Mr. James Geikie has been to see our glacial beds, and pronounces some of them true till. I am hardly disposed to agree with him, though I admit there are some things about the deposits that look like it. He also agreed with Mr. Skertchley that they had found *interglacial* flint implements. But I went over the ground one day with Skertchley and Belt, and I thought the case was, what is called in your part of the world, not proven. All that could be said was, that the implements occurred in a brick earth, and that there was a similar brick earth in the neighbourhood to be found beneath boulder clay. One section on which Skertchley chiefly relied, when I

came to examine it, certainly did not carry the weight he assigned to it; for what he thought to be boulder clay was, as I verily believe, only the top of the chalk rock covered with dirt, for so it turned out when I dug into it with my knife. Excuse this from an unoccupied invalid, and believe me, very truly yours, OSMOND FISHER.

15th March 1877.

Rev. Osmond Fisher, M.A.

MY DEAR SIR,—I am much obliged to you for sending me the account of your journey to Skertchley's ground. Mr. Skertchley's argument, if I understand it right, is that the brick earth in which the implements were found is the equivalent of those found in the neighbourhood between two boulder clays, and is therefore interglacial. To those who are familiar with the intercalated beds in Scotland, this would be a very natural inference; for nothing is more common than to find the same bed in one place between two boulder clays, and at another place the upper clay absent. If the two brick earths, the one between the two boulders clays and the other not, overlaid with clay be in all other respects the same, the probability is that the two belong to the same series. But, of course, till more information is obtained on the subject, Mr. Skertchley's conclusions can hardly be regarded as proven. I presume this is the view you take of the matter.

I have sent off your paper to Mr. James Geikie.

I have now determined to shake myself clear altogether of Geology, and devote all my leisure time to a subject I began some twenty years ago, but had to lay it aside when I got immersed in the climate business. If my head would only permit, I want to go into the abstract question on the determination of molecular motion and its bearing on Theism. It is the kind of study I feel most at home in, and the field is almost entirely new.—Yours very truly,

JAMES CROLL.

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

prefer the theory of floating ice rather than admit that the North Sea was filled with land ice. The Director is in Fife, along with his students, where he will be till the end of the week if he can manage to weather the storm. I have ordered 1 and 3 Inverness.—Yours truly,

JAMES CROLL.

EDINBURGH, 10*th July* 1877.

MY DEAR HORNE,—I have enclosed a packet of paper in your parcel which is sent on to-day. I could not manage to get your book out of the College Library yesterday; but if I see the Director to-morrow, I will try, and if I succeed I shall post it to you. I am not surprised at what you say about certain critics. But they must all come round in the longrun. You will require to look sharp, or perhaps they will have their views on Shetland out before you, which would be a pity. While I am writing, a letter has just come from Mr. J. Geikie. He says, beyond all doubt Harris is glaciated from the mainland; ice reaching up to 1650 feet, with erratics from Skye.—Yours truly,

JAMES CROLL.

EDINBURGH, 28*th November* 1877.

MY DEAR HORNE,—I have paid your account to Johnston, who will return you the receipt. I am glad to hear that your paper is so far advanced. But I would not place much faith in the *opinion* of geologists in general about the height of the land during the Glacial epoch, unless the *reason* was given; for the opinion is often based on the preconceived theory that the height of the land was the cause of the cold. In the particular case to which you refer, it is quite possible that Erdmann may be right. I have just sent to the railway people to come for your box.—Yours truly, JAMES CROLL.

CHAPTER XIX

CORRESPONDENCE WITH SIR J. D. HOOKER, MR. A. R. WALLACE, ETC

DURING the year 1878, Croll wrote a paper on "Le Sage's Theory of Gravitation," which appeared in the *Philosophical Magazine* of January 1878, and in the *American Journal of Science and Arts*, February 1878 (No. 70). This was followed by another paper on "The Age of the Sun in Relation to Evolution," which appeared in *Nature* on January 10, 1878 (No. 71). He continued this subject, and wrote a paper entitled "The Age of the Sun in Relation to Evolution," dealing with the motion of the stars, which appeared in *Nature* in April 1878 (No. 72). Thereafter he wrote a paper dealing with the origin of nebulæ, which appeared in the *Philosophical Magazine* of July 1878 (No. 73).

He returned to his geological studies, and wrote a paper on "Cataclysmic Theories of Geological Climate," which appeared in the *Geological Magazine* of September 1878, while a short abstract was published in the *American Journal of Science and Arts*, November 1878, p. 387 (No. 74).

This was practically his year's independent work, beyond his Survey duties. Mr. Croll was deeply interested in the results of the examination of the glacial phenomena of Shetland by Messrs. Peach and Horne during their holidays in 1878, as the following letters clearly show:—

EDINBURGH, *7th May* 1878.

MY DEAR HORNE,—I have sent on your map-case by book-post. In reference to your query about the ice,

the principle which determines the path of an ice particle pushed by force from behind is very simple indeed. It is this—the particle *will always take the path of least resistance*, whether that path be round the edge of the hill, or over the top of it. In the case to which you refer, sometimes the path of least resistance may be round the sides of the hill, and at other times it may be over the top of the hill. Without knowing the physical conditions of the hill, with its surroundings in the midst of ice-fields, it would be perfectly impossible for any one to predict whether the path of least resistance lay straight up the sloping face of the hill, or round its sides. When the striæ show that the ice has moved straight up over the hill, this proves that it had less difficulty in doing that, than it would have had in going round the sides. And when the striæ show that the ice had taken the round-about road, this is evidence that it did it to avoid the stey brae.—Yours truly, JAMES CROLL.

EDINBURGH, *19th July* 1878.

MY DEAR HORNE,—Peach gave me an outline of your wonderful discoveries. They are very remarkable, and prove beyond the possibility of a doubt that the Scandinavian ice-sheet passed over the Shetlands. The facts will rather annoy geologists in general who ignore the possibility of ice of such magnitude. You will find in my paper in the *Geological Magazine*, and in the chapter on "Climate and Time," everything which I know of regarding the glacial phenomena of Shetland. Prior to the appearance of my paper in the *Geological Magazine*, I do not think the idea that the Scandinavian ice filled the German Ocean ever entered any human being's mind. That the Shetland and Faroe Islands should have been glaciated by ice from Scandinavia was a conception which I hardly expected would have been so well received as it has been. By the way, it was in one of my papers in the *Reader*, about a dozen of years ago, that the idea was first suggested. I think I have the article at home,

should you wish to see it. I forget if any special reference was made to Shetland. It is a good idea to put in your paper a chart showing the path of the ice in North-western Europe. Would it not be a good idea to make a volume of your materials, and simply give a very full outline first to the Geological Society.—Yours truly,

JAMES CROLL.

EDINBURGH, *20th December* 1878.

MY DEAR HORNE,—The map and lines are *engraved on one plate.* The map from which the plate was taken was not one of Johnston's. I really at this moment forget where I got it, but perhaps I may find it out, and let you know. In the paper on the Antarctic ice, in the forthcoming number of the *Quarterly Journal of Science*, I have a section on the relation of the Antarctic ice to that of the Glacial epoch; and, to illustrate some points bearing on the physics of the question, I have inserted my map with the lines, but without the colour. When I go home to-night, I shall post you a copy of the map; I have none of the coloured copies, but this may serve your purpose. You should make a strong effort to get out your paper as early in the session as possible.—Yours truly,

JAMES CROLL.

The following interesting correspondence took place between Sir J. D. Hooker and Croll:—

ROYAL GARDENS, KEW,
6th November 1878.

DEAR MR. CROLL,—I thank you much for your valuable contribution to *Geological Climate*, just received and read with great interest.

The subject has been brought up again by a most ingenious paper, by Count Saporta, on the old Polar vegetation, "L'Ancienne Végetation Polaire," Extrait des Comptes Rendus du Congres internat des Sc. Geograph.,

Paris, apparently presented in 1875, printed in 1877, and not received here till autumn 1878, which is attracting so much attention amongst palæontologists that I must deal with his essay in my address to the Royal, though I should do this only in the way of a review of its contents. Saporta is a master in Vegetable Palæontology. He seeks to eliminate the contracted seasons of the poles by a greater diffusion of sunlight before the orb was condensed, and lays great stress on the uniformity of temperature being due to aqueous vapour, but has not hit upon your explanation of the latter, namely, the connection of the trade winds with the precession of the equinoxes.

I do not suppose that either solar or terrestrial physics will stand the strain of Saporta's hypotheses, or rather speculations, but I am not qualified to deal with that question. Too much stress is laid, I think, by you and others upon what I said at the R. S. *a propos* of the want of sunlight—I could not have meant to imply that the St. Petersburg Polars were matted down in absolute darkness for six months, though they certainly are deprived of direct sunlight for a long period,—not that the want of sunlight during the Arctic winter would not be very prejudicial to plants in general. What I said was, that it need not be prejudicial to dicotyledonous plants with deciduous leaves. The great difficulty is to account for the survival during Arctic winters of forests of evergreen dicotyledons, hence much depends on the evidence of such having existed in the Polar regions. This, again, depends on the importance attached to the apparent fact that many laurels characterised the flora, and that the laurels of those days were, like those of this day, for the most part evergreen. I am not at all convinced that they were so, but am not prepared, without further examination of them, to affirm the contrary.

Have you seen Dyer's lecture on Plant Distribution, as a field for geographical research? It is the best *résumé* of the condition of geographical botany which I

have seen, and he arrives at one of Saporta's conclusions by a very different route, namely, that the vegetation of the globe all started from the north.—Ever very truly yours, J. D. HOOKER.

EDINBURGH, *14th November* 1878.

Sir Joseph D. Hooker, C.B., D.C.L.

MY DEAR SIR,—Allow me to thank you very much for your letter of the 6th inst. and paper which you kindly sent, both of which I have read with great interest.

I am delighted to observe that you are devoting attention to that perplexing question of the existence of a warm condition of climate in Polar regions in former epochs. I shall be looking forward with much interest to your address. I am sorry I should have expressed myself somewhat incautiously in reference to your remarks at the Royal Society meeting. The fact is, I had forgotten your exact words on the subject, and had taken your views second-hand from some remark made at the Geological Society, by one of the members, at the close of a discussion on the subject of Polar climate.

I am much obliged to you for directing my attention to Count Saporta's and Dyer's articles, both of which were unknown to me.

I am very far behind in everything relating to palæontology.—I am, yours very truly,

JAMES CROLL.

In 1879, Dr Croll wrote a paper on "The Thickness of the Antarctic Ice, and its Relations to that of the Glacial Epoch," which appeared in the *Quarterly Journal of Science* of January 1879 (No. 75). Though he was unable, from the state of his health and the pressure of his other duties, to comply with the request of Professor Baynes to contribute articles on geological subjects to the *Encyclopædia Britannica*, yet at the urgent request of Sir Archibald Geikie he wrote an outline of "The Theory of Secular Changes of Geological Climate" (No. 76)

for his article "Geology" in that work, which was afterwards embodied in Sir Archibald Geikie's *Text-Book of Geology*, 1882. He also wrote a paper on "Interglacial Periods," which appeared in the *Geological Magazine* of October 1879 (No. 77); and another entitled, "Why the air at the Equator is not hotter in January than in July," which appeared in *Nature* on 17th December 1879, and in the *American Journal of Science and Arts*, February 1880 (No. 78).

At this time he received the following instructive letter from Mr. Agassiz, of America, relative to his theory of ocean currents:—

CAMBRIDGE, MASS., *15th May* 1879.

MY DEAR SIR,—During the two years I have been cruising in Gulf of Mexico and West India, the problem of the Gulf Stream has of course greatly interested me. From what has been accomplished the past winter by the *Blake* (Commander Bartlett), I am satisfied that we must look for the great supply of heated water to the northern hemisphere, not so much to the Gulf Stream proper, as it is measured by the amount of water passing through the Straits of Yucatan and finally emerging between Cape Florida and the Bahamas, and the immense mass of water pushed by the trade winds against the wall forming the islands between St. Thomas and Trinidad, and which, failing to find a passage through the comparatively shallow channels between them, as we have only between Sombrero and the Virgin Islands a deep channel 1000 fathoms, while not one of the other channels can be said to average more than 300 fathoms, and in many cases peaks and ridges of 40 fathoms are not uncommon; the other deep channel existing between Martinique and West Indiana, and for a small distance having a depth of 800 fathoms. Now, of course, of this immense body of water banked up against this wall of islands, but a small part forces its way into the Caribbean Sea; the rest must go north along the

eastern bank of the Bahamas, until it is deflected again easterly by the prevailing westerly winds of that region. Then, of course, a body of water 1500 fathoms deep and 20° in latitude cannot find its way across the wall closing the Caribbean Sea.

I hope in a few days to be able to send you the preliminary report of the work done last year, and should be glad of any criticisms you may find time to make. I hope shortly to publish some of the temperature sections also, and define the condition of things better that way. —Yours truly,

A. AGASSIZ.

Mr. Croll, having sent some of his recent papers to Professor M'Farland of America, gave rise to the following correspondence:—

OHIO STATE UNIVERSITY, COLUMBUS, O.,
28th March 1879.

James Croll, Esq.

DEAR SIR,—I write to acknowledge the receipt of pamphlets from time to time sent by you. I thank you for the favours, and allow me in return to say that your statements touching the South Polar ice-cap seem to leave nothing further to be said. It certainly appears to be established, if anything can be established, by the orderly array of facts. Some of Sir W. Thomson's assertions seem to me to be wild, and to have been hastily made. From the early part of the spring of 1878, I was so much engaged that I could not find time for further computations on the eccentricity of the earth's orbit. On 1st January of this year I resumed the work, and devote part of an hour every day to it. To-day I finished the work for 2,000,000 years by Stockwell's formula—time previous to 1850. I had a year or two ago extended it forward to 1,250,000 years. My intention is, if opportunity allow, to cover the time from 3,000,000 anterior to 1,250,000 years posterior to

1850, calculated both by Stockwell's and by Leverrier's formula at intervals of 10,000 years, and in specially interesting cases to divide the period into smaller portions, then to make a drawing of the two curves, and a table of eccentricities and longitude of perigee.

But the work is vast, and I am not sure that I can have the work finished before 1st January 1880.

I have no doubt that you will find a very interesting set of facts in the matter of the coal seams and their intervals apart, as found in the Hocking River Valley, in the south-east quarter of Ohio.

When my work is done, I shall have the pleasure, I hope, of sending you a copy.—Very respectfully,

R. W. M'FARLAND.

EDINBURGH, *21st June* 1879.

Professor M'Farland, A.M.

MY DEAR SIR,—I am very much obliged indeed for your kind letter of 28th March. I would have acknowledged receipt long ere this, had I not been suffering a good deal from pain in the head.

I am delighted to hear that you are still persevering in your laborious undertaking. When your calculations are finished, they will be of very great interest and value. I shall be looking forward with much interest to the publication of your results.

I am very much gratified that you are so well pleased with my paper on the Antarctic ice.

Professor Orton very kindly sent me a copy of his memoir on the Hocking River Valley coal seams. The facts narrated were very remarkable.—I am, yours very truly, JAMES CROLL.

Mr. Alfred R. Wallace having written an article on "Glacial Epochs" in the *Quarterly Review*, the following interesting correspondence took place between Croll and him:—

GEOLOGICAL SURVEY, EDINBURGH,
6th August 1879.

Alfred R. Wallace, Esq.

MY DEAR SIR,—I need hardly say that I read with intense pleasure the article on "Glacial Epochs" in the *Quarterly Review*, which has been very much heightened by learning this morning from Mr. Fisher who the author is. The article will do more for the theory of *C. and T.* than anything which has yet appeared on the subject. From your special knowledge of climate in relation to geographical distribution, as well as from your extensive acquaintance with all branches of natural science, I had long felt somewhat curious to learn your real opinion as to the bearing of the theory, and I certainly am not a little flattered as well as delighted to find it so favourable. I am glad to have this opportunity of expressing to you my best thanks.

I had a letter from Mr. James Geikie a few days ago, telling me how delighted he was with the article.

I am glad your article has set Mr. Fisher to the study of that important and much overlooked subject, the temperature of space. I hope he will make something out of it.—I am, yours most truly,

JAMES CROLL.

WALDRON EDGE, DUPPAS HILL, CROYDON,
10th August 1879.

James Croll, Esq.

MY DEAR SIR,—Your kind letter was very welcome to me, especially as it also conveys to me Mr. James Geikie's approval of my article. Well knowing my want of practical acquaintance with the phenomena of glaciation, I feel that it is presumptuous in me to write judicially on so complex a subject. My excuse is that for many years the question has been one of the intensest interest to me, and I have taken every opportunity of studying the chief writings bearing upon it. I can only hope that I have fallen into no serious errors

or misstatements such as dabblers in subjects that don't belong to them are always liable to. I shall take it as a favour if you will point out without hesitation any statements, whether of fact or of theory, that you think require correction, as the chief part of the article will be embodied in a chapter of a work I am now preparing on the causes which have affected the dispersal of animals.

Perhaps you can ascertain from some of the Edinburgh meteorologists whether there are any connected observations of the amount of solar radiation on or near the equator, so as to show how far the difference of the sun's distance at perihelion and aphelion makes itself apparent. It appears to have *no effect* on the temperature of the atmosphere, but it certainly ought on the direct radiation, and it would be a most important guide to know what effect it really has. I am a great admirer of the late Mr. Belt's writings, and long held to his view as to the causes of glaciation, but on fuller consideration gave them up. I am still, however, greatly fascinated by his theory of the blocking up of the drainage of continents by ice to explain the wide spread of loess and gravel. The ordinary explanations seem utterly inadequate if the facts stated in Belt's papers are anywhere *near* true. I can hardly accept, however, the mid-Atlantic glacier from Greenland to the Bay of Biscay. Might it not be possible that the confluent glacier of Norway, Scotland, Ireland, and Wales blocked up the North Sea and English Channel? This, with *some* elevation of the land and some lowering of the ocean, might give almost as much "damming up" as seems needful in Europe.—Believe me, yours very faithfully,

ALFRED R. WALLACE.

EDINBURGH, *12th August* 1879.

Alfred R. Wallace, Esq.

MY DEAR SIR,—Your article gives such a clear and correct account of the theory in all its bearings, that

I am really unable to suggest any alterations. With the exception of, perhaps, what you say in reference to the Antarctic ice, I don't remember, when reading the article, of differing from you on a single point. But the Antarctic ice is a point of so little importance in relation to the theories, that it is hardly worth while making any allusion to it. I have in a recent paper gone more fully into the subject of the Antarctic ice. When I go home to-night, I shall send you a copy. I fully agree with you that we have no evidence of there ever having been a North Polar ice-cap. I agree with you also in attributing the loess to the damming back of the Rhine and Elbe, or to land ice filling the English Channel and North Sea. See *Geological Magazine* for June 1870, p. 277. The observations of Messrs. Peach and Horne on the glaciation of the Shetland and Orkney Islands, which I hope will shortly be published, prove beyond all doubt that these islands were glaciated by *land ice* from the North Sea coming from Scandinavia.

I shall call on Mr. Buchan and see if he can give me any information as to observations made at the equator. The point to which you refer, like that of the temperature of space, I fear, has been much overlooked by physicists.

I am delighted to hear that you intend bringing out a work shortly on that interesting subject, the dispersal of animals, and that the leading idea of your article is to be embodied in it.—I am, yours ever truly,

JAMES CROLL.

WALDRON EDGE, DUPPAS HILL, CROYDON,
11th October 1879.

James Croll, Esq.

MY DEAR SIR,—I know so little of meteorology that I should prefer not to write to *Nature* on the subject. If you or Mr. Buchan would do so, it would, I am sure, receive more attention.

I should think Quito would be the best place for such observations, as I fancy the atmosphere is pr

clear all the year round, and there must be some European residents who could make such observations if instructed.

But could not valuable results be obtained at some of our regular meteorological observatories, such as Bombay, by taking a series of black bulb observations in July and January at *equal altitudes* of the sun, and with *equal transparency* of the air. The latter factor might, I should think, be determined sufficiently by observing *the colour of the sky*, as uncondensed vapour is, I believe, quite diathermous to direct rays. Of course, to make the observations comparable, the temperature of the air should also be equal, but a correction for this might probably be arrived at.

Am I not right in supposing that observations made at *any latitude* in July and January, with the same altitude of the sun, the same transparency of atmosphere, and the same temperature of air, ought to give the results we require, and though the extreme differences of air temperature in our latitude in July and January might prevent any good results being obtained, that would not be the case within the tropics.

Neither at Bombay nor at Batavia, where there are regular meteorological observatories, do they seem to make a single observation of direct sun heat. Mr. Buchan would be the proper person to call attention to this, and to show the interesting physical problems which a series of such observations would help to solve.—Believe me, yours very faithfully,

ALFRED R. WALLACE.

WALDRON EDGE, DUPPAS HILL, CROYDON,
23rd November 1879.

James Croll, Esq.

MY DEAR MR. CROLL,—I have been expecting with much interest to see your promised communication to *Nature* on the cause of the absence of change in the temperature of the equator at aphelion and perihelion.

I sincerely hope ill health may not be the cause of your silence on the subject. This, however, is not what I am now writing about. In the last few pages of my article in the *Quarterly* I touch very briefly on the great difficulty of the *alternation of climate* theory, the *total* absence of all indications of severe cold in the Arctic or Temperate regions, from the Miocene backward throughout all geological time. It is a difficulty that must not be shirked; for if the theory be the true one, I believe that *some* indications of the *fauna* and flora of these constantly recurring cold periods *must exist*, and can be found if sought for, especially during the later Secondary or earlier Tertiary formations. Your connection with the Geological Survey will probably enable you to direct me where to look for such indications with the best chance of finding them,—I mean in what works; or you may know some geologist who has worked at some extensive group of deposits in such detail as to be able to give the required information directly. It seemed to me that what is to be expected is, that in any extensive series of conformable strata of, say Eocene or Cretaceous age, there *ought* to occur at tolerably regular intervals a series of beds somewhat distinct in lithological character, and in which the usual fossils of the formation are either absent or partially replaced by *dwarfed forms*. Such beds might sometimes be very thin and insignificant, owing to the scanty deposition during an epoch when the bed was *completely* glaciated.

If any one such case can be found, either in this country or in a more northern latitude, it would give an immense support to the theory. You probably know if there are *any* of the geologists of the Survey who hold the theory firmly and *have* looked out for such evidences; or, if not, you can no doubt refer me to some one who can give me information on the subject, or to some *work* which gives the details of a series of beds and their fossils where such evidence is likely to be found.

Do not put yourself to any inconvenience to reply

to this letter until it is quite agreeable for you to do so. —Believe me, yours very faithfully,

ALFRED R. WALLACE.

EDINBURGH, *25th November* 1879.

MY DEAR MR. WALLACE,—Many thanks for your letter. I shall send my letter to *Nature* in the course of a few days. I would have done so sooner, but the head has been troubling me a good deal of late, and besides, I was half expecting that some meteorologist would remove this difficulty, as it is more apparent than real. The conclusion Mr. Fisher draws from Pouillet is perfect nonsense, and Mr. Hill's deductions from Dulong and Petit I do not believe to be correct.

In reference to the evidence of cold periods *prior* to the Miocene age, I think we have pretty good *physical* evidence of the existence of glaciation back to at least the Cambrian, if not to the Laurentian period. A great many of the facts I have detailed, as you will remember, in Chapter xviii. of *Climate and Time*; and in Chapter xvii. I have given what appears to me to be good evidence why we have not more physical indications of former Glacial periods.

Regarding the palæontological evidence I know but little. I suspect that few palæontologists ever dreamed or expected to find glacial animals in palæontological times. Perhaps, if they had been on the outlook, they might have found such evidence. Physical geologists never found physical evidence until they began actually to look for it. And I have little doubt that when palæontologists begin to search for evidence, that they will find it.

But in regard to far back periods, could we tell with certainty the class of animals that would indicate glacial cold? Of course I do not know, but you no doubt can tell whether such should be the case or not. We have in the Palæozoic times well-marked zones where life is scarce if not absent; this, of course, is not positive proof of cold, but it is a strong presumption in favour of it.

Professor Geikie says that, in the 2nd edition of Dana's *Manual of Geology*, there is something about palæontological evidence of cold periods during the Palæozoic age.

I shall ask Dr. James Geikie if he knows of any.

By the way, there is a very interesting article by Peach and Horne, two of our men, in the present number of the *Quarterly Journal of the Geological Society*, on the glaciation of the Shetlands. It is one of the most remarkable memoirs which have yet been written on glacial ice. It shows, beyond all doubt, that the North Sea was filled with *land ice* from Scandinavia. Lately, they visited the Orkneys and gave them a thorough survey for drift, and found that these islands have likewise all been overridden by North Sea ice. And just now one of our men engaged on the Survey to the north of Aberdeen has found evidence that the North Sea ice passed up on the land at that place, bringing with it broken and glaciated shells from the bed of the sea.—I am, yours truly, JAMES CROLL.

EDINBURGH, *2nd December* 1879.

MY DEAR MR. WALLACE,—I sent your letter to my colleague, Dr. James Geikie, and asked if he would be so good as to favour me with his opinion on the subject. This he has done in the enclosed letter. I observe that, like me, he does not place much weight on the fact that palæontologists have not recognised evidence of the existence of ancient Glacial periods. I think the difficulties in the way of detecting far back cold periods have not been fully realised either by geologists or by palæontologists as a rule. So strongly was I impressed with this, when engaged on the subject in *Climate and Time*, that I devoted a chapter to the details of those difficulties. Just consider how difficult, for example, it will be for the geologist of the future to detect traces of our last Glacial epoch a million of years hence. By that time all the boulder clay will be denuded off the surface of the land and washed into the sea.

The glaciated surface of the rocks will be disintegrated, and perhaps not a scratch left. Our raised beaches with Arctic shell beds, from which so much information has been obtained regarding the Arctic condition of our climate, will all be removed, and the shells mixed up with those now lying in our seas in such a manner that it will be difficult to say whether the strata formed from our present sea-bottoms will indicate a warm or a cold condition of climate. Now what will occur in reference to our recent Glacial epoch, has *actually occurred* in reference to all the Glacial periods of the past.

You need not return Dr. Geikie's letter. I have sent on my letter to *Nature*. I hope it may appear next week.

By the way, an American geologist, Professor Orton, thinks that he has found evidence of coal beds of Interglacial periods in the Carboniferous strata of Ohio in the Hanking Rock district. See *Geological Survey of Ohio*, vol. iii. 1878.—Yours truly, JAMES CROLL.

The following letter to Dr. Morison shows Croll's great desire to see the Swiss mountains and glaciers:—

8 JORDAN BANK, EDINBURGH,
21st February 1879.

DEAR DR. MORISON,—I am delighted to have a letter from you again. Mrs. Croll and I have long been looking for a visit from you. The first time Mrs. Morison and you are in Edinburgh, be sure and arrange to come out to Morningside and spend an afternoon with us.

I am glad to hear that you had such a pleasant trip to the Continent. There is not a spot in the globe I have a greater desire to see than Switzerland and its mountains; but, somehow or other, I have never managed to get so far from home. When we meet, we shall have a talk over the matter. I hope we shall have the pleasure of reading an account of your journey.—With kind regards to Mrs. Morison, I am, yours most truly,

JAMES CROLL.

The following letters by Croll to his colleague on the Survey, Mr. Horne, show something of his daily life and general interest in science at this period:—

EDINBURGH, *27th June* 1879.

MY DEAR HORNE,—I am glad to hear that the Geological Society is to print your map along with the paper. James Geikie and Helland have returned from the Faroe Islands. They found that these islands were glaciated from a common centre of dispersion in all directions. The sea around the Faroe Islands is evidently too deep to have allowed the Scandinavian ice to have reached them. It would probably break up in bergs in the deep channel to east of these islands. The channel is nearly 4000 feet deep, but the glaciation of these islands from a centre of dispersion is a most important fact, for it proves that it was done by land ice, and not by icebergs, as Milne Home & Co. would have it. Now here will run the argument. If we have to admit that the Faroes were glaciated by land ice, we have no reason to believe that the Shetlands were glaciated by any other agent; but the land ice which glaciated them did not belong to the islands themselves, as you have proved, but came from the North Sea. They found hills 1600 feet in height glaciated across their summits. I have no doubt, when you go to the Orkney Islands, you will find a good many facts which will throw more light on the glaciation of the Moray Firth; but no amount of facts, however hostile, derived from the regions between Elgin and Peterhead or between Aberdeen and Peterhead, will be able to overturn or to shake in the least degree your proof that the Scandinavian ice filled the North Sea to beyond the Shetland Islands or that the ice of the Moray Firth passed obliquely over Caithness. What has been said about the direction of the ice at Peterhead is just as much opposed to the direction of the ice along the coast to the south of Aberdeen as it is to the direction of the Norwegian ice. I have an impression

that you will find the striæ on the Orkneys more south and north than I have drawn them on the chart in *C. and T.* What wet, cold, and disagreeable weather we have had!—Yours truly, JAMES CROLL.

Helland is away to the Shetlands for Scandinavian boulders.

EDINBURGH, *25th July* 1879.

MY DEAR HORNE,—I have got back to work again. Helland was here yesterday, and he tells me that he did not succeed in getting Scandinavian boulders in Shetland, although he is convinced the Scandinavian ice must have gone over the islands. By the way, there is an excellent article in the present number of the *Quarterly Review* on "Glacial Epoch and Warm Polar Climate." The object of the article is to show that the eccentricity theory explains all the facts, and that all the other theories have failed. I have no idea who the author is. I hope you will get good weather for your holiday. How are you getting on with the abstract of your paper?—Yours truly, JAMES CROLL.

EDINBURGH, *30th August* 1879.

MY DEAR HORNE,—I have sent on the maps by post. Also your copy of the *Quarterly Journal of the Geological Society*, which Bennie and I took the liberty of opening the cover of, to see its contents, as no copy now comes to the office. Peach has just been in and told me of your great success in Orkney. Your Orkney paper will be a most important one; by all means try and get it ready by the opening of the session. Weather here still very unsettled.—Yours truly, JAMES CROLL.

EDINBURGH, *26th September* 1879.

MY DEAR HORNE,—I received your note. I am obliged for the information about the direction of the ice in Buchan. The only point now to be determined is

whether the Buchan ice, on reaching the sea, veered round to the north-west, or continued its motion southward along the east coast. If it took the latter path, it is difficult to conceive what should cause the Strathmore ice on approaching the coast to turn round almost at right angles and pass northward to Aberdeen. It would be of the utmost importance to find out what direction the Dee ice took, on reaching Aberdeen; for if it bent northward, like the Tay ice, it will prove that the Buchan ice could not have gone southward. Could you not take a run through to Aberdeen and see if you could get some light on the subject? Perhaps Jamieson of Ellon might be able to give you some information on the matter. You should write to him.—Yours truly,

JAMES CROLL.

Glad to hear that your paper is in the press. Try and get your Orkney one to follow up as closely after the other as possible. It is best to strike the iron while it is hot.

EDINBURGH, *22nd October* 1879.

MY DEAR HORNE,—Wilson sends me the following postcard:—"Ellon ground between this and sea covered with Old Red Sandstone drift. Must have come from south of Stonehaven, Jamieson thinks it is a post-Glacial marine deposit." I don't think it is at all likely that Jamieson's idea is correct. We know that the Stonehaven ice reached to about Aberdeen. This find of Wilson would seem to show that it must have continued its course onwards to Ellon at least. You should look after this important find of Wilson's.—Yours truly,

JAMES CROLL.

EDINBURGH, *1st November* 1879.

MY DEAR HORNE,—A parcel containing books from Douglas & Foulis, along with papers from Schenck & M'Farlane, was sent off by rail on Thursday.

Peach took out a number of copies of the paper, and stated that he would write you and let you know to whom they were sent, to prevent two copies being sent to one individual. I have sent on a parcel of wrapping paper, envelopes, etc. You simply say thin wrapping paper; as I am not sure what you mean, I have sent two kinds. In reference to Wilson's find, the fact that the red clay is perfectly unstratified and the same as ordinary boulder clay above 150 feet up to 400 feet, the height to which it reached, is strongly against the idea of its being a raised beach deposit. But, as you remark, it would be as well to make sure before saying much about it. It does not, of course, necessarily follow that the Red Sandstone came from the *land* to the south of Stonehaven. It may have come from the bed of the North Sea to the east of Aberdeen, for the Old Red Sandstone passes in a north-easterly direction from Stonehaven under the North Sea in the way indicated in the enclosed chart. But it is also quite possible that the Strathmore ice, which moved northwards along the coast to Aberdeen, may have continued its path on the land till it reached the Moray ice, as Wilson supposed. At anyrate, if the red clay is genuine boulder clay, it proves beyond all doubt that the Scandinavian ice must have been pressing hard against our coast, and that it must have been moving northward, not southward. Hume will write you to-day about the cabinet. Rocks arrived all right. List of paper enclosed.—Yours truly,

JAMES CROLL.

P.S.—Since the above was written, a second parcel has come from D. & F. I send along with wrapping paper by rail.

EDINBURGH, *18th November* 1879.

MY DEAR HORNE,—Yours just to hand. I have sent off the diagrams you want by rail. Peach's copy of *Quarterly Journal* has come. I cut it up

to look at your paper. His father was in the office at the time and took it away. It will make a mark.—Yours truly, JAMES CROLL.

EDINBURGH, *25th November* 1879.

MY DEAR HORNE,—I have sent off to Irvine, 163 Crown Street, Aberdeen, working copies of sheets 56, 57, and 66. As these sheets lie in the debateable ground, you should write to Irvine and explain to him Wilson's find, and tell him to keep a sharp lookout for Old Red Sandstone from the south. I would have written myself, but the head aches badly at present.—Yours truly,

JAMES CROLL.

In answer to an inquiry from his old friend, Rev. O. Fisher, Croll writes—

EDINBURGH, *4th August* 1879.

Rev. O. Fisher, M.A.

MY DEAR SIR,—The most elaborate article on the temperature of space is by M. Pouillet, 1838, a translation of which is to be found in Taylor's *Scientific Memoirs*, vol. iv. p. 44, 1846. I read it along with Herschel's many years ago, but I now forget all about it. I am glad you are going into the subject. It is a most important one, and one which has not received that attention from physicists which it deserves.

It is perhaps quite possible, as you state, that observations made at the equator might show something which would lead to results. I perceive your objection to Herschel's mode quite clearly, but I would require to grind up a little on the subject of absorption and radiation before I could venture an opinion. Speaking off hand, I would say with you that the temperature of the air at the confines of the atmosphere need not be exactly the *mean* below the temperature of the *earth's* surface and that of *space*. I have left physics altogether

in the meantime, and commence to the study of my old subject Theism, or rather Evolution in its philosophical aspects. Evolution in relation to Teleology is the subject I was working at twenty-five years ago. I want to finish something which has been lying over ever since. But I am so far behind in the literature of the subject that it will take a few years' reading before I learn what has been actually done since then.—Yours truly,

JAMES CROLL.

EDINBURGH, 21*st October* 1879.

Rev. Osmond Fisher, M.A.

MY DEAR SIR,—I have just read your letter in *Nature*, and I observe that your difficulty is a different one from what I thought it was. You ask for an explanation why the January temperature at the equator, when the sun is in perihelion, is not much higher than in July when in aphelion. But the temperature to which you refer is the ordinary temperature, as indicated by the shaded thermometer, which of course is simply the temperature of the air. I don't think it would be very difficult to show why the *air* in equatorial regions cannot be much hotter in January than in July. When my head gets better, I shall draw up a short paper on this point.

If it be true that the black bulb thermometer, which indicates, not the temperature of the air, but the direct heat of the sun, stands no higher at the equator in January than in July, then there will certainly be a difficulty in understanding how this can be the case, if the temperature of space is as low as $-239°$ F. But we have no certain evidence yet, so far as I can learn, that such is the case, and it was simply with the view of ascertaining if any of the readers of *Nature* could afford information if black bulb observations reliable have been made at any suitable station, that Mr. Buchan and I thought of writing to *Nature*.

I do not think that observations on the temperature

of the air can be of any value whatever in determining the temperature of space.

I have sent a few lines to *Nature*, pointing out the necessity for black bulb observations at the equator. I think it would be a pity to get into a discussion about the temperature of the air, which could be of no real service in the problems of the influence of the varying distance of the sun on temperature.—I am, ever yours,

JAMES CROLL.

EDINBURGH, 15*th December* 1879.

Rev. Osmond Fisher, M.A.

MY DEAR SIR,—Many, many thanks for your note. I daresay I may have stated in too strong terms that I thought you had not properly taken into account the modifying causes to which I referred. In reference to Dulong and Petit's formula, it is a purely empirical one, which has been found to agree pretty closely within the limits of actual observations; but it will not do to assume that it will equally do so in the case of temperature as low as that of space. For reasons which I have given in *Climate and Time*, and for others which I shall give in my next letter to *Nature*, the truth probably lies somewhere between that of Newton and Dulong and Petit.

In regard to the conclusion arrived at by Pouillet, to which you refer, there cannot be the shadow of a doubt that it is totally wrong. Pouillet's paper was written, you will remember, in 1838. At that time he was not possessed of the means of knowing how far he must have gone wrong. I do not now place the least reliance on Pouillet's determinations. Herschel's method appears to be more satisfactory, but still I feel assured, for reasons which I have also stated in *Climate and Time*, he assigns far too high a temperature to space. I have consulted one of the most eminent mathematical physicists of the day, and his opinion is that stellar space *cannot be much above absolute zero.*

I intend sending a letter to *Nature* on the unsatisfactory state of our knowledge regarding the temperature of space, but it may be some little time yet, as I am suffering a good deal from my old complaint.—With kind regards, yours ever truly,

JAMES CROLL.

During the year 1880, Dr. Croll suffered greatly from ill health; but he managed, in addition to his Survey duties, to write a paper entitled "Mr. Hill on the Cause of the Glacial Epoch," which appeared in the *Geological Magazine* of February 1880 (No. 79). This was followed by a paper on "The Temperature of Space and its Bearings on Terrestrial Physics," which appeared in *Nature* on April 1, 1880 (No. 80), and another on "Aqueous Vapour in Relation to Perpetual Snow," which appeared in *Nature* on July 1, 1880, in the *Geological Magazine* of August 1880, and in the *American Journal of Science and Arts*, August 1880 (No. 81).

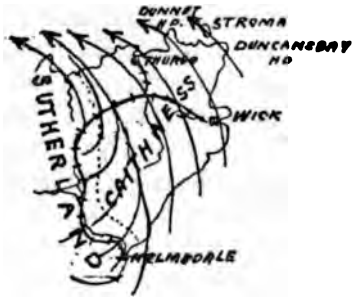
Knowing the interest which Dr. Croll took in the direction of the ice movement, as affecting the north of Scotland, his colleague on the Survey, Mr. Horne, wrote the following letter to him. Croll's reply, unfortunately, has not been preserved.

BANFF, *3rd January* 1880.

MY DEAR CROLL,—By this mail you will receive copies of the papers on Orkney by Peach and myself. You will be delighted that the evidence regarding the north-westerly movement of the ice is so convincing. I am specially pleased with the abundance of the Secondary rocks from Scotland in the boulder clay in Orkney. It establishes the conclusion that Orkney was glaciated by Scotch ice. Perhaps you may have heard that Milne Home attacked our Shetland paper in his presidential address to the Edinburgh

Geological Society last May. We thought it right to expose his error regarding the magnetic observations, and to answer other points. He sticks like a burr to Arctic currents. The reply is sent to the *Geological Magazine*, and should appear in this month's number or next month's.

Our holiday work in Caithness this summer was highly satisfactory. We made out beyond all doubt that the low grounds of Caithness were glaciated towards the N.W.



The foregoing rough sketch will show how the striæ in the valley and on the hill slopes between the county boundary and the Caithness plain point E. and N.E. Towards the edge of the plain they swing round to the N. and N.W. We got abundant evidence of that point. Then in numerous instances we proved, by means of the dispersal of the stones in the boulder clay, that the ice-flow must have been to the N.W. Further we got fair proofs of the extent of the later glaciation. I am busy at a joint paper on the subject, to be read at the Physical Society, Edinburgh, in February. The London men have had enough of that subject, I doubt, to publish anything more about it.

I am very sorry to hear you have been so ill, but I hope you will long be spared to work away quietly yet. I wish you very heartily all the compliments of the season. I have not heard how you have been keeping

for some time. If you can get an amanuensis, I should like very much to hear how you are.—With kind regards, sincerely yours, JOHN HORNE.

The following letters from Dr. Croll to Mr. Horne are of considerable interest :—

EDINBURGH, *13th January* 1880.

MY DEAR HORNE,—I have sent off a short reply to Mr. Hill. I have put some queries which will put him to some little difficulty to answer, according to his notions about the influence of fogs. It will appear in next number. Hill is wrong out and out. He appears to have little acquaintance with the physics of the subject.—Yours truly, JAMES CROLL.

EDINBURGH, *13th February* 1880.

MY DEAR HORNE,—I have sent off the two vols. you want. I was asking Peach this morning what length your paper is, but he does not seem to know. You may rest assured the sooner it is out the better, if you want to make a *mark*. Every day's delay will diminish the impression that the paper will make. It is not minute details that make the mark, but striking and unexpected facts or theories, and the man out first generally gets the credit. What fine weather we are getting!—Yours truly, JAMES CROLL.

EDINBURGH, *13th April* 1880.

MY DEAR HORNE,—You will no doubt have observed from the *Geological Magazine* and the *Geological Society Reports* how much your paper is really wanted. The conservative English geologists, S. Wood, O. Fisher, etc., seem to be in a state of alarm about the North Sea ice invading their land.—Yours truly,

JAMES CROLL.

EDINBURGH, *17th April* 1880.

MY DEAR HORNE,—I have read your letter to Peach. He is of the same opinion as myself, that it would not be advisable to postpone the paper. The facts to which you refer regarding Caithness and the Banff district are quite enough to form a separate paper to come after the Orkney one. It does not do to cram too many new and startling facts into one paper. What you have got to say on the Orkneys is quite enough for one paper. The great idea of your Orkney paper is, that the Scotch ice entering the North Sea passed over the Orkneys; and the facts you have to relate are more than sufficient to convince any human being capable of being convinced that such was the case. Let your readers get time to ponder over your Orkney facts, and then, in a future paper, bring out those additional facts collected during your new excursion. They will tell better after people have mastered your arguments from the Orkney facts. To enter into a discussion about Jamieson's ideas in your main paper would be apt to divert attention from the real drift of your argument. And, in addition to all this, it would necessarily curtail what you had to state regarding the Orkneys, as the Society limits you so much to space. I have sent off the parcel with the books which you require. By the way, you could append a note to your paper to the effect that a great mass of striking facts relating to the drift of the east coast of Scotland recently observed, go to corroborate the conclusion at which we have arrived. This will form the subject of a future communication to the Society; this would prepare your readers for what was to follow.—
Yours truly, JAMES CROLL.

Dr. Croll having sent his recent papers to Professor M'Farland, of America, the following correspondence followed:—

OHIO STATE UNIVERSITY, COLUMBUS, O.,
20th March 1880.

Mr. James Croll, Edinburgh.

MY DEAR SIR,—I have to acknowledge the receipt of other favours at your hands, for which favours please accept thanks.

It seems to me that some who are opposed to certain positions assumed or proved by you, touch off their guns before they are well loaded, and, in consequence, come to grief.

My computations are somewhat delayed in the matter of the eccentricity of the earth's orbit. Last summer I expected to devote ten weeks to the work; but from many causes, I did not work an hour. When our College year opens, in September of each year, my duties consume almost all the time. So when Christmas came, I was not ready, and I am not now, but could be ready for the engraver if I had ten or twelve days to devote to the work. I have used the new constants for 1850, as given by Stockwell, and computed by his formula for a period of 4,510,000 years, beginning before 1850; $3\frac{1}{4}$ million years, and running to 1,260,000 years hence; intervals 10,000 years, except at interesting points when the computations are at short intervals. By the formula which you used, I went over $1\frac{1}{4}$ million years at intervals of 25,000 years, but afterwards began the work over at intervals of 10,000 years, to extend over the $4\frac{1}{2}$ million years. On this part, the work is well advanced. Each computation is independent of the rest. The two curves follow the same general form, but give the maximum points at different places. In one or two cases Stockwell's form gives greater ordinates than the other. With no further detention, I may be ready by the middle of May; and it will probably be July before I can get the results in the *American Journal of Science.*

The Ohio coalfields seem to demand, by reason of the regularity of the intervals between seams, some great swinging motion, so to speak, which your theory

apparently fully and satisfactorily explains. Our President, Mr. Orton, fully accords with you in your explanation of the "times and the seasons."—Very respectfully, R. W. M'FARLAND.

EDINBURGH, *14th April* 1880.

Professor M'Farland.

MY DEAR SIR,—Your letter of the 20th ult. I duly received, and am delighted to learn that you have got so far advanced in your arduous undertaking. What an enormous amount of work you must have had! Your tables will be of great interest to astronomers and physicists, and will be of permanent value to science.

I shall write and get the editor of the *Philosophical Magazine* to reprint them in the *Journal* for the benefit of English readers, as soon as they appear in the *American Journal of Science.*

I am glad to know that President Orton has so favourable an opinion of the theory of geological climate discussed in my book. Some time ago he kindly sent me a report of some of the facts to which you refer.—Yours ever truly, JAMES CROLL.

EDINBURGH, *8th September* 1880.

Professor M'Farland.

MY DEAR SIR,—Many thanks for sending me a copy of your valuable table and chart. The labour it cost you must have been enormous. Those who glance over your table as they turn over the pages of the *Journal*, will form but a very inadequate idea of the amount of work embodied in these half-dozen of pages.

I am delighted to find that my results agree, in the main, so closely with yours.

I have written to the editor of the *Philosophical Magazine*, directing his attention to the importance of your table, and advising him to reprint it.—With kindest regards, I am, yours ever truly, JAMES CROLL.

Dr. Croll, approving highly of Professor Haughton's calculations and speculations, was anxious to have these published in America, and wrote the Professor on the subject. Dr. Croll's letter has not been preserved, but the following is the Professor's reply:—

TRINITY COLLEGE, DUBLIN,
9th June 1880.

James Croll, Esq.

MY DEAR SIR,—I feel much obliged by your letter of 2nd inst. and shall send copies of my paper to *Nature* and the *Philosophical Magazine*. I also send you three copies for any use you please. I undertook the experiments in order to find a co-efficient for estimating the tidal retardation in the earth's rotation, and hope to publish my results in the *Proceedings of the Royal Society*, but, *en passant*, it was easy to draw an inference about Carpenter's theory of oceanic circulation. If you write to Professor Dana on the subject, you might add the following, which came to my knowledge through an engineering friend who had seen my paper.

Mr. J. H. Longridge made observations in Hungary on "The Flow of Water through Level Canals" (*Proc. Inst. Eng.* vol. liii. 1877–8, Part iii.), where he had eighty miles of level water, with a difference of 2 feet level at the two ends. His co-efficient of

$$\text{"Drag"} = \frac{\text{friction } \textit{plus} \text{ viscosity}}{\textit{plus} \text{ friction of bottom and sides of canal}}$$

gives $f = \frac{1}{131{\cdot}6}$ in place of my co-efficient $f = \frac{1}{307}$

Mr. Longridge's co-efficient is about double mine, which is natural, as he had to deal with the "drag" of the water against the bottom and sides of the canal, as well as with the "drag" proper of water on water.—I am, yours sincerely,

SAMUEL HAUGHTON.

Croll thereupon transmitted the paper to Mr. Dana, the American publisher, who wrote as follows:—

NEWHAVEN, *3rd August* 1880.

DEAR MR. CROLL,—We have set up Professor Haughton's article which you sent us the copy of, exclusive of the most of the tables, and were intending to insert it in our September number. But, owing to a recent decision in England against the agent of an American journal, a trade journal of some kind, because the said American journal contained an article from a British journal of similar kind, one of our agents refuses to act for us unless we exclude articles previously published in England. From Trübner & Co., our chief agent in Great Britain, we have not heard. Under the circumstances we are forced to delay, at least, the publication of Professor Haughton's paper. We hope to hear that the English law is not so stringent as is here supposed.

We should be greatly indebted to you if you would learn from good authority whether we are no longer to have the privilege of citing from the Proceedings or the publications of British scientific societies, or from *Nature*, the *Philosophical Magazine*, or the *Geological Magazine*. Mr. Lockyer has sent us papers for republication. But, as the case now appears, our agents are liable to prosecution if citations of articles are ever made. We do not suppose that any such prosecution would take place, but we cannot satisfy agents except we have good authority to quote.

We have not often cited articles, being seldom able to do it because of the pressure from American contributors. Please give us your aid in the matter and you will greatly oblige,—Yours very truly,

JAMES G. DANA.

18th August 1880.

DEAR PROFESSOR DANA,—Your letter of the 3rd inst. is to hand, and I am astonished at the decision to which you refer. I had no idea that the English law in regard to the reproduction of articles in American journals was so stringent and absurd. I am at present confined to the house from indisposition; but, so soon as I am able to go out, I shall make particular inquiries in regard to the matter, and then let you know. Most English authors would be very sorry indeed to learn that their articles in future must be excluded from American journals.

I am sorry that you should have been put to so much trouble in this matter, but I hope to find on inquiry that the law does not apply to such purely scientific contributions as generally appear in your pages. Please to accept my very best thanks for being so kind as to give my short paper a place in your August number. I have just written to Professor Haughton and explained to him the circumstances.—I am, dear sir, yours very truly, JAMES CROLL.

Dr. Croll having written Professor Haughton referring to Dr. Carpenter's article in *Nature*, received the following reply:—

TRINITY COLLEGE, DUBLIN,
7th August 1880.

James Croll, Esq.

MY DEAR MR. CROLL,—I received your note, for which I am much obliged. I do not intend to answer Dr. Carpenter's statement in *Nature*, as he appears to me not to understand the fundamental conceptions of hydrostatical *pressure*, on which the motion must depend. He makes the same mistake about the ocean that Halley made about the air in his theory of Trade Winds. I hope, next winter, to send to the Royal Society a sketch of a mathematical theory of ocean currents caused by Trade and Anti-trade Winds.—I am, yours truly,

SAMUEL HAUGHTON.

Dr. Croll having written to Mr. Wallace regarding the Glacial epochs, the following correspondence between them ensued :—

PEN-Y-BRYN, ST. PETER'S ROAD, CROYDON,
19th July 1880.

James Croll, Esq.

DEAR MR. CROLL,—Many thanks for your interesting letter. The facts you mention are most curious and startling, and I shall look with great interest to Professor Geikie's detailed account of them. My chapter on "Glacial Epochs and Warm Polar Climates" has passed through the press some months back, and I am now near the end of my book. I can hardly venture to hope that my conclusions will satisfy *you*, but I may say here that they have been reached mainly by giving greater weight to your admirable views on ocean currents as affecting climate, than you seem to do yourself. I have endeavoured to show that a comparatively slight increase of the warm tropical waters carried to the Arctic regions, combined with somewhat less high land in those regions, would render glacial conditions impossible there, and this being done, there would necessarily result the state of climate that the Miocene Arctic vegetation showed did exist. I have further attempted to prove that the geological evidence shows that there *was*, during the greater part of the Secondary and Tertiary periods, such terrestrial conditions as *would* cause this great transference of equatorial heat northwards, and I have further argued, I hope soundly, that, such a condition of things existing, neither high eccentricity nor the changing phases of aphelion and perihelion would alter the general character of the climate, though it might affect the nature of the seasons. I also show that the changing phases of perihelion, etc., would only reverse the Polar glaciation when that glaciation was of small extent. When it was extensive and excessive, the change to winter in perihelion would not do away

with the Glacial epoch, but only cause a slight fluctuation of its southern limits.

This will enable you to form some idea of the nature of my argument, which I am sure you will weigh carefully when I have the pleasure of sending you a copy of the book, as I hope to do in a few months.—Believe me, yours very faithfully,

ALFRED R. WALLACE.

EDINBURGH, *24th July* 1880.

Alfred R. Wallace, Esq.

DEAR MR. WALLACE,—Many, many thanks for your interesting letter, giving me a brief outline of the general drift of your views on the cause of geological climate. I shall be looking with interest to the appearance of your book. Of course I cannot make out from your brief statement what those causes really are on which you rely: only I do trust you don't put much stress on the effects of changes in the physical geography of the globe. There is one serious objection to all theories of geological climate based on great changes in physical geography. These changes must be assumed to take place with irregularity, and also with extreme slowness, and it will be difficult to get them to harmonise with warm Interglacial periods. The amount of evidence uncollected, which lies scattered over the various journals and papers of America and the Continent, is perfectly astonishing. This will be brought out very strongly by Dr. James Geikie in his forthcoming work on *Prehistoric Man in Europe*. It is, I think, now beyond question that the Glacial epoch consisted of a succession of cold and warm periods, which must be accounted for in any theory of geological climate. In fact, these periods are the general test of theories of climate. It seems to me difficult to conceive how it is possible that they could have depended on changes in physical geography. They have all along convinced me that the only possible way of explaining them is by the hypothesis that these alternations were

due to the precession of the equinoxes at the time of high eccentricity. After fifteen years' study of the subject, I have been unable to find any serious objection to this theory. Nearly all the objections which I have seen have been based upon misapprehension of the subject.

There is another point on which I think considerable misapprehension prevails, that is, in supposing that the great accumulation of snow and ice on Arctic and Antarctic regions results from a high elevation of the land. True, on elevated mountains all over the globe we have perpetual snow; but this is not owing simply to elevation, but to certain physical conditions which come into operation at great altitudes, but these conditions are obtained on *Polar continents* without any elevation. This I have endeavoured to show in a letter in *Nature*, July 1. We have almost positive evidence that the Antarctic continent is a vast, flat plain, very little elevated above sea-level. It contains, no doubt, some elevated mountains, but these, of course, cannot determine the general character of the climate. Greenland does not appear to contain in the interior great mountain chains. Its mountains are simply a fringe along its western edge. True, the surface of the inland ice is found everywhere to slope gradually upwards towards the interior, but there is no evidence to conclude that the ground under the ice does so. The thickening of the ice, as we proceed inwards, is a necessary condition of an ice-sheet. Without this there could be no general dispersion of the ice. I hope you will excuse me for going into these details. I thought I might draw your attention to them, so that you might give them your consideration when you are treating this vexed subject, but it is very unlikely you have fallen into any of these traps.—I am, yours ever truly,

JAMES CROLL.

PEN-Y-BRYN, ST. PETER'S ROAD, CROYDON,
2nd December 1880.

James Croll, Esq.

MY DEAR SIR,—I am very sorry to hear of your continued ill health, so far as regards mental work. I trust, however, that you may be spared the affliction of paralysis, and may soon quite recover. Personally I should much regret losing the benefit of your criticism of my theory of geological climate, which is really founded almost wholly on your own researches, and which so very few people seem able clearly to comprehend.—With best wishes, believe me, yours faithfully,

ALFRED R. WALLACE.

CHAPTER XX

RESIGNATION OF APPOINTMENT ON GEOLOGICAL SURVEY

THROUGHOUT the whole of the year 1880, Dr. Croll suffered sadly from impaired health. His old complaint in the head became more serious, and the pain was so constant and severe that he was able to get but very little sleep. To make matters worse, he met with an unfortunate accident one day during the summer. Standing in an awkward position on a pair of steps in the office, he was trying hurriedly to remove some maps from a drawer, when suddenly he strained something in the region of the heart. The result was that, for several months, he was unable to make any physical effort or even to walk about with any degree of comfort. As Professor Sanders, whom he had formerly consulted with beneficial results, was by this time dead, he was now under the medical care of Professor Grainger Stewart. This gentleman thought it advisable to try whether the external application of aconite might have any effect in the way of relieving the pain in the head, and ordered a little to be applied to the afflicted part at times when the pain was severe. Dr. Croll carefully carried out the injunctions of the physician for some time, but the desired effect did not appear. On the contrary, the repeated application of the powerful drug seemed to aggravate the ailment. One evening, after applying it once or twice, Dr. Croll suddenly found that he had lost the power of speech, to such an extent that he could speak only in an unintelligible manner, like a

man paralysed. It seemed that the poison had seriously affected some of the nerves or muscles of the tongue or lips, and, although the power of intelligible speech returned in the course of a week or two, the injurious effects of the aconite did not wholly disappear for several years.

In the beginning of 1881, Dr. Croll felt very weak owing to want of sleep, pain in the head, and weak and irregular action of the heart, and he began to think that, although his duties in the office of the Geological Survey were comparatively light, he had now become physically unable to discharge them sufficiently. His colleagues, who had learned to regard him with the warm affection of friends, were ready to do everything in their power to lighten his toil; and they urged him to apply for a prolonged leave of absence, that he might see what the effect of thorough rest would be. But he was too tenderly conscientious to think of taking pay for months of ease, during which he would be unable to do any work, and he declined the friendly suggestion, and made up his mind to retire from his position on the Survey. Accordingly he sent in his resignation, and in the spring of 1881 he left the Government Service, in which he had spent thirteen of the best years of his life.

Professor Haughton, knowing the deep interest Dr. Croll took in the subject of the eccentricity of the earth's orbit, wrote him the following letter:—

TRINITY COLLEGE, DUBLIN,
21st February 1881.

James Croll, Esq.

MY DEAR MR. CROLL,—I was very much concerned to learn, from your letter of 2nd December 1880, that you are threatened with serious illness, but trust that complete rest from work may enable you to resume good work hereafter. I am to lecture in Glasgow on Thursday evening next, and intend going on to Edinburgh, in the hope of seeing you and some other friends.

During the last few months I have succeeded in stating in strict mathematical form the long irregularity in terrestrial climates depending on the perihelion longitude and eccentricity of the earth's orbit. I have sent it to the Royal Society. I will send you an early copy, as you have made the subject almost your own.—Yours very sincerely,

SAMUEL HAUGHTON.

The following letters from Dr. Croll to Mr. Horne record his doings about this period :—

EDINBURGH, *22nd April* 1881.

MY DEAR HORNE,—Being in the office, I take advantage of Robert's pen to answer your kind note. Mrs. Croll read the reply to Milne-Home you kindly sent. It is first-rate. I am glad to notice that you intend getting your Caithness paper put into the *Geological Magazine*. I had a visit from Jamieson of Ellon on Thursday. He is very much pleased with your Orkney and Shetland work, and I understand that he agrees with you on the various points. I have as yet had no information regarding the pension, but I hope it will be all right. I intend storing up my furniture in Edinburgh for a year, and will probably go north to Elgin about the end of May, and will stay with my sister-in-law Mrs. Findlay, 45 South College Street. Mrs. C. and I will be delighted to have a visit from you when we go north. I hope Mrs. Horne and your young daughter are keeping well.—With kindest regards, I am, yours,

JAMES CROLL.

45 SOUTH COLLEGE STREET, ELGIN,
21st July 1881.

MY DEAR HORNE,—I duly received your letter, but am sorry to find that we will have left Elgin before you reach it. The Director would no doubt tell you

that we have made up our minds to go to the south of England, and we leave here by the mail train on the 29th. We intend staying at Kingussie for two weeks on our way south. We would have remained longer here, but the place is not agreeing with me. I have been getting worse instead of better since I came to Elgin. I think the house is damp. Mr. Bennie read Mr. Milne-Home's paper to me, and your reply. I think your reply is first-rate. I am glad to hear that it will appear in the August number. I hope that Mrs. Horne, yourself, and little daughter are enjoying good health. Mrs. C. joins me in kind regards. — Ever yours,

JAMES CROLL.

You mention having sent the *Geological Magazine* to me. It has not come to hand. If not sent off, you need not, of course, send it.

BRIDGEND, PERTH, *18th August* 1881.

MY DEAR HORNE, — Would you kindly let me have a look at your *Geological Magazine* for August containing your paper. We will be here for a week or ten days.—Yours truly,

JAMES CROLL.

CUMBERNAULD, *30th August* 1881.

MY DEAR HORNE,—Allow me to congratulate you on your promotion. I am very glad indeed to hear of your appointment, and I wish you continued health and much success. Thanks for the copy of your paper. I met Mr. Peach at Perth, in the house of Mr. Marshall, Mrs. Peach's brother-in-law; and as he wished to get a reading of your article, I left your copy of the *Geological Magazine* with him, which he promised to

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,

but I do not like to ask the office copy. We came to Devonshire at the beginning of the month, and we are enjoying an almost Italian winter climate in this pleasant place. The roses and geraniums are in full bloom at present in the open air. I was delighted to hear of the Director's promotion.—Yours very truly,

JAMES CROLL.

On relinquishing his position in the Survey, Dr. Croll went north to Elgin, the native place of Mrs. Croll, in the hope that change of air and complete rest would benefit him. His health, however, did not improve, and he got rather worse than better there. Probably the air of this northern city was too keen for him. He thereupon moved to Perth for a short time, but did not find much benefit from this change either. As the winter was now approaching, Mrs. Croll and his friends grew anxious as to what the effects of a severe Scottish winter might be upon him in his enfeebled state of health, and it was resolved to remove to a warmer climate. Mrs. Croll and he accordingly removed in the beginning of December to a pretty little village called Dawlish, in Devonshire. This place he seemed to enjoy immensely, as, owing to the geniality of the climate, he could get out daily, and wander about in the open air. In the month of December the climate was so mild that, as he says himself, the roses and geraniums were in full bloom in the open air. Here he rapidly regained strength, and, the physical energy returning, the restless brain yearned for work. Dr. Croll had already returned to his favourite metaphysical studies, and written a paper on "Evolution in Relation to Force." This was sent to the *Contemporary Review*, but the magazine had such a plethora of lighter and more popular literature in hand, that it kept it for four months, and Croll, getting dissatisfied, asked for its return. He writes to his old friend Mr. Fisher on this subject.

15 STRAND, DAWLISH, DEVON,
4th February 1882.

DEAR MR. FISHER,—The *Contemporary*, after keeping my article on "Evolution in Relation to Force" for four months, has returned it, stating that, owing to the pressure on their space from political matters, they could not find room for my paper, although it had been carefully read and approved of. A new editor has just got into power, and I suspect it is he that has overturned the matter. The article has engaged my attention now and again for the past thirty years, and is by far the best thing I have ever written. I am therefore anxious that it should appear in some *good* journal. Can you tell me who are the principal men connected with the *Nineteenth Century*, or any other journal you might think suitable? The length of the article is against it. It will occupy at least twenty-five pages.—Yours ever truly,

JAMES CROLL.

Dr. Croll now began to yearn to return to Scotland, as the mild spring was approaching. Accordingly they resolved that so soon as the weather should become mild enough to justify their leaving Dawlish they would do so.

On 22nd March 1882, Dr. and Mrs. Croll, on the invitation of his old friend, the Rev. Osmond Fisher, paid a visit to him on their way home to Scotland, and spent a couple of days at the hospitable house of Harlton Rectory, Cambridge. The visit was a source of much mutual enjoyment to both those old scientific friends, regarding which Mr. Fisher writes: "My memory does not serve me to remember any remarkable incident of Dr. and Mrs. Croll's visit to me. I was much struck with the simple modesty of one of so grand attainments." This was a feature in Croll's character which struck every thinking person who came in contact with him and knew anything of his work. Though a giant in intellect, and an authority on all the subjects he studied, he had the

simplest, most modest, and unaffected manner, and would talk pleasantly to a little child, to the most illiterate or the most intellectual man with equal apparent interest, earnestness, and serenity. During an intimate acquaintance of over forty years, the writer never once saw his temper even ruffled.

Leaving Harlton, Dr. and Mrs. Croll journeyed home to Scotland, and returned to Perth. From there he writes his friend Mr. Horne, of the Survey, the following letter:—

PERTH, *18th October* 1882.

MY DEAR HORNE,—I return *Athenæum* with many thanks. Look at *Literary Gossip* for 23rd September. I have no idea of the price of the American edition of *C. and T.* I should think it would not be more than the half of the English. Thin, I have no doubt, would soon find you a second-hand copy. I asked the publishers what they would sell me a copy for, and the reply was seventeen shillings. This, with carriage, would come to about a pound. I then applied to Thin, and he gave me a cheap second-hand copy. This was about two years ago. When you get the volume on the Orkney and Shetland, perhaps you will let me have a look at it for a day. Glad to hear of our old friend Mr. Lawrence. I have not seen him for a generation. He is an agreeable and intelligent fellow.—Yours truly, JAMES CROLL.

Dr. Croll, being at leisure, wrote to Professor M'Farland as follows:—

GEOLOGICAL SURVEY OFFICE, EDINBURGH,
3rd April 1882.

Professor M'Farland.

MY DEAR SIR,—You will be sorry to learn that in consequence of pain in the head, I found it necessary to retire from the Geological Survey about a year and half ago. I am, however, happy to say that the long mental rest I have enjoyed has almost restored me to my former condition.

I hope you are still pursuing your scientific work, and in the enjoyment of good health. I felt annoyed and disappointed at the editor of the *Philosophical Magazine* for not reprinting your important tables of eccentricity. I think it was foolish in him, for they would have been of service to English astronomers, as they are by far the most complete ever computed, or probably ever will be attempted. I dare say the editor would have but little conception of the amount of labour embodied in your figures.

I need hardly say that I shall be glad to have a line or two from you when you find leisure.—Yours ever truly, JAMES CROLL.

OHIO STATE UNIVERSITY, COLUMBUS, O.,
30th April 1882.

Dr. James Croll, Edinburgh.

MY DEAR SIR,—Your letter of the 3rd inst. was received several days since, and before answering, I wished to see Dr. Orton, our Professor of Geology, who is also at present the State Geologist. I knew he would be glad to hear of your improved health, and to send greetings, as he does. Although our work is in close proximity to each other, our time is so fully occupied that it sometimes happens that we do not see each other for days. He is at work on the 5th volume of *Ohio Geology*, which he hopes to have ready for the press within the present year. When printed, a copy will be sent to you. Professor Orton is here considered one of the best geologists of the United States, and his very early perception of the value of your theory of geologic climate, in explanation of glacial action as manifested all over our broad State, did very much to bring your work prominently before the University and its patrons everywhere. It may be of interest to you to know that *Climate and Time* is regularly read by our classes, and discussed and spoken of as I think it should be; not as a text-book, but as a reference book with the classes in Geology, and also in Astronomy. Many students evince a thorough

knowledge of its salient features. And what is also matter for personal gratification—or may be so, at least —is the fact that the weight of reason for your conclusions seems so much to overbalance objections, that the work is a final and standard authority. Only a day or two before your letter reached me, my class in Astronomy had under discussion the astronomical part of the work; and afterwards they were all gratified when I said that I had received a note from you. We are all glad to hear of your improved health, and hope that many years of valuable work may yet be granted you. Indeed, your long silence had led me to fear the worst, and many a time within the year I have feared lest I should see your name in the list of those who "have joined the innumerable caravan."

My duties are numerous, having charge of two departments in the University, and directing the work in both; but my health is so good, that since the spring of 1856 I have lost not a single hour in College work by reason of sickness, and in the winter can kick the beam at more than 200 lbs. My appointment over a year ago as State Inspector of Railway Bridges has in that time somewhat interfered with my scientific work. We have almost 6000 miles of railway in the State; and myself and the two assistants are expected to examine yearly, and report on the condition of the bridges and trestles—the bridges number over 1200, and the trestles about 5000. But the work is done in the long summer vacation of three months.

Should your health or inclination ever permit you to visit this country, I assure you of a most cordial reception, and shall hope that we might be honoured with a call, and begin a personal acquaintance.

I need not assure you that I shall always be very glad to hear from you, and to know of your welfare.—Very truly your friend,

R. W. M'FARLAND.

P.S.—I send our last circular and catalogue, it may be of some interest.

Before Dr. Croll resigned, the Director of the Survey, Professor (now Sir Archibald) Geikie, who was all along a good friend, undertook to write an official letter to the Director-General, recommending that he should get an additional allowance added to the pension to which he was entitled under the regulations of the Civil Service. It was naturally to be expected that, when a man like Croll joined the Survey, at the request of the heads of the Department, in the full knowledge of his then advanced age, and with distinct indication that this would be taken into account, there would be no difficulty in obtaining this. The Superannuation Act authorises the Treasury to declare that when, for the due and efficient discharge of the duties of any office or class of offices, professional or other peculiar qualifications not ordinarily to be required in the public service are required, and that it is for the interest of the public that persons should be appointed thereto at an age exceeding that at which public service usually begins, . . . a number of years, not exceeding twenty, in addition to the actual number of his years of service, may be granted to him in computing the amount of superannuation. If any man had ever shown peculiar qualifications for a post on the Geological Survey not ordinarily required in the public service, that man was Dr. Croll. Indeed, it may be safely said that no stronger case for the granting of the utmost allowance authorised by the Act has ever been presented to the Treasury. But the very strength of his case in the eyes of all scientific, academic, parliamentary, and other competent men, was its weakness in the eyes of the Treasury. As a matter of fact, two assistant geologists, disabled for duty about the same time as he was, the one on the English and the other on the Irish staff, did get large additions to their pensions. The English assistant, in the computation of his allowance, had twenty years added to the twelve years which he had served, and thus received a pension equal to thirty-two sixtieths of his full salary; while the Irish

assistant, who was twenty-one years younger than Croll, had twenty-four years added to his thirteen years, so that his allowance amounted to thirty-seven sixtieths of his salary, *i.e.* within three sixtieths of the highest retiring allowance. When it was clearly seen that he had received an allowance too small to afford a maintenance, Professor Geikie kindly drew up a memorial to the Prime Minister on his behalf, praying that he would recommend the grant of a small sum annually from the Civil List. This memorial was signed by a number of the leading Fellows of the Royal Society of London, and by several influential members of Parliament. After Croll had been kept anxiously waiting for a period of *eighteen months*, an answer was received to the effect that the right honourable gentleman did not consider his claim to be one which he could recommend for the Civil List, but that he would grant a sum of £100 from the Queen's Bounty, This sum, of course, could in no way be considered as compensation for the deficiency of his pension, or an amount sufficient to meet his wants.

In 1882 the shabby behaviour of the British Treasury to one of its servants, also one of the most distinguished scientific men of the time, was the subject of much sharp criticism at the meeting of the British Association. As the result of that, it was resolved that a memorial should be drawn up and presented to the Lords of the Committee of the Council on Education, Science and Art Department, asking for an additional allowance to the pension awarded. The memorial, which was signed by most of the distinguished men of science of the day, by a large number of University Professors, and many members of Parliament, met with a like reception. In refusing this memorial, advantage was taken of the fact that the last memorial had been presented to the Prime Minister, and that, although he did not consider Dr. Croll's scientific services warranted him being put on the Civil List, he had made him a grant of £100 from the Royal Bounty. Undoubtedly it would have been better to have pre-

sented the first memorial to the Lords of the Committee of the Council on Education; but, even had that been done, there is no reason to believe that any different result would have been obtained. It is difficult to believe that the British Treasury actually allowed an eminent public servant to resign, in old age and sadly impaired health, on a pension of little more than one-fifth of his annual salary, and then refused to augment that pension on the ground that £100 had been doled out to him from the Royal Bounty. But such is the melancholy fact, and it is believed that for this, as for other facts, there was a cause. It has been hinted that a somewhat popular scientist, some of whose *current* theories Dr. Croll had vigorously and too successfully assailed, had prejudiced the Prime Minister against his assailant. We cannot for a moment believe that the Premier would listen to any paltry, untrue, and absurd charge of agnosticism or atheism brought against Dr. Croll; but we do sadly fear that he did not himself understand his brilliant contributions to geological science, and that, in an evil hour, he listened to the detractions of a prejudiced, or, perhaps, even spiteful man. Accordingly we are persuaded that the appeals made to the Government on Dr. Croll's behalf were never fully considered on their merits. Dr. Croll's peculiar qualifications for service on the Geological Survey were most notable, his contributions to geological science had been of the most brilliant order, and he had resigned in the firm belief that a liberal retiring allowance would be granted him when he had reached an age at which he could expect to enjoy the allowance for only a few brief years; yet he was informed, after four or five months' delay, that the Treasury had awarded him a superannuation allowance of £75, 16s. 8d., a sum equal merely to thirteen sixtieths of his annual salary. In the case of the English assistant, the pension was calculated on twenty *plus* the twelve actual years of service; in the case of the Irish assistant, on twenty-four *plus* the thirteen actual years of service; in the case of the Scotch

geologist, on the bare thirteen years of actual service. Thus Croll, the most eminent scientist on the staff of the Geological Survey Office, was, by her Majesty's Treasury, ruthlessly plunged into poverty.

As Lord Salisbury had himself signed the second memorial, it was thought when he took office that he might look more favourably on Croll's claims. Another attempt was accordingly made, a few years later, by his scientific friends on similar lines; but the Treasury, having once said No, never repents in a righteous cause; and again an answer of refusal came. Probably, had Croll been a popular retailer of other men's scientific discoveries, a studious member of a foreign royal family, or a *well-recommended*, although already highly-paid public servant, he would have received a liberal allowance from the Civil List; but because he did his duty well, because he had advanced the cause of science as much as any man of his time, because he had led, not only a blameless but an exemplary life, and because he had the strongest claims on all grounds to an increased pension,—the British Treasury thought fit to refuse him his just reward, and left him and his wife to subsist, or rather starve, on a beggarly allowance of £75 per annum.

Dr. Croll felt the manner in which he had been treated very keenly. He had not even got justice, being actually worse treated than two inferior men on the English and Irish Survey, who had not a tithe of the claim to recognition which he had. His first inclination was to write exposing the matter in the newspapers, but from this he was dissuaded. The following letters show how anxious he was about the matter, not so much on his own account as that of Mrs. Croll:—

19 NORTH METHVEN STREET, PERTH,
25th November 1882.

MY DEAR IRONS,—I shall take your advice and not write to the *Scotsman*. But something will have to be

done at once. I cannot, of course, apply direct to Mr. Mundella. My only chance, I think, is to write out a full statement of my case, and get some one of influence, who knows something about me, to present it to Mr. Mundella.

I enclose a statement which I have sketched out. I am sorry to trouble you, but I should like very much if you could manage to find time to go over it when it comes to hand, and make any necessary corrections or improvements which may suggest themselves. I have explained the conditions on which I agreed to enter the Survey, for I don't believe that they know that I was in a measure urged to join.

As I intend going through to Edinburgh on Wednesday morning to see what can be done, kindly report the statement, so that it will reach me on Tuesday.—Yours truly,

JAMES CROLL.

19 NORTH METHVEN STREET, PERTH,
3rd December 1882.

MY DEAR IRONS,—It has just occurred to me that, as you are a law agent, and the document sent bears the mark of having been copied by one of your clerks, there is a chance of Mr. Waddy falling into the mistake that I am one of your clients, and not, as I really am, an old and intimate friend, and that you have written in a business capacity, not out of kindness.

Should you think that there is even the ghost of a chance of him or of Mr. Mundella falling into this mistake, would it not be better to write at once and put the matter right? From a business point of view neither Waddy nor Mundella would take much interest in the matter.

Apologising for troubling you, I am, yours ever ruly,

JAMES CROLL.

19 NORTH METHVEN STREET, PERTH,
13th December 1882.

MY DEAR IRONS,—I am to meet Mr. Parker, M.P., here on Monday, along with Dr. Bower and Mr. Robert Pullar, about that pension business. As nothing has been heard from Mr. Waddy, I really do not know what would be best to ask Mr. Parker to do. If Mr. Waddy cannot see his way clear to approach Mr. Mundella, it would be a pity to lose the chance of getting Mr. Parker to do it. Mr. Pullar is chairman of Mr. Parker's committee, and would have great influence with him. What do you think should be done? One would not like to ask Mr. Waddy the question point blank, but could there not be some way of getting an answer from him, acknowledging at least the receipt of the document, before Monday? This would likely bring out the idea whether he is to do anything or not. At anyrate, you might drop me a line or two, by the end of the week, as to what you think would be best to ask of Mr. Parker. Frost very severe here at present, but not much snow.—Yours ever truly,

JAMES CROLL.

19 NORTH METHVEN STREET, PERTH,
20th December 1882.

MY DEAR IRONS,—I saw Mr. Parker this morning, and had a long talk with him. He is very much interested in my case, and will do everything he can to aid me. He knows Mr. Mundella well, and will speak to him. He thinks, however, that there will be great difficulty in reopening the pension question. He believes that there is more chance of getting something further from Mr. Gladstone. He mentioned that Lord Rosebery might be of service. I shall come through to-morrow, Thursday, and see you for a few minutes at 10 A.M.—Yours truly,

JAMES CROLL.

PERTH, *24th December* 1882.

MY DEAR IRONS,—As I have some business in Edinburgh, I think I shall come through on Tuesday and see what can be done to get at Lord Rosebery before Mr. Gladstone comes to stay with him.—Yours truly,

JAMES CROLL.

PERTH, *29th December* 1882.

MY DEAR IRONS,—I have just time to catch the post. Make out the memorial the way you think best. Get a proof from printer, and then let me see it. If it is addressed to My Lords, Professor Geikie will forward it. Your friend appears to think it can be so addressed.

I got proof of list of papers this morning and have returned it corrected to printer.

I trust Dr. Allon will send on a few copies of my article.

I should have a dozen of proofs of memorial at least.

I hope Mr. Campbell's knee is improving. Mrs. C. is slowly getting round.—Yours ever truly,

JAMES CROLL.

CHAPTER XXI

EVOLUTION BY FORCE IMPOSSIBLE

THE article which Dr. Croll had sent to the *Contemporary* was afterwards sent to the *British Quarterly Review*, where it appeared in January 1883, under the title "Evolution by Force Impossible: a New Argument against Materialism."

His residence in Devonshire during the winter of 1881–2 had greatly invigorated him, and the rest he enjoyed there had so much improved his health, that he felt he would soon be able to resume his studies. Fortunately, his expectations were not disappointed, and he was soon able to enter upon a considerable correspondence regarding his metaphysical paper, as well as his climatic and geological investigations.

The matter of his superannuation allowance, which was vital to him, still occupied his attention, and he wrote the following letters on that subject:—

19 NORTH METHVEN STREET, PERTH,
3rd January 1883.

MY DEAR IRONS,—Yours with memorials are to hand. I shall send one with the letter to the Duke of Argyll, who is at Mentone at present; and when I get his signature, I shall then send it, or get some one else to send it, to the Duke of Devonshire. Dr. Bower will get Sir D. Currie and the other Perth names. If we manage to get at Lord Rosebery before Mr. Gladstone comes through, it will do. I will find means of getting the Glasgow M.P.'s, when once I get a few

copies of the list of papers. Dr. Allon has not sent on the copies of article. I suppose the printer would not get them thrown off till after the *Review* was printed. If they do not come to hand by the time the list is ready, I will send you the proof I have.

The better plan will be to send the copy of *Climate and Time* through by rail, and I shall write Earl Rosebery's name on it.

We must not append too many names, but first-rate what there are. A lot of commonplace men would weaken the memorial.

Wishing you all a happy New Year, I am, yours ever truly, JAMES CROLL.

PERTH, *5th January* 1883.

MY DEAR IRONS,—Book to hand. As it is considered rude to present a book to a nobleman without obtaining his permission, I have not sent it direct, but have forwarded it by book-post to you. I have enclosed a copy of list of my papers, received from printer this morning, along with a copy of article from *British Quarterly*, which came to hand last night, so that you need not trouble opening the parcel, but put a label with Lord Rosebery's address over the present address, and it could be sent to him when Lord Moncreiff forwards the memorial. His lordship might mention in his note to Lord Rosebery that a copy of my book is sent, which the author hopes may be accepted.

I enclose a copy of article for Lord Moncreiff, which might be handed to him along with list of papers.

I telegraphed to the printer to send on immediately a dozen of copies of list of papers, and hand a few to you. Be sure and send list of papers to every one along with every copy of memorial.

I have only got the two or three copies of the *Quarterly* article which I wrote for, but I do trust they will let me have more. I shall send one to Argyll.—Yours ever truly, JAMES CROLL.

19 NORTH METHVEN STREET, PERTH,
8th January 1883.

Sir Joseph Hooker, M.D., D.C.L., LL.D.

DEAR SIR,—About two years ago, owing to the state of my health, I was under the necessity of resigning my situation on the Geological Survey. By some mishap or other I received too little superannuation allowance. I have been advised by my friends to memorialise the Lords of the Science and Art Department for an addition to my pension. I enclose a copy of the memorial, and I need hardly say that I shall feel greatly obliged for the benefit of your name. I am glad to say that the memorial is being signed by a large number of members of Parliament and leading scientific men.

My head, I am happy to say, is much improved by the long rest, and I expect to be able to resume my studies shortly.—I am, yours most truly,

JAMES CROLL.

19 NORTH METHVEN STREET, PERTH,
11th January 1883.

MY DEAR IRONS,—Another day, and still no copies! Nearly a fortnight has been lost waiting on the printer. I believe I would get M.P.'s and professors by the dozen, had I the confounded lists. I am sorry to put you to so much trouble, but I shall feel greatly obliged if you would call on the printer and see that a dozen or two are posted instantly, and at least a hundred copies sent on by rail with as little delay as possible.

As the list will be of use to me for other purposes, I have ordered a thousand copies in all from the printer. Of course there is no special hurry for these, but we must have for this memorial business a supply without further delay.—Yours very truly,

JAMES CROLL.

19 NORTH METHVEN STREET, PERTH,
12th January 1883.

MY DEAR IRONS,—Yours to hand. I have got the Duke, and a number of other important names. I am to write to the Duke of Devonshire to-day; and when we get him, as I feel certain we shall, and Lord Breadalbane, the better plan would be to get them all printed on the memorials, and then we will have a better chance of securing such men as Lords Moncreiff and Rosebery, who know so little about me. The lists have now all come to hand.—Yours truly, JAMES CROLL.

Head aches badly to-day.

FRITH HILL, GODALMING,
13th January 1883.

DEAR MR. CROLL,—I have much pleasure in signing your memorial, and sincerely hope you may be successful, as I think you have been very badly treated.

I read your article in the *British Quarterly* yesterday. It is very forcible and well reasoned, yet I doubt if it will produce much effect. The extreme materialists will reply that matter and its forces being eternal and infinite, all the motions of matter necessary to produce organised beings have been determined by preceding motions, back to all eternity. They will maintain that these conceptions of eternal matter and forces are no more difficult than the conception of a determining power, which is neither matter nor force.

I am very glad to hear you are better. Have you read Stallo's *Concepts and Theories of Modern Physics*, one of the International Scientific Series? It contains some most acute criticisms of modern scientific views, especially of the kinetic theory of gases and of non-Euclidean geometry. I think you would be interested in it.—With best wishes, yours faithfully.

ALFRED R. WALLACE.

19 NORTH METHVEN STREET, PERTH,
19th January 1883.

MY DEAR IRONS,—You will see, from the two letters enclosed, that there are some points in the memorial which appear to have given dissatisfaction to one or two of Professor Geikie's London friends. As the points referred to are of no great importance to us, I have made some alterations which will get rid of the objections.

Please read over my alterations, and then send it to the printer, and tell him to send me a proof without a moment's delay, as I can do nothing until I get the altered copies. Tell him to send, say, six proofs. These will keep me going till the proof is examined. I have got His Grace the Duke of Devonshire, and have written to the Earl of Breadalbane. I shall try and get at the Duke of Buccleuch and a few other nobles. The great stars of science are all signing with hearty goodwill. What about Mr. Waddy? Have you got his and Mr. Buchanan's names? I have got five or six M.P.'s from the west, and expect more shortly.—Yours truly,

JAMES CROLL.

19 NORTH METHVEN STREET,
22nd January 1883.

MY DEAR IRONS,—I have written to Mr. Paton, Glasgow, — who has the memorial containing the signatures of the M.P.'s, who got it to try and obtain Mr. Charles Tennant and some others,—to post it direct to you. Try and get not merely the M.P.'s of Edinburgh, but the legal luminaries to whom you refer, and also the Lord Provost. The sooner we can get them the better. I have got the greater part of the great names of science in England. These I got without any trouble, as I am known to them all. The difficulty is with the M.P.'s, who know nothing whatever about me. What we must now do is to collect all the names

which have been obtained, and I shall make out a list giving all their titles, and get them printed on a number of the memorials. The list will be a very imposing one indeed. I would let Lord Rosebery alone till we get the list printed. I am not sure if it would be good policy to present him with a copy of the book. I rather think not. But when we get out our list, I think the better plan would be for me to write and ask if he would be willing to sign. After this, we might try and get a talk with him and the Lord Advocate before they go south. When I get the altered memorial, I shall make an onset on the Edinburgh Professors. I observe that the Right Hon. J. Inglis, D.C.L., LL.D., is Chancellor of Edinburgh University. We must get him. Lord Rosebery, LL.D., I see is Rector: I shall write after I get the Edinburgh Professors, and ask Lord Rosebery to sign as Rector.

The memorials have not yet come from the printer. I hope he will not again disappoint us.—Yours truly,

JAMES CROLL.

The following is an interesting letter to Mr. Bennie, regarding the great ice-cap:—

PERTH, 22nd January 1883.

DEAR MR. BENNIE,—I am greatly obliged for the trouble you have taken, and also for your long and interesting account of Agassiz's great ice-cap. It is singular that, amongst all the discussions we have had on ice, I should never have heard about the paper to which you refer. It is long since I first heard of Agassiz's great cap of ice extending down to near the equator, but I was not aware that we had the full account of it, in an English, far less in an Edinburgh magazine. In so far as the mere filling up of the North Sea with solid ice is concerned, Agassiz has been a quarter of a century before me. One who holds that an ice-cap of enormous thickness covered our northern hemisphere down to near the equator, must of course

necessarily hold that not only the North Sea, but every sea over which the cap spread, must have been filled with ice. But this could hardly be considered an anticipation of the outline of the path of the ice of the Glacial epoch in North-western Europe, given by me in *Geological Magazine* and *Climate and Time.* Of course, to a great extent I am speaking in the dark, as I do not know the particulars of Agassiz's theory. I am surprised that the parcel sent from London through Williams and Norgate has not reached the office. When you happen to be in Princes Street, you might kindly call and see what has become of it.—Yours truly,

JAMES CROLL.

19 NORTH METHVEN STREET, PERTH,
3rd February 1883.

MY DEAR IRONS,—It is a great pity you did not send your letter to me a week ago. I have been waiting with the utmost impatience for the list of M.P.'s and other names which may have been got in Edinburgh to enable me to get the whole arranged and printed on the memorial, so as to enable me to get at Lord Rosebery. I have no other means of getting at him. I might write to him and get his name, but that alone would not be of much service. If all the influential names obtained were on the document, it would not only produce an impression on him, but would be sure to make him take an interest in my case. Had we succeeded in getting an introduction to him through the Lord Advocate, Lord Moncreiff, or some of these great men, we might have arranged for an interview; but without getting Lord Rosebery first interested in the matter, it is needless to do anything.

I shall now arrange the names, come through to Edinburgh and get them printed at once. No time must be lost; I shall be through on Monday morning.—Yours ever truly,

JAMES CROLL.

19 NORTH METHVEN STREET, PERTH,
10th February 1883.

MY DEAR IRONS,—Mr. Parker wishes the memorial sent up to him, so as to get the signatures of the Scottish M.P.'s when Parliament meets, so we must get it ready immediately. If you have got Mr. Waddy or Lord Moncreiff, write at once and let me know, as I intend posting the corrected copy to the printer to-morrow. If you send off your letter by to-night's mail, I will get it to-morrow morning by calling at the post office. Mr. Buchanan sent me a letter from Lord Rosebery's secretary, saying that his lordship is to do what he can for me.

JAMES CROLL.

19 NORTH METHVEN STREET, PERTH,
16th February 1883.

Sir J. Dalton Hooker, K.C.G.I., C.B., etc.

DEAR SIR,—I enclose a copy of memorial containing the names of those who have kindly signed the recommendation for a reconsideration of my case. Notwithstanding the large and influential list of names on the document, I fear that, without the personal influence which you so kindly offered, there may be some difficulty in getting the case reconsidered.

Mr. Parker, M.P. for Perth, is endeavouring to get some more of the Scottish members to support the memorial. I shall write and let you know when it is sent in to the Department, which, I expect, will be in a few days.—I am, yours most sincerely,

JAMES CROLL.

19 NORTH METHVEN STREET, PERTH,
8th March 1883.

MY DEAR IRONS,—You will see from the enclosed that the memorial was sent in on the 26th ult. Mr. Parker sent it direct to Mr. Mundella at his request. Previously, Professor Geikie sent me a letter that he would do all he could to favour it. The Department, I

understand, sent memorial to Professor Geikie to report on.

Might you not send a copy of names to your London friend who favoured you with his advice?

Please to accept of a copy of *Climate and Time.* The package encloses a copy of the article on Evolution. As I am very scarce of copies, perhaps you would kindly let me have it back when you read it.

I have been knocked up with cold for some time back.—Yours ever truly, JAMES CROLL.

FRITH HILL, GODALMING,
24th March 1883.

DEAR MR. CROLL,—I am glad to see you have such an array of signatures to your memorial. It must surely receive attention.

I shall be glad to read your article again, though I think I fully see the drift of your argument. No doubt *force* alone is no explanation of anything, but all materialists begin with *definite* forces acting according to definite laws, and the whole question is whether, these definite forces being *infinite* or practically infinite in number, the atoms of physicists would not by their interaction through *infinite* time produce all the results of the *material universe* without any further "determinism" than that implied in the definite nature of the original forces. This view, if I remember rightly, you do not discuss, and it affords a complete answer to some of your arguments. Is not this so?—Yours very truly,

ALFRED R. WALLACE.

19 NORTH METHVEN STREET, PERTH,
29th March 1883.

MY DEAR IRONS,—You will be wondering at my long silence. I was down at Gourock five weeks ago, got a chill, and have been confined to the house ever since.

I have, of course, been able to do nothing as regards your application, but trust these dreadful east winds will

let me out of doors shortly. I do trust you will be successful, and am sorry I am so little known in Perth; but I shall certainly do what I can.

The memorial has not yet reached the Treasury. Professor Geikie failed to send in his report in time for last meeting of Council, and then came Easter holidays.

I am sorry to say that a sister of Mrs. Croll, a Mrs. Findlay, Elgin, died a week or two ago.

Mr. Mundella, you will observe, is poorly. I hope this will not damage our case.—Yours ever truly,

JAMES CROLL.

Croll had obtained from the publishers copies of his article "Evolution by Force Impossible," which appeared in the *British Quarterly Review*, and was anxious to obtain the opinion of some metaphysicians on the validity of the argument. He accordingly wrote the following letter on the subject to his old friend Dr. Morison.

19 NORTH METHVEN STREET, PERTH,
17th April 1883.

DEAR DR. MORISON,—The publishers of the *British Quarterly Review* have sent me one or two copies of my article on Evolution. I have much pleasure in sending you a copy, which please to accept. I need hardly say that I would be delighted to have, at your leisure, a few lines with your opinion of the argument, more particularly in regard to points where you may *differ* from me. Before writing the concluding portion I am anxious to learn the objections which may be urged by our leading thinkers. We have not as yet taken up house. I have memorialised the Science and Art Department for the addition to my pension which should have been obtained for me at the time I left the Service. You will see from the enclosed list how strongly my memorial has been supported. The feeling is that I have been somewhat hardly treated. I am daily expecting to hear the decision of the Treasury. I hope that Mrs. Morison

and yourself are in the enjoyment of the best of health. I am happy to say we are both well.—With kindest regards, I am, yours ever truly, JAMES CROLL.

The following interesting correspondence took place between Dr. Shadworth Hodgson and Dr. Croll regarding the same paper:—

19 NORTH METHVEN STREET, PERTH,
20th March 1883.

Shadworth H. Hodgson, Esq.

DEAR SIR,—I enclose a copy of an article from the *British Quarterly Review*, on a new aspect of Evolution. Please to accept the copy. Its main drift is this: I have endeavoured to show that the fundamental question in organic evolution is, What determines molecular motion and force? Consequently, the grand principle of evolution is *Determinism*. The next question is, What is the determining cause? What is that which determines molecular motion and force? I have endeavoured to prove that it is *absolutely* impossible that this can be motion, force, or anything of the nature of an act. If this conclusion be correct, it follows that Mr. Spencer's theory of Evolution by Force, is absolutely impossible, and that force cannot be the ultimate of ultimates.

I shall be delighted to hear at your leisure what you think of the argument.—I am, yours most sincerely,

JAMES CROLL.

45 CONDUIT STREET, REGENT STREET,
LONDON, *5th April* 1883.

James Croll, Esq., F.R.S.

DEAR SIR,—Many thanks for so kindly sending me your article "Evolution by Force Impossible," and for the letter which accompanied it. It reached me at a time of great occupation, no uncommon thing with me, or I should have responded before this.

It seems to me that you have clearly demonstrated your main point, that force alone, motion alone, or any

sort of act alone, that is, undetermined to any particular direction, is totally unable to account for evolution, because it is a mere abstraction or *ens rationis*. Every force, motion, or act must be determined and concrete, and its determination being, as it is, part of itself, requires accounting for. Consequently, the initial state or act of any evolution has a prior condition which determines it to be the determinate state or act which it is, though we may not know what that prior condition is.

So far I go with you completely, at least if I rightly apprehend your meaning. But in the latter part of your article, "Determinism in Relation to Theories of Life," and onwards (I speak with great hesitation), are you not doing very much the same thing with regard to the "objective idea of life," as evolutionists do with abstract force, motion, or act, that is, setting it up *alone* as a determining cause of force?

Evolution as a whole is, by its very statement, *teleology*, "definite heterogeneity" is its *telos*. It is a larger and vaguer name for "objective idea of life." The *telos* cannot be transferred from the end as result, to the beginning as plan, so as to act as efficient or determining cause. You might as well explain the tangential and centripetal forces in the planetary motions by saying that they were forces planned to produce elliptical paths. Now the Evolution theory seems to me to aim right, when it aims at accounting for its *telos*, "definite heterogeneity," by the play of efficient forces, without including its *telos* in their definition. But it never can completely realise its object, because (as you have shown) its first efficient force must always be determinate and not an abstraction, and therefore must require an anterior condition to make it what it is. There is always a point in the evolutionary regress, where its explanation comes to an abrupt stop, leaving its ultimate term unexplained, yet requiring explanation.

Thus we have to refer the whole evolution to a

condition beyond itself, unknown to us by observation or experiment, but which we have to conjecture from what we know of the evolution which is its effect. In forming which conjecture, we have only the facts of our moral nature to go upon, and cannot attain a knowledge *constitutive*, as Kant would say, of the objects conjectured; that is to say, our ideas are ideas of realities, but inadequate as knowledge of them. Mr. Spencer's *great* error, in my opinion, is his taking this prior condition as *wholly unknowable*, and thus seeking in the evolution an absolute beginning. The evolution is not self-sufficing or self-evolved from abstract force, one of its own elements, or from abstract idea, which, if transferred to the beginning of the evolution, would be but another element of it. Its unknown condition must be thought of as the condition of their combination. This is to me one great philosophical basis of Theism. Nature works as if she had a previous plan, but the presence of the plan at the beginning of the evolution is not demonstrable, and even were it so, it would not be a concrete determinate force, such as science directs its efforts to discover.

I hope you will not think I have abused the liberty you gave me by your invitation to say what I thought of your argument. I think you have established the main point. I shall look for the *British Quarterly*, with your Teleology article in it, though I fear I may find some things there with which I shall not be in entire accordance.

Let me beg your acceptance of two addresses of mine to the Aristotelian Society, which I send by book-post, though, as they are only "Metaphysics," I do not venture to say, Read them, but only do as you like.—Believe me, dear sir, sincerely yours,

SHADWORTH H. HODGSON.

19 NORTH METHVEN STREET, PERTH,
6th April 1883.

Shadworth H. Hodgson, Esq., LL.D., etc.

DEAR SIR,—Many, many thanks for your interesting

letter. It is just the very sort of thing I want, and pleases me better than though you had said, "I wholly agree with you." I don't think we will differ so much on the "*objective idea*" when I come to explain myself.

Thanks for your two addresses, which I shall read with much interest, and none the less that they are metaphysical. Metaphysics is my favourite study.—Yours most sincerely, JAMES CROLL.

19 NORTH METHVEN STREET, PERTH,
7th April 1883.

Shadworth H. Hodgson, Esq., LL.D., etc.

DEAR SIR,—On reading your letter, it struck me that there must be a misunderstanding between us somewhere, as it seemed strange that you should agree with the first and main part of the argument and yet disagree so completely with the latter part, seeing that it is, in my opinion, a necessary deduction from the former.

To-day, on going over your letter more carefully, I find where the mistake lies. I fear I have been using certain words unguardedly, to which common usage has assigned a different meaning from what I have given them. For I find that you have misapprehended the sense in which I use that unfortunate expression *objective idea*. By "objective idea" I do not mean a something which the determining cause perceives and makes its determinations to agree with. The objective idea is not a something which the determining cause takes as its plan of operation. By objective idea I simply mean the *form* according to which the determining cause, be it conscious or unconscious, *actually* makes its determinations. If the leaf of a tree is being formed, this cause must, as a *matter of fact*, be determining the motions of the molecules according to the objective idea of a leaf, or, in other words, according to a figure of the form of a leaf, or no leaf would be formed. This is not asserting anything conjectural or hypothetical. It is a bare statement of what is actually taking place. I think

I have stated this plainly at page 27, middle paragraph.

I have an impression that, with this explanation, the difference between us is greatly diminished.

The application of all this to Mr. Spencer's theory of Evolution by Force is obvious. Force can never explain how the molecules are determined according to the idea of the thing which is being formed. In fact, his philosophy can never touch the real problem of Evolution.

I am sorry thus to trouble you, but if you would point out the misleading terms I have been using, I shall esteem it a kind favour. Again thanking you for your important and, to me, interesting remarks on the objective idea,—I am, yours most sincerely,

JAMES CROLL.

45 CONDUIT STREET, REGENT STREET,
LONDON, *12th April* 1883.

James Croll, Esq.

DEAR SIR,—Thanks for your two letters. I am very pleased to find from the second, of April 7th, that I had misunderstood your view, and that you do not look at an antecedent plan as determining cause of resulting form, as, for example, of a leaf.

I had taken particular notice of your expressions at page 27, "simply stating what actually takes place," and had jotted down in pencil against them, "Very true, and for that reason it is *not* an *explanation* of what actually takes place."

I think the expressions which chiefly misled me are on the next page, 28, "It is *because* these molecular movements, etc.," and below, "One element in *this cause of determination* must be intelligence or thought."

How intelligence or thought could be an agency is what I never could construe to myself. This is Hegel's assumption, derived from Kant, derived from Scholasticism, derived from crude, or unanalysed, common-sense notions.

I confess I thought that you were wishing to substitute a better, that is, more adequate cause than Spencer's absolute force for his force as the originator of the world-history of Evolution. My objection to Spencer is not so much that his force is inadequate as an explanation of the design we find in nature, as that *any* theory of world-history, *any* evolution theory, is inadequate as an explanation. Science, since Bacon's time, consciously abstains from using final causes as efficient; that is, abstains from seeking a *total* explanation of the world-phenomena. It consciously *restricts itself* to a partial one, that is, to showing the steps by which, in order of efficient causation, what we call organic design, or objective idea, is realised. Consequently, *ex hypothesi*, organic design cannot be used as a cause, for it would be a step realising itself.

My view accordingly is, that this confessedly, purposely partial character of scientific explanations—this *partial character of science*—is the true basis of Theism. If you base Theism on the necessary presence of an objective idea, you limit your conception of God by that idea. Whereas, if you say, There is a cause of the world *beyond* science, you leave your conception of God to be determined by our felt, practical relations (speculatively unknown) to the unknown cause. And this view of the basis of Theism, I believe, is chiefly due to Kant, the great permanent result of his system, when noumenon-phenomenon puzzles have been eliminated—puzzles which have tripped up Mr. Spencer. He has taken the faulty and left the sound in Kantianism. The same thing, *in another respect*, may be said of Hegel.—Believe me, very sincerely yours,

SHADWORTH H. HODGSON.

Dr. Croll forwarded a copy of his article on Evolution to Mr. Herbert Spencer, accompanied by the appended note, to which Mr. Spencer replied in the letter which follows:—

19th April 1883.

I send you a copy of an article on Evolution from the *British Quarterly*, which I hope you will accept. On many points we agree, but in regard to the philosophy of Evolution you will see we are almost poles asunder.

I hope I have not in any way misrepresented your views; if I have, I shall be glad to put matters right in the concluding part of the article. I know that no one is more desirous that the whole matter should be freely discussed than you are yourself.

38 QUEEN'S GARDENS, BAYSWATER, W.,
23rd April 1883.

James Croll, Esq.

DEAR SIR,—You must excuse me on grounds of health from entering at length into any discussion on the points raised in the article you have been good enough to send me.

I will say only that your view as to the determination of force is one which puzzles me as coming from a man of science. I hold that the determination of any force depends on the proceeding distribution of forces in their amounts and directions; and I accept in full your alleged implication that this distribution depends upon a preceding distribution, and so on through infinite past time; and if you allege that this is not an ultimate or satisfactory interpretation, I simply reply that there exists in it just the same ultimate inscrutability that exists with respect to the nature of force and the nature of matter.

Your criticism seems to me, as the criticism of most others, to tacitly assume that the hypothesis of Evolution as set forth by me professes to be an interpretation of things in their ultimate causes, whereas, professedly alleging the ultimate cause to be unknowable, alike in nature and mode of operation, the sole aim is to generalise the order of the manifestations.—I am, truly yours,

HERBERT SPENCER.

CHAPTER XXII

CORRESPONDENCE WITH PROF. G. J. ROMANES

DR. CROLL likewise sent a copy of his paper on Evolution to the late Professor G. J. Romanes, and the following correspondence which ensued has acquired a peculiar interest through the publication of (1) Dr. Romanes' *Thoughts on Religion*, edited by the Rev. Canon Gore, and (2) of his *Life and Letters*, edited by Mrs. Romanes. Both Dr. Romanes and Dr. Croll, although known to the world chiefly through their scientific achievements, were men of a profoundly religious nature, and the first, as well as the last published work of each dealt with a great theological subject. In 1873, while a student at Cambridge, Dr. Romanes gained the Burney prize for an essay on "Christian Prayer and General Laws," but even by this time, his biographer tells us, he had "entered on that period of conflict between faith and scepticism which grew more and more strenuous, more painful, as the years went on. . . . Step by step, he abandoned the position he had maintained in his Burney Prize Essay, with no great pauses, rather, as it seems, with startling rapidity, and with sad and reluctant backward glances he took up a position of agnosticism, for a time almost of materialism. He wrote a book, published in 1876, which was entitled *A Candid Examination of Theism*, . . . marked throughout by a lofty spirit and profound sadness, and a belief (which years after he criticised sharply) in the exclusive right of the scientific method in the court of reason. . . . Anyone who reads carefully the conclusion of the *Candid Examination*, will see the note of longing and thirsting

for God. . . . The reaction against the conclusions of the essay set in far sooner than has been at all suspected. Perhaps the first published mark of reaction is the Rede Lecture of 1885;" but the long conflict between faith and scepticism "never really ceased until within a few weeks of his death," although it ended "in a chastened, a purified, and a victorious faith."

18 CORNWALL TERRACE, REGENT'S PARK, N.W.,
6th May 1883.

DEAR SIR,—I have been much interested in the very able article which you have been so kind as to send me, and for which I now write to thank you—the more so because I did not happen to have seen it in the *British Quarterly*. I wish very much, for my own sake, that we could meet and have a conversation upon the points which you urge, for these are so new to me that I am not sure how far I am yet in a position to offer the criticism which you solicit. By this I do not mean that I have failed to appreciate the lucidity of your exposition, but such a number of questions occur to me which I should like to ask you, that I think conversation would be a better medium of understanding than correspondence. Therefore *in lumine* I may ask whether there is any prospect of your being in town between this and the end of next month, and if so, whether you would care to have a talk with me upon the subject-matter of your essay.

Failing this, I should like to ask you two general questions, which are the most important ones that I have to ask.

1. Is it not true that the determination of force depends upon the conditions, statical and dynamical, under which force operates? If so, is it not further true that the determination of any given *quantum* of force must depend upon the previous determinations of other *quanta* of forces, and so on *ad infinitum*? Lastly

if this is so, does it not follow that, on the supposition of force and matter being permanent, every physical change, or every particular determination of force, is the mechanically necessary outcome of all such previous changes? But to say that it is so appears to me equivalent to saying that every determination of force is itself determined by the action of previous forces, and therefore, if force is persistent and matter persistent, that every determination of force must take place in the way that it does take place—just as in the parallelogram of forces the resultant must always be determined in magnitude and direction by the *m* and *d* of the components.

2. The other point I have to ask you about is touching your criticism on my theory of causation. I should have thought that the essential element in the idea of causation consists in that of equivalency; not only must it be true that everything which happens must have a cause, but also that like causes should always produce like effects, or that there should invariably be a quantitative relation between the cause and the effect. But this would not be the case if, according to the above illustration, a falling mass produced a different amount of mechanical effect on different days; that is, there would be no causation in such a case, for there would not be such a collocation of conditions as invariably produced a particular consequent. Which is another way of saying that if force were not persistent, causation would not be existent. I should be very glad if you could dislodge me from my views on these points, and I look forward with interest to the appearance of your essay on Teleology.

Although I write to you direct, I should be obliged to you not to divulge my authorship of the essay on Theism to which you allude. Some day I intend to do so myself, but not until my opinions are more matured.—Yours very faithfully,

G. J. ROMANES.

Letter to G. J. Romanes, Esq.

Causation.—A few lines on the two questions to which you refer. I shall begin with the one relating to causation, as it requires to be considered first.

The principle of causality, as I understand it, is "Every event or everything which comes to pass must have a cause"; or, expressed in other words, "Everything which happens or begins to be must have a cause." The principle never can reveal to us anything regarding the nature of the cause. This is known from the nature of the effect. It will not do to affirm, as some do, that cause *must* be something of the nature of an act or exertion of force; for there are many things which come to pass which cannot be produced by such causes, as, for example, the determination of acts or the determination of forces, and yet these must have causes.

The principle that the same cause acting under the same conditions will always produce the same effect is not primary, but a deduction from the more general principle that every event must have a cause. This is evident, because, if the cause acting under absolutely the same conditions would not always produce the same effect, but should produce one kind of effect at one time and a different kind of effect at another time, then here would be something coming to pass without any cause, namely, the change from the one kind of effect to the other, a thing impossible according to the principle of causality. Were it not for the principle of causality, there would be nothing to prevent us believing that the same cause under the same conditions might not always produce the same effect; for if an event could happen without a cause, there would be no reason for believing that the same cause, under the same conditions, should *always* produce the same effect.

The principle of "Equivalence" to which you refer, I think, is also a deduction of the same nature from the more general principle that "every event must have a

cause." To affirm that a cause must always be equivalent to the effect which it produces is simply affirming a proposition logically deducible from the principle of causality. For if there should be something more in the effect than could have been produced by the cause, then this something must be either an effect without a cause or the effect of some other cause. If we suppose the former, then we contradict the principle of causality. If we suppose the latter, namely, that this something was the effect of some other cause, then it follows that the cause in question was equal to all that it actually produced.

Take the case of the stone in my illustration. Suppose we found that the stone, in descending a given distance, performs ten foot-pounds of work the one day, but only five the next. According to the principle of causality, there must have been a cause for the disappearance of the five foot-pounds. Either the stone must have lost half of its weight, or the five missing foot-pounds must have gone to produce heat or some other sort of work. Assume the former to be the true explanation. The principle of causality simply affirms that there must have been a cause for the loss of weight, or, in other words, *for force ceasing to be persistent.* This does not in any way affect the principle. It is the same as regards the principle, whether force persists or not. It only guarantees that the force could not cease to persist without a cause. The domain of causality is far wider than that of force. Countless millions of effects are continually taking place that have no relation to force, as, for example, determination; and, for this reason among others, the persistence of force could not be substituted for the principle of causality.

Determination.—I will willingly admit, for the sake of argument, that the determination of a force depends upon the conditions under which the force operates, and this again on the previous determination of another force or forces, and so on *ad infinitum.* In short, that every

particular determination is the necessary outcome of all such previous determinations. But I cannot admit the conclusion which you draw from this, namely, "that to say so is equivalent to saying that every determination of force is itself determined by the *action* of previous forces." It is determined by the *determination*, not by the *action* of the previous forces. The determination is the result not of the *actions*, but of the *determination* of those actions. In no conceivable way can determination result from force or action, or force or action result from determination. The two things are *absolutely* different in their essential nature. The one can never, even in thought, take the place of the other. Neither force nor the action of force will explain evolution; it is the determination or direction given to these that explains everything. But determination is not effected by either force or action, but by prior determinations. Here is the weak point in Mr. Spencer's philosophy of evolution. In making force the ultimate of ultimates, he completely overlooks the fact that determination, not force, is the fundamental thing in the explanation of the process. Mr. Spencer will doubtless say that he does not profess to give an ultimate explanation, as such an explanation lies in the region of the unknowable. But this will not remove the objection; for, supposing our intelligence were infinite, it would not in any way enable us to explain evolution in terms of force, because such a thing is in itself absolutely impossible.

More than this, the mystery of evolution is to account for the determinations occurring according to an objective idea. This is not asserting anything hypothetical, it is simply stating what actually takes place. In the formation of a tree, for example, every molecule must have its motion directed and determined according to the objective idea of a tree, or no tree would be formed. At page 64 of the article I have endeavoured to show that natural selection can never be the cause of the determination of molecular motion. But, even sup-

posing it could be the cause, this would not show that force had anything to do in the matter; for natural selection is not produced by force or energy. It results not from force, but from the way in which forces are adjusted. The superior adaptation of a thing to the conditions under which it exists determines its selection. It is not the mere force of an animal that secures its existence, but the way in which that force is adapted to the conditions under which it lives. Blind force, unless properly directed, is of little avail in the struggle for existence.

18 CORNWALL TERRACE, REGENT'S PARK, N.W.,
23rd May 1883.

J. Croll, Esq., F.R.S.

MY DEAR SIR,—I am greatly obliged to you for your kindness in answering my question so fully. You have made the matter more clear to me, and I begin to think you are right.

I can see that your views on causality follow logically from your views on determination. But I should like to ask you one more question with regard to determination. You say that "in no conceivable way can determination result from force or action." But take such a simple case as the parallelogram of forces: would you not allow that the determination of a resultant force in magnitude and direction is effected by the *magnitude* and *direction* of the component? And if so, would you not also require to allow that each of any number of components was itself previously determined, as to its magnitude and direction, by the similar operation of previous forces, and so *ad infinitum*?

This is my chief difficulty. Each swing of a pendulum, for instance, seems to me to be determined, not by occult power, but by the previous swing *plus* gravity. Hence—and only hence—it is that the course and the amplitude of the next swing admits of being calculated or predicted.

I have just published a little poem anonymously called "The More Excellent Way," which I think might interest you. Macmillan & Co. have the copyright, or I should send you a copy, but I daresay you can procure one from a circulating library.—I remain, yours very truly,

G. J. ROMANES.

Answer.

Most certainly I would allow that the determination was effected by the *magnitude* and *direction* of the forces; but this is simply to allow that the determination is the effect, not of the forces, but of the determination of the forces. *Magnitude* and *direction* are not forces, but simply certain determinations of the forces. Magnitude is the *determined quantity* of the forces. Direction is the *determined path* taken by the forces, or the *determined direction* in which they happen to act.

The swing of the pendulum is explained in a similar manner. The mere motion of the pendulum is, of course, effected by the force of gravity; but the direction of that motion is not the result of force, but of the direction in which the force acts.

19 CORNWALL TERRACE, REGENT'S PARK, N.W.,
26th May 1883.

I am greatly obliged to you. I think it will have to end in another edition of my anonymous book, undoing the principal argument. In this case would you object to a statement of my obligation to you?

G. J. ROMANES.

19 CORNWALL TERRACE, REGENT'S PARK, N.W.,
20th April 1886.

MY DEAR SIR.—The paper to which you refer is in type for the *Contemporary Review*, under the changed title of "The World as an Eject." The Rede Lecture, of which it is a complement, appeared in the *Contemporary Review* for July 1885. I have no separate copies, or

I would send you one. If you read it, I should much like to have your opinion. But wait until you see both the papers. It was our previous correspondence that led to them.—Yours very truly,

G. J. ROMANES.

CHAPTER XXIII

CORRESPONDENCE WITH PROFESSOR M'FARLAND AND SIR J. D. HOOKER

DR. CROLL had written Professor M'Farland, asking him to get one of his papers inserted in the *American Journal of Science*, when the following correspondence ensued:—

OHIO STATE UNIVERSITY, COLUMBUS, O.,
9th April 1883.

To Dr. James Croll, Perth, Scotland.

DEAR SIR,—Your letter of 17th ult. was received on the 31st,—the list of papers by you came about the same time. I had occasion to address Professor J. D. Dana, one of the editors of the American *Journal of Science*, the very day your letter came. I wrote, in addition to what claimed my personal attention, asking him what chance there might be to print your paper. He writes in reply, that he will take pleasure in giving space for it, but that his present engagements will require that he name August as the earliest date. This letter will probably reach you about the 24th or 25th of April—then, when the mail shall bring the return, it will be about the middle of May, and the delay from that date to the time when the printers will take hold will be only about seven or eight weeks. Sometimes long articles are divided, and part appears one month, another part later. You know the size of the journal.

Your article may be sent either directly to Professor Dana or to myself. Use your pleasure: if it comes to me, I will see that it goes to the hands of Professor Dana.

I have no means of telling which of the two publica-

tions has the wider circle of readers, the *Philosophical* or the *Geological Magazine*, and inasmuch as the American journal is at your service, I suppose it does not matter very much which may be best known on this side of the Atlantic. But I know your forthcoming article will be read by our whole body of scientific men, and you know our country is pretty large, and has a good many stirring men in it. New Haven, the town where the journal is published, is about five hundred miles from Columbus by rail, yet I had Professor Dana's answer by mail in three days.

It will be gratifying to your many friends in this country to know that your health is improved. *Climate and Time* is read by a large body of young men—several copies have to be purchased for the library and reading-room of the College. I have heard a good many students speak of the work in such a way as to show a mastery of the chief features. I send a copy of our last circular and catalogue; being a "State" document, it is not fully under control of the University, but several parties have their say. Dr. Orton, our geologist, still works in the field a part of the year. I will see whether the last "Ohio" report has been sent you by him, and if not, I will forward it. At least one more volume is to come—perhaps two; six or seven have already appeared. As Ohio contains about forty thousand square miles, and there are thirty-seven other States and large sections called "territories," all the States and United States besides, being engaged in promoting geological researches, it is easy to see that the geological literature is growing to large dimensions. It might be better if their "Utica" should be "pent up" more than it is, or life be prolonged somewhat, so as to allow larger margins for work.—Very truly yours, R. W. M'FARLAND.

19 NORTH METHVEN STREET, PERTH,
24th April 1883.

Professor M'Farland, Ohio State University.

MY DEAR SIR,—Your letter of the 9th inst. came duly to hand yesterday. I am glad to learn that you

have succeeded in obtaining a place for my paper in the American *Journal of Science.* Many, many thanks. I have always found Professor Dana kind and ready to oblige. I shall not send the paper to any of the journals here till after it appears in America. In this case I shall send you the manuscript in course of a few days, and as I would like to get a proof, and also a revised proof, before it appears, August will be quite soon enough to allow of the transmissions of the proofs backwards and forwards.

I am delighted to learn that my book is so much read by the students of your University. They are the hope of the future.

I am greatly obliged to you for the documents which you have kindly sent on. They will, no doubt, come to hand in the course of a day or two.—Yours ever truly,

JAMES CROLL.

Dr. Croll was engaged writing two papers on Mr. Alfred Wallace's "Modification of the Physical Theory of Secular Changes of Climate," and had consulted Sir Joseph Dalton Hooker in regard to some points in these, on which the following correspondence ensued:—

25th November 1883.

DEAR MR. CROLL,—I have been reading your two papers which you were so good as to send me, with great interest. I am now so immersed in Botany and Horticulture, official and unofficial, scientific, popular, and economic, that I have no time to think on the subject, and very little to recollect even. There are, however, some points in connection with Antarctic ice to which I would like to call your attention, premising that your paper is full of good remarks, and what I take to be for the most part sound conclusions.

One point is this, that the Antarctic Continent, so called, that is, the only piece of land of any extent found in the Antarctic regions, is Victoria Land. Of this Sir W. Thomson knew nothing, and it is absurd to class it with groups of islands bound by an ice-sheet. From

Cape North, where we made and left it, in latitude 71°, to Mount Terror in latitude 79°, it is a continuous, lofty, continental mass, rising in rugged peaks the whole way, probably nowhere below 4000 feet, with peaks of 6000 to 8000 or upwards, and brown or black cliffs showing out here and there. The only other Antarctic land of any consequence of which anything is definitely known, is that south of Cape Horn, and this differs wholly in character from Victoria Land, and may well be a group of islands, bound, not by an ice-sheet, but by pack ice. The other real or supposed Antarctic lands are mere specks, immensely distant apart, and all far north of latitude 70°. They may be small islands (some are cloud-shadowed), but it is rash to suppose that they are held together by an ice-sheet. They are surrounded by pack ice, as is nearly the whole South Polar area, north of 70°, and no one has ever attempted to pierce that pack but Ross, who did so twice, finding open water beyond it, which led on to nearly 80°.

Cook, Weddell, and Ross have alone, I think, penetrated beyond 70°, the two former in open water. Ross tried Weddell's route (who got to 74°, and turned back, leaving open water ahead), and spent the whole season trying to penetrate the pack in all directions east of Graham's Land, but could not get beyond 62° in the very meridian where Weddell got to 74° in open water.

You may guess what a disappointment this was, when I tell you that Weddell's route was kept as a *bonne bouche* for that expedition, third year of Polar exploration, so sure were we of getting to south of where Weddell left off.

With exception of the Great Barrier in latitude 79°, we know nothing of ice-sheets in the South Polar Ocean. That is one, no doubt, but it may be quite a local phenomenon, and merely a great bay blocked with snow-falls. True, we have huge icebergs accurately described by you and all, but their origin is not proved. My *guess* was, and is, that they are formed of accumulations of snow on pack ice, and that the barrier itself so formed

is the mother of many of these. The whole coast of Victoria Land was blocked by them, of all heights and areas, extending for miles from the coast. No doubt many originated in glaciers sent down from the valleys of Victoria Land, but they were caught in pack ice, and were subsequently covered by the successive snowfalls, and hence grew as in the tabular form.

I think it quite possible that the Antarctic Polar area is in the main aqueous, and covered with pack ice, formed in the sea, and increased by the perennial snowfalls. My impression is that the snowfall in the Antarctic is enormous, summer and winter. I cannot suppose that there is land enough in the South Polar area to supply the astounding number and gigantic size of the icebergs that infest the ocean between 50° and 70°, and this to the total exclusion of bergs of the northern type. The physical conditions of the bergs and pack in the two polar areas are wholly different, and suggest wholly different factors. Ross's constant observation was, "we had nothing like this and that in the north."

The burden of my tale is, that it is not right to speak of an Antarctic Continent at all, except as a pure speculation. If Victoria Land is a part of one, it is after all a mere speck in that vast area south of 70°, and the great rarity of earth or stones on Antarctic bergs is another very suggestive fact—quite as much as is the horizontal stratification, so uniform in southern bergs, so rare in northern. So, too, in the enormous area of the individual southern bergs, it is difficult to conceive that these should have originated in glaciers, and yet be flat-topped and horizontally stratified.

On the other hand, it is perfectly conceivable to me that the Great Barrier, which we followed for 450 miles east and west, in latitude 78° and 79°, and which, I think, is supposed to rest on bottom, may girdle the Antarctic area, and still have originated in pack ice, snowed upon for ages. All along its flanks there were bergs much lower than itself, all tabular. And, lastly, its nucleus may be an Antarctic Continent,

continuous with Victoria Land. I have often wondered whether the Australian colonies would ever send out an expedition to explore their own Polar Seas. If they had a commander who, like Ross, would dare to pierce the pack, I do not doubt but that great discoveries would follow.—Very truly yours, J. D. HOOKER.

The motions of the great southern girdle of pack ice is another phenomenon worth studying. It is suggestive of a vast open ocean. If, as in the north, it was formed near land and floated off, it would not be so uniform in position, nor, when it does move, do so in such vast areas at a time. It nowhere streams off in currents suggestive of straits, between lands farther south, but occupies thousands of square miles, where there is certainly not even an islet to hold it, presenting a barrier to navigation in water of profound depth.

WOLFCRAG, BRIDGE OF ALLAN,
29th November 1883.

DEAR SIR,—Many, many thanks for your long and most interesting letter. I hope your views as to the origin of the Antarctic ice may be correct. It would be, so far, the better for my theory of change of climate. My chief aim in writing was to show that Mr. A. R. Wallace's theory that permanent ice can exist nowhere but on high lands, and that the so-called Antarctic Continent is a lofty mountainous region, is wholly incorrect. I wished to show, from the very character of the icebergs, that his view could not be upheld. In this I hope I have so far succeeded. Your views may be correct, but certainly Mr. Wallace's are wrong.

I am greatly obliged to you for information and many suggestions which your letter contains. I have, however, a strong impression that the greater part of the Antarctic ice rests on a comparatively shallow sea-bottom, in a manner similar to what we now know was the case in the German Ocean during the Glacial epoch. I can

hardly think the ice from which the bergs are derived is floating, although it may have, as you think, originated on floating ice which may have sunk down as layer after layer was heaped upon it.—I am, yours sincerely,

JAMES CROLL.

My head is much improved of late, though reading or writing is still like a surgical operation.

3rd December 1883.

DEAR MR. CROLL,—At the risk of tiring your head, though with the hope of relieving it, I write to say that I can quite go along with you in supposing the mass of southern fixed ice to be resting on a shallow sea-bottom. The great Southern Barrier which we sailed along for some four hundred miles is such a mass. The difficulty I have is in explaining how, if it is a matter of the great southern bergs, they get detached and floated away, for, the snowfall being far in excess of the melting, when once grounded it is annually added to, and thus more irrevocably fixed to the bottom again. Off the coast of Victoria Land, which Ross describes as six to ten thousand feet high, and as "covered with glaciers projecting several miles into the sea," the soundings gave sixty to ninety fathoms, when still two and a half to four miles from shore. Now this depth of water would not float even a small berg of these seas, and along the shore, where the glaciers did project, no doubt to form icebergs, the water must be quite shallow—this shallowness extended many miles from the land. He describes *solid* fields of ice "not yet broken up for the season" on January 20! My impression is that these never would break up, but become, by successive snowfalls, giant bergs, finding depth of water as they drifted north.

Again, he describes the land ice, that is, in the pack along the coast as "blending imperceptibly with the snow that descends from the mountains and extends far into the sea." The result would be the glaciers driving the pack ice out to sea.

The Great Barrier, two to three hundred feet high, one hundred and fifty to two hundred in another place, looked as if it might be the mother of the great Antarctic bergs, forming, as it does, a vertical cliff; but "not the smallest appearance of any rent or fissure could we discover throughout its whole extent," and there were only small fragments of ice along its base. Its outer edge was afloat, however, for we found four hundred fathoms at no great distance from it. But Ross remarks, "After running a hundred miles along the barrier, we passed several heavy pieces of ice, evidently fragments of the barrier, or broken-up bergs, of which it is very remarkable we had not seen one berg during a run of one hundred and sixty miles along this barrier, from which, no doubt, some must occasionally break away." Ross adds, however, that in winter, when the air is probably 40° to 50° below zero, irregular expansion would produce separation of large masses. I doubt this. I cannot suppose that in the south the thermometer ever sinks far below zero, and if it did, and this was the effect, the bergs would not float away so fast as that we should see none of them in the following summer.

However, it is not the origin of the bergs that concerns you, but the formation of the ice. I cannot doubt but that the bergs have originated from barrier ice, and what I suspect is that this barrier ice originated in pack ice over very shallow bergs increased by successive snowfalls. The quantity of snow that falls in summer is enormous south of 50° to 60°—certainly it fell on half the days of each summer month during the three seasons we spent in those seas, and I think in one month some fell every day. Add to this that the pack in lat. 70° to 80° consolidated in February to be snowed upon ever after, and you have the conditions that would make pack into bergs along shore or wherever it was quiet enough.—Very faithfully yours, J. D. HOOKER.

N.B.—There is no summer melting of snow and ice in the Antarctic as there is in the Arctic. It is the only

region known to me where there is perpetual snow not glacier on land at sea-level.

10th December 1883.

DEAR MR. CROLL,—You are most welcome to make any use you please of my information, premising that I should like to see it first, for really I have thought little of the subject for forty years, when your interesting papers brought it up, and I was in a sad fog about it when in the Antarctic myself.

I think the whole subject is one of immense interest; in fact, I think the condition of the two polar areas is the most absorbing of our telluric problems. I quite follow your line of reasoning in respect of the land nature of the Antarctic ice and bergs, and the dispersion of the sheet from centres, probably not one polar cap, but from archipelagoes of shallows and the origin of the big bergs.

We must not, however, forget when we speak of "Antarctic land" that all we know of it which is worth the name, is a very lofty, rugged, mountainous coast, four to twelve thousand feet high, some four to five hundred miles long, from lat. 72° to 79°, namely, Victoria Land. It is after all a mere scratch on a 14-inch globe. We know nothing of its continuation W. or S. It may be part of a stupendous Mount Nucleus, from which the shallow water barrier ice radiates for hundreds of miles in all directions,—or it may be a narrow strip of mountainous land like Spitzbergen, New Zealand, etc., but its great height would rather indicate great extent. I cannot recall any coast land so high, except, perhaps, the west coast of the middle island of New Zealand. Victoria Land is not a thing we sighted and left—we spent weeks along it, and returned to it after leaving it.

Have you ever *probed* Wilkes's and D'Urville's accounts of the barriers they saw, and the various sealers' narratives (Bulley's, etc.). When I remember the rapidity with which the ice formed and closed round us in the end of February all along the foot of the barrier in 78° in a vast sheet, and the constant subsequent snowfalls, I cannot resist

the idea that such a solidified pack would, by superincumbent snowfalls, ground, become a tabular ice sheet, and break up into bergs, to be floated away and continue increasng in height till they drifted beyond melting point. Many thanks for the new pamphlet, which I shall study.—Yours very sincerely, J. D. HOOKER.

13th December 1883.

DEAR DR. CROLL,—I have read your paper on the thickness of the Antarctic ice, and I think that the general principles and conclusions are quite satisfactory. Many thanks for it. I can only demur to one or two statements of detail, and then refer to matters as to which I would venture to suggest caution.

I do not know whether any writer had laid down the broad distinction between the *pack ice* and the *true barrier ice*, of the south. In so far as any one has seen, I take it that the barrier ice is a very small matter as to extent compared with the pack, which is generally met with at 68° or north of it, and is too often miscalled barrier. Ross regarded it as very different from the pack ice of the Arctic and far more formidable, but still it was pack. It consists of broken pieces of floating ice 6 to 10 feet out of water. This was its character everywhere and is, I suppose, the accumulation of nine to ten months out of the twelve of frozen ocean water and snow 70° south of it and drifted north. No ships have even attempted to enter it but Ross's, and they went through it in two meridians, near together, and fearful work it was, totally different from boring the northern pack.

It is supposed to girdle the whole Antarctic from 65° to 70°, but I don't believe that it does. We twice rounded it coming north; Cook attained 72° without it; and Weddell 74½° without seeing it even! The great tabular bergs from the ice-sheet or barrier get entangled in it, and delayed in their northward voyages thereby. You say at bottom of p. 3 that, in the "Antarctic, we have not great masses of land in one part, and of sea in another."

This is scarcely correct; except for pack and bergs, the sea east of Victoria Land is certainly a great mass of sea, and so must the meridian be, in which Weddell went south-east of Graham's Land, and probably Cook too (p. 4). I dispute W. Thomson's assertion that the region south of 61° is continuous solid. Nor is there the smallest authority for asserting that the South Pole may be safely assumed to be in the centre of an ice-sheet. Still less can it be said that "no reason can be assigned for supposing the conditions in separate areas upon the same parallel of latitude to differ." Facts are against it even in the little we know, and what we do know is just nothing at all, considering the vastness of the area in question. Weddell, if I remember aright, did not see even ice blink ahead when he turned north!

At p. 25, you assume that barrier or cap to extend to 70°. I do not know that it does this anywhere; for what the southern navigators call the barrier is the floating pack, and no barrier at all. Again, p. 28, "The Antarctic Continent is generally believed to extend on an average from the South Pole down to lat. 70°." Now surely this is a gigantic assumption, when it is considered that we know absolutely nothing of lat. 80° to 90°, and that, with the exception of some scraps of islets, Victoria Land is the only bit of Antarctic Continent of any extent known to us, or the only other supposed considerable mass, Graham's Land, the violence of the currents rendering it almost certain that it is an archipelago.

I for one am inclined to think that, there is not so very much land south of 70°, but that, where there is, you have in some places ice-sheet or cap or barrier, just as you assume in others lofty rugged land. *We saw both.* One of my reasons is, that almost all the rocks taken for bergs were volcanic. After all, it will serve your purpose as well to assume considerable areas of low land, whether joined or not, into a continent of which we know nothing, and whether at or about the Pole.

We certainly saw more of circumpolar ice than any other, and we blockaded the pack along upwards of a

thousand miles, lat. 60° to 70°, but nowhere except in 78° did we see barrier ice, or ice-sheet on low land. Further, I think that it requires no great area of barrier ice in the cap to furnish all the bergs of the South Ocean, when you consider how slow they were, how entangled they get in the pack, how much they are added to by snow. I have seen berg masses, with the positions of all visible north of 60°, and was struck rather with the paucity than the quantity of them. Then, too, remember that they don't voyage due north, but are first driven west and then far farther east by the roaring forties which extend to 60°; in fact, they take very oblique courses northward. Even in 75°, if I remember aright, we did not see one for a day or two over the whole horizon, though we were only 140 miles from the barrier.

The upshot is, that, according to my view, there are in the South Polar area hundreds of miles of ocean covered with pack ice. Considering that the enormous area of pack ice between 60° and 70° all must have come from ocean south of it, and that it melts fast compared with the bergs, being dashed to pieces by the violence of the ocean, it follows that there is a huge area of Southern Ocean to supply it, to one of land with barrier ice. But there is still enough of the latter for your wants, that is, barrier sheets of vast thickness covering low land and shallow water, and doing what you so well describe.

Your chapters on temperature of ice, compression, friction, melting points, are most interesting, and, I doubt not, sound,

It is an old axiom not to assume more for our purposes than is absolutely necessary, and this is the burden of my tiresome song; pray forgive its discordance.—Ever sincerely yours,

J. D. HOOKER.

If there was an ice-sheet continuous up to 70°, the number of bergs would be so great that the South Ocean would be unnavigable up to 40° or 48°.

WOLFCRAG, BRIDGE OF ALLAN,
19th December 1883.

MY DEAR SIR,—Pain in the head, my old enemy, has prevented me, till now, thanking you for your two very interesting communications.

In assuming that the Antarctic ice-sheet probably extends to latitude 70°, I simply adopted the prevailing opinion on the subject amongst recent investigators into the condition of the Antarctic regions. Every chart which I remember having seen represents the permanent ice as extending down to that latitude. I, of course, do not pass any positive opinion of my own on the subject, further than I believe that the greater part of the ice is resting on shallow sea-bottoms, and that the so-called continent is probably a mass of detached land, or islands united in one continuous sheet by ice.

Since I have been obliged to live on my niggardly pension, I have had to store up my books along with my furniture in a warehouse, and go into lodgings. In this case, I am unable to give you a reference to the various articles and papers on the subject from which I borrowed the assumption as to the extent of the Antarctic ice. If you will, however, turn up Wallace's *Island Life*, p. 133, you will find a chart by Petermann. Many years ago I read Ross, Wilkes, Cook, Weddell, and others. But, at the time they wrote, no correct notions regarding the necessary physical conditions of land ice existed.

I may mention to you that in August last, Mr. Parker, M.P. for Perth, who is a very intimate personal friend of Mr. Gladstone, laid my whole case before him, along with the memorial and list of names supporting it, and earnestly requested that a small allowance might be granted out of the Civil List. But I have just learned that he will neither sanction anything from the Civil List, nor an addition to my pension!—Yours sincerely, JAMES CROLL.

BRIDGE OF ALLAN, *20th December* 1883.

Sir Joseph Dalton Hooker, C.B., D.C.L., F.R.S.

MY DEAR SIR,—I send you a copy of my proof.

You will find the part where I refer to your views at pages 101 and 102. Make any corrections, additions, or alterations which you might prefer, and let me have the copy back at your earliest convenience.—Yours ever truly, JAMES CROLL.

KEW, *23rd December* 1883.

MY DEAR MR. CROLL,—I have read your article herewith returned with the greatest interest; it seems to me unassailable (p. 85). It is no doubt these winds that tend to preserve the ice cliffs of Siberia, and of the Arctic sea-coast of North-west America. Did it ever occur to you that those ice cliffs are relics of the Glacial epoch? (p. 86). Is W. correct in supposing that, except in Greenland and Grinnell Land, there is in the Arctic regions no accumulation of permanent ice. I am aware that there is no perpetual snow at the sea-level in the Arctic regions, but I thought that the interior of the large Polar islands (?) was ice-capped.

Same page. You cannot say that the Antarctic region consists of land probably not much above sea-level, for, firstly, we know of very little land compared to the amount known of ocean. Secondly, the only considerable stretch of land known from 72° to 79° is enormously high. This, however, does not affect your argument. I can quite suppose that there are vast areas "probably not much above sea-level," buried under ice, and with barrier cliffs all round. Graham's Land is something of this sort, I think (p. 90). Is not "interaction" better than "mutual reactions on one another." I write with great diffidence to you, a physicist!

P. 96. The (?) is my father-in-law, Mr. Symonds (of Pendock).

P. 101. I cannot admit that "the Antarctic Continent" is a low dismembered group of flat islands. There may be such a continent as you guess, but no one has seen it. What Ross saw may not be the true Antarctic Continent, or even a part of it, but it is the only one ever seen or heard of. We surveyed its coast

from 72° to 79°, measured its peaks 4000 to 14,000 ft. continuously, saw far distant interior mountains, and saw no break in the land, and this by Ross and Cragie, in two ships independently surveying, and Ross the most experienced of Arctic navigators. I have many sketches of the coast line.

There is plenty of room in the Antarctic Ocean for all the flat islands you may assume, and I don't object to your assuming a great amount of those, but this ignoring the only known great mass of continuous land is to me incomprehensible.

P. 101. "Cut notches in the ice-cap." No "ice-cap" has been seen in these meridians, only loose or tight pack ice. It would be accurate to say that those masses of land "cause a southern deflection of the position of the pack."

P. 101 and lines from the bottom, "weight of evidence" —would it not be better to say that "there is much to favour the assumption, and I would assent to such a statement."

P. 103. Surely neither Patagonia nor Fuego is in a glaciated condition.

My father-in-law, Mr. Symonds, is staying in our house at Sunningdale; he too thinks your paper is a splendid one. He is very ill with cardiac asthma, and has been obliged to give up his living. He is sending you one of his tracts, and would much like any of yours that you could spare; he has read them all as they came out. I am exceedingly concerned to hear of Gladstone's obduracy—he does not love science; we must try the next Ministry. Yours is indeed a very, very hard case. We can spend any amount in providing for the comforts as well as needs of lunatics, felons, and paupers, not of incapacitated students!

I think I must get out my Antarctic journals, plans, and sketches, and take a turn at the Antarctic Ocean in a paper for the Geographical Society.—With every good wish of the season, very truly yours,

J. D. HOOKER.

I think Balleny Islands are a group *soldered* by ice.

They are also most mountainous, 10,000 ft. if I remember aright, and have barrier ice cliffs all round.

WOLFCRAG, BRIDGE OF ALLAN,
28th December 1883.

Sir J. Dalton Hooker, C.B., F.R.S.

MY DEAR SIR,—I am glad to find that you are so well pleased with my paper. Thanks for pointing out the several inaccurate statements. These I shall attend to. The want of reference to Victoria Land, both in the present paper and in the last, was an accidental omission. The papers were both written before I received your letters. The mountainous character of Victoria Land has been known to me since I read Ross's *Antarctic Regions* a quarter of a century ago.—I am, yours ever truly,

JAMES CROLL.

BRIDGE OF ALLAN, *28th December* 1883.

Sir Joseph Dalton Hooker.

MY DEAR SIR,—When I wrote in the forenoon, I quite forgot to say how pleased I was to learn that you intend drawing up a paper on the Antarctic regions, for the Geographical Society.

Would it not be well to urge on the Society the desirability of moving for an expedition to that mysterious region.

Please to express to Mr. Symonds my best thanks for his interesting memoir.—Yours ever truly,

JAMES CROLL.

CHAPTER XXIV

RESUMPTION OF PHYSICAL STUDIES

IN 1884 Dr. Croll returned with renewed vigour to his former studies on *Climate and Time*. He wrote a paper entitled "An Examination of Mr. Alfred Wallace's 'Modification of the Physical Theory of Secular Change of Climate,'" which appeared in the *Philosophical Magazine*, Series 5, xvii. 81 (No. 83). This was followed by "Remarks on Professor Newcomb's Rejoinder," which appeared in the *Philosophical Magazine*, Series 5, xvii. 275 (No. 84), and a "Further Examination of Mr. Alfred Wallace's 'Modification of the Physical Theory of Secular Changes of Climate,' Part II. Geological and Palæontological Facts in relation to Mr. Wallace's 'Modification of the Theory,'" which appeared in the *Philosophical Magazine*, Series 5, xvii. 367 (No. 85). He also wrote a paper on "The Cause of Mild Polar Climates," which appeared in the *Philosophical Magazine*, Series 5, xviii. 268 (No. 86).

On 15th February 1884, the Geological Society of London awarded to Dr. Croll part of the proceeds of the Barlow-Jamieson Fund, in recognition of his recent scientific researches as embodied in his works. The following is an excerpt from the minutes of the Society with reference to this:—

"The President then handed to Professor Bonney, D.Sc., F.R.S., for transmission to Dr. Croll, a portion of the proceeds of the Barlow-Jamieson Fund, and said—'Professor Bonney, — The Council, in recognition of the value of Dr. James Croll's researches into "The

Later History of the Earth," and to aid him in further researches of like kind, has awarded to him the sum of £20 from the proceeds of the Barlow-Jamieson Fund. Dr. Croll's work on *Climate and Time in their Geological Relations*, and his numerous separate papers on various cognate subjects, including "The Eccentricity of the Earth's Orbit," "Date of the Glacial Period," "The Influence of the Gulf Stream," "The Motion of Glaciers," "Ocean Currents," and "The Transport of Boulders," by their suggestiveness have deservedly attracted much attention. In forwarding to Dr. Croll this award, the Council desires you to express the hope that it may assist him in continuing these lines of research.'

"Professor Bonney in reply said—'Mr. President, I have been charged by Dr. James Croll to express to the Society his regret that his weak health, and the great distance at which he resides, prevent him from being present in person to-day to receive this award. He desires me to express his deep sense of the honour which is done to him in this renewed mark of the appreciation of his work; and he gives us the cheering news that, though still at times suffering, he is now able to do a little work, a proof of which, in a paper on Mr. Wallace's remarks on the "Theory of Climate," reached me yesterday. Deeply though I regret Dr. Croll's absence, I feel honoured in representing a man who has done such original, suggestive, and valuable work.'"

The following correspondence between Dr. Croll and Sir J. Dalton Hooker, F.R.S., is of much interest:—

WOLFCRAG, BRIDGE OF ALLAN,
11th January 1884.

Sir Joseph Dalton Hooker.

MY DEAR SIR,—I forgot to mention, when I wrote last, the reason why I prefer "mutual re-action of the physical causes," to the comprehensive term "interaction." Interaction, as I understand it, implies both action and *re*-action. I have already discussed the

influence of the mutual action of the agents, and I wish now to consider the other factor—the mutual *re*-actions. Were I to use the term "interaction," it might lead to confusion when considering the effects of re-action *only*.—I am, yours respectfully, JAMES CROLL.

MONTREAL COTTAGE, PERTH,
28th March 1884.

DEAR DR. CROLL,—I wish I could answer your question at all. I know no subject more hopelessly unintelligible than the Arctic explorers' accounts of the woods they found.

It is impossible to say whether they mean recent driftwood, lignites, or Tertiary deposits. I talked with M'Clure and Osborn about the Banks Land deposits, which must be very extraordinary, but could arrive at no clear idea as to their age or origin.

The woods which I examined from time to time were, as far as I recollect, pieces of drift timber—poplar and white spruce—evidently drifted down the Coppermine or Mackenzie in modern times, though possibly many centuries ago, for the conservative power of continuous cold is great.

M. Belcher's *Last of the Arctic Voyage*, vol. i. p. 380, describes the stump of a tree embedded in frozen clay, together with portions of leaves, etc., and he describes peat in the immediate neighbourhood. This was in Wellington Channel. I examined specimens brought to me, and they had all the appearance of being driftwood of *Abies alba*, white spruce. No leaves nor peat were brought, and no scientific man was present at the digging. Belcher, you know, was a notoriously untruthful man, and an officer of his ship whom I questioned pooh-poohed the story of the digging the tree out of frozen soil. Belcher was the best and most deservedly hated man of his day in the Navy, and one must not pin faith on what his enemies say of him. If his account is

correct, it is clear evidence of an Interglacial mild period, I should suppose.

I will inquire of Admiral Richards, who was in Belcher's ship, and see what he knows. Dr. Lyall, I think it was, who described the whole of Belcher's story, and it is a significant fact that the ship's boatswain, who discovered the wood, thought it was the "top-gallant mast of a ship," and the carpenter's mate, who was one of the party, was of opinion "that it was a worked spar of about eight inches diameter!"

I forget whether wood has been found with the mammoth bones in the Buckland Cliffs; if so, that would be fair evidence of a warm period,—but the elephants themselves, are we to suppose that all the bones found over so many degrees of longitude were all washed down from lower latitudes, is a great stretch. I will write again when I hear from Admiral Richards.—Ever truly yours,

J. D. HOOKER.

MONTREAL COTTAGE, PERTH,
1st April 1884.

DEAR DR. CROLL,—As I anticipated, the answers to my queries are most unsatisfactory. Dr. Lyall, who was naturalist on the Belcher expedition, writes me that he was away on a sledge expedition at the date of the discovery of the tree, and adds that Admiral B. "had a very fertile imagination."

On the other hand, Admiral Richards writes: "I perfectly remember the piece of tree; it was 16 to 18 inches long, and 6 to 7 inches in diameter, and I should say it was unquestionably fossil. I am under the impression that there were a lot of stumps standing, about the same height, in a valley or ravine, but I cannot call to mind whether I saw them, or a sketch of them by Belcher. I don't think I can be mistaken as to their fossil character. I remember carefully examining the specimen, and at one time I had some loose pieces of the petrifaction in my possession. I remember the stump lying about the

upper deck for some time, and that pieces got disintegrated off it. It was regarded at the time a remarkable discovery."

Now the specimens given to me to examine were assuredly not petrified, for I made the slices for microscopic examination with a razor! and as they were unaccompanied with any account of their position, etc. (which appeared afterwards in Belcher's narrative), it never occurred to me to treat them as anything but driftwood.

The only way I can reconcile the conflicting accounts is, that the boatswain and carpenter's mate found driftwood, and that the fossils were another story, and that Belcher has jumbled them up and sent me the driftwood, which I identified with *Picea alba*. What the fossil wood is, or what its age, it is impossible to say; it is most likely to have been Miocene.

There is a statement in Nares's voyage, to the effect, I think, that there are no recent fossil woods in the North. The book is at my place in the country. If I can find it, I will copy the passage for you.

I have read most of the Arctic narratives, and think I should have noted any account of remains that would indicate Interglacial warm periods.—Yours very truly,

J. D. HOOKER.

AUTON COTTAGE, DAVID STREET, BLAIRGOWRIE,
2nd April 1884.

Sir Joseph D. Hooker.

MY DEAR SIR,—I am greatly obliged for your interesting letter, but disappointed at the uncertainty that seems to hang over Belcher's Arctic tree. From the description of the wood given by your father, I had concluded it to be almost certain that the tree had grown in the Arctic regions. I had a chapter on the subject in *Climate and Time*, an old proof of which I enclose. Might I ask you to glance over the facts which are given, and jot on the margin any remarks which might be of

service to me? I am much interested in the subject, and would like, if possible, to examine what has been written more fully. But unfortunately I am away from my books, they being all stored up in Edinburgh along with my furniture. Can you tell me where I could get a full account of the Buckland Cliff of Siberia, containing the elephant remains? I don't remember having read the full accounts.—I am, yours sincerely, JAMES CROLL.

I shall look with interest for Admiral Richards' opinion.

KEW, *6th April* 1884.

DEAR MR. CROLL,—With every wish to find botanical evidence of Interglacial warm periods, I am unable to say that the facts you have so well marshalled are conclusive. M'Clure's great accumulation appeared to me to be that of driftwood, the bark notwithstanding. If *in situ*, they must apparently have grown one atop of another without intervening soil! for, as I understood at the time, they were piled upon one another to a great thickness—a solid mass of trunks of all sizes; this is not like growing *in situ*. The argument from the presence of bark is inconclusive, as driftwood with bark is not uncommon in the Polar islands, I believe. Nor is distance from sea of any moment. Nor incombustibility, for the soaking in salt water would account for that.

Fielding in Nares's narrative mentions seaweeds of existing Arctic species occurring in mud beds 200 feet above the sea, retaining their peculiar seashore odour. He adds, no evidence was discovered in the mud beds of Grinnell's Land to encourage the idea that any of these trees[1] had grown *in situ*, or that during the period occupied by the elevation of this country to 1000 feet it had experienced an Interglacial period during which such trees might have flourished.

Nares himself says: "In considering former reports

[1] He alludes to finding coniferous trees still retaining their buoyancy.

of the finding of fossil wood, and trees said to be *in situ*, it is noticeable that the positions where such petrifactions and stumps of trees have been found, not excepting the case reported by Sir Edward Belcher, *Last Arctic Voyage*, vol. i. p. 380, are all in the near neighbourhood of where the water currents are now collecting drift timber, and whither we could have expected them to have borne it over the land even at a lower level than it is at present, which all the data in our possession proves to have been the case in very recent geological times." No doubt the drift timber all comes from the Mackenzie and Coppermine rivers, and the greatest accumulations of it would hence be to the westward on the shores of Banks Land.

Then again, an interglacial warmth that would allow of the growth of trees more than a foot in diameter in the Polar islands must have been a startling one, and of which unmistakable traces would be found all over the Polar islands,[1] for we must bear in mind that these are not cases of deposits covered by subsequent rock beds of thickness, such as overlies the Miocene, but would be comparatively, if not actually, surface deposits. I do not know to which researches of Heer's you allude as putting it beyond doubt that the drift theory of the recent wood must be abandoned. I remember the evidence as to the Miocene plant not being drifted.

I will try and find out where Lieutenant Anjous published, and also where the best account of Buckland's Cliffs is to be had; I think, Seemann's *Voyage of the Herald.*

Probably Nordenskjöld and other Arctic explorers have described the glacial deposits that contain the mammoth bones. I am disposed to regard these bones as the best evidence of an Interglacial warm period, and that a careful examination of the ice cliffs that have under my view survived from a very early date of the Glacial period would clear up the subject.

[1] The answer to this is, that the constant ice movements would soon obliterate evidence.

I am going to Paris for a fortnight on Thursday, and shall not have time to hunt up the Arctic voyage before I go, but will as soon as I return.

It is rather against existing evidence of Interglacial periods that none have been forthcoming from Spitzbergen, the only scientifically explored Arctic land.—Yours very truly, J. D. HOOKER.

If you again have occasion to allude to the specimens of wood from Belcher, which I examined, it would be well to add that there is no proof that these came from the "fossil stump *in situ*"; they certainly were not petrified as Admiral Richards says the stump was, and which would refer it to the Miocene more probably than to Interglacial age, for wood buried in ice does not petrify.

Dr. Croll, having sent his paper on "The Physical Theory of Secular Changes of Climate" to Professor Adams, Cambridge, with reference to a calculation as to the probable amount of precession of the ecliptic, received the following reply:—

OBSERVATORY, CAMBRIDGE,
19th March 1884.

James Croll, Esq., LL.D., F.R.S.

MY DEAR SIR,—I have been so very busy lately that I have only just now had time to look at your paper for the *Philosophical Magazine*. I now find that the passage which you have marked is quite inaccurate. I am not aware that Professor Darwin has done anything new with reference to precession. Certainly the fact that the rate of precession depends mainly on the obliquity of the ecliptic at the time, is no discovery of his.

The statement that the more the obliquity, the greater is the rate of precession, is the reverse of the truth, and there is some strange mistake in reference to my calculations of the probable amount of the precession

10,000 years ago. I can only account for it by supposing that 69·87″ has been taken instead of 49·87″, which I may perhaps have given as a probable estimate, though a very rough one, of the precession 10,000 years ago. The fact is, that the rate of change of precession for very distant epochs has not yet been satisfactorily investigated. It is one of the problems which I have been intending for some time to set about.—Believe me, yours very truly,

J. C. ADAMS.

Dr. Croll sent a copy of some of his papers to the Rev. Mr. Baxter, and announced his intention to abandon physical science and resume the study of metaphysics.

BURNBANK ROAD, HAMILTON,
5th November 1884.

Rev. G. C. Baxter.

DEAR SIR,—Will you please accept of a copy of one or two papers written recently? They are somewhat controversial, being intended to remove some misapprehensions regarding the "Physical Theory of Secular Changes of Climate," a subject on which I have been working for the past twenty years. I intend abandoning physical studies, and returning to my old and favourite subject, metaphysics, and the one or two papers sent are, I hope, about my last words in physics.

When I take up house again and get into my library, I shall send you any of my former papers which I may find remaining.—I am, dear sir, yours sincerely,

JAMES CROLL.

HAMILTON, *6th November* 1884.

Rev. G. C. Baxter.

DEAR SIR,—I find I have a spare copy of a paper bearing on one of my old and favourite lines of thought. I have sent it, as I have an impression some of the ideas might interest you.

We shall probably leave this place for Rothesay before the dead of the winter.—Yours very sincerely,

JAMES CROLL.

Towards the close of the year 1885, Dr. Croll resumed his astronomical studies, as bearing on climate and time, with special reference to the sun's heat and effects thereof. He followed this up during the years 1886–9, by an investigation into the origin and action of nebulæ in the solar system; and during these years, his attention was devoted, at such intervals as he was able to read and dictate, to writing out the results of these investigations, and to putting down on paper his thoughts on philosophy. By this time his health had become so enfeebled that he was unable to do more than about an hour or two's work at any time, and even that was only accomplished by the aid of an amanuensis, who read to him, and to whom he dictated such short passages on both subjects as he could compose without bringing on the pain in his head. After moving about for five years from place to place in an unsettled condition, with no permanent home, he longed for this to get rest. Having during the time obtained, through the kindness of friends, a little increase to his income, he resolved on taking up house again. He naturally gravitated to the quiet, ancient city of Perth, near his own native place, and was fortunate in obtaining a lease of a comfortable house in the suburbs of the city. At the end of the summer of 1886, he accordingly took up his permanent abode there.

From Perth he wrote to Professor Darwin, Cambridge, sending a copy of *Climate and Cosmology*, on which the following correspondence ensued:—

5 PITCULLEN CRESCENT, PERTH,
3rd November 1886.

Professor Darwin, M.A., F.R.S.

MY DEAR SIR,—I have read in *Nature*, with

much interest indeed, your address at Birmingham, and feel greatly obliged for the prominence you have given to my speculations. It is very encouraging. If your address has been printed in a separate form, I should feel obliged for a copy, if you have one to spare.

Will you please to accept of a copy of my recent book, *Discussions on Climate and Cosmology*, which I have forwarded by parcel post? If you can find time, I should like you to read Chapter viii. I have there shown that the facts of geology are wholly opposed to Mr. Wallace's modification of my theory. Facts show that the more intense were the cold periods of the Glacial epoch, the more *warm* and *equable* were the Interglacial periods, and that as the cold periods became less severe, the corresponding Interglacial periods became less equable. Professor James Geikie's *Prehistoric Europe* contains a perfect store of facts on this point. It is strange that many geologists are so reluctant to admit Interglacial periods, which so much upset the theories of climate.—Yours sincerely,

JAMES CROLL.

NEWNHAM GRANGE, CAMBRIDGE,
7th November 1886.

MY DEAR SIR,—I beg leave to thank you for your great kindness in sending me a copy of your book. I will begin my reading of it with the chapter you point out.

I cannot see as yet that Wallace betters the case in any one point; on the contrary, it seems worse.

I see Ball has recently made a pronunciamento in your favour. For myself, I hope you are right; but, as I say in my address, the matter is very complex, and must remain doubtful at present. I send you a copy of the address by post.—Yours very sincerely,

G. H. DARWIN.

5 PITCULLEN CRESCENT, PERTH,
9th November 1886.

Professor Darwin, M.A., F.R.S.

MY DEAR SIR,—Thanks, many thanks for the two copies of your address. What you say as to Professor Ball's opinion excites my curiosity. If he has published anything on the subject, will you kindly give me a reference to it.—Yours truly,

JAMES CROLL.

The "pronunciamento" here referred to was made by Sir Robert Ball in a paper read at the Royal Irish Academy on 14th May 1886, in which he says: "The following calculation has convinced me that Mr. Croll's theory affords an adequate explanation of the Ice age." He then proceeds to give the geometrical formulæ establishing this proposition, which will be found in *Nature*, vol xxxiv. p. 607.

On Sir Robert Ball's proposition being challenged by one of Croll's critics, Mr. W. H. Monck, in October 1896, Sir Robert wrote the following reply:—

"In your issue of November 4, p. 7, my friend Mr. W. H. S. Monck asks one or two questions relative to the paper on 'The Astronomical Theory of the Great Ice Age,' which you did me the honour to reprint.

"I take as a convenient unit the mean daily sun heat on one hemisphere. The amount of this unit is indicated by the fact that it continuously maintains the earth's temperature some 300° more or less above what it would be were the sun's heat withdrawn.

"The calculations I gave showed that in the glacial winter, the mean daily receipt of heat sunk to ·68 of a unit, while in the brief glacial summer, the mean daily receipt was 1·38 unit.

"Considering the magnitude of the unit, it is obvious that fluctuations like this must correspond to vast climatic changes of the kind postulated in the Ice age.

Here, it seems to me, lies the great originating cause of the Ice age, and to dwell on the minor phenomena merely obscures the real point.

"If it be said that no great climatic change takes place because the total sun heat in the year remains the same, then I remark, as I did at the Royal Institution, that on this principle it would be the same thing to give a horse 15 lbs. of oats a day for six months, and 5 lbs. a day for the other six months, as to give him 10 lbs. of oats a day all the year round.

ROBERT S. BALL.

THE OBSERVATORY, CO. DUBLIN, 11*th November*."

The following are the remarks made by Professor G. H. Darwin at the British Association meeting at Birmingham in 1886, referred to in Dr. Croll's letter:—

"Mr. Croll claims to prove that great changes of climate must be brought about by astronomic events of which the dates are known or ascertainable (*Climate and Time*). The perturbation of the planets causes a secular variability in the eccentricity of the earth's orbit, and we are able confidently to compute the eccentricity of many thousands of years forward and backward from to-day, although it appears that in the opinion of Newcomb and Adams no great reliance can be placed on the values deduced from the formulæ at dates so remote as those of which Mr. Croll speaks. According to Mr. Croll, when the eccentricity of the earth's orbit is at its maximum, that hemisphere which has its winter in aphelion would undergo a Glacial period. Now, as the date of great eccentricity is ascertainable, this would explain the great Ice age and give us its date.

"The theory has met with a cordial acceptance on many sides, possibly to a great extent from the charm of the complete answer it affords to one of the great riddles of geology.

"Adequate criticism of Mr. Croll's views is a matter of

great difficulty on account of the diversity of causes which are said to co-operate in the glaciation. In the case of an effect arising from a number of causes, each of which contributes its share, it is obvious that, if the amount of each cause and of each effect is largely conjectural, the uncertainty of the total result is by no means to be measured by the uncertainty of each item, but is enormously augmented. Without going far into details, it may be said that these various concurrent causes result in one fundamental proposition with regard to climate which must be regarded as the keystone of the whole argument. That proposition amounts to this, that climate is unstable.

"Mr. Croll holds that the various causes of change of climate operate *inter se* in such a way as to augment their several efficiencies. Thus the trade winds are driven by the difference of temperature between the frigid and torrid zones, and if from the astronomic cause the northern hemisphere becomes cooler, the trade winds on that hemisphere encroach on those of the other, and the part of the warm oceanic current which formerly flowed into the cold north zone will be diverted into the southern hemisphere. Thus the cold of the northern hemisphere is augmented, and this in its turn displaces the trade winds further, and this again acts on the ocean currents, and so on, and this is neither more nor less than instability.

"But if climate be unstable, and if, from some of those temporary causes for which no reasons can as yet be assigned, there occurs a short period of cold, then surely even an infinitesimal portion of the second link in the chain of causation must exist, and this should proceed as in the first case to augment the departure from the original condition, and the climate must change. In a matter so complex as the weather, it is at least possible that there should be instability when the cause of disturbance is astronomic, whilst there is stability in an ordinary sense. If this is so, it might be explained

by the necessity for a prolonged alternation in the direction of prevailing winds, in order to affect oceanic currents.

"However this may be, so remarkable a doctrine as the instability of climate must certainly be regarded with great suspicion, and we should require abundant proof before accepting it. Now there is one result of Mr. Croll's theory which should afford almost a crucial test of its acceptability. In consequence of the precession of the equinoxes, the conditions producing glaciation in one hemisphere must be transferred to the other every 10,000 years. If there is good geological evidence that this has actually been the case, we should allow very great weight to the astronomical theory, notwithstanding the difficulties in its way. Mr. Croll has urged that there is such evidence, and this view has been recently strongly supported by M. Blytt (*Nature*, July 8 and 15, 1886). Other geologists do not, however, seem convinced of the conclusiveness of the evidence.

"Thus Mr. Wallace (*Island Life*), whilst admitting that there was some amelioration of climate from time to time during the last Glacial period, cannot agree in the regular alternations of cold and warm demanded by Mr. Croll's theory. To meet this difficulty he proposes a modification. According to his view, large eccentricity in the earth's orbit will only produce glaciation when accompanied by favourable geological conditions. And when extreme glaciation has once been established in the hemisphere which has its winter in aphelion, the glaciation will persist at some diminution of intensity when precession has brought round the perihelion to the winter. In this case, according to Wallace, glaciation will be simultaneous on both hemispheres.

"Again, he contended that if the geological conditions are not favourable, astronomic causes alone are not competent to produce glaciation."

CHAPTER XXV

DISCUSSIONS ON CLIMATE AND COSMOLOGY—CLOSE OF PHYSICAL STUDIES

IN 1885, Dr. Croll had another serious illness, which confined him to the house for a considerable time. The slow, weak action of the heart, and his desire for constant open air exercise, were not very compatible with each other. He tried to go out every day; and in the winter he caught cold, which brought on certain dropsical symptoms, which however, with rest and judicious medical treatment, passed off. He was able to write a paper on "Arctic Interglacial Periods," which appeared in the *Philosophical Magazine*, Series 5, xix. 30. This was the last original paper he wrote on these climatic subjects on which he had earned so great a reputation. During the year he collected all the papers he had written on climatic subjects since the publication of his great work, *Climate and Time*, in 1874. These he revised down to date and published in the end of 1885, under the title *Discussions on Climate and Cosmology*.

This completed his work on geology and physics, and he quietly, but deliberately, closed his reading and writing on these subjects, which had engrossed his attention for a period of twenty-five years, in which he had earned a world-wide reputation, reaped the richest rewards in honours which he could desire, and through which he formed friendships with the most distinguished scientists of the time. He did this to turn to his favourite theme of metaphysics; and now he resumed the

subject, of which he had never completely lost sight since his earliest manhood.

BURNBANK ROAD, HAMILTON, *29th April* 1885.

DEAR DR. MORISON,—Glad to have a letter from you again and to learn that you are well. If the weather continues good, Mrs. Morison and you will enjoy Prestwick.

The severe pain in the leg turned out not to be sciatica. I don't know what it was, but it terminated with swelling and water in the feet and ankles. They are getting better, and I hope soon to be able to get on my boots.

I have not seen anything of the author to whom you refer, but have taken a note of the volumes. I am done with reading and writing in physics, and am looking forward with pleasure to my old studies after I get a long rest.

I have a volume supplementary to *Climate and Time* ready for the press. I am just arranging with the publisher, and wish I saw the last proof out of hands. —With kindest regards to Mrs. Morison and yourself, I am, yours sincerely,

JAMES CROLL.

AVON COTTAGE, STRATHAVEN, *22nd August* 1885.

DEAR DR. MORISON,—I have just finished a rough draft of my Preface, but am not altogether pleased with it. It is a sort of thing I am not good at.

If it is not too much to ask, would you kindly look over the MS. and make any corrections or alterations which you may think necessary or advantageous.

As I now finally abandon the whole subject of climate, I have added a paragraph to that effect. It will prevent me having to reply to any criticisms on the book.—With our kindest regards to Mrs. Morison and yourself, yours sincerely,

JAMES CROLL.

The *Saturday Review*, October 1887, in criticising *Climate and Time in their Geological Relation: a Theory of Secular Changes of the Earth's Climate*, and *Discussions on Climate and Cosmology*, said—

"We hope it is not too late to refer to the handy reprint of Dr. Croll's classical *Climate and Time*, and to his new companion volume, entitled *Discussions on Climate and Cosmology*. We cannot venture here to re-discuss Dr. Croll's well-known theories. In the new volume he himself endeavours to reply to the many criticisms which these have called forth. Some of these criticisms were fair and honest enough, and difficult to answer. Others, we regret to say, bore evidence of strong prejudice, and even personal animosity. Whatever objections may be fairly taken to certain of Dr. Croll's positions, every honest scientific investigator will admit that his writings have had the most radical influence on cosmological speculation. In certain directions his influence has been nearly as great as that of Darwin's in biology. It is now allowed that, in speculating on the past condition of the earth, he has laid too much stress on cosmical causes, and made too little allowance for simple but influential geographical changes. But when all deductions are made, there can be no doubt that Dr. Croll's name will in the future be allotted a very high position in the science of our time. It will certainly be a reproach to our generation that a man who has spent his life in the disinterested pursuit of pure science, and that with such magnificent and far-reaching results, should, when incapacitated by infirm health and old age, be allowed only the miserable retiring pittance due to him as a servant of the Geological Survey—a pittance about equal to the hire of a day labourer."

Dr. Croll sent copies of *Climate and Cosmology* to Sir Joseph Dalton Hooker and Mr. Alfred Russell Wallace, upon which the following correspondence ensued:—

ROYAL GARDENS, KEW,
11th November 1885.

DEAR DR. CROLL,—I am much gratified by your sending me *Climate and Cosmology*, which I shall at once read, and, I need not say, with great interest; for, though I had read the articles, I never put them together before me.

I have been thinking much of Antarctic ice, and, looking over my sketches, I cannot account for the small, often very small, tabular bergs, all along the coast of Victoria Land, and of the Barrier, otherwise than as the wreck of great floes formed by snowfalls. Of course, these do not interfere with the (?) tabular bergs (?) of the Barrier.

My own impression still is that the Antarctic area is a water one in the main. How else can one account for the enormous area of the pack, which in continuous sheets or island-like masses, it occupies. This prodigious development of pack, extending in some meridians to 58°, and which certainly originates in (?) ice formed south of 70°, must require a prodigious area south of 70° of water for its formation, and it is all drifting north.

Whatever land there is, is no doubt ice-capped, but the excessively rugged outline of the coast of Victoria Land, with peaks and precipices, the latter showing black cliffs several thousand feet above sea-level, proves that there is no such ice-cap there as in Greenland. This rugged and lofty broken outline was the character of the coast for 7° = 500 miles, but contrast this with any 500 miles of the Greenland coast with its ice-cap. Victoria Land precisely resembles the Alps snow-clad to their base. The Barrier, only reached in 78°, is another matter. It is the edge of an ice-field, whether originating either as the seaward margin of an ice-cap, or the terminus of a huge glacier occupying a huge broad bay, is uncertain. I incline to the former—it is immaterial whether the land it covers is island or continuous. What I do stick to is, that were there such an ice-cap in the longitude of Victoria Land, as there is in Greenland, the contour of the

Victorian Land coast would be impossible. The fact of Ross having found open sea in 78° and Weddell in 74° shows how much water there is in the south, but the so-called barrier of most Antarctic voyages, met with in 62°–70°, is only the edge of the pack, which Ross alone penetrated, or even attempted to penetrate, and he had his reward by finding open sea beyond it,—where Wilkes had written land. The fact is, we know nothing of the Antarctic regions south of 70°, except Victoria Land, and it wants a more careful consideration than it has ever had. I don't believe W. Thomson's speculation that it is a chain of islands, and it is far too continuous, lofty, and rugged, to be considered ice-capped, though it is ice-*clothed*.—Ever truly yours,

J. D. HOOKER.

I forwarded the petition to Lord Salisbury a month ago with a private letter, and he answered that he sent it to Lord Iddesleigh.

13th November 1885.

DEAR DR. CROLL,—Has it ever been noticed that the bifurcation of the great ice floes of Norway, as represented in your map (p. 133), in the German Ocean, is over the Dogger Bank? I take for granted that it has, but when I pointed it out to Professor Judd, it was new to him.

You have demolished Wyville Thomson, who appears to me to have known as little of the physics of sea as he did of the geography of the Antarctic land.—Ever yours truly, J. D. HOOKER.

FRITH HILL, GODALMING,
12th December 1885.

Dr. James Croll, F.R.S.

DEAR MR. CROLL,—I have been reading, with very great interest and pleasure, the two last chapters of your book on the origin of sun's heat and of nebulæ. Your

theory of the light and heat of all suns, stars, and nebulæ being due to arrested motion seems to me most simple, suggestive, and probable, and supported by a mass of weighty fact and argument. It seems the complement of the molecular theory of gases, the molecules in this case being the vast molar masses of the stellar universe.

There is, however, a point connected with your discussion of the age of the sun's heat and of geological time that does not seem to me so clear. You say that the sun can only have been giving heat at the present rate for 20 million years if heat is due solely to gravitation, and that geology requires far more than 20 million years. Your arrested motion gives any possible amount of heat to begin with, and thus you get a longer period of emission.

But this seems to me rather beside the point. Whatever heat the sun originally had, it had cooled down to the present temperature by radiation, losing heat less and less rapidly as it became cooler. In doing so, it must have passed through the point at which life, such as we find in the earlier Palæozoic rocks, first became possible, say a temperature such as would have heated the ocean to 120° or 150° F. Would not the time from that epoch to now be a *fixed* period, whatever the primitive heat of the sun? If so, this period is what we want to arrive at, not the whole period during which the sun has been giving out heat, which has no bearing on the problem. Thus let H be the heat of the sun at starting, H – N the heat now. It has cooled according to some definite law.

H . . H—a . . H—3 . . H—c . . . . . H—N

If H—c represents the temperature at which life became possible on earth, then the time H—c . . . to H—N is what we want, and whether the time between H and H—c was 5 million or 5000 million years makes no difference whatever. Another limiting factor is the time required for the earth to cool from the highest possible life temperature to present temperature. This

you allude to at end of Chapter xviii. as not calculable within sufficiently close limits, but you do not, I think, refer to the fact that the time of the sun's cooling, from the limits of terrestrial life temperature to present temperature, is not affected by the total store of its original heat. This point seems to me to want further elucidation.—Yours very faithfully,

ALFRED R. WALLACE.

8 BATTERY PL., ROTHESAY,
19th December 1885.

DEAR MR. WALLACE,—I am delighted to find that you are so well pleased with my theory of the origin of the sun's heat. It is very encouraging. Your considerations strengthen my arguments that the gravitation theory cannot account for the sun's heat. Assuming it as correct that gravity could only have supplied the present rate of radiation for 20 million years, then the time from H—c, when life became possible, to H—N, the present time, must be far less than 20 million years, for the rate of radiation at H—c was greater than at present. Geology, however, proves that the time in question must have been more than twice 20 million years.

Your difficulty seems to arise, I think, from your not taking into account the fact that the length of time between H—c and H—N will depend upon the nature of the cooling process. Were the sun solid, or possessed of a solid crust like that of the earth, its rate of cooling would depend mainly on the conductive power of this crust for heat. Were the crust a bad conductor, the surface would rapidly cool down, and the store of internal heat would last for countless ages. Were it, on the other hand, a good conductor, like copper, the surface would remain long hot, and the store would, of course, be sooner exhausted. The sun, however, is not solid, but a gaseous mass, and cools by an entirely different process. Owing to its gaseous condition, and the way

in which it cools, into the consideration of which I need not here enter, the temperature of the surface decreases with excessive slowness.

The great length of the interval between H—c and H—N is mainly due to three circumstances—(1) the great mass of the sun; (2) the fact that this mass is in a gaseous condition; and (3) the enormous store of heat that this mass possesses.

The *store of heat* is therefore an important factor. I have left physics once and for all, I trust, and hope I may be able to do a little at my old favourite subject, though terribly crippled by pain in the head.—Yours sincerely, JAMES CROLL.

Dr. Croll did not publish anything between 1885 and 1889. In 1887 he was busy working at *Stellar Evolution* and *The Philosophical Basis of Evolution.* Owing to the state of his health, progress with these works, which were both of a very abstruse, speculative nature, was necessarily slow. He worked, however, very consistently at them, and did not allow himself to be distracted either by correspondence or the discussions which were going on in science. There is therefore little to record in 1887. The following letter from Professor M'Farland, and Dr. Croll's reply, however, show that he had not lost his interest in science.

MIAMI UNIVERSITY, OXFORD, OHIO,
20th March 1887.

Dr. James Croll, Edinburgh.

MY DEAR SIR,—With the College above named I was connected from 1856 to 1873—was then transferred to the University at Columbus, O., during the closing of this on account of financial embarrassment—and on the reopening here in 1885, I was asked to take charge and resuscitate the College. In this work I have been so much occupied that I write little for the journals, and my correspondence is chiefly on business. Oxford is in

the south-west corner of Ohio, near Cincinnati. We take our name from the English town. Our country is large, as you know. Oxford is 120 miles from Columbus. Dr. Orton in his geological tours reaches our village occasionally—was here a few weeks ago. I send you a copy of a little astronomical journal printed 600 or 700 miles north-west of Oxford. In this country we have a good many men whose chief business it is to "object." It is not always important, in their judgment, to be conversant with the theory to which they object—their business is *to object.* You will notice a little article over my name replying to one of these unfledged objectors. They would be amusing if they were not annoying. I send you our last catalogue, also a photograph of myself—at my daughter's request.

Hoping that you may be in the enjoyment of fair health now, I am,

R. W. M'FARLAND.

5 PITCULLEN CRESCENT, PERTH,
6th April 1887.

MY DEAR SIR,—I am delighted to have a letter from you again, and glad to find that you are well. I had long been looking for one. Many thanks for the copy of *The Sidereal Messenger* containing your short and telling reply. I confess I have not read Woekoff's paper. I glanced over a page or two, and, observing that he did not appear to have grasped the problem, I laid it aside. I have requested the publishers to send you a copy of my recent book, which please to accept. I am much pleased to have your photograph, and also the catalogue of the University. The education afforded by your University is of a high order certainly, and I have little fear but that the institution will prosper under your care.

I am glad to say that my health has much improved since I left the Survey, although I am still very much troubled by my old complaint, pain in the head. I get

one to do my reading and writing, which helps me greatly.

I have now settled permanently in Perth, and have returned to my old studies in philosophy, in order to finish some work which has been laid aside for upwards of a quarter of a century.

I shall be delighted to have a letter now and again when anything turns up of interest.—Yours sincerely,

JAMES CROLL.

Having got settled in a house, he thought he would like to visit his native village, and the Rev. Mr. Baxter having invited him to spend a day, the following is the reply:—

5 PITCULLEN CRESCENT, PERTH,
12th July 1887.

DEAR MR. BAXTER,—Many thanks for your kind invitation. We shall be delighted to come through on Tuesday the 19th, and spend the day amongst my old and esteemed friends. We shall leave after breakfast and will probably reach you somewhere about ten o'clock.

Perhaps the Thomsons, or some one in the village who has a horse, will give us a drive down to Cargill Station in the evening to catch the 8.31 train for Perth. —With kindest regards, yours sincerely,

JAMES CROLL.

Mr. Baxter alludes to this visit in a note from him which is given later on. Dr. Croll again writes him as follows:—

5 PITCULLEN CRESCENT, PERTH,
5th September 1887.

DEAR MR. BAXTER,—I observe the Christian Conference is to be held on the 13th, 14th, and 15th inst.

Mrs. Croll and I will be very much pleased if you will take up your abode with us during the time. As

we have but one spare bedroom, I am sorry, as I mentioned to you before, that we cannot well invite your father to come along with you. The bed, however, as I have stated, is a wide one.

Come up *both* and take some dinner with us. I suppose the better plan will be to dine at one o'clock and tea in the afternoon. Or would you prefer dinner at four? Either time is the same to us.—With kind regards, yours sincerely,

JAMES CROLL.

5 PITCULLEN CRESCENT, PERTH,
23rd November 1887.

DEAR MR. BAXTER,—If the *Athenæum* is of any interest to you, I can let you have it. (The copy is Dr. Bower's.) When he is done with it, he hands it to me, and as he does not require it back, he lets me send it to any one I choose. I mentioned your name to him, and he was much pleased. It, of course, will cost you nothing, and you will not require to send it back. Hoping you are well, and with kind regards, yours sincerely, JAMES CROLL.

5 PITCULLEN CRESCENT, PERTH,
21st December 1887.

Shadworth H. Hodgson, Esq., LL.D., etc.

MY DEAR SIR,—I am greatly obliged to you for being so good as to favour me with a copy of your valuable address. It is on a subject in which I am specially interested.

I have for the past thirty years been in search of a copy of Anthony Collins's famous *Philosophical Enquiry concerning Human Liberty*, published, I think, without his name in 1716. I have repeatedly advertised in the *Bookseller*, but to no purpose. This is strange, seeing that the book went through at least three editions. I have Dr. Samuel Clark's reply to Collins, published in 1717.

Can you oblige me with the address of some London second-hand bookseller who might be likely to have Collins's book.

Excuse this not being in my own handwriting.

JAMES CROLL.

45 CONDUIT STREET, REGENT STREET, LONDON,
26th December 1887.

DEAR SIR,—I write (from the country) to say that I think you will find Mr. John Wilson, 12 King William Street, The Strand (not City), London, W.C., a very good second-hand bookseller for such books as that you are in search of. He will, I feel sure, do his best to find the book for you, if you mention my name; I have long been a customer of his. I am sorry to see you are obliged to employ an amanuensis.—I am, very truly yours,

SHADWORTH H. HODGSON.

CHAPTER XXVI

STELLAR EVOLUTION

NO better evidence can be adduced of the sympathy which was felt for Dr. Croll by scientific men regarding the manner in which he had been treated by the Treasury, and the esteem in which he was held, than is shown by the following letter, honourable in its tone and terms, both to the learned President and the Society over which he presided:—

HURSTLEIGH, KEW, *24th January* 1888.

MY DEAR DR. CROLL,—The circumstance having been brought to my knowledge, as President of the Geological Society, that the long and unfortunate delay about the Civil List pension was causing you inconvenience, I communicated with the Committee of the Scientific Relief Fund, privately, upon the subject. In reply, they inform me that, owing to recent calls upon the funds at their disposal, their balance is at present very small, but that balance they at once awarded to you. In forwarding it, allow me, on the part of the Committee, as well as myself, to express our regret at the smallness of the amount, and our earnest hope that your great claims may ere long be recognised by your name being placed upon the Civil List for an adequate pension.—Believe me to remain, yours very faithfully,

JOHN H. JUDD.

Dr. Croll was now busy on *Stellar Evolution*, and was desirous of seeing *World-Life*, the work of Professor

Winchell of America, which he could not obtain here, whereupon the following correspondence took place between them:—

5 PITCULLEN CRESCENT, PERTH, SCOTLAND,
21st April 1888.

Professor Alexander Winchell.

DEAR SIR,—It is only quite recently that I became aware of the existence of your important work, *World-Life*. Being engaged at present on an article on the "Nebular Hypothesis," I am reluctant to let the article out of my hands before having the pleasure of seeing your book, and have, therefore, made every effort to get hold of a copy, but without success. I wrote to all the principal libraries in this country, where such a book is likely to be found, but it seems that none of them possesses a copy. I would order a copy through a bookseller, but I could not receive it through that source in time to serve my purpose. Might I ask if you would kindly let me have a copy by return of book post, in exchange for a copy of my recent book, *Climate and Cosmology*, which I forward to you by to-day's mail. I would have sent you a copy of *Climate and Time* also, had it not been that you are likely to have one already. But, should this not be the case, let me know, and I shall let you have one.—Yours very truly,

JAMES CROLL.

ANN ARBOR, *7th May* 1888.

Professor James Croll, LL.D., Perth, Scotland.

MY DEAR SIR,—Your esteemed letter of 21st April reached me to-day. Your *Climate and Cosmology* arrived three days ago, just as I was starting on a journey, and I took it with me for a companion. I have read about one-third of the book, and embrace this opportunity to express my satisfaction that you have so easily and conclusively answered Professor Newcomb's criticisms of your theory. Your *Climate and Time* came to my hands for review in 1876, and I reviewed it in the

International Review, New York. I was fascinated by your reasonings, the gist of which I attempted to condense in my article, a copy of which I send you.

It gives me pleasure to send you a copy of my *World-Life*. In fact, I had determined to do this before receiving your letter and request. The book was a large undertaking, as Professor G. H. Darwin says (in *Nature*), but I felt that it needed to be done and I felt like doing it. I am quite aware that I have made some slips, and one or two quite unaccountable ones, since I have not expressed the views which I entertained. But I hope you may find something to approve.

As you are a well-known geologist, I beg to send you also a copy of my *Geological Studies*, and I would be gratified by your candid judgment on the method which I have pursued in this text-book.

With the view of introducing myself more fully, I take the liberty to mail also a few pamphlets recently from my pen on scientific and philosophic questions, with one of an exegetical character. To indicate that I am actually in the field as a geologist, I mail the *Annual Report on the Geology of Minnesota* for 1886. I shall have one also for 1887.

I hope you will not feel deluged or embarrassed by this plenitude of trifling acknowledgments of the gratitude which I feel for your large views of the action of geological causes and the pleasure which I have received in gaining casual insight into your character.

I am dreaming of visiting Scotland this season on occasion of the meeting of the International Congress at London. I have a special problem to investigate concerning the age and affinities of the upper portion of the Old Red Sandstone. Its assumed Devonian age has ranged, as Devonian, in America, certain sandstones, which on other grounds, I think to be Carboniferous. Your *Holoptychius*, for instance, has been found also in America. But I am not quite certain of being able to cross the Atlantic this summer.—Very sincerely yours,

ALEXANDER WINCHELL.

5 PITCULLEN CRESCENT, PERTH,
25th May 1888.

Professor Winchell, LL.D.

MY DEAR SIR,—Your two volumes and separate papers, which you have so kindly sent me, came duly to hand on the 18th inst., for which please accept my most sincere thanks.

World-Life is a most interesting and important work, and I promise myself a treat in the reading of it. It must have cost you a very great deal of study and research. It is a pity the book is so little known in this country, as it is one much required.

Your *Geological Studies* must prove a useful manual, and some of the papers you sent are on subjects that interest me much.

By the way, you style me in the book a professor. I am only a plain, self-educated man; and although I was a good few years on the Geological Survey, yet I know but very little about geology proper, my studies being principally in physics and philosophy, but, owing to pain in the head, from which I have long suffered, I do not do much in either.

I am glad to learn that you purpose visiting Scotland, and I hope you will have time to give me a call, though I cannot promise you much that will interest you in the way of geology.

Some thirty years ago I read with much interest a work on the Will, of three volumes, by a professor in your University, Henry P. Tappan. I suppose he won't be alive.—Again thanking you for your esteemed favour, I am, yours very sincerely,

JAMES CROLL.

In connection with Stellar Evolution, Dr. Croll wrote to Professor Darwin for information on the subject. Croll's first letter has unfortunately not been preserved, but the letters here given sufficiently elucidate the point he wished cleared up.

NEWNHAM GRANGE, CAMBRIDGE
24th October 1888.

MY DEAR SIR,—I do not think your solution will hold. It would do so, of course, if it were not for the mutual attraction of the various bodies revolving about the sun. Laplace has indeed shown that a certain comet which got entangled with Jupiter probably made two revolutions round the planet and then required a hyperbolic velocity and was expelled from the solar system. Whether or not this was really the fate of that comet, such a thing might happen.

The problem of the three bodies is an unknown field as yet, and the ellipse, hyperbola, and parabola are only orbits described when there are only two. The only cases of three bodies as yet solved and that approximately, is where the perturbing influence is small.—Yours very truly,

G. H. DARWIN.

5 PITCULLEN CRESCENT, PERTH,
5th November 1888.

Professor Darwin, F.R.S.

MY DEAR SIR,—I am greatly obliged to you for being so good as to answer my query. I am still under a difficulty. The case of the comet I used simply to illustrate what I had always regarded as a general principle in celestial mechanics. But from your letter I am beginning to fear that probably I may be wrong.

The principle to which I refer is this: If a body at absolute rest is drawn into a system, say our solar system, by the attractive force of all its members, the body can never acquire a velocity which will carry it out of the system into boundless space; for to suppose this would be to suppose that the attractive energy which drew the body towards the system was greater than the attractive energy which drew the body back, which I suppose is certainly not the case. If the comet of Laplace to which you referred had a projected velocity before it

was drawn into our solar system, then I can understand how the attractive force of Jupiter could have deflected the comet and made it pass out of the system in a hyperbolic path. My difficulty is to understand how Jupiter could have done this, had the comet been previously moving in a closed curve.

I hope you will pardon me for again troubling you, as it is a point in which I am at present very much interested.—Yours ever truly,

JAMES CROLL.

NEWNHAM GRANGE, CAMBRIDGE,
8th November 1888.

MY DEAR SIR,—I do not think your conclusion is correct.

The solar system has a certain amount of total energy, part being potential—the sum of the products of the masses two and two divided by their mutual distances—and part kinetic. The proportion which is potential and the kinetic vary from time to time as the bodies move. I leave out of account rotations and frictions, and suppose the system "conservation," so that the total energy is constant. Now, if another body falls from rest at infinity, and is entangled for a time in our system, and afterwards is expelled with high velocity, all that the principle of energy teaches is, that after its expulsion, the total energy of our system is less than it was before. For when the body is again at infinity, it possesses kinetic energy, and thus must have been abstracted from our system.—I remain, yours very sincerely,

G. H. DARWIN.

5 PITCULLEN CRESCENT, PERTH,
12th November 1888.

Professor Darwin, F.R.S.

MY DEAR SIR,—I am again greatly obliged to you for being at so much trouble in answering my queries,

I can gather now from your letter that the principle to which I referred is perfectly correct, only I had incautiously expressed it in too unqualified terms. Of course, I believe that there is energy enough in our solar system to expel any number of comets, and that if Jupiter in his orbital motion should have struck the one to which Laplace referred, it would undoubtedly without much trouble have projected it into infinite space.

In the principle to which I referred, I took into account only the direct effect of gravity: that a body at rest falling into our solar system, if not struck by any of the planets in their orbital or rotary motion, would pass through the system to the same distance from which it fell. The attractive force of the whole system would not carry the body an inch beyond that distance, and consequently, if Laplace's comet passed out of the system, it must have either been *struck* by Jupiter, or it must have possessed a projected motion prior to its entering our system.

I think you will from this understand what I really mean, and I have little doubt you will agree that the principle in the sense in which I understand it is correct. Should it, however, really be the case that I am still wrong, perhaps you will kindly again send me a few lines and let me know.—Yours very sincerely,

JAMES CROLL.

13th November 1888.

Impact is absolutely excluded from Laplace's conclusion. I do not think you are right. A body entering the solar system may run across planet after planet in such a way that its velocity is augmented by each, and it at last may get up such a speed as to get expelled. I shall be glad to write again if you wish it.

G. H. DARWIN.

5 PITCULLEN CRESCENT, PERTH,
15th November 1888.

Professor Darwin, F.R.S.

MY DEAR SIR,—Thanks, many thanks, for the post-card. I see I am still wrong. "A body entering the solar system," you say, "may run across planet after planet in such a way that its velocity is augmented by each, and at last may get up such a speed as to get expelled."

Would you kindly tell me in *ordinary language* how this is effected? I had been under the impression that the motion transferred would have all been restored to the system, and that the body would have been held a prisoner. It is one of those subjects to which I have given little thought, and which at present has incidentally come in my way.

I never like to commit myself to an opinion before making sure that I am right.—Yours sincerely,

JAMES CROLL.

NEWNHAM GRANGE, CAMBRIDGE,
17th November 1888.

MY DEAR SIR,—After giving you the following quotation, I must leave you to look up the originals, 1885 (Clerk's *History of Astronomy*, p. 120):—"By their (*i.e.* Jupiter or Saturn) influence the comets were in all probability originally fixed in their present tracks, and by their influence exerted in an opposite sense, they may in some cases be eventually ejected from them. A curious instance of such capricious dealing on the part of Jupiter was afforded by the comet of 1770, found by Lexell of St. Petersburg to perform its circuit of the sun in five and a half years, but which had never previously, and has never since, been seen. The explanation of this anomaly suggested by Lexell, and fully confirmed by the analytical sequences both of Laplace and Leverrier, was that a very close approach to Jupiter in 1767 had com-

pletely changed the character of its orbit and brought it within the range of terrestrial observation, while in 1779, after having only twice traversed its new path, at its second return it was so circumstanced as to be invisible from the earth. It was by a fresh encounter directed into one entirely different.

Note.—Leverrier showed (*Comptes Rendus*, t. xxv. p. 564, 1847) that the problem of the disturbance suffered by Lexell's comet was far less determinate than it had been made to appear in the *Mecanique Celeste.* It is possible that this body may in 1779 have been finally thrust out of our system; it is also possible (as Laplace concluded) that it may be revolving too far from the sun to be accessible to our view; but it is much more probable that its orbit still retains a family likeness to the one temporarily assigned to it by Jovian influence in 1767, in which case Leverrier's calculations afford evidence for its eventual identification.

This was what I was referring to in my former letter, but I had not referred to it.

I still do not see that there is any violation of the principles of energy in the expulsion of a body from the planetary system with hyperbolic velocity. I suppose that a small body might enter our system without any sensible disturbance of the motion of the planets, and that as it passed through it might pass near a succession of planets, so that *each one might accelerate it and none retard it, and so come to move faster and faster.* I conceive that such a case of motion is only now to be treated by arithmetical processes and not by general analysis. If even a far more general solution of disturbed motion is attainable, we may learn the possibilities of disturbed motion like this, and so far, all I can see is that the hypothesis of expulsion does not violate the principles of energy. I have not read Laplace's or Leverrier's investigations, and it would take me too long to do so now.—I remain, yours very sincerely,

G. H. DARWIN.

5 PITCULLEN CRESCENT, PERTH,
26th November 1888.

Professor Darwin, F.R.S.

MY DEAR SIR,—Many, many thanks for your letter, which I have carefully thought over. I have also read the paragraph in Clerk's *History of Astronomy*, and likewise those in Grant's *History of Astronomy*, pp. 102–106. I am sorry that my difficulty still remains. My difficulty is one that lies within very narrow bounds. In fact, you expressed it in your letter in almost one sentence, when you said that "a body might enter our system and pass near a succession of planets, so that *each one might accelerate it and none retard it*, and so come to move faster and faster." How such a thing as this is possible, is just my difficulty. I have no other. It seems to me that in whatever way a body may approach or recede from a planet, gravity will exert the same energy in pulling the body back toward the planet as it did in pulling it forward, so that its motion will be as much retarded as it had been accelerated. The disturbing influence of the planets might make the body take at times an excessively elliptic path, but surely it would never become so eccentric as to make the body cease to return to the system.

It would seem that Lexell's comet is still moving in an ellipse.

Apologising again for troubling you so very much, yours sincerely, JAMES CROLL.

5 PITCULLEN CRESCENT, PERTH,
24th December 1888.

Professor Darwin, F.R.S.

MY DEAR SIR,—I fear you will be beginning to regard me as a bore. I am now ready for the press, and if you could give me a hint as to *how* a body can pass from planet to planet and get its motion accelerated by each and retarded by none, it would be esteemed a special favour.—Yours very sincerely,

JAMES CROLL.

5 PITCULLEN CRESCENT, PERTH,
27th December 1888.

Professor Darwin.

MY DEAR SIR,—Many, many thanks for favouring me with a reading of Sir William's letter. It is all that I require. It is strange that his second illustration is just about what I had supposed would likely be the way that the body abstracted energy from Jupiter.—Yours truly,

JAMES CROLL.

5 PITCULLEN CRESCENT, PERTH,
31*st December* 1888.

Professor Darwin.

MY DEAR SIR,—I have just come upon a most extraordinary statement of Sir William Thomson. There is surely some mistake. Would you kindly look at the page of MS. which I enclose, and give me your opinion.

Again apologising for troubling you, I am, yours sincerely,

JAMES CROLL.

Although in 1885, Dr. Croll had definitely resolved to terminate his studies, both in climatology and in physical science in general, he found his theories so misrepresented and misunderstood that he felt obliged once more, in the year 1889, to defend and further explain these theories. Accordingly, he wrote a paper on the "Rate of Sub-aerial Denudation," which appeared in the *Geological Magazine* on December 3, 1889, and another on "Prevailing Misconceptions regarding the Evidence which we ought to expect of former Glacial Periods," which appeared in the *Quarterly Journal* of the Geological Society.

During this year also he embodied the results of his investigations and speculations in astronomical science during a long period of years in a volume entitled *Stellar Evolution and its Relation to Geological*

Time. This was published about the month of May 1889; and the following notice, by Mr. A. Fowler, of the Royal College of Science, South Kensington, slightly revised by him, which appeared in *Nature* in June of that year, gives a view of the aim and scope of the work.

Dr. Croll's book, though chiefly dealing with the question of stellar evolution from the astronomer's point of view, calls in the evidence afforded by geology in favour of the theory which is set forth in its pages. The particulars of the theory are clearly stated, and the new facts which have been gathered since the theory was first published are fully considered.

Dr. Croll accepts the nebular hypothesis of Kant and Laplace, and deals mainly with the question of the pre-nebular condition. According to his theory, large, cool, dark bodies moving with enormous velocities were either created or were eternal, and these colliding with each other here and there, the evolution of the celestial bodies was accomplished. With regard to the origin of these bodies endowed with motion, Dr. Croll states: "We are perfectly at liberty to begin by assuming the existence of stellar masses in motion, for we are not called upon to explain how the masses obtained their motion any more than we have to explain how they came into existence. If the masses were created, they may as likely have been created in motion as at rest, and if they were eternal, they may as likely have been eternally in motion as eternally at rest" (p. 3.) It is argued that the heat energy which would have been derived from gravitation alone could not possibly have been equal to that which the solar system originally possessed. But there is absolutely no limit to the amount of available energy from Dr. Croll's point of view. The most important argument against the gravitational theory is undoubtedly the geological and biological one. The whole question of geological time rests on an estimation of the time during which the

sun has been radiating its heat, and on this point Dr. Croll remarks: "If gravitation be the only source from which the sun has derived its heat, then life on the globe cannot possibly date further back than 20,000,000 years, for under no possible form could gravitation have afforded, at the present rate of radiation, sufficient heat for a longer period" (p. 35). The adoption of Langley's value (1·7 times that of Pouillet) for the rate of solar radiation reduces Helmholtz's estimate of 20,000,000 years to 12,000,000 years, and even this would not be available for plant and animal life, as millions of years must have undoubtedly elapsed before the earth was prepared for them. Professor Tait (*Recent Advances in Physical Science*, p. 175) has shown that from the physical point of view 10,000,000 years is about the utmost that can be allowed for all the changes that have taken place on the earth's surface since vegetable life of the lowest known form was capable of existing there. Lord Kelvin states his conclusions on this point thus: "In the circumstances, and taking fully into account all possibilities of greater density in the sun's interior and of greater or less activity of radiation in past ages, it would, I think, be exceedingly rash to assume as probable anything more than 20,000,000 years of the sun's light in the past history of the earth, or to reckon on more than 5 or 6 million years of sunlight for time to come" (*Popular Lectures and Addresses*, p. 390).

It is not necessary here to enter into details of the various methods by which geologists and biologists have attempted to estimate the length of time which must have elapsed since the earth first received the heat of the sun. On this point Dr. Croll says: "The grounds upon which the geologists and biologists found the conclusion that it is now more than 20 or 30 millions of years since life began on the earth, are far more certain and reliable than the grounds upon which the physicist concludes that the period must be less" (p. 68). Here again it may be well to quote Lord Kelvin, who

says: "What then are we to think of such geological estimates as 300,000,000 years for the 'denudation of the Weald'? Whether is it more probable that the physical conditions of the sun's matter differ 1000 times more than dynamics compel us to suppose they differ, from those of matter in our laboratories, or that a stormy sea with possibly channel tides of extreme violence should encroach on a chalk cliff 1000 times more rapidly than Mr. Darwin's estimate of one inch per century?"

But, granted that the geological evidence is against the gravitation theory, it remains for us to see how Dr. Croll's theory bears the strain put upon it when the details of the evolutionary processes are inquired into.

According to the impact theory of Dr. Croll, "meteorites are but the fragments of sidereal masses which have been shattered by collision" (p. 12.) The result of such a collision would be mainly to produce a gaseous mass, but some of the exterior fragments would have velocity sufficient to carry them beyond the influence of the central mass. This view is obviously in direct contradiction to the opinion held by Mr. Lockyer, who looks upon meteorites as the parents and not the children of sidereal systems. The explanation of the thumb-marks and the heterogeneous structure of meteorites which has been given by Mr. Lockyer (*Proceedings of the Royal Society*, vol. xliii. p. 151) would apply equally to Dr. Croll's view.

Comets, according to the impact theory, have a similar origin to meteorites, Dr. Croll apparently agreeing that they are nothing more than swarms of meteorites. Those with elliptic orbits probably had their origin in the collision which produced the nebula out of which the solar system has been evolved, whilst those with parabolic and hyperbolic orbits are probably the outcasts of other systems.

The first condition of a nebula, according to Dr. Croll, is that in which it consists of broken fragments

scattered through a gaseous mass of excessively high temperature. Mr. Lockyer's recent researches are consistent with this view as far as meteorites and interspaces are concerned, but they point to the opposite conclusion with regard to temperature. Mr. Lockyer's spectroscopic work has shown that the highest temperature is in all probability only reached by a nebulous mass as the completed volatilisation of all the meteorites composing it, and he has shown that the intermediate stages which should occur on this supposition are actually represented amongst the stars. This, therefore, furnishes a strong argument against the high temperature theory of nebulæ.

According to the impact theory of Dr. Croll, the meteorites scattered through the gaseous mass have nothing whatever to do with the luminosity; whereas, from Mr. Lockyer's point of view, the luminosity is in great part, if not entirely, due to collisions between the meteorites. Dr. Croll objects to this latter view because it "does not appear to afford any rational explanation of this banging about of the stones to and fro in all directions; for, according to it, the only force available is gravitation, and this can only produce merely a motion of the meteorites towards the centre of the mass" (p. 20). Dr. Croll has evidently given but little thought to this theory, originally advanced by Professor Tait, for it is obvious that all the meteorites would not lose all their momentum by collision during their first movements towards the common centre of gravity. Those which escaped collision would move on beyond the centre of gravity with considerable velocities, and would continue to oscillate to and fro until all their momentum was converted into heat by collisions. The banging about might therefore go on for a very long time; and the observations made by Dunér and classified by Mr. Lockyer tend to show that this is probably the case. The increase of temperature would accordingly take place gradually, and not suddenly,

as Dr. Croll supposes, and, further, the highest temperature would be associated with a certain class of stars, and not with the nebulæ themselves.

The subject of new stars has an important bearing on theories of cosmogony, but the subject is dismissed with very few words. The general view adopted by Dr. Croll seems to be that in such a case as *Nova Cygni* the outburst was due to the collision of a star with a swarm of meteorites when it was just fading from our view. Its spectrum, as observed during its later stages by Copeland and others, was that of a planetary nebula. If, therefore, a nebula is at a higher temperature than a star, *Nova Cygni* must have got hotter as it got dimmer. The same would be true also of the more recent new star in Auriga, and this may be taken as confirmation of the view that the temperature of nebulæ is relatively low.

Dr. Croll shows that his theory explains other details of the structure of our universe, including the proper motions of stars and the origin of binary systems, but these need not be more than mentioned.

Assuming that Dr. Croll has established that gravitation alone would have been incompetent to produce the heat originally possessed by the solar nebulæ, it is only necessary to reconcile this with the low-temperature theory of nebulæ, as the high-temperature theory has been shown to be inconsistent with the facts.

It may be suggested that instead of the dark stellar masses endowed with motion which Dr. Croll supposes to have been the pre-nebular condition, meteorites at great distances apart were endowed with similar velocities. In the first groupings the collisions would only occur very rarely, and there would be more grazes than anything else, so that the average temperature might still be low in the earlier stages. Professor G. H. Darwin has shown that the conception of fluid pressure which is demanded by Laplace's nebular hypotheses is not difficult to reconcile with the meteoric hypothesis.

If we substitute meteorites in collision for the molecules of a gas impinging against each other, there would be a quasi-fluid pressure, as the average result of the impacts of the meteorites and the separation of the planets from the meteorite swarm would take place exactly as in a gaseous mass.

Dr. Croll having sent a copy of *Stellar Evolution* to Professor Winchell, the following correspondence ensued:—

ANN ARBOR, *13th April* 1889.

Dr. James Croll, Perth, Scotland.

MY DEAR SIR,—I received, a few days since, a copy of your new work on *Stellar Evolution*, and I need not assure you that I read it with eagerness and great satisfaction. The grand themes covered by your discussion have always possessed great fascination for me. I feel complimented by the references which you have made to my work.

I have received from the author a copy of Professor G. H. Darwin's *Memoir*, and feel gratified that he finds mathematical support for that conception of the physical constitution of a nebula which I had in print before Mr. Lockyer began writing on the subject. I have read Preston's and Darwin's recent articles in *Nature*.—Very sincerely yours, ALEXANDER WINCHELL.

MINNEAPOLIS, MINN.,
20th April 1889.

Professor James Croll, Perth.

DEAR SIR,—I have received the two volumes of which your favour of March 26 gave me advice, and I desire to express my appreciation of your kindness and of the value of the works. Your publications I prize among my most valued ones, and shall study them carefully, and with great interest.

You will find in vol. ii., *Final Report of the Minnesota Survey*, a re-discussion of the Recession of the Falls of St. Anthony. The data there employed are

reliable, and they seem to fix the second Glacial epoch at a comparatively recent date. Several geologists have told me that the result based on the data used seems to be the most exact and reliable yet attained.—Very truly, A. H. WINCHELL.

5 PITCULLEN CRESCENT, PERTH,
20th May 1889.

Professor A. Winchell, LL.D.

MY DEAR SIR,—I have sent you by book-post the uncorrected proof of a most important paper by Dr. Huggins read before the Royal Society on the 2nd inst. Dr. Huggins' results will, I suspect, go a good way to overturn the views held by Mr. Lockyer, and recently advocated by Professor Darwin, on the meteoric constitution of nebulæ. He finds that there is no evidence to support Mr. Lockyer's assertions as to the magnesium flame bands ("cool magnesium") in the spectra of the gaseous nebulæ; and, consequently, Mr. Lockyer's further statement on the meteoric nature of these bodies cannot be regarded as more than a hypothesis.

Mr. Huggins is our highest authority on all subjects connected with the spectra of the stars.

The paper might have appeared in *Nature*. Mr. Lockyer, however, is the editor, and this may explain it.

Is Professor Tappan, formerly Chancellor of your university, still alive? I read with much interest, some forty years ago, his work on the Will. Jonathan Edwards' *Treatise on the Will* is, however, my favourite book.—I am, dear Sir, yours faithfully,

JAMES CROLL.

ANN ARBOR, *7th June* 1889.

Dr. James Croll.

MY DEAR SIR,—I am under very great obligations to you for the opportunity to read so soon Dr. Huggins'

important memoir. One cannot fail to be impressed and prepossessed by the pains which he takes to attain exactness and certainty. I do not hesitate to give credence to the conclusions which he indicates, even when adverse to those of Mr. Lockyer, because I think I find in the latter's method a somewhat more incautious and wholesale procedure—quite noticeable, indeed, in the quotations made by Dr. Huggins.

If, however, we assent to all Dr. Huggins' positions, I do not see that we are precluded from the supposition that with the gas and intense heat revealed by spectroscopic research, and the failure to detect magnesium in particular, there may exist also non-luminous solid matters not optically revealed. It was all along my own conception that matter in various conditions is embraced within the nebula. When we consider the enormous distances and magnitudes concerned, I do not think such a conception inadmissible. If the co-existence of various conditions of matter is admitted, then it remains to ascertain the antecedents of the solid portions. Reasoning *à priori*, I discover no fatal objection to your theory of a grand "smash," nor to the theory of a gradual aggregation of cosmical matter. Dr. Huggins recognises the compatibility of a gaseous constitution with your theory, but he seems to think that the collision must have converted the colliding masses wholly into gas. Now, as you suggest in your book, some portions may not have been thus converted, and even if they were, cooling (perhaps peripheral) would reproduce solid conditions, while yet much of the nebulous mass might remain gaseous. Dr. Huggins does not mention the compatibility of a gaseous constitution with the theory of meteorital aggregation, but he does not deny it, and his reason is perhaps not inexplicable, whatever he thinks. On my part it has long seemed entirely admissible that from collisions of meteorital masses there may have resulted liquid and gaseous—even dissociated—conditions of matter. I do not feel, therefore, that Dr. Huggins'

determinations are destined to produce any great revolution in nebular theories. Both Professor Darwin and Dr. Huggins have referred to Roberts' photograph of the nebula in Andromeda, and Huggins speaks of it as "showing a planetary system at a somewhat advanced stage of evolution," and he quotes *Monthly Notices R. A. S.* That interests me exceedingly. If you could aid me in procuring a copy of the published view, I would most willingly refund all expenses to the proper person or office.

You inquire about ex-President H. P. Tappan. He withdrew from the University in about 1866, and settled in Europe. He died about three years ago in Switzerland. I will send you a copy of a memorial address.—Very sincerely, ALEXANDER WINCHELL.

5 PITCULLEN CRESCENT, PERTH,
8th July 1889.

Professor A. Winchell.

MY DEAR SIR,—When I received your letter, I wrote to Dr. Huggins inquiring about the photographs. A copy of his reply I herewith enclose.

I then wrote to Mr. Roberts to see if copies of the photographs themselves could be obtained, but I have not as yet heard from him. The probability is, that he is away from home.

I have sent by book post three copies of *Knowledge*, containing reproductions of Mr. Roberts' photographs on a larger scale. It is probable they will give you as good an idea, if not better, than the actual photographs themselves. When I hear from Mr. Roberts, I shall write you.

Many thanks for the two memorial addresses which you sent, and which interest me greatly.

Stellar Evolution is beginning to make an impression here, especially amongst those familiar with the subject. This, I think, is in a great measure due to the evidence given in Part II. regarding the age of the earth.—Yours sincerely, JAMES CROLL.

5 PITCULLEN CRESCENT, PERTH,
10th July 1889.

Professor A. Winchell.

MY DEAR SIR,—I have just received the enclosed reply from Mr. Roberts in reference to his photographs. I am glad to find that he will be able to furnish you with an enlarged copy.—Yours sincerely, JAMES CROLL.

Shortly after the publication of *Stellar Evolution*, the following interesting and instructive correspondence on the subject thereof took place between Dr. Croll and Mr. Alfred Wallace:—

PARKSTONE, DORSET, *25th August* 1889.

James Croll, Esq.

MY DEAR SIR,—I have now been able to read your very interesting *Stellar Evolution*. I think your general idea of the stellar universe a very probable and suggestive one, as it accounts for the proper motion of the stars and other phenomena, which a general nebulous theory will not do. But I do not think that this theory in any way gets over the difficulty of the amount of solar heat and the duration of geological time. You appear to have overlooked the fact that the initial heat of the solar nebula has no bearing on the question at all, but only the heat at the time when it (the nebula) had contracted to the *diameter of the earth's orbit* and had thrown off the earth. Now, the heat at that epoch is a fixed quantity, not affected by the initial heat of the nebulæ. It matters not, therefore, whether the original heat of the solar system was sufficient to expand the solar nebula to the diameter of the orbit of Neptune, or to ten times that diameter; when it had cooled down to the dimensions of the earth's orbit, it would have reached the same temperature and have contained the same store of heat. What we require is not a *greater original* heat, but some agency by which the sun continued to generate heat, if not so fast as it gives it out, yet sufficiently to delay the cooling almost indefinitely. Two such causes are con-

ceivable and have been suggested,—one derived from the constant rain of meteorites on the sun from the stellar space through which it passes during its proper motion through the stellar universe; the other the continued absorption of fresh gaseous matter from the ethereal spaces it is passing through—as suggested by Mr. Matthew Williams in his *Fuel of the Sun*. This objection seems to me so obvious that I cannot understand how it is you have not even noticed it. For your purpose it was necessary to show that, after the earth had been thrown off, and its surface had cooled sufficiently to allow of the condensation of water and the formation of sea and land, the sun could *possibly* contain many times more heat than was required to keep it at the diameter it had then attained. You must show, in fact, that it is possible to have two suns, *identical in constitution, in mass*, and in dimension, yet one containing many times as much heat as the other, *because* it had had a higher initial temperature. That seems to me to be physically impossible. Is it not so?—Yours very faithfully, ALFRED R. WALLACE.

5 PITCULLEN CRESCENT, PERTH,
9th September 1889.

Alfred R. Wallace, Esq.

MY DEAR SIR,—Referring to your letter of 25th ult., I think your difficulty arises from overlooking the fact that, according to the Impact theory, condensation can only take place as the nebula loses its heat, whereas, according to the Gravitation theory, condensation is required to produce the heat. Take the case of the solar nebula. According to the latter theory, when the materials had condensed so as to occupy only the space, say, included within the earth's orbit, there would be comparatively little heat generated, and the store of energy possessed by this nebula, which we shall now call the sun, would be small indeed. I have not computed the amount, but I am certain that it would not be sufficient

to supply the present rate of solar radiation for many thousand years. The mass would require to be condensed to a space far less than that included within the orbit of Mercury, before a store of heat sufficient to supply the present rate of radiation for at least one or two millions of years. In regard to the Impact theory, by the time the nebula had cooled and condensed to the earth's orbit, it might probably possess some hundreds of millions of years' heat.

I trust that these considerations will make the matter plain.

Have you seen Dr. Huggins' paper on the great nebula in Orion? In case you have not, I have sent you by post a copy which you can return at your leisure. It goes a good way to establish the theory. If that nebula had originated by condensation, it could not consist of a gas so tenuous, and of such a low density and high temperature.—Yours very truly, JAMES CROLL.

PARKSTONE, DORSET, *12th September* 1889.

DEAR MR. CROLL,—You seem to assume that in the nebular hypothesis the primitive nebula is assumed to be *cold*, and its condensation to be due solely to gravity. But did anybody ever hold this theory? I find in the article "Solar System" in the *English Cyclopædia* written by Professor D. Morgan, the theory is stated to be that "condensation takes place arising *from loss of heat*," and again he refers to the necessary consequences "*of the cooling of a nebulous atmosphere.*" And, surely, any other conception is absurd and physically inconceivable. Can anybody suppose the whole physical constitution of the universe to be so different from what it is now, that iron and all the other metals and solid elements could exist as cold vapours. The point of *liquefaction* of each metal, as well as the points of *vaporisation* of every solid element, are *physical constants*, and cannot be conceived to have been *totally different* from what they are now in any rational theory of the universe.

I cannot therefore see that the theory of a "cold nebula," not leading to heat enough to produce the sun or the solar nebula by gravitation alone, has any bearing on the problem. The sun, at a certain epoch, say that of the Laurentian epoch, had condensed to a certain diameter, and it has since condensed to the diameter it now possesses. Each of these diameters, assuming the mass to be a fixed quantity, implies a definite amount of heat, which amount would not be affected by the fact of the original nebula having been *hotter*, and therefore *larger*, or *cooler*, and therefore smaller, since it must anyhow have been once at least as large as to fill the orbit of Neptune. This objection your letter does not touch.

I return Dr. Huggins' paper, which I had seen in *Nature*.—Believe me, yours faithfully,

ALFRED R. WALLACE.

5 PITCULLEN CRESCENT, PERTH,
14th September 1889.

Alfred R. Wallace, Esq.

DEAR MR. WALLACE,—If you will examine the history of Gravitation theories of the origin of nebulæ, and of the sun's heat (no easy matter to get at), you will find that I am right. I doubt you will not get much on the subject in any of our encyclopædias. You will, however, find a great deal on the literature of the matter in Winchell's *World-Life*, a book referred to in *Stellar Evolution*, p. 22.

Laplace assumes that the primitive solar nebula was hot (see *Stellar Evolution*, p. 30); but then, this was not the Gravitation theory of the origin of the sun's heat.

What you say as to the absurd consequences which follow from supposing the heat of the nebula to be due to condensation by gravity is perfectly true, but so much the worse is it for the Gravitation theory under every form.—Yours sincerely, JAMES CROLL.

The Rev. Mr. Baxter, Free Church minister, Cargill,

wrote Dr. Croll, inviting him and Mrs. Croll to spend a day with him, and received the following reply:—

5 PITCULLEN CRESCENT, PERTH,
28th June 1889.

MY DEAR MR. BAXTER,—We shall be delighted, Mrs. Croll and I, to take advantage of your kind invitation, and come out and spend a day with you. Tuesday 9th will suit us well.

We shall drive out direct in the forenoon, and perhaps you will find some one who will kindly take us down to Cargill station to meet the evening train.—Yours sincerely, JAMES CROLL.

Mr. Baxter writes regarding Dr. Croll's visits to him as follows:—

"My own personal knowledge of Dr. Croll was confined to his later years. It was my privilege, however, during these years to see him both in his own house and in my own. Our connection arose from his belonging to this district. In making one of his visits to this scene of his earlier years, his affection for which he never lost, he called on me here at the manse, and from that time he was accustomed every year, during the two or three years preceding the last seven of his life, to come and spend a day with me, visiting the scenes of his childhood. These days were to me days of great enjoyment, both from the kindliness and geniality of nature characterising Dr. Croll, and from the originality, intellectuality, and power attaching to his conversation. I can still recall the interest of his various references to the great theory of the secular changes, the interest attaching to his description of the Glacial period, and its effects on this locality in particular. I can remember how passionately he declared his preference for metaphysics over physics, and his intention, if spared, to return to what he called his first love, and the intimation he made to me of his being at that very time engaged on a study of Kant, with a view to ultimate publication of his views,

if permitted to fulfil his purposes. I remember, too, an allusion he made on one occasion to an improvement he claimed to have suggested on the subject of Telegraphy, consisting, if I remember rightly, of a proposed method of economising the electric current at angles of the wire-points at which, in consequence of the momentum acquired by the current of electricity in its course, it necessarily overleaped the junction, and was thus lost—falling to the ground, as it were. Dr. Croll claimed to have suggested that the weakness might be remedied by simply allowing of a little void space at end-points, so that the current in its momentum, leaping beyond the point of the line it was leaving, leaped only the length of the line it sought to reach, and was then safely received without any loss whatever. On this principle, if understood aright, all telegraph lines are now conducted. His conversation on these and on topics in general was very attractive, his utterances being always distinguished by a most startling lucidity and force of argument. As incidents connected with Dr. Croll's visits to this neighbourhood, I remember two that are perhaps possessed of sufficient interest to render them worth retailing and preserving. He was wont to revisit, at such times, the cottage still standing, in which he spent the years of his childhood and earlier manhood. Looking round with searching eyes on the old familiar walls, I remember his remarking, as his eye fell on the window of the chamber, 'And there are the very shutters I made with my own hands'; and, stepping up to them, he opened and closed them fondly. A mantelpiece of wood also that remained from these days, and was an original contrivance of his for suspending the 'crusie,' he pointed to with loving interest and remarked on its survival. That mantelpiece I secured at a later time, when it was being removed in connection with some repairs, and still retain as a precious relic of Dr. Croll's memory. At another time, on my asking if any of his old playmates and friends remained in the village whom he might go and

see to revive old memories, he inquired after several, all gone, till at last he mentioned the name of Robert Young, an intelligent tailor still surviving in Wolfhill. I said, 'Robert is yet alive, we will go and see him.' On arriving at the door of the house, it was opened to my knock by Robert himself. I pointed to Dr. Croll and said, 'Do you know this gentleman, Robert?' 'No,' he said hesitatingly, 'I do not.' 'This is Dr. Croll,' I said. 'Ay, ay?' he at once responded, his eyes sparkling with delight. 'Jamie Croll, Jamie Croll'; and the two proceeded to live over again the days of their boyhood, and reflect on the diversity of fortune the revolving years had brought to them severally. Robert Young in his own sphere is a man of more than average intelligence. One anecdote of Dr. Croll, not, however, through my own experience, but which I obtained on his own authority from the person mentioned in it, Andrew Stewart of Kinrossie, a lifelong friend of Dr. Croll's, and one whom Dr. Croll highly esteemed, and used to visit to the close of his life. Andrew Stewart told me that in Dr. Croll's early days, when he was still a joiner and working at the erection of Kinrossie Free Church for Dr. Andrew Bonar, he lived in his town. One day Dr. Bonar remarked to him (Andrew Stewart), 'So you have James Croll residing with you? How do you get on with him?' 'Oh, very well,' said Andrew Stewart. 'We have sometimes keen discussions and debates, but on the whole we get on very well.' 'Do you know,' continued Dr. Bonar meditatively, 'that James has a very metaphysical mind?' The anecdote is worth reproducing, as showing at once the early representation of mental power and Dr. A. Bonar's keen discernment of character. Andrew Stewart was a man far above his compeers in both attainments and intelligence, and I enclose a partial critique of his, relative to one of Dr. Croll's later works which I had lent him, that, though written in great feebleness and much pain, yet shows that he possessed powers rendering him not unworthy to be a friend of Dr. Croll's."

Dr. Croll had sent a copy of *Climate and Time* and also of *Discussions on Climate and Cosmology* to Dr. Nansen, from whom he received the following letters:—

ASGARDSTRAUR, *21st August* 1889.

DEAR SIR,—Accept my heartiest thanks for your very great kindness in sending me your highly interesting book, *Discussions on Climate and Cosmology*. I need not tell you that this important work has for me just at present a keen interest, and I am going to read it with great pleasure. The only thing I have to send you at present is a paper I read before the Geographical Societies of London and Edinburgh. I regret to say that the copy I send is not complete. By mistake the conclusion has not been printed in the *Geographical Magazine*; as it now is, it stops in the middle of a sentence. You will find it complete in the "Proceedings of the Geographical Society of London." I have, however, no copy of that.

Do you come to the meeting of the British Association in Newcastle? If so, I hope we may meet there.—I am, dear sir, yours faithfully, FRIDJOF NANSEN.

ASGARDSTRAUR, *27th August* 1889.

DEAR SIR,—To-day I received your highly interesting book, *Climate and Time*. For this beautiful present I send you my most hearty thanks. I am going to read it with keen attention and much pleasure, and will devote much time to it, as I think it to be a work of the highest importance.

I regret to say that I have not got anything of interest to send you, but hope soon to be able to send you something.

I also thank you most heartily for your very kind letter, which I got some days ago. I wrote to you before that time to thank you for your previous present. I hope you have already got that letter.—I am, dear Sir, yours faithfully, FRIDJOF NANSEN.

CHAPTER XXVII

THE PHILOSOPHICAL BASIS OF EVOLUTION—CLOSING DAYS

DURING the years 1889 and 1890, Dr. Croll was busily engaged in writing and preparing for the press the work which it had been his chief aim in life to accomplish. We can scarcely say "writing," for by this time he was unable to write, and could only dictate to an amanuensis. Some idea of the strain involved in this may be gathered from the fact that, owing to his extreme weakness and his old enemy, pain in the head, he was able to dictate for only about half an hour a day. He had, however, determined to accomplish his work; and, by a marvellous force of will and concentration of his very limited energy, he persevered day by day till he finished the manuscript. When that was done, he was scarcely able to do more. The Rev. D. Caird, then Congregational minister in Perth, now of Edinburgh, kindly took in hand the correspondence with the publishers; and, with Croll's guidance, he had the proof-sheets revised and the work carried through the press to the publisher's hands. The work was issued in the month of November 1890, and was entitled *The Philosophical Basis of Evolution.* A melancholy satisfaction attaches to the fact that Dr. Croll was just spared to see a copy of his last work in print, and to hear read to him the following favourable review of it, which appeared in the *Times* on 27th November 1890:—

"Dr. James Croll's works, dealing with some of the

broader problems of science, are so well and so favourably known, that his new essay on *The Philosophical Basis of Evolution* (Edward Stanford) is assured of a respectful reception. It will not, however, be welcome to those who see in evolution, pure and simple, the ultimate explanation of the universe. It will probably be repudiated in certain quarters as unadulterated metaphysics, although Dr. Croll declares himself that 'the present volume is not of a speculative or hypothetical character. The reader,' he adds, 'will readily perceive that all the main conclusions are, without exception, deduced from facts or from fundamental principles. Most of them, indeed, are in some form or other necessary consequences of the great principle of causality, namely, Everything which comes to pass must have a cause.' Dr. Croll, in fact, traces back the conclusions of evolution to the premises of theism, and his reasoning, albeit metaphysical in the original and legitimate sense of the word, is stringent, cogent, and coherent."

The following review of Dr. Croll's last work by Mr. T. Whittaker, which appeared in *Mind* (vol. xvi. p. 268), gives an excellent account of the nature and scope of the book:—

"The author of this book, eminent for the cosmological width of sweep which he gave to his geological inquiries, died near the close of last year, a few weeks after its publication. In many ways it is a work of special interest. Although the best part of Dr. Croll's life had been given to scientific pursuits, it is really a return to the problems that had been the first to occupy him. The conditions under which he had devoted himself to science as well as to philosophy had not been the most favourable; but if we are to make the comparison, his interest in philosophy seems to have come to him most from nature, and his interest in science from circumstances. The present work bears the mark both of philosophical consideration applied to science and of the influence of the scientific spirit on a

philosophical mind that had never become estranged from theology. So far as results are concerned, the author may be described as a theist who accepts scientific evolution in its full sense, and who places philosophical determinism at the base of this. The detailed argument is carried through with great clearness and vigour, and brings to light the interaction of new and old elements of thought that are all in their different ways powerful at the present time.

"The first point to be noted is the author's clear view of the necessity for definiteness in scientific explanation, and for permanent distinctions between the different sciences. This he enforces by very decisively separating the problems of the 'production' and the 'determination' of motion. The process of evolution is perfectly continuous, and beneath it there is a constant ground which it is for the physicist to define. The changes in which evolution consists can all be expressed as motions, and motion may rightly be described as always 'produced' by force. The history of the production of things may be traced back indefinitely, and nowhere is the ground of production found to be other than constant. This, however, does not exhaust the scientific view, nor even express its most important side. Molecular forces, to which all others may ultimately be reduced, have their particular directions and points of application, and enter into action at fixed moments of time. These definite and particular determinations can only be explained from previous determinations which are equally definite and particular; and those again from others. In the transformations of energy we say that the energy is the same all through. The effects, however, are different. Thermal and electrical energy, for example, manifest themselves in different ways; and it is because of their different modes of manifestation that we call them different energies. Just as we are not to call electricity a form of heat because the energy of electricity can be transformed into the energy of heat, so

it is not admissible to say that the energy manifested in organic life is merely physical and chemical because it can be transformed into various physical and chemical energies, and because these energies are at the ground of vital processes. The modes of energy being different, the uniformities of connection between them can never be all reduced to a single physical law.

"This view has its application to mind also. Mental evolution, like material evolution, is continuous. What is constant in it is 'mind'; but here again true scientific explanation depends on regarding mind as definitely determined. To speak of the production of acts by the will, for example, may be correct as far as it goes; but every act is a determinate act, and the determination has to be explained by something other than 'will.' The act of choice in its definite character is rather to be explained by the 'agreeableness' or 'reasonableness' of a suggested direction than the direction by the mere act. In mental even more than in natural science the definite determination, and not the mere production, is the important thing.

"The idea of 'determination,' thus scientifically generalised, is made use of to arrive at a philosophical view of nature and mind. From the impossibility, or at least excessive difficulty, of admitting an infinite regress in the series of past events, the author infers that the world must have had a beginning. Its determination can then, he holds, only be explained by theism. We must suppose all determination in the world to take its origin from a determination in the mind of God. The series of events in time is not eternal, but their determination is eternal. This determination Dr. Croll is led to conceive of as strictly necessary. His doctrine is therefore at once a philosophical and a theological determinism.

"Dr. Croll's theism implies a teleological view of things; and this view he seeks to defend against some interpretations of organic evolution. He succeeds in

showing that natural selection does not exclude every kind of teleology; though, of course, something might be said against his contention that teleology in the form of 'objective ideas' of species and so forth is still required by science. The better course here, from his own point of view, would have been simply to show the independent range of philosophical interpretation. 'Natural selection' itself, as a scientific theory, Dr. Croll remarks in more than one passage, derives all its efficacy from being a theory of 'determination.' It is not a theory of the 'production' of forms of life, but explains how determinate forms arise under definite conditions.

"The question of free will is discussed in an especially interesting way. From the body of the work the author seeks to exclude it as irrelevant to his general scientific and philosophical positions. Advocates of free will, he argues, do not really intend to deny the universal validity of the law of causation. Express discussion of the question is relegated to an appendix. Here Dr. Croll shows, in spite of the concession made elsewhere, that the believer in free will ought if he is consistent to deny the absolute uniformity at least of psychological law. He himself, both on scientific and philosophical grounds, refuses to admit the exception involved in free will. Many well-pointed arguments against the indeterminist position might be quoted from the chapters he devotes to the subject. The illusory belief in an undetermined will is explained especially from the determination of action by psychological states that are not brought into clear consciousness. 'I am directly conscious of the act of the will; but not, at the moment, of the manner in which it was determined.' As soon as particular mental states are distinctly recognised, they become, as it were, 'objects,' like those of the external world, and are thought of as something that is not the Ego. Then the notion arises of an exertion of will that is independent of these, as of other particular objects. An act of which the causes are not known at the moment is not, however,

an arbitrary 'act of will' pure and simple, but has really been determined by states that only require a new effort of introspection to bring them into clear consciousness.

"When we regard an action as 'our own,' we hold ourselves 'morally responsible' for it, however necessarily it may have been produced. It is our own action because it is the result of our own nature. The relation between action and internal nature in general is this, that 'the fruit is bad because the tree is bad' and 'good because the tree is good.' When an action is compulsorily determined, or proceeds from something outside our own nature, we do not feel that it is our own, hence a mechanical conception of the necessity of human actions, or a conception of them as necessarily determined from outside, may tend to produce theories of irresponsibility. This, however, is not really the conception of the philosophical determinist. Necessity, in its philosophical sense, is simply 'the certainty that is in things themselves.' By insisting on the inevitable consequences of actions, the theological form of determinism, far from weakening, has strengthened the sense of responsibility.

"This last contention of Dr. Croll's can be justified negatively as well as positively. When they apply their view to ethics, the theological and the philosophical determinist are on the same ground. Both must hold that actions necessarily determined quite rightly carry with them not merely the consequences that depend directly on the agent, but also consequences depending on the nature of men in general and on the requirement of definite means, for the attainment of social ends. And the ideas of 'irresponsibility' now in the air proceed really from the doctrine of free will. Accept determinism in the full sense, and it becomes evident that some criterion of the 'imputability' of an action to a person will have to be sought other than the absence of necessity; since actions are all necessarily determined, either

from without or from within. On the contrary, adopt a view of guilt and merit essentially identical with that which is derived by Catholic theologians from their doctrine of indetermination, then every action that can be proved scientifically to be necessitated is at once regarded as something for which no responsibility can be imputed. The demoralising consequences that may be drawn do not, however, spring from the scientific proof of necessity, but from its combination with that doctrine of free will which is regarded by many as the one support of moral responsibility. With philosophical determinism there is no danger of any such consequences.

5 PITCULLEN CRESCENT, PERTH,
3rd November 1890.

MY DEAR FOSTER,—You will be sorry to hear of the condition of my health. A few months ago, just when I had about finished my new volume on *The Philosophical Basis of Evolution*, which will be issued shortly, my health suddenly gave way, and I am now so weak as to be able to do little more than move about the house. The breakdown began as follows: I fell on the floor and lay insensible for above an hour; and when I recovered consciousness, I found I was not so strong either physically or mentally as formerly. I have since then had four or five cases of unconsciousness. What I am suffering from is a slow loss of power in the heart. This state of things will probably go on till the heart stops, an event which I am enabled to contemplate with the utmost composure, as it will be but the way of entrance to a better land.

There is one thing, however, which is weighing a little on my mind. I have no relations in Perth, or, indeed, almost anywhere. Mrs. Croll belongs to Forres, a small town about a hundred miles to the north, and I am anxious to see her settled there amongst her relations in the event of my death. The expense of

removal will be a considerable drain on her comparatively small means. Do you think that a sum, say £50 or so, might be obtained from the Royal Society Relief Fund for this purpose? You might kindly write and let me know your opinion on the matter.

I hope that Mrs. Foster, yourself, and family are keeping well.—Apologising for troubling you, I am, yours most sincerely,

JAMES CROLL.

Professor Foster, having to confer with the Committee of the Royal Society, was not able to respond definitely for a few days; but in December he wrote to Mrs. Croll the letter which will be found printed on p. 488.

Dr. Croll's nephew says that "his last illness came on so suddenly that it was feared he would not live to see the book. Then Mr. Caird wrote the publisher and got him to hurry up a copy, so that he might be gratified thus far. Knowing this, he eagerly watched every post as it came round, and when at last it did come, he examined it outside and inside, and seemed very pleased with it; then remarking, while he handed me the book, 'Of making books there is no end.' Then he said, 'My work is now done. I leave the world without a regret save one,'—I understood him here to refer to Mrs. Croll, —'trusting to the merits of the Lord Jesus Christ as my only, but, thank God, all-sufficient ground of hope for the future.' The powerful thinker and ardent worker thereupon felt that his mission on earth was finished, and he calmly composed himself with full faith to await the end that meets all men."

The Rev. Mr. Caird writes:

"It was not my good fortune to know Dr. Croll except by name till the autumn of 1888, when I became pastor of the Congregational Church at Perth; and by that time his health had failed so sadly that any kind of

intercourse which might have been valuable for the wider purposes of biography was practically impossible. One could not help feeling the influence of the keen intellect, the genial spirit, the tender, affectionate nature, which had won so many friends in former days: and it was always a delight and a true inspiration to spend even a passing hour in his company. To talk with him on anything beyond the conventionalities of the day was invariably to receive some thought or some impression that made the physical world seem more vast and glorious, or the spiritual world more real and powerful, if at times more mysterious in its depths of truth and its complexities of desire and purpose and will. But at no time during those two brief years of our friendship could he speak or listen much without suffering pain; and one was ever conscious of the necessity of sparing him as much as possible, even in the discussion of questions which more immediately absorbed his interest. The shadow of death was in fact already upon him, and it gave that strange and hallowed reserve to fellowship which one can always feel deeply, but seldom describe in living speech. Now and then the change of every week seemed to be more or less apparent, but the consciousness of declining strength never disturbed his peace of mind, or turned him aside from work. I remember him saying one day, in the course of casual conversation, 'It is but little I can do now, yet the better thing is just to work on till the end comes.' And work on he did, with a composure, a steady purpose, and a heroism which were extremely touching in their simplicity and their influence on his own mind.

"From 1885, if not earlier, Dr. Croll's work was not only limited, but greatly interrupted by recurring illness. Latterly he could spend only an hour a day in his study, and even then he could do little more than think out the details of the problems he had in hand. One day he would listen to his amanuensis reading from a work of reference of criticism, and the next he would dictate a

new paragraph of his own treatise. And this is but a faint indication of the difficulties and disadvantages under which *Stellar Evolution* and *The Basis of Evolution* were produced. That both books suffered from circumstances one can hardly doubt. At times Dr. Croll felt this very keenly himself. He would fain have made them larger and more thorough in their treatment of the subjects under review, and yet, taken as they are, they undoubtedly contain the substances of all he could have written. He worked so slowly and was such an exact and penetrating thinker that all he put on paper was for him practically a final judgment. These two books absorbed all his attention during the time I knew him; and, so far as I can remember, his thoughts did not turn seriously on any other questions pertaining to science or philosophy. He liked, however, to keep up a modified interest in current history and to maintain his touch of the outer world. As a rule, he glanced over the newspaper from day to day, marked the reports or articles he wished to have read, and then, afterwards, he was glad to hear a friend talk of anything that had specially interested him, or even of incidents he had not been able to overtake. His heart, however, was in his books, and in the larger concerns of life and destiny which now pressed upon him. The publication of *The Basis of Evolution* had to be greatly hurried in the end, and it looked after all as if the author were not to see his book in print, his last illness developed so rapidly. But Stanford, with great consideration, hurried on a rough copy, completely bound, and I can remember well the gleam of sunshine which broke upon his pillow as he grasped the book in his hand. Two or three days later the first review appeared in the *Times*, and he heard it read with great satisfaction. It seemed to be the last greeting from the outer world that he really cared for, and it was all the more welcome because of his feeling that the book gave a certain completeness to his life-work. Philosophy was his first love and his last, and

nothing but the force of circumstances turned him aside from it during the greater part of his life.

"From a very early period Dr. Croll was a Christian believer, and he could talk in a most interesting way of the philosophies and the theories of the day in their relation to the religion of Christ. Any modern treatment of theism kindled his attention and his critical faculty. But it was very noticeable that in the later months of his life he passed almost unconsciously from interest in speculative questions to simple faith in Christ as the one and only Saviour and Lord of men. He recognised the difficulty of reconciling many apparently contradictory truths which he held with confidence, and yet he was never really disturbed by this. He rather ridiculed the position of those who seem to imagine that the unity in which all things are to be resolved is to be found within the individual himself, or within the domain of abstract reason. To him the doctrine of evolution, and indeed most forms of modern thought, were but flashes from the great central orb, and flashes which should be welcomed as helping every man to a wider understanding of the world as it is, and of God who stands behind it all. What he frequently said in effect was this: 'Truth can never be inconsistent with herself. Meantime, some things look very dark, and some mysteries seem well-nigh insoluble, but when we get a little further on—when we know more of science and of everything here, the light will break, and the impossible sometime will come. And in the end some will be surprised at their fear, others at their folly.' The day before his death he spoke with great tenderness of some visions of Christ which had flashed before his eyes. He felt as if, even then, he had passed from the earth into what he called 'The eternal fellowship.' I asked him then if he had any conception of the spiritual realm beyond. 'No,' he replied, 'the time for that has not yet come. But my ignorance gives me no pain. It is sufficient now to lie still and wait for God, for He knows

all, and will do all things well.' The end was not long delayed, for the weary spirit, which had so humbly and so bravely battled with its weariness, passed on the dawn of day into eternal peace.

"One other thing I should like to add. In the closing months of his life Dr. Croll found great delight in going back by way of reminiscence on his early days. He spoke with deep gratitude of his home, of the faith and devotion of his father, of the wise and gracious Providence that had shaped his own destiny, of the love of many friends which had ever been to him a sacred treasure. Not less characteristic was his thought of the church of which he remained to the end an honoured member. In youth he was greatly helped by his connection with this fellowship, and as I think again of the last year of his life, there comes back with a strange, deep pathos the memory of a forenoon when he went for the keys of the old building in Mill Street, and, opening it with his own hand, went and sat for a little while in the seat in the gallery which his father used to occupy when he himself was a boy. Nor was this mere sentiment. It was one of the last acts of a true man who loved to trace back to its source the main stream of influence which had made him what he was, and who could not forget the mercy and the truth of God, upon whose world he had spent his thought, and by whose Spirit he had lived."

Mr. Paton of the Glasgow Corporation Gallery writes:

"I saw Dr. Croll two days before he died. At that time he was very weak and exhausted, but mentally he was as clear and eager as in his best days. He was exceedingly anxious to discuss with me some of Mr. Herbert Spencer's fallacies, but his wife had warned me to stay only a very brief time with him. His effort to speak brought on a fit of coughing, to relieve which he was getting whisky in teaspoon measures. In that

connection he made almost the only little joke I ever heard him utter. 'I'll take a wee drop o' that,' he said. 'I don't think there's much fear o' me learning to drink now!' It showed how bright, cheerful, and heart-whole he remained to the last."

He continued in this state to the end, retaining his mental vigour, his contented, bright spirit full of faith, and passed away peacefully without pain or any apparent struggle on the 15th of December 1890.

He was buried in the family burying-ground in the churchyard of the parish of Collace on the 18th of December 1890. Among the mourners were Dr. Bower, R.A.; Dr. Miller and Dr. Thomson, Perth Academy; Rev. John Brown, Kinclaven; Rev. Robert Findlay and Rev. Peter Grant of Perth; Rev. David E. Irons, B.D., Glasgow; Mr. Peach, Geological Survey, Edinburgh; Mr. Paton, Glasgow; J. Campbell Irons, Edinburgh; Mr. D. A. Macdonald, J.P., Cumbernauld, and Mr. William Macdonald, Forres, brothers-in-law; and others. Like the day on which he was born, the day on which he was buried was a cold, snowy, wintry day. On the tombstone which he had been at some pains to get erected himself are inscribed the characteristically simple words—

JAMES CROLL, LL.D., F.R.S., ETC. ETC
Died 15 Dec. 1890 · Aged 69 Years

A week after the funeral, Professor Foster, of London, wrote the following letter to Mrs. Croll:—

LONDON, W., *23rd December* 1890.

DEAR MRS. CROLL, — This must be a mournful Christmas and New Year for you, now that you have lost the companion of so many years. I would not intrude upon you at such a time, but I should like to offer you the expression of my sincere sympathy, and

to say how thoroughly I loved and respected your husband.

My chief business, however, in writing just now is to say that I had the satisfaction yesterday afternoon, at a meeting of the Scientific Relief Fund Committee, of voting a grant of £100 for your benefit. This sum is to be entrusted to Professor Geikie, at his request, and £50 is to be paid almost immediately, and the remainder in six months' time. I hope this assistance may be of service to you at this time, and that you will receive it as an indication of the honour in which Dr. Croll was held by scientific men in London. I only wish I had been able to announce the grant earlier, so that he might have known of it.—I am, dear Mrs. Croll, yours very truly, G. CAREY FOSTER.

Little or nothing has been said as yet regarding the relationship between Dr. and Mrs. Croll, chiefly because there was really nothing to record on the subject. Their life together, through fortune and misfortune alike, was an unbroken record of domestic felicity and happiness. He, on the one hand, was tenderly considerate and attentive to a degree almost unspeakable, while she, on the other hand, was a devoted and attached helpmeet to him. When he had resigned his position on the Survey, and his income by way of pension was reduced to starvation point, his first consideration was for Mrs. Croll. His niggardly pension would of course die with him, and nothing would be left for her maintenance. He therefore, out of the proceeds of the sale of his household furniture, and what he had derived from the proceeds of his book in the year 1880, purchased a small joint annuity on the life of himself and Mrs. Croll, and in 1881 he purchased another small annuity on her life alone. In 1886, from the proceeds of his book entitled *Discussions on Climate and Cosmology*, he purchased another small annuity on her life, the whole three annuities only amounting to £55 per annum. This

serves to show, however, his great consideration for her, and the amount of self-denial which he exercised for her future welfare. Indeed, in his declining years, his chief concern was how to make provision for her in case of his death; and the last letter he wrote to Professor Foster is one of the most pathetic illustrations of the tender consideration which he had for one who had lived and struggled with him in his marvellous career of adversity and prosperity.

At a very early age Dr. Croll gave evidence of the possession of some, at least, of those remarkable intellectual powers which afterwards distinguished him, and enabled him, in the face of almost overwhelming difficulties, to accomplish so much good work in science and philosophy. His faculty of perception was, from the first, so delicate and keen that, as a mere infant, he grasped the incidents connected with his brother David's baptism with an energy that enabled him to retain them clearly in his mind throughout life; and when, in later life, he made occasional excursions to the Pentland Hills and the Firth of Forth, his discovery of glacial evidence in the one case and of two river channels in the other, proved the possession of an eye as thoroughly trained as that of one who had, for a lifetime, been a practical geologist. Nature had long had her charms for him; he had looked upon her as a lover; and when he came to examine her in the interests of science, he read her secrets at a glance. Whatever the subject which attracted his attention, he brought to bear upon it the whole force of his richly gifted nature. The pursuit of truth was to him a solemn duty and an exquisite joy. In order that he might fulfil his duty and enter into his joy, he lived wholly and solely for the question of the hour. All else was, for the time, as if it were not; and this rare capacity of concentration—of both heart and mind—exercised often amid circumstances most distracting, largely accounts for the amount of work which he succeeded in doing.

He was but little of a geologist, still less of a chemist, for mere facts, however interesting in themselves, had little charm for him apart from general principles. Even as a lad, when studying mechanics, etc., he instinctively rose from the facts which absorb most boys to the great laws which they illustrate. He was by nature a philosopher rather than a scientist. He was ever straining toward the most abstract,—the origin of the material universe or the great First Cause. He had a most tenacious memory—nothing that ever interested him escaped his grip. What he saw he saw so clearly that it remained for ever photographed on his mind. Even the scientific facts, which he seemed to forget when he had laid hold of their laws, were in that way made only more completely his own, so that they were easily recalled by the mere statement of the laws. Moving with wondrous freedom through the varied realms of physics, celestial and terrestrial, he was largely indebted to a vivid power of imagination for his theories. (See Professor Tyndall's letter referring to Croll's picture of "hammers.") There was something of the poet in him, although he had but little power of poetic expression. He loved to look at facts pictorially, and interesting is his own confession that, had he been reading for mere pleasure, he would have read the works of poets chiefly. Most remarkable of all his intellectual features, however, was his reasoning power, so strong and clear, of which we need only say *Climate and Time* remains an enduring monument. The keenness and directness of his marvellous logical faculty is, however, well illustrated in other ways. It appears in his correspondence; was evinced in the *Philosophy of Theism*; is manifest in all his papers, specially in the *Philosophical Basis of Evolution*; and is by no means least clearly demonstrated in his marvellous calculations of millions of years and intricate mathematical problems, which, as he knew little of mathematics, he worked out by a process of figure-logic entirely his own.

On the day on which he opened the first number of the *Penny Magazine*, young Croll became a student, and a student he continued to the end. His intense desire for a liberal education at school and college could not be gratified, and unfortunately the books in his father's humble home were few and little suited to his taste. Books, however, he would have, and books he succeeded in getting in different ways. The perusal of Joyce's *Scientific Dialogues* and some other works enabled him to master the principles of mechanics and physics, and gave him a taste for natural science; while Dick's *Christian Philosopher*, combined with his own private thoughts on religion, and probably some other influences, stirred his metaphysical powers and led him to the prolonged study of philosophy. The laborious examination of Edwards on the Freedom of the Will provided valuable discipline to his mind, prepared him to tackle Kant and other writers, and produced a confidence in his own powers which led to the publication of a pamphlet on *Predestination* in 1854, and of the *Philosophy of Theism* in 1859. Tempted by the opportunities afforded in the Andersonian, he renewed his scientific reading, and, having already tasted the joy of communicating his thoughts, he soon began that long list of papers which extended over a period of more than twenty-five years, and led to a series of minute researches in almost all departments of science. Few men, probably, in this age, have gone so thoroughly into so many departments of knowledge; fewer still have made more valuable contributions on so many varied themes. Croll was an incessant worker. He lived to work, and whatever he did, he did with all his might. If he could not do a thing thoroughly, he would not do it at all. On one occasion, when he had been invited to prepare a popular hand-book, it was suggested that he might accept the invitation and have the work done by an assistant acting under his direction; he at once repelled the suggestion, almost with scorn. His patience and perseverance were marvellous, marvel-

lous his economy of time and energy. Few men more thoroughly enjoyed the society of friends, few were more truly alive to the charms of music, yet he was almost never seen at concerts or social gatherings. Even his country walks were generally taken alone, that he might be able to muse on the great themes that engrossed his attention. With old age creeping upon him, and health sadly broken, he renewed his theistic studies; and after much new reading, he, with great labour, succeeded in finishing his last book, just as the day was sinking into the night when no man can work.

The amount and variety of the work done by Dr. Croll during the years of his strangely chequered career may be guessed, though by no means measured by a reference to the list of his published book and papers. The time has, perhaps, not yet arrived when an accurate estimate of the value of his researches and speculations in science and philosophy can be formed. He was a born metaphysician, and there can be little doubt that, had he enjoyed the advantage of an early and thorough scholastic training, had he even been permitted, in riper years, to concentrate his energies on the work to which he was devoted, he would have achieved distinguished success as a powerful teacher of philosophy. Through the irony of life, he was compelled, for many years, to toil for bread in the most uncongenial spheres of labour; and when at length he found rest for the sole of his foot, he was led into a series of purely scientific researches which consumed the best years of his life. In these, however, his metaphysical acumen proved of invaluable service, for the themes which chiefly attracted him were of the most complicated and abstract nature; and it may be safely said that the world-wide favour accorded to *Climate and Time*, and its place as probably the most remarkable work connected with geology published during the last half-century, are largely due to the singular philosophic power of its author. *Climate and Cosmology* served largely to develop, illustrate, and confirm Croll's

theory of the secular change of the earth's climate, which, even if it be not in all respects faultless and complete, will for ever be acknowledged as having opened up the way to any thorough explanation of the phenomena with which it deals. In *Stellar Evolution*, Croll treats of a subject which he, with his well-nigh encyclopædic knowledge of science, was peculiarly fitted to discuss. Here, too, he deals in the most careful manner with the almost numberless phenomena involved, and elaborates a theory of the origin of suns and systems, which has already secured the approval of eminent astronomers, and will, at the least, assuredly find its place in the history of astronomical speculation for all time. With this brilliant bit of work Dr. Croll bids farewell to science, and returns to philosophy.

Some of the merits of the *Philosophy of Theism* have already been pointed out in the letters of the late Professor Ferrier of St. Andrews and Principal Cairns of Edinburgh; and we shall only add here that in this early work Dr. Croll was successful in laying down the lines of an argument which adequately meets all the objections to theism which can be suggested by the modern theory of evolution. In the *Philosophical Basis of Evolution*, he returns to this old argument and elaborates it. He clearly states the principle of determinism, which he declares to be the foundation-stone of evolution. He carefully examines its relation to Spencerianism and Darwinism; he proves that force, matter, and motion can never be determined by force, matter, or motion, and he concludes that the universe, in all its beauty, joy, and fulness of life, can never be explained in terms of matter, motion, and force: so that the whole process of evolution, natural selection included, evidently points to theism. The argument, so far as it goes, is simply irresistible; and we can only regret that he returned to his first love too late, and with too little strength left to enable him to prepare an all-round argument for theism worthy of philosophy. The *Basis of Evolution* is, perhaps, chiefly

valuable as the confession of faith made by a master-mind most intimately acquainted with all that science and philosophy can suggest against religion. With all the energy of his nature he had laid hold of the secret power which upholds and informs the universe: he felt that that power is spirit; that though it be infinitely greater than all we can know or conceive, still it is so closely related to us, so essentially one with us, that we may gladly pursue our labours throughout the day, as children working in the Father's vineyard, and when the night comes, lay us down to sleep in perfect peace upon the Infinite Mother's breast.

Of the moral character of Dr. Croll little need be said. To those who knew him best it seemed without a flaw. An inheritance, it was broadened, deepened, and enriched by his strong personal religion. His devotion to truth and right was simply perfect; in his presence falsehood, meanness, or injustice could not exist. His sense of honour was keen even to a fault. In circumstances in which most men would have clung to office for years, he resigned his position in the Survey for the childlike reason that he could not do the work for which he drew his pay; and the noble recognition of his chivalry made by those in power forms a melancholy illustration of what the late M. Amiel calls the law of irony. He was warmly affectionate, strongly attached to relatives and friends. Deep below the apparently austere surface of his nature lay a fount of tenderness which poured forth streams of help and healing all around. Out of his scanty means he liberally gave to relieve distress. Again and again he made the widow's heart to sing for joy, as he sought, with the exquisite delicacy of a Christian gentleman, to brighten her desolate home. Little children loved him well. Deeply sensible of the smallest kindness received, he proved grateful to the end, and ever strove to show his gratitude for what he never could pay back by giving to others such help as had been given to him. Innocent of the slightest taint of

envy or jealousy, he rejoiced in the success of his friends, and spared no pains to bring it about. Again and again he was invited to deliver lectures, and write popular books, which might have brought him fame and monetary reward; but with singular devotion to duty, he declined them all, that he might consecrate his whole time and strength to the pursuit of pure science.

Dr. Croll was a man of a deeply religious nature, which he inherited from his honoured parents; and this nature was so carefully trained in the home, even in childhood, that he may be said to have been a Christian from his earliest days. But, strange to say, for many years after his intellectual new birth, he seems to have devoted no earnest thought to religious subjects. It was only after he had attained to early manhood that the great realities of the spiritual life attracted his attention. Then, however, he brought the whole force of his powerful mind to bear upon the subject of his relation to God, and he was led to recognise Christ as the only Redeemer of sinful men. His entire nature was stirred to its centre as he meditated on the all-important theme, and he surrendered himself wholly—heart and mind and soul—to the Saviour as Lord and Master. It was no mere theory or plan of salvation which Croll accepted. He had entered the presence, he had come beneath the sway of the living, personal Christ, and he yielded himself in absolute submission and devotion as a loyal subject to his rightful King. Thenceforward for him there could be no doubts on the great subject of God revealed in Christ. Other eminent thinkers might in the course of their studies be led to question the great realities of the spiritual world, might need to pass through awful crises in their spiritual history, in which the light of Heaven would seem to be quenched, and might be able to regain their hold of the central truths of religion only after agonies of doubt and conflict. Croll had seen and heard and felt the things of the Spirit, and through all his life of ardent scientific and philosophic research,

he remained a humble, childlike, and consistent Christian.

From the beginning, the earnestness and intensity of his religion impressed his friends and neighbours. In Kinrossie he was held in the highest esteem by Dr. Bonar and all who knew him. In Paisley, where he joined the little band of those who had embraced the theological views of the late Dr. Jas. Morison, of Glasgow, he was elected to fill the important office of deacon; and in Glasgow he was for several years a member of Elgin Place Congregational Church. The religion of Croll was a religion of the deed. He was rarely heard, even by his most intimate friends, to speak on religious topics. When, however, theological subjects were discussed in his presence, he freely granted to earnest thinkers the liberty of thought which he claimed for himself. Sometimes, indeed, when a young man, confident in a little acquaintance with science, was getting beyond his depth, he quietly told him that he knew nothing about the subject. But if the slightest degree of levity or irreverence was shown, he at once rebuked the speaker, yet with a dignity so grave and calm that no sting was left behind; and on one such occasion, at least, of which we have heard, the rebuke was afterwards gratefully recognised. His religious feelings were to him more sacred than even those which a man cherishes toward the mother who gave him birth; they formed his prop and stay amid all the trials of his strangely chequered life; they gave him perfect peace and boundless hope in the solemn hour of death.

Altogether James Croll may be regarded as one of the most remarkable and most typical Scotsmen of his age. Others there may have been who possessed equal intellectual endowments, learning similarly wide and varied, a character as pure, noble, and generous; but for a combination of these eminent properties he was certainly unsurpassed by any of his contemporary fellow-countrymen. None of them has more strikingly displayed

the unwearied industry, the indomitable perseverance, the tenacity of purpose, that are so characteristic of the Scottish people. None has by his own unaided efforts, in the face of almost overwhelming difficulties, raised himself from a beginning so lowly to a position so high in the ranks of the world's thinkers. Like Thomas Carlyle, he was the son of a working stonemason; like him he had to do battle against lifelong physical weakness, and the ever-repeated buffets of misfortune; and, like him, he attained to a world-wide reputation as a profound and brilliant teacher. Croll, however, owed nothing to the early training of grammar school or university; his physical ailments were far more serious, and his misfortunes were continued to the very end. But amid all his trials his faith, his hope, his courage never failed, his serenity of soul remained unmoved. Unweariedly while the day lasted he toiled as one who was ever in the great taskmaster's eye, and only when the shades of night had gathered round his head did he lay him down to rest, leaving a noble record of work behind. But the man was greater far than his work. Those who enjoyed his friendship counted it one of their rarest privileges, and they cherish his memory as that of one of nature's noblemen.

OBITUARY NOTICES

THE following obituary notice by Lord Kelvin was read to the Royal Society, shortly after Dr. Croll's death:—

James Croll, who died on the 15th of December, at the age of sixty-nine, presented in his life a rare case of inborn passion for philosophy and science conquering all obstacles and attaining to the object of lifelong devotion to scientific research and philosophic speculation. Dependent wholly on his own work for his support, he commenced earning a livelihood as a beginner in a merchant's office; and with his ability he might no doubt have earned promotion and become a successful merchant. But the superior attraction of philosophy prevailed, and he wrote a book on *The Philosophy of Theism*, which was published in a large octavo volume, I believe, while he was still working in the merchant's office. After being one of about seventy unsuccessful candidates for the post of underkeeper of the Hunterian Museum of the University of Glasgow, he was appointed in 1859 to the post of janitor of Anderson's College, Glasgow. About this time the Geological Society of Glasgow was founded, and became the centre of an active company of geologists who took up the study of the traces of the Glacial period, so striking and abundant in the West of Scotland. Croll and his successful competitor for the University post, John Young, both of them with characteristic ardour, threw themselves into the work of geology. Croll, according to his peculiar bent of mind, was drawn chiefly into the more speculative lines of geological inquiry, and in 1864 published his "Essay on the Physical Cause of the Changes of Climate during the

Glacial Epoch," which deservedly gained the careful consideration both of geologists and of astronomers. This speculation undoubtedly presented a *vera causa* for some of the changes of climate which have occurred in geological history, although we can scarcely consider it adequate to be so powerful and exclusive a factor as Croll endeavoured to make it. His vigorous dispute with Carpenter regarding oceanic circulation rightly enforced attention to the importance of wind as the prime mover of some of the great ocean currents, but did not overthrow Carpenter's very important views regarding the effects of heat according to which differences of temperature in the water itself in different regions and at different depths have paramount efficacy in producing some of the great oceanic circulations. After serving for eight years as janitor in Anderson's College, Glasgow, Croll was selected by Sir Archibald Geikie to take charge of the maps and correspondence of the Geological Survey in Edinburgh. But according to rule he must be examined, and the Civil Service examiners plucked him in arithmetic and English composition. On the strong urgency of Sir Roderick Murchison (who asked me, from my personal knowledge of Croll, to write a statement of my opinion regarding his qualifications), the Civil Service Commissioners, with a wisely liberal relaxation of their rules, accepted his great calculations regarding the eccentricity of the earth's orbit and the precession of the equinoxes during the last 10,000,000 years as sufficient evidence of his arithmetical capacity, his book on *The Philosophy of Theism* and numerous papers published in scientific journals as proof of his ability to write good English. He was therefore allowed to receive the appointment on the Geological Survey in Edinburgh, though he had failed to pass the qualifying examination. During the rest of his life he was thus kept in relation with the great practical work of the Geological Survey in Scotland, and was allowed time to devote himself to speculative study

and writing in geological physics, astronomy, and philosophy. During the last year of his life he sent to press his last work, published a few weeks before his death, entitled *The Philosophical Basis of Evolution.*[1]

The following obituary notice appeared in *Nature* on 25th December 1890, and which Messrs. Macmillan & Co., publishers, have kindly authorised to be republished here:—

By the death of this well-known writer, geological literature loses one of its most voluminous and able contributors. Though not in the proper sense of the word a geologist, he had made himself well acquainted with many geological problems, and first attracted notice more than twenty-five years ago by the brilliance and suggestiveness of his attempts to solve them. He was born in 1821, at Little Whitefield in Perthshire, and after the usual brief schooling of a peasant's son, he was apprenticed as a millwright in his native village. The employment allowed him leisure for reading, and he devoted himself with ardour to the study of philosophy and of physical science. At the age of twenty-four, however, the effects of an accident which he had met with in boyhood compelled him to seek a less laborious vocation, and eventually he became agent for an insurance company. These early years gave but little promise of the particular bent of his genius by which he would attain distinction. Eventually the general acquirements, and the zeal with which he was known to devote his spare time to philosophical reading, attracted the interest of the governing body of the Andersonian University and Museum in Glasgow, and in 1859 he was appointed keeper of that establishment. He had already found his way into print, by publishing anonymously a volume on *The Philosophy of Theism.* But his new position in an institution devoted largely to the

[1] Remarks by Lord Kelvin in *Proceedings of Royal Society of London*, 1891–92, p. 219.

teaching of science, led him to throw himself more fully into the study of physics. In 1861 he published in the *Philosophical Magazine* his first contribution to scientific literature, a paper on "The Electrical Experiments of Ampère."

About that time the Geological Society of Glasgow was founded, and became the centre of an active company of geologists who specially took up the study of the traces of the Glacial period so striking and abundant in the West of Scotland. Croll was drawn into the prevalent enthusiasm, and soon, with characteristic ardour and enthusiasm, began an investigation of some of the physical difficulties which had arisen in the course of geological inquiry. In 1864 he published his remarkable essay, "On the Physical Causes of the Changes of Climate during the Glacial Epoch." This paper speedily attracted the notice of men of science. In it the author endeavoured to find a true cause for the extension of snow and ice during the Ice age, far beyond their present limits. For this purpose he invoked the aid of astronomic and terrestrial physics, and he provided an explanation which captivated geologists by its simplicity as well as by the wide range of phenomena which it helped to elucidate. It was this paper which laid the foundation of his scientific reputation. It was likewise the means of opening up for him a new and more convenient employment, for it led to his being selected by the present Director-General of the Geological Survey to take charge of the maps and correspondence of the Survey in Edinburgh. He was appointed to this office in 1867, and found himself able to prosecute with more vigour than ever the researches in physical geology which had now so great a charm for him. The question of the origin of climate led him into a far wider field of investigation than he had at first contemplated. It brought him face to face with many theoretic problems which geologists had been unable to solve. These he attacked with characteristic energy. He enforced his

arguments with a single eye to the discovery and establishment of truth, and exposed without reserve views which seemed to him erroneous. With no intention of rousing controversy, he soon found himself in collision with other writers who disputed his arguments. One of the most interesting and vigorous of these disputations was with the late Dr. W. B. Carpenter regarding the theory of oceanic circulation. Croll maintained with great force and with general approbation the position for which he contended, that the prime motors in the circulation of the ocean were the winds. After publishing many papers on this and cognate subjects, he collected, continued, and partly re-wrote these, adding fresh materials to them, and issuing the whole as his well-known work on *Climate and Time in their Geological Relations*, which appeared in 1875. Though much division of opinion was aroused as to the real value of some of his views in relation to the establishment of sound geological theories, there was a general recognition of the originality and acuteness of his mode of dealing with accepted facts and principles and of the value of his writings as strengthening and directing inquiry. He was accordingly elected a Fellow of the Royal Society in 1876, and the University of St. Andrews conferred on him the degree of LL.D.

By degrees, however, Dr. Croll's health began to fail. He suffered so intensely from pains in the head that he was compelled in 1881 to resign his appointment in the Geological Survey, and retire on the miserably small pension to which, by the rigid rules of the Civil Service, his length of service only entitled him. By exercising the greatest care, he was still able at intervals to resume his studies in geological physics, and to publish occasional papers, partly in reply to his critics, who were now increasing in number and pertinacity. In 1885 he published a small volume embracing some of these papers and much new material, under the title of *Discussions in Climate and Cosmology.*

Dr. Croll's investigations into the geological history of terrestrial climate had led him to consider the question of the origin of the sun's heat, and thence to reflect on the possible condition and development of nebulæ and stars. The latter chapters of the volume just mentioned were devoted to these subjects, which he would fain have discussed more at length had not the increasing failure of his bodily powers warned him that if he wished still to return to that philosophy which was his first love, he must husband his remaining strength. Nevertheless the attraction of these astronomic problems proved insuperable. He continued to work at the subject, enlarging the scope of the investigation until it embraced not the earth and the sun merely, but the origin and development of the whole material universe. At last he followed his usual method, gathered together his various contributions to the subject, trimmed, enlarged, and modified them, and published them in a separate volume, entitled *Stellar Evolution in its Relation to Geological Time.*

The publication of that work marks the close of his labours in more definite scientific inquiry. He was now free with such remaining strength as he could command to re-enter the field of philosophic speculation in which he had spent his earliest years of mental exertion, and which for nearly thirty years, through all the engrossing attractions of geological inquiry, had never lost its fascination for him. Accordingly he betook himself once more to the study of such subjects as force, matter, causation, determinism, evolution, etc., and proceeded to apply the facts and principles with which he had in the interval been dealing so actively to the problems in philosophy that had aroused his thoughts in the early years of his life. In spite of his increasing infirmity, he persevered in committing to writing the ideas which he had now formed, and this year he sent to press his last work, published only a few weeks ago, *The Philosophical Basis of Evolution.*

Of all recent writers who have contributed so much

to current scientific literature, probably no one was personally so little known as Dr. Croll. His retiring nature kept him for the most part in the privacy of his own home. But he endeared himself to those who were privileged with his friendship by his gentleness and courtesy, his readiness to help, and the quiet enthusiasm with which he would talk about the topics which absorbed his thoughts. After quitting the Geological Survey of Scotland, he tried residence at different places, in hopes of finding one where his failing bodily health would least impede the powers of his mind, which he retained with singular freshness up to the close. He settled at last in the town of Perth, where he spent the few remaining years of his life. Struggling on in spite of ominous warnings, he finished his last book just before the stroke which carried him off on Monday the 15th inst.

The following obituary notice by J. Horne, F.R.S.E., F.G.S., of H.M. Geological Survey of Scotland, was read to the Geological Society, on the 19th February 1891 :—

Since the opening of the present session, this Society has lost one of its most distinguished Fellows, and the science of geology one of its devoted followers. As an original thinker and powerful investigator in that branch of geology relating to the physical cause of climatic change during geological epochs, Dr. Croll was among the foremost, if not the first, of his time. He has left behind him a brilliant series of researches, of which Scotchmen may well be proud. That he achieved so much without the advantages of special training in his early years, and in spite of a delicate frame, only serves to throw into relief his great intellectual strength and iron will.

Apart from his scientific researches, the story of his life may be briefly told. He was born at St. Martin's, Perthshire, on the 21st January 1821, of parents of humble rank, who were devoted members of the Scotch

Congregational Church. Following in early life the craft of a millwright, he was compelled to abandon it by an injury which he sustained. For a time he tried various occupations, with indifferent success. After years of patient waiting, he caught at last the flowing tide. By the kind intervention of the late Mr. Walter Crum of Thornliebank, he obtained a situation at the Andersonian University, Glasgow — an appointment which proved a great boon to him. It gave him leisure to pursue those studies on electricity, heat, and the physical cause of climatic change, which formed the subjects of his earlier papers. It placed within his reach the University Library and the Library of the Philosophical Society; while the publication of his researches soon brought him into contact with the great leader of science in this country, Lord Kelvin.

But a better fate was yet in store for him. His papers on geological climate and glacial submergence naturally arrested the attention of geologists at home and abroad. The present Director-General of the Geological Survey, Sir A. Geikie, promptly recognised the value of these researches. Thanks to his administrative ability, Dr. Croll was appointed secretary to the Scotch staff of the Geological Survey, when the Survey was reorganised in 1867. For fourteen years he discharged the duties of this office—a period of extraordinary intellectual activity in his career. During his term of service, no note of discord was ever heard from him. His rare modesty, combined with his great intellectual power, won the esteem of all his colleagues, who gladly placed at his disposal any fresh observations in the field bearing on glacial geology.

Eventually the prolonged intellectual strain told on his health, which was never very robust. Serious symptoms supervened in 1881, which compelled him to sever his connection with the Survey. But, though invalided, he was not vanquished. By watchful care, he so husbanded his strength, that he was able at intervals

to pursue his favourite researches for a further period of nine years. A friend who visited him during his last illness says that his powerful mind remained clear and eager to the last, revolving thoughts on favourite themes which his feeble voice could hardly utter. On the 15th of December 1890 he passed away, at the age of sixty-nine.

Turning now to his long series of contributions to science and metaphysics, it will hardly be possible, within the limits of this notice, to do more than give a brief summary of his more important researches, laying special emphasis on those on which his reputation will ultimately rest.

His first work, entitled *The Philosophy of Theism*, published in 1857, at the age of thirty-six, endeavoured to define the relation of Theism to the determination of molecular motion. He tried to show that, for the production of any organism, two things are necessary —first, motion; second, the determination of motion. Mere vital force might account for motion, but the determination of motion implies an idea, design, and a directing mind.

The publication of *The Philosophy of Theism* plainly showed the philosophical bent of his mind, and his early metaphysical bias. But, after his removal to Glasgow, he turned his attention to scientific subjects. In 1861, at the age of forty, appeared his first scientific paper on "Ampère's Experiment on the Repulsion of a Rectilinear Electrical Current on Itself." From this date till his retirement from the Geological Survey,—a period of twenty years,—he published nearly eighty papers, extending over a wide range of subjects. The titles of his earlier communications, prior to 1864, showed the varied character and abstruse nature of his researches, including, amongst others, the following:—"Chemical Combination in Relation to Specific Heat"; "The Cohesion of Gases"; "The Mechanical Power of

Electro-Magnetism"; "The Relation of Chemical Affinity to Vital Force."

In 1864, however, he published a remarkable paper, "On the Physical Cause of the Change of Climate during Geological Epochs," which formed the starting-point for a brilliant series of investigations extending over many years. In connection with this memoir, it is interesting to recall the views then held in this country regarding the phenomena of the Glacial period. Notwithstanding the highly suggestive paper of Agassiz in 1840, in which he showed how the *roches moutonnées*, striations, and glacial deposits indicate the former existence of land ice in Scotland, geologists were slow in accepting his opinions. For years nearly every geologist in Britain clung resolutely to the theory of the iceberg origin of the drift. At length it was vigorously assailed by Sir Andrew Ramsay, Sir Archibald Geikie, and Mr. Robert Chambers. From a careful examination of evidences of ice-action in this country, Canada, and the Continent, Ramsay felt convinced that the theory was no longer tenable. In like manner, Sir Archibald Geikie, who in his early years had accepted the old explanation, was compelled to abandon it in 1861, after an extended series of observations in different parts of Scotland. He prepared an elaborate memoir on the subject, giving a detailed description of the phenomena, and his reasons for attributing them to the action of land ice. While this memoir was in preparation, another eminent Scotch glacialist, Mr. Jamieson of Ellon, arrived at similar conclusions from his own independent observations, and Sir Charles Lyell also adopted the same explanation.

Sir A. Geikie's paper was read in abstract to the Geological Society of Glasgow in 1862, and published as a separate memoir in 1863, appearing subsequently in the first volume of the *Transactions*. When we consider the date of publication of this elaborate memoir, nearly two hundred pages in length, geologists will readily admit that it is of special importance in connection with

the history of glacial geology in Britain. There can be little doubt that it paved the way for the final rejection of the iceberg hypothesis in this country.

The cogent arguments advanced by Sir A. Geikie in favour of the former existence of land ice in Scotland had doubtless a powerful influence on Croll's philosophic mind. He evidently realised that the iceberg theory was doomed, and that well-nigh twenty years had been lost by geologists in this country owing to their stubborn refusal to adopt the suggestions of Agassiz. Accepting the land ice origin of the boulder clay and moraines, Croll proceeded, with characteristic boldness, to grapple with the question of the probable cause of climatic change. To a man of his originality and power, the existence of glacial conditions in Temperate latitudes during former geological epochs must have been a problem of absorbing interest. Various theories have been advanced to account for such alternations of climate. Some have suggested that they might be due to a change in the position of the earth's axis of rotation; others, that the earth may have passed through hot and cold regions of space; while Sir Charles Lyell strenuously advocated the doctrine that they may have been caused by changes in the distribution of land and sea, on the assumption that elevation of land about the poles would lower the temperature of the globe, and that elevation round the equator would raise it. Recent researches, however, are rather opposed to the belief in such enormous terrestrial changes, and seem to point to the permanence of continental and oceanic areas from primeval time.

Owing to an early suggestion of Sir John Herschel, the attention of geologists was directed to the probable effect of cosmical causes in producing climatic change. In 1830, he showed that during a period of high eccentricity, the hemisphere whose winter occurs in aphelion will experience a long and exceptionally cold winter and a hot summer; while the opposite hemisphere will enjoy equable climatic conditions. But he subse-

quently held that the cold of the Glacial period could hardly be due to the direct effects of high eccentricity, because each hemisphere must receive precisely the same amount of heat; and, further, the deficiency of heat resulting from the sun's greater distance would be equalised by the excess of heat received during the short but hot summer.

To Dr. Croll belongs the rare merit of showing that, though glacial cycles may not arise *directly* from cosmical causes, they may do so *indirectly*. As already indicated, his first contribution to the subject was published in 1864, but the development of his theory resulted in a series of brilliant researches extending over a period of eleven years, to 1875. He was led to investigate the problem of the eccentricity of the earth's orbit and its physical relations to the Glacial period. By means of Leverrier's formulæ, he calculated tables of eccentricity for three million years in the past and one million years in the future, with the view of determining the periods of high eccentricity which, according to his theory, were coincident with cycles of extreme cold.

He was further led to consider the various physical agencies affecting climate resulting from periods of high eccentricity, of which by far the most important is the deflection of ocean currents. In connection with this question he called attention to the influence of the Gulf Stream as an agent in the distribution of heat on the surface of the globe. He pointed out that the quantity of heat transferred by the Gulf Stream from equatorial regions into the North Atlantic is enormously greater than was previously imagined, amounting to no less than one-fifth part of the entire heat possessed by the North Atlantic. He contended that "to such an extent is the temperature of the equatorial regions lowered, and that of high temperature and polar regions raised, by means of ocean currents, that were they to cease, and each latitude to depend solely on the heat received directly from the sun, only a very small portion of the globe

would be habitable by the present order of beings." It is obvious, therefore, that if from any cause the Gulf Stream were deflected into the Southern Ocean, the temperature of the northern hemisphere would be greatly lowered.

These latter researches led him to inquire into the physical cause of ocean currents, which proved the most difficult and perplexing part of his investigations. Two theories had been advanced to account for oceanic circulation, viz., the gravitation theory and the wind theory. He controverted that form of the gravitation theory advocated by Lieutenant Maury, who ascribed the currents of the ocean to difference of specific gravity resulting from difference of temperature between the sea in equatorial and polar regions. He also keenly opposed the phase of the gravitation theory expounded by Dr. Carpenter, who maintained that difference of temperature between the sea in equatorial and polar regions produces a general movement of the upper portion of the sea from the equator to the poles and a counter-movement of the under portion from the poles to the equator. On the other hand, he contended that ocean currents are caused and maintained, not by the impulse of the trade winds alone, but of the prevailing winds of the globe regarded as a general system.

These various lines of research are intimately associated with the fundamental question of the physical cause of climatic change.

As Dr. Croll's reputation as an original thinker will ultimately rest on these remarkable memoirs, which, with other researches, were eventually published in one volume, entitled *Climate and Time*, in 1875, a brief statement of his theory may here be given as presented in Chapter iv. of that volume.

With the eccentricity at its superior limit, and the winter occurring in aphelion, the earth would be about eight millions of miles farther from the sun than at present. The reduction in the amount of heat received

from the sun owing to his increased distance would lower the mid-winter temperature to an enormous extent. In temperate regions, the greater part of the moisture of the air is at present precipitated in the form of rain, and the small portion, which falls as snow, rapidly disappears. But in the foregoing circumstances, the mean winter temperature would be lowered so much below the freezing point that what now falls as rain would then fall as snow. But the winters would then not only be colder than now, they would also be much longer. At present the winters are nearly eight days shorter than the summers, but with the eccentricity at its superior limit, and the winter solstice in aphelion, the length of the winters would exceed that of the summers by thirty-six days. The lowering of the temperature and the lengthening of the winter would both tend to increase the amount of snow accumulated during the winter. The result would be, that at the commencement of the short summer, the ground would be covered with the winter's accumulation of snow. The presence of so much snow would lower the summer temperature, and prevent to a great extent the melting of the snow.

There are three ways by which accumulated masses of snow and ice tend to lower the summer temperature.

First. By means of direct radiation.

Second. Because the rays, falling on snow and ice, are to a great extent reflected back into space. Those that are not reflected, but absorbed, do not raise the temperature, as they disappear in the mechanical work of melting the ice. Whatever may be the intensity of the sun's heat, the surface of the ground will be kept at 32° so long as the snow and ice remain unmelted.

Third. Snow and ice lower the temperature by chilling the air and condensing the vapour into thick fogs. The great strength of the sun's rays during summer, due to his nearness at that season, would tend to produce an increased amount of evaporation. But the presence of snow-clad mountains and an icy

season would chill the atmosphere, and condense the vapour into thick fogs. The thick fogs and cloudy sky would effectually prevent the sun's rays from reaching the earth, and the snow in consequence would remain unmelted during the entire summer.

But the cause which, above all others, tends to produce changes of climate is the deflection of ocean currents. A high condition of eccentricity produces an accumulation of snow and ice on the hemisphere whose winter occurs in aphelion. This accumulation tends in turn to lower the summer temperature, to cut off the sun's rays, and so to retard the melting of the snow. It tends to produce on that hemisphere a state of glaciation. But on the other hemisphere, which has the winter in perihelion, opposite effects take place. There the shortness of the winters, and the increase of temperature owing to the proximity of the sun, combine to prevent the accumulation of snow. The general result is that the one hemisphere is cooled, and the other heated. This state of things now brings into play those agencies which tend to the deflection of the Gulf Stream. When the northern hemisphere is being glaciated, the north-east trade winds of this hemisphere will far exceed in strength the south-east trade winds of the southern hemisphere. The median line between the trades will lie considerably to the south of the equator. The effect of the northern trades blowing across the equator to a great distance will be to impel the warm water of the tropics over into the Southern Ocean. By this means the Gulf Stream would be deflected to the south of Cape St. Roque, and would flow along the Brazilian coast into the Southern Ocean. The deflection of the Gulf Stream, combined with the other causes already mentioned, would, according to Dr. Croll, place Europe under glacial conditions, while the temperature of the Southern Ocean would be enormously raised. He shows that but for the Gulf Stream and other currents, London would have a mean annual temperature, 40° lower than at present.

Such is a brief outline of Croll's ingenious method of explaining the occurrence of an Arctic climate in Temperate latitudes in former geological epochs. In support of his theory he appealed with confidence to the evidence in favour of warm Interglacial periods, which he regarded as a crucial test. Reviewing the data which had been gradually accumulated by geologists in favour of the belief that the Ice age was not an epoch of continuous cold, but consisted of a succession of cold and warm periods, he contended that this sequence follows as a necessary consequence from his theory of secular changes of climate.

But in addition to the luminous memoirs bearing on the foregoing theory, Dr. Croll pursued other lines of research, the results of which were published in various periodicals, and subsequently incorporated in his volume on *Climate and Time*. In the *Philosophical Magazine* for 1850 Mr. Alfred Tylor published a paper in which he estimated the amount of sediment brought into the ocean by denuding agents. He inferred that one foot removed off the general surface of the land during that period would raise the sea-level three inches. At a later date Croll approached this question, and pointed out that the rate at which the materials are carried off the land is measured by the rate at which sediment is carried down by our river systems. Hence, in order to determine the present rate of sub-aerial denudation, we have only to ascertain the quantity of sediment annually carried down by the river systems. From the estimates of the materials discharged by the Mississippi, furnished by Humphreys and Abbot, he inferred, in 1868, that the rate of denudation is about one foot in six thousand years. Taking the mean elevation of the land, given by Humboldt at 1000 feet, he contended that the whole would be carried down into the ocean by our river systems in about six million years, if no elevation of the land took place. He further showed the value of this method as a measure of geological time.

Another ingenious investigation relates to the displacement of the earth's centre of gravity by a polar ice-cap, resulting in submergence. This suggestion was first advanced by M. Adhemar, in his work *Revolutions de la Mer*, in 1842, but when Croll published his views on the subject in the *Reader*, he was unaware of Adhemar's conclusions. In connection with this question Croll estimated the probable thickness of the Antarctic ice-cap, and computed the rise in the level of the ocean, resulting from the transfer of an ice-cap two miles thick from the southern to the northern hemisphere. According to the method which postulates the rise at the pole to be equal to the extent of the displacement of the earth's centre of gravity, he inferred that the rise at the North Pole would be about 380 feet, and the rise in the latitude of Edinburgh would be 312 feet. By this means he endeavoured to account for the submergence during the Glacial period, instead of ascribing it to a subsidence of the land.

Following up the idea of the existence of Continental ice-sheets during the Glacial period, he independently suggested that the Scandinavian and Scotch ice-sheets coalesced on the floor of the North Sea, moving westwards towards the Atlantic, thereby accounting for the marine shells and boulders of Secondary rocks in the Caithness boulder clay.

In connection with the movement of Continental ice-sheets and glaciers, he was led to investigate that perplexing question in physics, viz., the physical cause of glacial motion. He reviewed the various theories which had been advanced to explain this phenomenon, indicating various objections to them. He ultimately advanced an ingenious explanation of his own, which may here be briefly summarised from his statement of the theory in *Climate and Time.*

Ice is not absolutely solid throughout. It is composed of crystalline particles which are not packed so closely together as to include interstices. They are united to

one another at special points determined by their polarity, and on this account they require more space. It will be obvious, then, that when a crystalline molecule melts, it will not merely descend by gravitation, but capillary attraction will cause it to flow into the interstices between the adjoining molecules. The moment that it parts with the heat received it will of course resolidify, but it will not solidify so as to fit the cavity which it occupied in the fluid state. For the liquid molecule in solidifying assumes the crystalline form, and of course there will be a definite proportion between the length, breadth, and thickness of the crystal; consequently it will always happen that the interstice in which it solidifies will be too narrow to contain it. The result will be that the fluid molecule in passing into the crystalline form will press the two adjoining molecules aside in order to make sufficient room for itself between them. The crystal will not form to suit the cavity, the cavity must be made to contain the crystal. And what holds true of one molecule holds true of every molecule which melts and resolidifies. This process is therefore going on incessantly in every part of the glacier, and in proportion to the amount of heat which the glacier is receiving. This internal molecular pressure, resulting from the solidifying of the fluid molecules in the interstices of the ice, acts in the mass of the ice as an expansive force tending to cause the glacier to widen out in all directions.

The lateral expansion of the ice from internal molecular pressure, according to Dr. Croll, explains how rock basins may be excavated by means of land ice. It also removes the difficulties experienced in accounting for the movement of ice up a steep slope. Nay, further, he called attention to the fact that the ice which passed over Strathmore must have been over 2000 feet in thickness. An ice-sheet 2000 feet thick exerts a pressure on the rocky floor of upwards of 51 tons per square foot. When we reflect, he contended, that ice under such enormous pressure, with grinding materials

lying underneath, was forced by irresistible molecular energy up an incline of one in seven, it is not at all surprising that the hard lava should be ground down and striated. It also helps us to realise how the softer portions of the rocky surface over which the ice moved should have been excavated into hollow basins.

The speculations of physicists regarding the limit of geological time prompted him to investigate the question of the probable age and origin of the sun. Accepting gravitation as the only conceivable source of the sun's heat, he reviewed the two forms in which this theory had been presented, first, the meteoric theory advocated by Meyer, and, second, the contraction theory, expounded by Helmholtz. Even if we postulate 100 millions of years as the limit of geological time, he maintained that gravitation will not account for the supply of the sun's heat during so long a period. According to the foregoing theories, it is assumed that the matter composing the sun, when it existed in space as a nebulous mass, was not originally possessed of temperature, but that the temperature was developed as the mass condensed under the force of gravitation. He suggested that the nebulous mass might have been possessed of an original store of heat previous to condensation. Proceeding to consider how the sun's mass could have become possessed of energy in the form of heat previous to condensation, he argued that it was readily explained by means of the dynamical theory of heat. "Two bodies, each one-half the mass of the sun, moving directly towards each other with a velocity of 476 miles per second, would by their concussion generate in a single moment the 50,000,000 years' heat." He further contended that we are led from physical considerations regarding the age of the sun's heat to the conclusion that the geological history of our globe must be limited to 100 millions of years.

His great work, *Climate and Time*, embodying the foregoing researches, which appeared in 1875, produced a profound impression on geologists at home and abroad.

His opponents, as well as those who adopted his opinions, admired the originality and philosophic grasp displayed by the author. Hardly eleven years had elapsed since the publication of his first paper bearing on the physical cause of climatic changes, and yet, quite a revolution had taken place in the method of interpreting glacial phenomena. The marvellous development of opinion regarding glacial questions had been signalised by the publication of Professor James Geikie's epoch-making volume, *The Great Ice Age*, in 1874. The views so ably expounded by these authors regarding the former extension of land-ice in the northern hemisphere during the Glacial period, have been generally accepted, and now form part of the common stock of geological knowledge all over the world.

But the theory developed by Dr. Croll of the physical causes of secular changes of climate, and the researches on which that theory was based, gave rise to prolonged controversy. Though one of the most modest of men, he was a keen controversialist. The numerous replies to his antagonists appeared chiefly in the pages of *Nature* and the *Philosophical Magazine*. They were subsequently collected and published in 1885 in *Climate and Cosmology*, which is to a large extent a further development of his previous work. In 1889 he published a volume on *Stellar Evolution*, in which he states the geological argument against the views held by Thomson and Tait regarding the age of the sun.

The few remaining months of his life were devoted to a wholly different field of inquiry. He reverted to those philosophical questions which had fascinated him in his early years. In his last work, *The Philosophical Basis of Evolution*, which issued from the press about three months before his death, he contended that evolution by force was impossible. He maintained that the production of motion and the determination of motion, the production of force and the determination of force, are absolutely and essentially different. Reviewing the theory

of development advocated by Darwin and Spencer, he associates the phenomena of evolution with this continuous direction of motion, which to his mind betoken will and purpose. To those who enjoyed his friendship, he occasionally referred to this question in terms which plainly indicated that he did not halt between two opinions. The idea that a Supreme Will directed the course of Nature was the cardinal point in his faith.

Of his private life it may be truly said that "whatever record leaps to light, he never shall be shamed"; of his career as a man of science it may be confidently asserted that he has nobly sustained the reputation of the Scottish school of geology, which was founded by the genius of Hutton and Hall.

PROFESSOR M'FARLAND'S LETTER

THE following letter from Professor M'Farland, of America, throws some light on the Croll-Newcomb controversy, and shows how Dr. Croll was esteemed in America:—

CONING, O., *22nd March* 1895.

James Campbell Irons, Esq., Edinburgh.

DEAR SIR,—It is a week since I returned from my home in Oxford, but press of business has hindered me from writing for a few days.

My chair was Mathematics and Astronomy—not Geology, although I have spent a good deal of time on the latter subject, especially in its connection with astronomy in accordance with Dr. Croll's theory.

I give a general view of the subject, in as brief a form as I can, so that I may not be unduly tedious to you.

Oxford is nearly 200 miles almost south of the west end of Lake Erie, and is not far from the Ohio River. For a thousand miles east and west, and for an average of 150 north and south, along the whole southern boundary of our great lakes, the country is, I may say, covered with boulders, and these, in places, extend many feet down below the surface. In this whole region there is scarcely an acre without boulders. The rocks are of the same kind as found in the Canada Highlands, 200 miles north of Lake Erie. In some places which I have seen, you could walk for hundreds of yards, some places for half a mile on the boulders, stepping from one to the other, without putting a foot on the ground. In many places I have seen fields of 12 or 15 acres fenced

entirely with boulders piled up as a wall, and enough left on the ground to make it look as a place from which the forest had been but lately cut. These are of all sizes, from that of a pint cup to many tons weight. These facts troubled the geologists, and set them to speculating. Our first State Geological Survey of Ohio, was made in 1836–37, before there was any very definite conception of the Glacial period. Of course, after the preliminary suggestions of De Saussure and Agassiz in Switzerland, the American geologist seized upon the suggestions and *theorised bountifully.* Ohio had two colleges nominally under its care, but it had never furnished a dollar for either, until a period twenty years subsequent to our great Civil War. In 1856, I was called to the Chair of Mathematics and Astronomy in the one situated at Oxford, near Cincinnati, in the south-west corner of the State. During the progress of the War, the Congress of the U.S. granted to the several States 30,000 acres of the public lands, still unoccupied, for each Senator and Congressman from the several States. Ohio, having nineteen members of the Lower House and two Senators, came into possession of 630,000 acres. The proceeds of the sale were to found and sustain an institution to be called "The Agricultural and Mechanical College." The name was subsequently changed to "The Ohio State University." Edward Orton, State Geologist of Ohio, was made President of the new college, and inasmuch as the finances of the older State colleges were in a state of collapse from the effects of the war, the college at Oxford was closed, and I was transferred to the one at Columbus, the capital of the State. When Dr. Croll's book came out in 1875, Dr. Orton said to me that if Dr. Croll's calculations could be trusted, the *great geological enigma would be fully explained.* He asked me if I understood that sort of calculation. I replied that I did, but that I had not made any such myself, that any astronomer knew how it was done. He requested me to test the computations,

at least in part. You will see a little of this referred to in the pamphlet which I published, and which contains the eccentricity and the longitude of the perihelion for four million years. Almost immediately Dr. Croll's theory was attacked by Simon Newcomb, who was of the astronomical staff of the Naval Observatory, at Washington City, and who was also directly concerned in the *Nautical Almanac* of the United States. He had said that the results of Croll's work were not to be relied on. President Orton was greatly concerned at this unexpected criticism. But Newcomb was one of the Regents of the Smithsonian Institution of Washington City. Of course you know something of that institution. Now John U. Stockwell, one of our astronomers, in his very full and excellent treatise on the "Theory of the Moon," into which he introduced the disturbing effects of Neptune, had calculated the eccentricity of the earth's orbit for 1,000,000 years at intervals of 4000 years. This treatise had been highly praised by Newcomb, and as one of the Regents of the Smithsonian, he had voted to print Stockwell's work, and it was done. It is in the eighteenth volume of the *Smithsonian Contributions*. Here was a good opportunity to test Mr. Newcomb. I knew him to be a man extremely well posted in some things, and extremely unversed in many others, and also inclined to hasty judgments. I have long known him personally. *He had highly commended and published Stockwell's work, he denied the validity of Croll's.* I went over the calculations for 1,100,000 years by *both* formulæ —Leverrier's, which Croll had used, and Stockwell's independent formula, which Newcomb had praised. On showing the results in the progress of the work, Dr. Orton became greatly interested. For about 350,000 years the curves by the two methods differed but little, for 70,000 years they were almost identically the same. Of course Dr. Croll's work was corroborated. Dr. Orton urged me to publish the result. It was done in the *American Journal of Science*, at Yale College, in

June 1876. I gave no names but those of Croll and Stockwell, but the result "knocked" Newcomb out of the ring. Mr. Croll saw that publication, and at once wrote to me his gratification in the unexpected corroboration. We continued our correspondence once or twice a year from 1876 to the time of his death. A good many brief articles were published from time to time by different persons on this side the sea—to most of which Dr. Croll replied—and with the natural result that some of our scientists were completely overwhelmed, and the silly criticisms drowned with them. Some one in the *Sidereal Messenger* also tried his hand, and I replied to him in the same journal. Dr. Croll mentions this in one of the letters which I send you. So far as I know, it was the universal opinion here that the critics of Dr. Croll's theory had been put completely *hors de combat.* The above remarks will fully explain the introductory statements made in the pamphlet above referred to. Dr. Croll, in the third chapter of his *Climate and Cosmology*, refers to this work, as you will see.

In my judgment, Dr. Orton stands leading among the geologists of the United States. He at once adopted the theory as a "good working hypothesis," and, so far as I know, it was accepted more or less by all geologists.

Of later years there seems to be a desire among many of the geologists, especially among the noisiest, even when they are not the most learned, to want some additional help to Dr. Croll's theory. The general import of these views is well given by Le Conte, a most able geologist of California, in his work on geology, published a few years ago. In my view, the sinking of the border land on both sides of the boundary line between Canada and the United States, which is admitted to have occurred, *is a direct effect of the theory*, and is a strong confirmation of its general truth. Dr. Croll nowhere says that no other causes operated,—and, of course, such of the effects as that just referred to, belong to and constitute a part of the theory.

The objectors, so far as I know, are among the younger geologists.

I send you a transcript of a letter which was published in New York, in the April number of the *Popular Science Monthly*, 1894. It is folded separately from these sheets, and is marked "A." On reading it, you see it fully explains itself, and the occasion of its being written. Of course you may know that the *Prestwich* therein named belongs to your side of the Atlantic. If you will read paper "A" right here, it will make plain what follows immediately hereafter.

The gorge of the Niagara has played a considerable part in the estimation of those geologists who think the "Ice age" ended "80,000" years ago, as Mr. Prestwich says. The same error (not to say ignorance of the question) is widespread among our geologists. The length of the gorge and its present rate of recess—about 2·4 feet per year—would require about 15,000 years to pass over the seven intervening miles from Lake Ontario to the present position. Now some of our theologians call this 7000 years, and so belittle it, by ignoring the facts, and substituting their own views. I show in letter "A" that 30,000 years is as remote a period as that gorge can be pushed back to,—20,000 or 25,000 is a much more probable one. Rev. G. Frederick Wright of Oberton College, Ohio, has for ten or twelve years taken great interest in the Ice age—has written two books on the subject, and finds unmistakable evidence that man was in this continent in the Ice age, having left his mark in the débris; hence, the "Ice Age" is, in his view, a recent event. I am acquainted with him; he is able and honest. He was one of the original discoverers of the old outlet of Lakes Huron, Michigan, and Superior, northward through Lake Nipissing to the Ottawa River in the province of Ontario. In a letter explanatory of this outlet, published in the New York *Nation*, September 22, 1892, he says: "Since the scenes of the earlier times, many thousand years have rolled by, and mean-

while the northern country, *which had been so much depressed beneath its load of glacial ice* has been slowly returning towards its former position. The current of the Detroit River has been reversed, and now we have Niagara, whose age is pretty well known. At the present rate of the recession of the falls, less than 10,000 years would be required to form the gorge above Queenstown." (This is a mistake, as I have shown above. I was present at Niagara a few years ago, when our Lake Survey Company measured the present position of the falls. Hall of New York State, who was State Geologist in 1842, was also present, and assisted. He had marked the position in 1842. From this measurement, which I looked on and saw, the 2·4 feet is obtained.) Mr. Wright continues: "Of late this has been taken as a glacial chronometer. But this discovery of an earlier northern outlet for the Great Lakes will, as Mr. Gilbert some time ago surmised, *considerably lengthen our calculations*." After the publication of letter "A," I called Mr. Wright's attention to the facts therein mentioned—he did not then, and has not since, offered any objection to the assertions of my letter. You easily see that all these things tend to the establishment of the substantial truth of Dr. Croll's theory. Mr. Wright's "considerably lengthen our calculations," and his "many thousand years" mentioned a few lines above, forced him, willy-nilly, into the position of one who acquiesces in Croll's theory. A year or two ago, I reviewed Le Conte's statements, and published in *Science* in New York,—but the review covers ground not essentially differing from the above. I have no copy of that publication at hand. In concluding this part of the subject, I say that I believe every objection to Dr. Croll's theory, having any weight whatever, has been explained in a satisfactory way, or has been shown either to proceed directly from that theory, or to harmonise with it. After remaining from 1873 to 1885 in the State University at Columbus, Ohio, the trustees requested me to take charge of the College

in Oxford, and resuscitate it. For a limited time I took charge of it, resigning my position in 1888, after seeing the institution well under way again. Nearing the old limit, "threescore and ten," I am not now teaching, but my old studies are continued. In moving back from Columbus to Oxford, 120 miles, my letters were put promiscuously in several boxes to fill up corners. They were unpacked at Oxford, and several thousand thrown miscellaneously into a large box. I searched as carefully as I could among these, and found the letters, which I herewith inclose. I think I have all but four or five. The correspondence was kept up at irregular intervals, when something which occurred was deemed of sufficient moment to communicate. The letters will all be plain, when taken in connection with this long letter. Somewhere among my papers are many brief printed articles on the general theory in question. But I do not know where to begin to look for them. Still, most of them first saw the light in Scotland, and the others in different numbers of the *American Journal of Science*,—edited by "Dana" who is mentioned in one of these letters.

These were generally answers to ill considered or ignorant statements made by persons who, if they really were not, should have been, on the learner's bench, and not elsewhere. And our general correspondence touched whatever was floating in the breeze on this general topic.

When your publication is made, I shall be glad to pay for a copy, that thereby I may add a little to the sum which you hope to realise.—I am yours, sir, very cordially, for our friend's sake,

R. W. M'FARLAND,

Late Professor of Mathematics and Astronomy, Ohio State University, and, for a time, President of the State College at Oxford.

APPENDIX

LIST OF SCIENTIFIC PAPERS AND WORKS

1857

1. *The Philosophy of Theism.* London: Ward & Co.

1861

2. Remarks on Ampère's Experiment on the Repulsion of a Rectilineal Electrical Current on itself. *Philosophical Magazine*, April 1861.

1862

3. On Chemical Combination in Relation to Specific Heat. Read before the Chemical Society, March 6, 1862. *Chemical News*, March 8, 1862.
4. Remarks on Ampère's Experiment on the Repulsion of a Rectilinear Current on itself. *Philosophical Magazine*, May 1862.
5. On the Cohesion of Gases, and its Relation to Recent Experiments on the Thermal Effects of Elastic Fluids in Motion. *British Association Report*, 1862, p. 21 (Sections).
6. On the Mechanical Power of Electro-Magnetism. *British Association Report*, 1862, p. 24 (Sections).
7. Ampèrian Repulsion. *Philosophical Magazine*, Oct. 1862.

1863

8. On the Relation of Chemical Affinity to Vital Force. *Chemical News*, May 16, 1863.

1864

9. On Supposed Objections to the Dynamical Theory of Heat. *Philosophical Magazine*, March 1864.
10. On the Influence of the Tidal Wave on the Earth's Rotation, and on the Acceleration of the Moon's Mean Motion. *Philosophical Magazine*, April 1864.
11. On the Nature of Heat Vibrations. *Philosophical Magazine*, May 1864.
12. On the Cause of the Cooling Effects produced on Solids by Tension. *Philosophical Magazine*, May 1864.
13. On the Physical Cause of the Change of Climate during Geological Epochs. *Philosophical Magazine*, August 1864.

1865

14. On the Physical Cause of the Submergence of the Land during the Glacial Epoch. *The Reader*, Sept. 2, 1865. *Natural History Review*, 1865, p. 594.
15. On the Submergence of the Land. *The Reader*, Oct. 14, 1865.

    With first suggestion as to the Invasion of the North Sea by Land Ice during the Glacial Epoch.

16. On Glacial Submergence. *The Reader*, December 2 and 9, 1865.

1866

17. On the Eccentricity of the Earth's Orbit. *Philosophical Magazine*, Jan. 1866.
18. Glacial Submergence on the Supposition that the Interior of the Globe is in a Fluid Condition. *The Reader*, Jan. 13, 1866.
19. Glacial Submergence. *The Reader*, March 3 and 26, 1866.

20. On the Physical Cause of the Submergence and Emergence of the Land during the Glacial Epoch, with a note by Prof. Sir Wm. Thomson. *Philosophical Magazine*, April 1866.
21. On the Influence of the Tidal Wave on the Motion of the Moon. *Philosophical Magazine*, Aug. and Nov. 1866.
22. On the Reason why the Change of Climate in Canada since the Glacial Epoch has been less complete than in Scotland. *Transactions of the Geological Society of Glasgow*, 1866.

1867

23. On the Eccentricity of the Earth's Orbit and its Physical Relations to the Glacial Epoch. *Philosophical Magazine*, February 1867.
24. On the Reason why the Difference of Reading between a Thermometer exposed to Direct Sunshine and one shaded diminishes as we ascend in the Atmosphere. *Philosophical Magazine*, March 1867.
25. On the Change in the Obliquity of the Ecliptic; its Influence on the Climate of the Polar Regions and Level of the Sea. *Transactions of the Geological Society of Glasgow*, vol. ii. p. 177. *Philosophical Magazine*, June 1867.
26. Remarks on the Change in the Obliquity of the Ecliptic and its Influence on Climate. *Philosophical Magazine*, Aug. 1867.
27. On certain Hypothetical Elements in the Theory of Gravitation, and generally received Conceptions regarding the Constitution of matter. *Philosophical Magazine*, Dec. 1867.

1868

28. On Geological Time, Part I. Method of Determining the Rate of Sub-aerial Denudation. *Philosophical Magazine*, May 1868.

29. On Geological Time, Part II. Tables of Eccentricity of the Earth's Orbit. *Philosophical Magazine*, Aug. 1868.

30. On Geological Time, Part III. Inquiry into the Effects of Icebergs, Interglacial Periods, etc. *Philosophical Magazine*, Nov. 1868.

With the Suggestion that the Warm and Cold Periods of the Glacial Epoch explain the commingling of Mammalia of Subtropical and Arctic Types in the Cave and River Deposits.

1869

31. On the Physical Cause of the Motions of Glaciers. *Philosophical Magazine*, March 1869. *Scientific Opinion*, April 14, 1869.

32. On the Influence of the Gulf Stream. *Geological Magazine*, April 1869. *Scientific Opinion*, April 21 and 28, 1869.

33. On Mr. Murphy's Theory of the Cause of the Glacial Climate. *Geological Magazine*, Aug. 1869. *Scientific Opinion*, Sept. 1, 1869.

34. On the Opinion that the Southern Hemisphere loses by Radiation more heat than the Northern, and the supposed Influence that this has on Climate. *Philosophical Magazine*, Sept. 1869. *Scientific Opinion*, Sept. 29, Oct. 6, 1869.

35. 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. *Transactions of the Geological Society, Edinburgh*, vol. i. p. 330.

1870

36. On Ocean Currents, Part I. Ocean Currents in relation to the Distribution of Heat over the Globe. *Philosophical Magazine*, Feb. 1870.

37. On Ocean Currents, Part II. Ocean Currents in relation to the Physical Theory of Secular

Changes of Climate. *Philosophical Magazine*, March 1870.

38. On the Path of the Ice-Sheet in North-Western Europe and its Relations to the Boulder Clay of Caithness. *Geological Magazine*, May and June 1870.

    Reprinted, with a slight rearrangement of paragraphs, in *Climate and Time*, chap. xxvii.

39. On the Cause of the Motion of Glaciers. *Philosophical Magazine*, Sept. 1870. Short abstract by Editor in *Geological Magazine*, Dec. 1870.

40. On Ocean Currents, Part III. On the Physical Cause of Ocean Currents—Examination of Lieut. Maury's Theory. *Philosophical Magazine*, Oct. 1870.

1871

41. On the Transport of the Wastdale Granite Blocks. *Geological Magazine*, January 1871.

42. On a Method of Determining the Mean Thickness of the Sedimentary Rocks of the Globe. *Geological Magazine*, March 1871.

43. Mean Thickness of the Sedimentary Rocks. *Geological Magazine*, June 1871.

44. On the Age of the Earth as determined from Tidal Retardation. *Nature*, Aug. 24, 1871. *English Mechanic*, Sept. 1, 1871.

45. Ocean Currents: On the Physical Cause of Ocean Currents. Examination of Dr. Carpenter's Theory. *Philosophical Magazine*, Oct. 1871.

1872

46. Ocean Currents. *Nature*, Jan. 11, 1872.

47. Ocean Currents: Reply to Mr. Ferrel. *Nature*, March 21, 1872.

48. Ocean Currents: Proof that Ocean Currents are not due to Gravity. *Nature*, April 25, 1872.

49. Ocean Currents: Reply to Mr. Ferrel. *Nature*, July 25, 1872.
50. What Determines Molecular Motion?—The Fundamental Problem of Nature. *Philosophical Magazine*, July 1872.
51. Kinetic Energy. *Nature*, Aug. 15, 1872.
52. Oceanic Circulation: Reply to Professor Everett and Mr. Wallace. *Nature*, Oct. 3, 1872.

1874

53. Ocean Currents: Further Examination of Gravitation Theory. *Philosophical Magazine*, February 1874.
54. Ocean Currents: The Wind Theory of Oceanic Circulation. *Philosophical Magazine*, March 1874.
55. Ocean Currents: Reply to Dr. Carpenter. *Nature*, May 21, 1874.
56. On the South of England Ice-Sheet. *Geological Magazine*, June 1874.
57. The Physical Cause of Ocean Currents. *Philosophical Magazine*, June 1874. *American Journal of Science and Arts*, September 1874, p. 228.
58. On the Physical Cause of the Submergence and Emergence of the Land during the Glacial Epoch. *Geological Magazine*, July and August 1874.

1875

59. *Climate and Time in their Geological Relations: A Theory of Secular Changes of the Earth's Climate.* London: Daldy, Isbister & Co.
60. *Climate and Time.* Letter in *Nature*, Aug. 26, 1875.
61. The *Challenger's* Crucial Test of the Wind and Gravitation Theories of Oceanic Circulation. *Philosophical Magazine*, September 1875. *American Journal of Science and Arts*, September 1875, p. 222. *British Association Report*, 1875.

62. The Wind Theory of Oceanic Circulation. Objections Examined. *Philosophical Magazine*, October 1875. *American Journal of Science and Arts*, January 1876, p. 58.
63. Oceanic Circulation: Reply to Dr. Carpenter. *Nature*, October 7, 1875.
64. Oceanic Circulation: Expansion of Sea Water as Determined by Professor Thorpe, in Relation to. *Nature*, Nov. 25, 1875.
65. Further Remarks on the Crucial Test Argument. *Philosophical Magazine*, December 1875.

1876

66. Remarks on Mr. Burns's Paper on the Mechanics of Glaciers. *Geological Magazine*, August 1876.
67. On the Transformation of Gravity. *Philosophical Magazine*, Oct. 1876. Short abstract in *British Association*, 1876.
68. On the Tidal Retardation Argument for the Age of the Earth. *British Association Report*, 1876. *Nature*, Sept. 28, 1876. *American Journal of Science and Arts*, Dec. 1876, p. 457.

1877

69. On the Probable Origin and Age of the Sun. *Quarterly Journal of Science* for July 1877.

1878

70. Le Sage's Theory of Gravitation. *Philosophical Magazine*, Jan. 1878. *American Journal of Science and Arts*, Feb. 1878, p. 146.
71. The Age of the Sun in Relation to Evolution. *Nature*, Jan. 10, 1878; Feb. 21, 1878. *American Journal of Science and Arts*, March 1878, p. 226.
72. Age of the Sun in Relation to Evolution: Motion of the Stars. *Nature*, April 11, 1878.
73. The Origin of Nebulæ. *Philosophical Magazine*, July 1878.

74. Cataclysmic Theories of Geological Climate. *Geological Magazine*, Sept. 1878. Short abstract in *American Journal of Science and Arts*, Nov. 1878, p. 387.

1879

75. The Thickness of the Antarctic Ice, and its Relations to that of the Glacial Epoch. *Quarterly Journal of Science*, Jan. 1879.
76. Outline of Secular Theory of Change of Geological Climate, for article "Geology," in *Encyclopædia Britannica*, vol. x., and Professor Geikie's Text-Book of Geology, 1882, p. 23.
77. Interglacial Periods. *Geological Magazine*, Oct. 1879.
78. Why the Air at the Equator is not Hotter in January than in July. *Nature*, Dec. 17, 1879. *American Journal of Science and Arts*, Feb. 1880, p. 142.

1880

79. Mr. Hill on the Cause of the Glacial Epoch. *Geological Magazine*, Feb. 1880.
80. The Temperature of Space and its Bearings on Terrestrial Physics. *Nature*, April 1, 1880.
81. Aqueous Vapour in Relation to Perpetual Snow. *Nature*, July 1, 1880. *Geological Magazine*, Aug. 1880. *American Journal of Science and Arts*, Aug. 1880, p. 103.

1883

82. Evolution by Force Impossible: A New Argument against Materialism. *British Quarterly Review*, Jan. 1883.

1884

83. Examination of Mr. Alfred Wallace's Modification of the Physical Theory of Secular Changes of Climate. *Philosophical Magazine*, Series 5, xvii. 81.

84. Remarks on Professor Newcomb's Rejoinder. *Philosophical Magazine*, Series 5, xvii. 275.
85. Examination of Mr. Alfred Wallace's Modification of the Physical Theory of Secular Changes of Climate. Part II. Geological and Palæontological Facts in Relation to Mr. Wallace's Modification of the Theory. *Philosophical Magazine*, Series 5, xvii. 367.
86. On the Cause of Mild Polar Climates. *Philosophical Magazine*, Series 5, xviii. 268.

1885

87. On the Arctic Interglacial Periods. *Philosophical Magazine*, Series 5, xix. 30.
88. *Discussions on Climate and Cosmology.* 8vo. Edinburgh. Pp. 1–327.

1889

89. Rate of Sub-aerial Denudation. *Geological Magazine*, Dec. 3, vi. 526.
90. On Prevailing Misconceptions regarding the Evidence which we ought to expect of Former Glacial Periods. *Quarterly Journal Geological Society*, xlv. 220.
91. *Stellar Evolution and its Relations to Geological Time.* 8vo. London. Pp. 1–118.

1890

92. *The Philosophical Basis of Evolution.* 8vo. London. Pp. 1–204.

MEMORIAL TO TREASURY

Unto the Right Honourable

THE LORDS OF THE COMMITTEE OF COUNCIL ON EDUCATION,

Science and Art Department.

MEMORIAL of JAMES CROLL, LL.D., F.R.S., formerly of the Geological Staff for the Survey of Scotland.

MY LORDS,—I beg to submit the following statement of my case for your Lordships' consideration:—

At the time of the reorganisation of the Geological Survey in 1867, Professor Geikie, who had been appointed Director for Scotland, wrote to me asking if I would accept an appointment on the Scottish staff. I wrote stating that I was too far advanced in life, being then forty-six years of age, to think of entering upon the laborious duties of a field geologist. He replied, mentioning that as it would be necessary for one of the officers to be resident in Edinburgh, to take charge of the office work, I would receive that post if I desired. After consulting my friends, and duly thinking over the matter, I agreed to enter the Survey on these conditions. On 20th August 1867, I received a notice of my appointment. I then resigned my situation in Anderson's University, Glasgow, and on 2nd September entered upon the duties of my office. These I have endeavored to perform to the best of my abilities.

For a number of years I have been suffering from a somewhat mysterious pain in the heart, which, however, never in any way affected my general health, or prevented

me for a single hour from attending to my ordinary office duties. One day in the autumn of 1880, when hurriedly removing some maps from one of the drawers in the office, while standing in an awkward position on a pair of high steps, I accidentally gave my heart a slight strain. It had the effect of intensifying to such an extent the pain in my heart that it entirely disabled me for work. It was accompanied by a sleepless condition, which terminated in a slight attack of paralysis. I was then advised by an eminent physician to try to retire, if possible, from the active duties of life. As I had at the time been only thirteen years in the service, and, being married, the amount of superannuation to which this period would entitle me was insufficient to meet my daily wants, it became rather a serious matter to give up my situation.

The Director, Professor Geikie, who sympathised with me very much, kindly stated that he would endeavour to obtain a little more than the bare amount of pension that might be allotted for the time I had served. He mentioned that he would write an official letter to the Director-General to that effect, which would probably be forwarded to the Department, backed up by one from the Director-General himself, and that in all probability the request would be complied with. On the faith of this promise, which, I have every reason to believe, was fulfilled, I at once resigned my situation. After a lapse of four or five months, I received a notice, dated 5th May 1881, to the effect that the Treasury had awarded me a superannuation allowance of £75, 16s. 8d., a sum which was simply equal to thirteen-sixtieths of my annual salary. I thus received no additional sum beyond what my length of service strictly entitled me to, and no account whatever appears to have been taken of the fact that my age was over sixty.

By Section iv. of the Superannuation Act, 1859, it is provided: "It shall be lawful for the Commissioners of the Treasury from Time to Time, by any Order or Warrant, to declare that for the due and efficient

Discharge of the Duties of any Office or Class of Offices to be specified in such Order or Warrant, professional or other peculiar Qualifications, not ordinarily to be acquired in the Public Service, are required, and that it is for the Interest of the Public that Persons should be appointed thereto at an Age exceeding that at which Public Service ordinarily begins; and by the same or any other Order or Warrant to direct that when any Person now holding or who may hereafter be appointed to such Office or any of such class of Offices shall retire from the Public Service, a Number of Years not exceeding twenty, to be specified in the said Order or Warrant, shall, in computing the Amount of Superannuation Allowance which may be granted to him under the foregoing Section of this Act, be added to the Number of Years during which he may have actually served; and also to direct that in respect of such office or Class of Offices the Period of Service required to entitle the Holders to Superannuation may be a Period less than Ten Years, to be specified in the Order or Warrant; and also to direct that, in respect of such Office or Class of Offices, the Holder may be entitled to Superannuation, though he may not hold his Appointment directly from the Crown, and may not have entered the Service with a Certificate from the Civil Service Commissioners: Provided always, that every Order or Warrant made under this Enactment shall be laid before Parliament."

I humbly submit that my case falls within this Section, and that in computing the amount of superannuation to be granted to me, I may fairly expect to have a number of years, not exceeding twenty, added to the number of years I actually served.

I may here refer to the fact that two assistant geologists, disabled for duty about the same time as I was, the one on the English and the other on the Irish staff, did get large additions to their pensions. The English assistant, in the computation of his allowance,

had twenty years added to the twelve years which he had served, and thus received a pension equal to thirty-two-sixtieths of his annual salary. The Irish assistant had twenty-four years added to his thirteen years, so that his allowance amounted to thirty-seven-sixtieths of his salary, *i.e.* to a sum within three-sixtieths of the highest retiring allowance; yet this gentleman is twenty-one years younger than I am.

When it was found that I had received an allowance too small to afford a maintenance, Professor Geikie kindly drew up a memorial to the Prime Minister on my behalf, praying that he would recommend the grant of a small sum annually from the Civil List. This memorial was signed by a number of the leading Fellows of the Royal Society of London, and by several influential members of Parliament. After anxiously waiting for a period of eighteen months, an answer to it has been received, to the effect that the right honourable gentleman does not consider my claim to be one which he could recommend for the Civil List, but he would grant a sum of £100 from the Queen's Bounty. This sum, of course, can in no way be considered as compensation for the deficiency of my pension, or an amount sufficient to meet my wants.

The foregoing statement explains how application was not made at an earlier date, but looking to the fact that I was asked to enter the service in the knowledge of my then advanced age, that I have served faithfully till my health would not permit of further service, that the Superannuation Act specially provides for such cases as mine, and that precedents exist in the Department for such a course, I humbly submit that in the circumstances I have a good claim for the reconsideration of my case, and for the award of a higher pension than that granted to me, which is quite inadequate for my maintenance.—I am, Your Lordships' obedient servant,

JAMES CROLL.

19 NORTH METHVEN STREET, PERTH.

We, the undersigned, strongly commend the claims of Dr. Croll to the favourable consideration of the Lords of the Committee of Council.

His Grace the Duke of Devonshire, K.G., M.A., D.C.L., F.R.S., etc., Chancellor of the University of Cambridge.

The Most Noble the Marquess of Salisbury, K.G., M.A., Chancellor of the University of Oxford, and Vice-President of the Royal Society.

His Grace the Duke of Argyll, K.T., D.C.L., F.R.S., etc., Chancellor of the University of St. Andrews.

His Grace the Duke of Buccleuch and Queensberry, K.G., D.C.L., etc., Chancellor of the University of Glasgow.

His Grace the Duke of Richmond and Gordon, K.G., D.C.L., etc., Chancellor of the University of Aberdeen.

The Right Honourable John Bright, M.P., Lord Rector of the University of Glasgow.

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W. Kitchen Parker, F.R.S., F.Z.S., F.L.S., etc., Hunterian Professor of Comparative Anatomy in the Royal College of Surgeons.

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David J. Hamilton, M.B., Professor of Pathological Anatomy in the University of Aberdeen.
Balfour Stewart, M.A., LL.D., F.R.S., Professor of Physics in Owens College, Manchester.
Henry E. Roscoe, B.A., LL.D., V.-P.C.S., Professor of Chemistry in Owens College, and President of Literary and Philosophical Society, Manchester.
W. Boyd Dawkins, M.A., F.R.S., F.G.S., Professor of Geology and Palæontology in Owens College, Manchester.
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Alexander Macalister, M.D., F.R.S., Sec. R.I.A., Professor of Anatomy in the University of Dublin.
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Valentine Ball. M.A., F.R.S., Professor of Geology and Mineralogy in the University of Dublin.

We, the undersigned Members of Parliament, also strongly commend the claims of Dr. Croll to the favourable consideration of the Lords of the Committee of Council.

George Anderson, M.P. for the City of Glasgow.
Joseph C. Bolton, M.P. for Stirlingshire.
Thomas R. Buchanan, M.P. for City of Edinburgh.
James A. Campbell, LL.D., M.P. for the Universities of Glasgow and Aberdeen.
Charles Cameron, LL.D., M.P. for the City of Glasgow.
Alexander Crum, M.P. for Renfrewshire.
Andrew Grant, M.P. for Leith.
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Duncan M'Laren, Ex-M.P. for City of Edinburgh.
Charles S. Parker, M.P. for the City of Perth.
Samuel D. Waddy, Q.C., M.P. for the City of Edinburgh.

LIST OF THOSE WHO SIGNED THE RECOMMENDATION OF DR. CROLL'S MEMORIAL, PRESENTED FEBRUARY 26, 1883.

We, the undersigned, strongly commend the claims of Dr. Croll to the favourable consideration of the Lords of the Committee of Council.

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George H. Darwin, M.A., F.R.S., Plumian Professor of Astronomy in the University of Cambridge.

We, the undersigned Members of Parliament, also strongly commend the claims of Dr. Croll to the favourable consideration of the Lords of the Committee of Council.

George Anderson, M.P. for the City of Glasgow.
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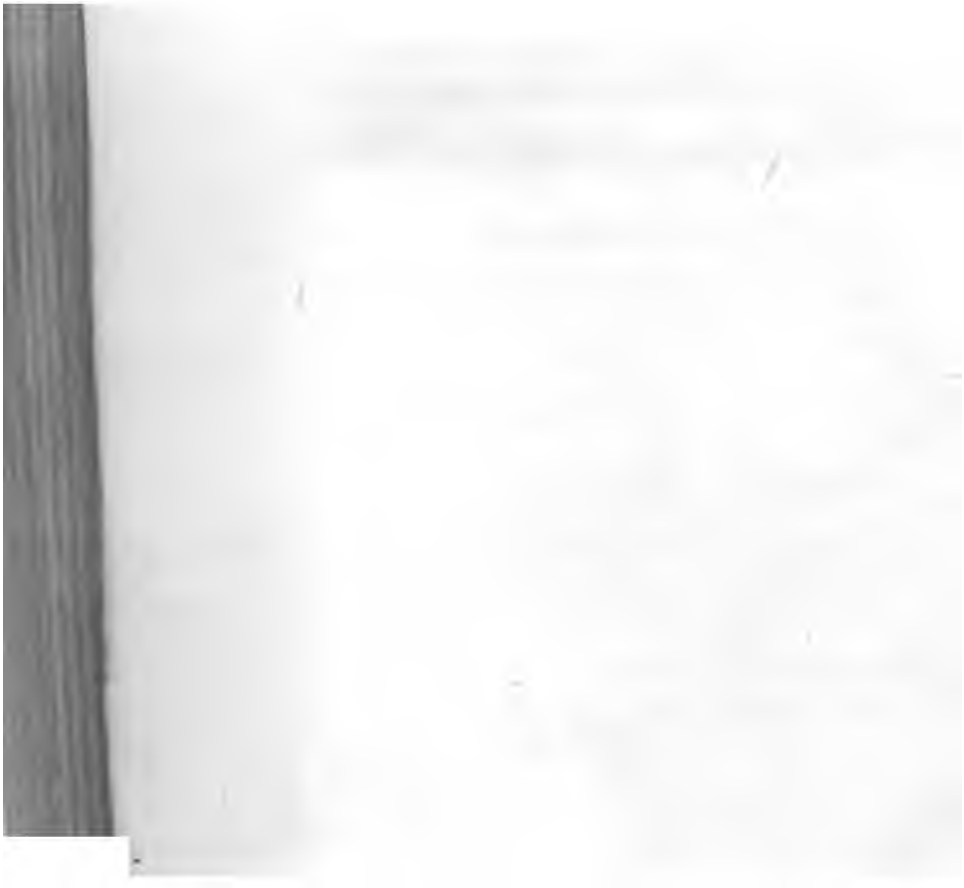
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