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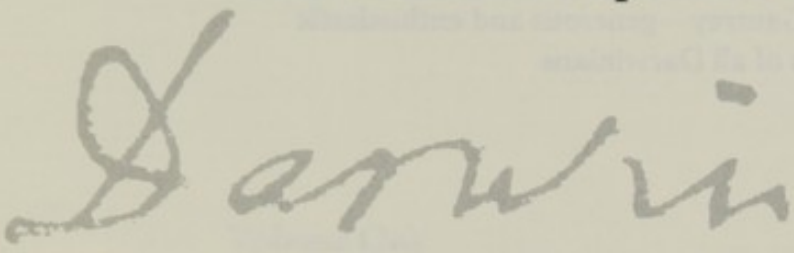
The Collected Papers of Charles Darwin

Volume One

The Collected Papers of Charles Darwin



Edited by PAUL H. BARRETT

With a Foreword by Theodosius Dobzhansky

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Foreword

Charles Darwin is one of the greatest among the creators of modern science. The phenomenon of creativity is little understood, and the inner workings of a creative genius are an enigma. Anything that can throw light on these phenomena is interesting. Darwin's personality, motivations, and the flow of his ideas are still far from completely clear, and this despite much of his voluminous correspondence having been published, and the reminiscences of some people who came in contact with him being available. Darwin's biography, after the voyage on the *Beagle*, was uneventful. A wealthy recluse seldom venturing away from his country estate, suffering from some undiagnosed illness which made him feel constantly unwell, and yet working steadily on his scientific endeavors, living beyond the threescore and ten mark. What sort of inner compulsion made this Victorian-style family man overcome his bodily ailments and leave a scientific heritage far greater than a man of vigorous health could be expected to produce in a lifetime of similar duration?

The major works of Darwin published in book form are readily available, some of them in many editions and in many languages. Professor P. H. Barrett has earned the gratitude of all students of Darwin and of biological evolution by preparing this two-volume anthology of Darwin's shorter articles. Most of the articles in the anthology are, of course, available in the journals in which they were originally published. But, in the first place, only some of the oldest and largest libraries, especially in the United States, possess files of these old periodicals. Second, it is far easier and more convenient to study these writings of Darwin's assembled in an anthology than to be forced to wade through numerous scattered old volumes. I can make a confession: despite a lifelong interest in Darwin, I had read, before receiving the manuscript of Professor Barrett, only four of the 152 articles in the anthology!

The anthology includes Darwin's writings ranging in time from 1832 (an extract from a letter written by Darwin to Professor Henslow from Rio de Janeiro, Brazil, on 18 May of that year), to six articles published in

1882, the year of Darwin's death. Even a cursory perusal of the anthology shows how remarkably far-ranging were Darwin's interests. He published on zoology, botany, paleontology, geology, ethology, and even meteorology. No natural phenomenon seemed uninteresting to him. Many articles are letters of inquiry asking for information about things that occupied his attention. These letters were published in such magazines as *Journal of Horticulture and Cottage Gardener* and *Gardeners' Chronicle*. Although the readers of these magazines were mostly not trained scientists, Darwin hoped that some of them were keen and accurate observers of nature who could furnish him reliable information pertinent to the problems he was working on. Reading these tidbits one can often see the unstated relevance of his inquiries to the major works which he was preparing at the time. Thus, several articles in 1855 and 1857 are concerned with the ability of seeds to survive more or less prolonged immersion in sea water. Darwin is obviously preoccupied with the problem of the occurrence of the same species on different continents and on islands. Were these species independently created in several places, or did they migrate by being passively transported by sea currents? Or take a short letter of inquiry about "Mouse-coloured Breed of Ponies" in *Gardeners' Chronicle and Agricultural Gazette* for 1857. This color pattern, now known to be due to a dominant gene, produces dark stripes on the legs of the horses, which are reminiscent of the common patterns in domestic and wild species of asses. The question that prompted Darwin to make this inquiry can, without being a mind reader, be stated thus: "Have the species horse and ass descended from a common ancestor?" In 1868, when Darwin was working on *The Descent of Man* and *The Expression of the Emotions*, he composed a list of seventeen "Queries about Expression for Anthropological Inquiry," and in 1873 he published several short papers, one of which was "Origin of Certain Instincts."

Darwin generally avoided polemics which the publication of his evolutionary theories unleashed throughout the world, leaving the defense of his views to his supporters, such as T. H. Huxley. But not entirely. Already in 1860, a few months after the publication of *The Origin of Species*, he apologized for having been unaware of one of his partial anticipators, Patrick Matthew. In 1863 he published in *Athenaeum* an incisive countercritique of the objections raised by a certain Dr. Carpenter, to the effect that "lowly organized animals" are products of spontaneous generation, and that their existence at our time level contradicts evolution. In 1871 Darwin attempted to defend his theory of pangenesis against Galton, whose experimental evidence contradicted this theory. This, as we know at present, was one of the rare instances when even Darwin, being human, was wrong!

Darwin, the recluse, was far from indifferent to ethical and socio-political problems of his time. As early as 1836 he published an article jointly with Robert FitzRoy, the captain of the *Beagle*, in defense of the work of British missionaries, chiefly in Tahiti and New Zealand. Although most of the text of this article is probably FitzRoy's, it includes substantial quotations from Darwin's diaries, doubtless with the author's permission. "Vermin and Traps," published in 1863, is an appeal against infliction of suffering on animals, asking that the use of steel traps for catching animals be discontinued. According to the style of his time, Darwin asserted that "An English gentleman would not himself give a moment's unnecessary pain to any living creature, and would instinctively exert himself to put an end to any suffering before his eyes," and wondered "how such cruelty can have been permitted to continue in these days of civilization." By no means inconsistently with this, Darwin supported, in 1881, a law authorizing experiments on living animals, when necessary for the benefit of scientific understanding of the body functioning.

This anthology, added to the mass of published works and biographical materials on Darwin, shows ever more clearly the greatness of the man whose life was completely dedicated to science. To him science was obviously not a "profession," not even a way toward recognition and fame. It was a necessity to sustain his mind, just as the food he ate and the air he breathed were necessities for his body.

THEODOSIUS DOBZHANSKY

Preface

Unlike his books, which are readily available in libraries and elsewhere, Charles Darwin's articles have been difficult to find because they are scattered in numerous and often obscure periodicals. Also, previously published bibliographies of Darwin's papers are incomplete. To help overcome these deficiencies, all his known articles, except for three long ones already published in book form, have been collected and are reprinted here. Included are eighty-seven listed in *The Life and Letters of Charles Darwin*, and an additional sixty-five, most of which have been located by searching the pages of journals and other periodicals to which Darwin most commonly submitted papers. Some were found by friendly librarians, fellow scientists, and interested scholars. Without the much appreciated assistance of these sympathetic and enthusiastic contributors, this collection could never have reached its present state of completion.

The papers reprinted here are not only ordinary articles, but letters to the editor, notes, and prefaces to books. A few ancillary articles are placed in the appendix. These include two papers by Darwin's sons, and a previously unpublished paper read by Darwin at the Plinian Society when he was a student at the University of Edinburgh Medical School. Also in the appendix is a list of publications in which species collected by Darwin during the voyage of the *Beagle* are named and/or described, and a bibliography of books written by Darwin. This two-volume work, together with Bibliography II, constitutes the most complete bibliography of Charles Darwin presently available.

The collection as a whole gives renewed evidence of Darwin's scientific and literary genius. If he was, as he himself suggested, of ordinary intellect, then the capacity for creativeness and productivity of most humans is truly amazing. His mastery of many specialties—geology, zoology, botany, ecology, physiology, psychology, evolution—is once again brought forcefully to our attention. His understanding and use of science as a method for the discovery of new facts is art in the classic Michelangelo-Leonardo tradition. His talent in employing both empirical

and theoretical traits of science is amply demonstrated. On occasion he comments on the importance and the role of theory in leading to new lines of investigation. His excitement and obvious pleasure at making predictions, and of having his predictions at times confirmed, his habit of writing dozens of letters to friends, colleagues, and the public at large, show how and why his scientific methodology was so successful. His concern for accuracy, coupled with an open candidness, is shown by the number of times he corrects mistakes he has made. His sense of humor, his sense of righteousness, and his sense of humaneness emerge with a conviction not previously felt.

For the benefit of the general reader, footnoted annotations of many of the technical terms used by Darwin have been provided by the editor, and are designated as such by a dagger. Darwin's own footnotes have been preserved in the form in which they originally appeared, although they have been renumbered to conform with a uniform style. Original spelling, grammar, and punctuation have been retained. Proper names often vary from modern usage. Headings and closures of letters to the editor are omitted. No posthumous papers are included in this collection. The arrangement of the papers is kept as chronological as possible. R. B. Freeman has earlier listed books published by Darwin in *The Works of Charles Darwin: An Annotated Bibliographic Handlist* (London: Dawsons, 1965). Freeman's *Handlist* does not cite Darwin's articles, except for the four Ray Society volumes on barnacles. The three long papers which have already been reprinted in book form are (1) "On the Movements and Habits of Climbing Plants," *Journal of the Linnean Society of London (Botany)* 9:(1867) 1–118 (read 2 February 1865), reprinted in *On the Movements and Habits of Climbing Plants* (London: Murray, 1875); (2) "On the Character and Hybrid-like Nature of the Offspring from the Illegitimate Unions of Dimorphic and Trimorphic Plants," *Journal of the Linnean Society of London (Botany)* 10(1869): 393–437 (read 20 February 1868); and (3) "On the Specific Difference between *Primula veris*, Brit. Fl. (var. *officinalis*, of Linn.), *P. vulgaris*, Brit. Fl. (var. *acaulis*, Linn.), and *P. elatior*, Jacq.; and on the Hybrid Nature of the Common Oxlip: With Supplementary Remarks on Naturally-Produced Hybrids in the Genus *Verbascum*," *Journal of the Linnean Society of London (Botany)* 10(1869): 437–54 (read 19 March 1868), reprinted in *The Different Forms of Flowers on Plants of the Same Species* (London: Murray, 1877).

PAUL H. BARRETT

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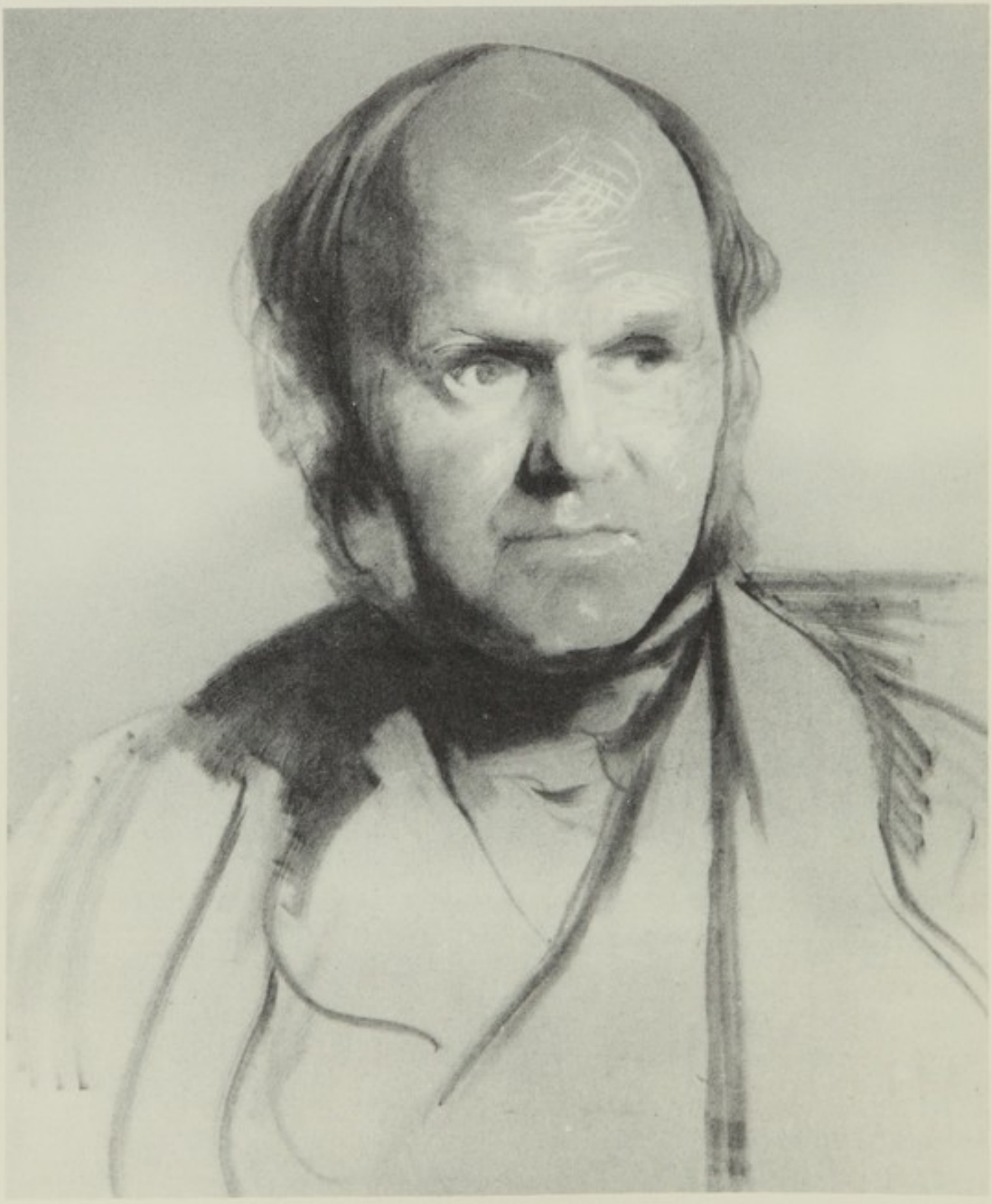
halls, and being a wonderful companion at home and on innumerable trips to various libraries in America and Britain. All of my children, Paul H., Thomas E., and Margaret Anne (now Mrs. Loren Rathert), sacrificed to this and my other Darwin projects many precious hours and years of their early lives. For financial assistance I am indebted to Mr. Walter A. Weiss, of the Arts Fund, Inc., New York; Michigan State University All-University Grants; and the Michigan State University Department of Natural Science Faculty for contributions from the Royalty Funds.

The Collected Papers of Charles Darwin

Volume One

Sketch of Charles Darwin by Samuel Laurence in 1853. From a photograph of the original by Edward Leigh of Cambridge. Reproduced by permission of the Sedgwick Museum, Department of Geology, University of Cambridge.



Extracts from Letters Addressed to Professor Henslow[1]

For Private Distribution

The following pages contain Extracts from Letters addressed to Professor Henslow by C. Darwin, Esq. They are printed for distribution among the Members of the Cambridge Philosophical Society, in consequence of the interest which has been excited by some of the Geological notices which they contain, and which were read at a Meeting of the Society on the 16th of November 1835.

The opinions here expressed must be viewed in no other light than as the first thoughts which occur to a traveller respecting what he sees, before he has had time to collate his Notes, and examine his Collections, with the attention necessary for scientific accuracy.

Cambridge, Dec. 1, 1835

Extracts, &c.

Rio de Janiero, May 18, 1832

We started from Plymouth on the 27th December 1831—At St Jago (Cape de Verd Islands) we spent three weeks. The geology was pre-eminently interesting, and I believe quite new: there are some facts on a large scale, of upraised coast that would interest Mr Lyell . . . . St Jago is singularly barren, and produces few plants or insects; so that my hammer was my usual companion . . . . On the coast I collected many marine animals, chiefly gasteropodous mollusca (I think some new). I examined pretty accurately a Caryophyllia,[2] and, if my eyes were not bewitched, former descriptions have not the slightest resemblance to the animal. I took several specimens of an Octopus, which possessed a most marvellous power of changing its colours; equalling any chamelion, and evidently accommodating the changes to the colour of the ground which it passed over . . . . We then sailed for Bahia, and touched at the rock of St Paul. This is a serpentine formation . . . . After touching at the Abrothos,[3] we arrived here on April 4th . . . . A few days after arriving, I started on an expedition of one hundred and fifty miles to Rio Macao,[4]

which lasted eighteen days . . . . I am now collecting fresh water and land animals: if what was told me in London is true, viz. that there are no small insects in the collections from the Tropics, I tell entomologists to look out and have their pens ready for describing. I have taken as minute (if not more so) as in England, Hydropori, Hygroti, Hydrobii, Pselaphi, Staphylini, Curculiones, Bembidia, &c. &c. It is exceedingly interesting to observe the difference of genera and species from those which I know; it is however much less than I had expected . . . . I have just returned from a walk, and as a specimen how little the insects are known, Noterus, according to Dic. Class.[5] consists solely of three European species. I, in one haul of my net, took five distinct species . . . . At Bahia, the pegmatite and gneiss in beds had the same direction as was observed by Humboldt to prevail over Columbia, distant thirteen hundred miles.

Monte Video, Aug. 15, 1832

My collection of plants from the Abrothos is interesting, as I suspect it contains nearly the whole flowering vegetation . . . . I made an enormous collection of Arachnidae at Rio. Also a good many small beetles in pill-boxes: but it is not the best time of year for the latter . . . . Amongst the lower animals, nothing has so much interested me as finding two species of elegantly coloured planariae (?) inhabiting the dry forest! The false relation they bear to snails is the most extraordinary thing of the kind I have ever seen. In the same genus (or more truly, family) some of the marine species possess an organization so marvellous, that I can scarcely credit my eyesight. Every one has heard of the discoloured streaks of water in the equatorial regions. One I examined was owing to the presence of such minute Oscillatoria,[6] that in each square inch of surface there must have been at least one hundred thousand present . . . . I might collect a far greater number of specimens of invertebrate animals if I took up less time over each: but I have come to the conclusion, that two animals with their original colour and shape noted down, will be more valuable to naturalists than six with only dates and place . . . . At this present minute we are at anchor in the mouth of the river: and such a strange scene it is. Every thing is in flames—the sky with lightning—the water with luminous particles—and even the very masts are pointed with a blue flame.

Monte Video, Nov. 24, 1832

We arrived here on the 24th of October, after our first cruize on the coast of Patagonia, north of the Rio Negro . . . . I had hoped for the credit of dame Nature, no such country as this last existed; in sad reality we coasted along two hundred and forty miles of sand hillocks; I never knew before, what a horrid ugly object a sand hillock is: the famed country of the Rio Plata in my opinion is not much better; an enormous brackish river bounded by an interminable green plain, is enough to make any

naturalist groan. . . . I have been very lucky with fossil bones; I have fragments of at least six distinct animals; as many of these are teeth, shattered and rolled as they have been, I trust they will be recognized. I have paid all the attention I am capable of, to their geological site; but of course it is too long a story for a letter. lst. the tarsi and meta-tarsi, very perfect, of a cavia[7]; 2d. the upper jaw and head of some very large animal, with four square hollow molars, and the head greatly produced in front. I at first thought it belonged either to the megalonyx or megatherium. In confirmation of this, in the same formation I found a large surface of the osseous polygonal plates, which "late observations" (what are they?) have shewn to belong to the megatherium. Immediately I saw them I thought they must belong to an enormous armadillo, living species of which genus are so abundant here. 3d. The lower jaw of some large animal, which from the molar teeth I should think belonged to the edentata[8]; 4th. large molar teeth, which in some respects would seem to belong to some enormous species of rodentia; 5th. also some smaller teeth belonging to the same order, &c. &c.—They are mingled with marine shells, which appear to me identical with existing species. But since they were deposited in their beds, several geological changes have taken place in the country. . . . There is a poor specimen of a bird, which to my unornithological eyes, appears to be a happy mixture of a lark, pigeon, and snipe. Mr Mac Leay[9] himself never imagined such an inosculating creature. . . . I have taken some interesting amphibia; a fine bipes[10]; a new Trigonocephalus,[11] in its habits beautifully connecting Crotalus[12] and Viperus[13]: and plenty of new (as far as my knowledge goes) saurians. As for one little toad, I hope it may be new, that it may be christened "diabolicus." Milton must allude to this very individual, when he talks of "squat like a toad." . . . Amongst the pelagic crustaceae, some new and curious genera. Among Zoophites some interesting animals. As for one Flustra,[14] if I had not the specimen to back me, nobody would believe in its most anomalous structure. But as for novelty, all this is nothing to a family of pelagic animals, which at first sight appear like Medusa, but are really highly organized. I have examined them repeatedly, and certainly from their structure it would be impossible to place them in any existing order. Perhaps Salpa[15] is the nearest animal; although the transparencyof the body is almost the only character they have in common. . . . We have been at Buenos Ayres for a week. It is a fine large city; but such a country; every thing is mud; you can go no where, you can do nothing for mud. In the city I obtained much information about the banks of the Uruguay. I hear of limestone with shells, and beds of shells in every direction. . . . I purchased fragments of some enormous bones, which I was assured belonged to the former giants!!

April 11, 1833

We are now running up from the Falkland Islands to the Rio Negro (or Colorado.). . . . It is now some months since we have been at a civilized

port; nearly all this time has been spent in the most southern part of Tierra del Fuego. It is a detestable place; gales succeed gales at such short intervals, that it is difficult to do any thing. We were twenty-three days off Cape Horn, and could by no means get to the westward. —We at last ran into harbour, and in the boats got to the west of the inland channels. —With two boats we went about three hundred miles; and thus I had an excellent opportunity of geologizing and seeing much of the savages. The Fuegians are in a more miserable state of barbarism than I had expected ever to have seen a human being. In this inclement country they are absolutely naked, and their temporary houses are like those which children make in summer with boughs of trees. . . . The climate in some respects is a curious mixture of severity and mildness; as far as regards the animal kingdom the former character prevails; I have in consequence not added much to my collections. The geology of this part of Tierra del Fuego was to me very interesting. The country is non-fossiliferous, and a common-place succession of granitic rocks and slates: attempting to make out the relation of cleavage, strata, &c. &c. was my chief amusement. . . . The Southern ocean is nearly as sterile as the continent it washes. Crustaceae have afforded me most work. . . . I found a Zoea,[16] of most curious form, its body being only one-sixth the length of the two spears. I am convinced, from its structure and other reasons, it is a young Erichthus.[17] I must mention part of the structure of a Decapod, it is so very anomalous: the last pair of legs are small and dorsal, but instead of being terminated by a claw, as in all others, it has three curved bristle-like appendages; these are finely serrated and furnished with cups, somewhat resembling those of the Cephalopods. The animal being pelagic, this beautiful structure enables it to hold on to light floating objects. I have found out something about the propagation of that ambiguous tribe the Corallines. . . . After leaving Tierra del Fuego, we sailed to the Falklands. . . . I had here the high good fortune to find amongst the most primitive looking rocks, a bed of micaceous sandstone, abounding with Terebratula[18] and its subgenera, and Entrochites.[19] As this is so remote a locality from Europe, I think the comparison of these impressions with those of the oldest fossiliferous rocks of Europe will be pre-eminently interesting. Of course they are only models and casts; but many of them are very perfect.

Rio de la Plata, July 18, 1833

The greater part of the winter has been passed in this river at Meldonado. . . . We have got almost every bird in this neighbourhood, (Meldonado) about eighty in number, and nearly twenty quadrupeds. . . . In a few days we go to the Rio Negro to survey some banks. . . . The geology must be very interesting. It is near the junction of the Megatherium and Patagonian cliffs. From what I saw of the latter, in one half hour, in St Joseph's bay, they would be well worth a long examination. Above the great oyster-bed there is one of gravel, which

fills up inequalities in its interior; and above this, and therefore high out of the water, is one of such modern shells that they retain their colour and emit a bad smell when burnt. Patagonia must clearly have lately risen from the water.

Monte Video, November 12, 1833

I left the Beagle at the Rio Negro, and crossed by land to Buenos Ayres. There is now carrying on a bloody war of extermination against the Indians, by which I was able to make this passage. But at the best it is sufficiently dangerous, and till now very rarely travelled. It is the most wild, dreary plain imaginable, without settled inhabitant or head of cattle. There are military "postas" at wide intervals, by which means I travelled. We lived for many days on deer and ostriches, and had to sleep in the open camp. . . . I had the satisfaction of ascending the Tierra de la Ventana, a chain of mountains between three and four thousand feet high, the very existence of which is scarcely known beyond the Rio Plata. After resting a week at Buenos Ayres, I started for St Fé. On the road the geology was interesting. I found two great groups of immense bones, but so very soft as to render it impossible to remove them. I think, from a fragment of one of the teeth, they belonged to the Mastodon. In the Rio Carcarana, I got a tooth which puzzles even my conjectures. It looks like an enormous gnawing one. At St Fé, not being well, I embarked and had a fine sail of three hundred miles down that princely river the Parana. When I returned to Buenos Ayres, I found the country upside down with revolutions, which caused me much trouble. I at last got away and joined the Beagle.

E. Falkland Island, March 1834

I have been alarmed by your expression "cleaning all the bones," as I am afraid the printed numbers will be lost: the reason I am so anxious they should not be, is, that a part were found in a gravel with recent shells, but others in a very different bed. Now with these latter there were bones of an Agouti,[20] a genus of animals, I believe, peculiar to America, and it would be curious to prove that some one of the same genus coexisted with the megatherium; such, and many other points entirely depend on the numbers being carefully preserved. . . . I collected all the plants which were in flower on the coast of Patagonia, at Port Desire, and St Julian; also on the eastern parts of Tierra del Fuego, where the climate and features of Tierra del Fuego and Patagonia are united. . . . The soil of Patagonia is very dry, gravelly, and light. In East Tierra, it is gravelly, peaty, and damp. Since leaving the Rio Plata I have had some opportunities of examining the great southern Patagonian formation. I have a good many shells; from the little I know of the subject, it must be a tertiary formation, for some of the shells (and corallines) now exist in the sea. Others, I believe, do not. This bed, which

is chiefly characterized by a great oyster, is covered by a very curious bed of porphyry pebbles, which I have traced for more than seven hundred miles. But the most curious fact is, that the whole of the east coast of the southern part of South America has been elevated from the ocean, since a period during which muscles have not lost their blue colour. At Port St Julian I found some very perfect bones of some large animal, I fancy a Mastodon: the bones of one hind extremity are very perfect and solid. This is interesting, as the latitude is between 49° and 50°, and the site far removed from the great Pampas, where bones of the narrow toothed Mastodon are so frequently found. By the way this Mastodon and the Megatherium, I have no doubt, were fellow brethren in the ancient plains. Relics of the Megatherium I have found at a distance of nearly six hundred miles in a north and south line. In Tierra del Fuego I have been interested in finding some sort of ammonite[21] (also I believe found by Capt. King[22]) in the slate near Port Famine; and on the eastern coast there are some curious alluvial plains, by which the existence of certain quadrupeds in the islands can clearly be accounted for. There is a sandstone with the impression of leaves like the common beech tree; also modern shells, &c., and on the surface of the table land there are, as usual, muscles with their blue colour, &c. . . . I have chiefly been employed in preparing myself for the South Sea, and examining the polypi of the smaller corallines in these latitudes. Many in themselves are very curious, and I think undescribed: there was one appalling one, allied to a Flustra, which I dare say I mentioned having found to the northward, where the cells have a moveable organ (like a vulture's head, with a dilatable beak), fixed on the edge. But what is of more general interest, is the unquestionable (as it appears to me) existence of another species of ostrich besides the Struthio ostrea. All the Gauchos and Indians state it is the case: and I place the greatest faith in their observations. I have the head, neck, piece of skin, feathers, and legs of one. The differences are chiefly in the colour of the feathers and scales; in the legs being feathered below the knees; also in its nidification, and geographical distribution.

Valparaiso, July 24, 1834

After leaving the Falklands, we proceeded to the Rio Santa Cruz; followed up the river till within twenty miles of the Cordilleras: unfortunately want of provisions compelled us to return. This expedition was most important to me, as it was a transverse section of the great Patagonian formation. I conjecture (an accurate examination of the fossils may possibly determine the point) that the main bed is somewhere about the meiocene period (using Mr Lyell's expression); judging from what I have seen of the present shells of Patagonia. This bed contains an *enormous* mass of lava. This is of some interest, as being a rude approximation to the age of the volcanic part of the great range of the Andes. Long before this it existed as a slate and porphyritic line of hills. I

have collected a tolerable quantity of information respecting the various periods and forms of elevations of these plains. I think these will be interesting to Mr Lyell. I had deferred reading his third volume till my return; you may guess how much pleasure it gave me; some of his wood-cuts came so exactly into play, that I have only to refer to them, instead of redrawing similar ones. . . . The valley of Santa Cruz appears to me a very curious one; at first it quite baffled me. I believe I can shew good reasons for supposing it to have been once a northern strait, like that of Magellan. . . . In Tierra del Fuego I collected and examined some corallines: I have observed one fact which quite startled me. It is, that in the genus Sertularia[23] (taken in its most restricted form as by Lamouroux[24]), and in two species which, excluding comparative expressions, I should find much difficulty in describing as different, the polypi quite and essentially differed in all their most important and evident parts of structure. I have already seen enough to be convinced that the present families of corallines, as arranged by Lamarck, Cuvier, &c. are highly artificial. It appears to me, that they are in the same state in which shells were, when Linnaeus left them for Cuvier to re-arrange. . . . It is most extraordinary I can no where see in my books a single description of the polypus of any one coral (excepting Lobularia (Alcyonium) of Savigny). I found a curious little stony Cellaria (a new genus), each cell provided with a long toothed bristle capable of various and rapid motions. This motion is often simultaneous, and can be produced by irritation. This fact, as far as I see, is quite isolated in the history (excepting of the Flustra, with an organ like a vulture's head) of Zoophites. It points out a much more intimate relation between the polypi, than Lamarck is willing to allow. I forget whether I mentioned having seen something of the manner of propagation in that most ambiguous family, the corallines: I feel pretty well convinced that if they are not plants, they are not Zoophites: the "gemmule" of a Halimeda contains several articulations united, ready to burst their envelope and become attached to some basis. I believe that in Zoophites universally, the gemmule produces a single polypus, which afterwards or at the same time grows with its cell, or single articulation. The Beagle left the strait of Magellan in the middle of winter: she found her road out by a wild unfrequented channel; well might Sir J. Narborough[25] call the west coast South Desolation, "because it is so desolate a land to behold." We were driven into Chiloe, by some very bad weather. An Englishman gave me three specimens of that very fine lucanoidal insect,[26] which is described in the Cambridge Philosophical Transactions,[27] two males and one female. I find Chiloe is composed of lava and recent deposits. The lavas are curious, from abounding with or rather being composed of pitchstone. . . . We arrived here the day before yesterday; the views of the distant mountains are most sublime and the climate delightful: after our long cruise in the damp gloomy climates of the South, to breathe a clear dry air, and feel honest warm sunshine, and eat good fresh roast beef, must be the summum bonum of human life. I do not like

the looks of the rocks half so much as the beef, there is too much of those rather insipid ingredients mica, quartz, and feldspar. . . . Shortly after arriving here I set out on a geological excursion, and had a very pleasant ramble about the base of the Andes. The whole country appears composed of breccias, (and I imagine slates) which universally have been modified, and often completely altered by the action of fire; the varieties of porphyry thus produced are endless, but no where have I yet met with rocks which have flowed in a stream; dykes of greenstone are very numerous. Modern volcanic action is entirely shut up in the very central parts (which cannot now be reached on account of the snow) of the Cordilleras. To the south of the Rio Maypo, I examined the tertiary plains already partially described by M. Gay.[28] The fossil shells appear to me to differ more widely from the recent ones, than in the great Patagonian formation; it will be curious if an eocene and meiocene formation (recent there is abundance of) could be proved to exist in South America as well as in Europe. I have been much interested by finding abundance of recent shells at an elevation of thirteen hundred feet; the country in many places is scattered over with shells, but these are all *littoral* ones! So that I suppose the thirteen hundred feet elevation must be owing to a succession of small elevations, such as in 1822. With these certain proofs of the recent residence of the ocean over all the lower parts of Chili, the outline of every view and the form of each valley possesses a high interest. Has the action of running water or the sea formed this ravine? was a question which often arose in my mind, and was generally answered by my finding a bed of recent shells at the bottom. I have not sufficient arguments, but I do not believe that more than a small fraction of the height of the Andes has been formed within the tertiary period.

Valparaiso, March 1835

We are now lying becalmed off Valparaiso, and I will take the opportunity of writing a few lines to you. The termination of our voyage is at last decided on. We leave the coast of America in the beginning of September, and hope to reach England in the same month of 1836. . . . You will have heard an account of the dreadful earthquake of the 20th of February. I wish some of the geologists, who think the earthquakes of these times are trifling, could see the way in which the solid rock is shivered. In the town there is not one house habitable; the ruins remind me of the drawings of the desolated eastern cities. We were at Valdivia at the time, and felt the shock very severely. The sensation was like that of skating over very thin ice; that is, distinct undulations were perceptible. The whole scene of Concepcion and Talcuana is one of the most interesting spectacles we have beheld since leaving England. Since leaving Valparaiso, during this cruise, I have done little excepting in geology. In the modern tertiary strata I have examined four bands of disturbance, which reminded me on a small scale of the famous tract in

the Isle of Wight. In one spot there were beautiful examples of three different forms of upheaval. In two cases I think I can show that the inclination is owing to the presence of a system of parallel dykes traversing the inferior mica slate. The whole of the coast from Chiloe to the south extreme of the Peninsula of Tres Montes is composed of the latter rock; it is traversed by very numerous dykes, the mineralogical nature of which will, I suspect, turn out very curious. I examined one grand transverse chain of granite, which has clearly burst up through the overlying slate. At the Peninsula of Tres Montes there has been an old volcanic focus, which corresponds to another in the north part of Chiloe. I was much pleased at Chiloe by finding a thick bed of recent oyster-shells, &c. capping the tertiary plain, out of which grew large forest trees. I can now prove that both sides of the Andes have risen in this recent period to a considerable height. Here the shells were three hundred and fifty feet above the sea. In Zoology I have done but very little; excepting a large collection of minute diptera and hymenoptera from Chiloe. I took in one day, Pselaphus, Anaspis, Latridius, Leiodes, Cercyon, and Elmis, and two beautiful true Carabi; I might almost have fancied myself collecting in England. A new and pretty genus of nudibranch mollusca which cannot crawl on a flat surface, and a genus in the family of balanidae,[29] which has not a true case, but lives in minute cavities in the shells of the concholapas,[30] are nearly the only two novelties.

Valparaiso, April 18, 1835

I have just returned from Mendoza, having crossed the Cordilleras by two passes. This trip has added much to my knowledge of the geology of the country. . . . I will give a very short sketch of the structure of these huge mountains. In the Portillo pass (the more southern one) travellers have described the Cordilleras to consist of a double chain of nearly equal altitude, separated by a considerable interval. This is the case: and the same structure extends northward to Uspellata.[31] The little elevation of the eastern line (here not more than six thousand or seven thousand feet) has caused it almost to be overlooked. To begin with the western and principal chain, where the sections are best seen; we have an enormous mass of a porphyritic conglomerate resting on granite. This latter rock seems to form the nucleus of the whole mass, and is seen in the deep lateral valleys, injected amongst, upheaving, overturning in the most extraordinary manner, the overlying strata. On the bare sides of the mountains, the complicated dykes and wedges of variously coloured rocks, are seen traversing in every possible form and shape the same formations, which, by their intersections, prove a succession of violences. The stratification in all the mountains is beautifully distinct, and owing to a variety in their colouring, can be seen at great distances. I cannot imagine any part of the world presenting a more extraordinary scene of the breaking up of the crust of the globe, than these central peaks

of the Andes. The upheaval has taken place by a great number of (nearly) north and south lines[32]; which in most cases has formed as many anticlinal and synclinal ravines. The strata in the highest pinnacles are almost universally inclined at an angle from 70° to 80°. I cannot tell you how much I enjoyed some of these views; it is worth coming from England, once to feel such intense delight. At an elevation of from ten to twelve thousand feet, there is a transparency in the air, and a confusion of distances, and a sort of stillness, which give the sensation of being in another world; and when to this is joined the picture so plainly drawn of the great epochs of violence, it causes in the mind a most strange assemblage of ideas. The formation which I call porphyritic conglomerates, is the most important and most developed in Chili. From a great number of sections, I find it to be a true coarse conglomerate or breccia, which passes by every step, in slow gradation, into a fine clay-stone porphyry; the pebbles and cement becoming porphyritic, till at last all is blended in one compact rock. The porphyries are excessively abundant in this chain, and I feel sure that at least four-fifths of them have been thus produced from sedimentary beds in situ. There are also porphyries which have been injected from below amongst the strata, and others ejected which have flowed in streams: and I could shew specimens of this rock, produced in these three methods, which cannot be distinguished. It is a great mistake to consider the Cordilleras (here) as composed only of rocks which have flowed in streams. In this range I nowhere saw a fragment which I believe to have thus originated, although the road passes at no great distance from the active volcanos. The porphyries, conglomerates, sandstone, quartzone-sandstone, and limestones alternate and pass into each other many times (overlying clay-slate, when not broken through by the granite.) In the upper parts the sandstone begins to alternate with gypsum, till at last we have this substance of a stupendous thickness. I really think the formation is in some places (it varies much) nearly two thousand feet thick. It occurs often with a green (Epidote?) siliceous sandstone and snow-white marble: and resembles that found in the Alps, from containing large concretions of a crystalline marble of a blackish-gray colour. The upper beds, which form some of the higher pinnacles, consist of layers of snow-white gypsum and red compact sandstone, from the thickness of paper to a few feet, alternating in an endless round. The rock has a most curiously painted appearance. At the pass of the Puquenas[33] in this formation, where a black rock (like clay-slate, without many laminae) and pale limestone have replaced the red sandstone, I found abundant impressions of shells. The elevation must be between twelve thousand and thirteen thousand feet. A shell which I believe is a Gryphaea is the most abundant. There is also an Ostrea, Turritella, Ammonites, small bivalve, Terebratula (?) Perhaps some good conchologist will be able to give a guess to what grand division of the continents of Europe these organic remains bear most resemblance. They are exceedingly imperfect and few; the Gryphites[34] are most perfect. It was late in the season, and the

situation particularly dangerous, from snow-storms. I did not dare to delay, otherwise a good harvest might have been reaped. So much for the western line. In the Portillo pass, proceeding eastward, I met with an immense mass of a conglomerate, dipping to the west 45°, which rests on micaceous sandstone, &c. upheaved, converted into quartz rock, penetrated by dykes, from a very grand mass of *protogene* (large crystals of quartz, red feldspar, and a little chlorite.) Now this conglomerate, which reposes on and dips from the protogene at an angle of 45°, consists of the peculiar rocks of the first described chain, *pebbles* of the black rock with shells, green sandstone, &c. &c. It is here manifest also that the upheaval (and deposition at least of part) of the grand eastern chain is entirely posterior to the western. To the north, in the Uspellata pass, we have also a fact of the same class. Bear this in mind; it will help to make you believe what follows. I have said the Uspellata range is geologically, although only six thousand or seven thousand feet, a continuation of the grand eastern chain. It has its nucleus of granite, consisting of grand beds of various crystalline rocks, which I can feel no doubt are subaqueous lavas alternating with sandstone, conglomerates, and white aluminous beds (like decomposed feldspar) with many other curious varieties of sedimentary deposits. These lavas and sandstones alternate very many times, and are quite conformable one to the other. During two days of careful examination I said to myself at least fifty times, how exactly like, only rather harder, these beds are to those of the upper tertiary strata of Patagonia, Chiloe, and Concepcion, without the possibility of their identity ever having occurred to me. At last there was no resisting the conclusion. I could not expect shells, for they never occur in this formation; but lignite or carbonaceous shale ought to be found. I had previously been exceedingly puzzled by meeting in the sandstone with thin layers (a few inches to some feet thick) of a brecciated pitchstone.[35] I now strongly suspect that the underlying granite has altered such beds into this pitchstone. The silicified wood (particularly characteristic of the formation) was yet absent; but the conviction that I was on the tertiary strata was so strong in my mind by this time, that on the third day, in the midst of lavas, and heaps of granite, I began an apparently forlorn hunt in search of it. How do you think I succeeded? In an escarpement of compact greenish sandstone I found a small wood of petrified trees in a vertical position, or rather the strata were inclined about 20° or 30° to one point, and the trees 70° to the opposite; that is, they were before the tilt truly vertical. The sandstone consists of many horizontal layers, and is marked by the concentric lines of the bark (I have a specimen). Eleven are perfectly silicified, and resemble the dicotyledonous wood which I found at Chiloe and Concepcion: the others, thirty to thirty-four in number, I only know to be trees from the analogy of form and position; they consist of snow-white columns (like Lot's wife) of coarsely crystalized carbonate of lime. The largest shaft is seven feet. They are all close together, within one hundred yards, and about the same level; no where else could I find any. It cannot be doubted that the layers of fine

sandstone have quietly been deposited between a clump of trees, which were fixed by their roots. The sandstone rests on lava, is covered by a great bed, apparently about one thousand feet thick, of black augitic lava, and over this there are at least five grand alternations of such rocks and aqueous sedimentary deposits; amounting in thickness to several thousand feet. I am quite afraid of the only conclusion which I can draw from this fact, namely, that there must have been a depression in the surface of the land to that amount. But neglecting this consideration, it was a most satisfactory support of my presumption of the tertiary age of this eastern chain. (I mean by tertiary, that the shells of the period were closely allied to, and some identical with, those which now lie in the lower beds of Patagonia.) A great part of the proof must remain upon my ipse dixit of a mineralogical resemblance to those beds whose age is known. According to this view granite, which forms peaks of a height probably of fourteen thousand feet, has been fluid in the tertiary period: strata of that period have been altered by its heat, and are traversed by dykes from the mass: are now inclined at high angles, and form regular or complicated anticlinal lines. To complete this climax, these same sedimentary strata and lavas are traversed by very numerous true metallic veins of iron, copper, arsenic, silver, and gold, and these can be traced to the underlying granite. A gold mine has been worked close to the clump of silicified trees. When you see my specimens, sections, and account, you will think there is pretty strong presumptive evidence of the above facts. They appear very important; for the structure and size of this chain will bear comparison with any in the world: and that all this should have been produced in so very recent a period is indeed remarkable. In my own mind I am quite convinced of it. I can any how most conscientiously say, that no previously formed conjecture warped my judgment. As I have described, so did I actually observe the facts. . . . On some of the large patches of perpetual snow, I found the famous red snow of the arctic regions. I send with this letter my observations and a piece of paper on which I tried to dry some specimens. . . . I also send a small bottle with two Lizards: one of them is viviparous, as you will see by the accompanying notice. M. Gay, a French naturalist, has already published in one of the newspapers of this country a similar statement, and probably has forwarded to Paris some account[36] . . . . In the box there are two bags of seeds, one ticketed "valleys of Cordilleras five thousand to ten thousand feet high": the soil and climate exceedingly dry; soil light and strong; extremes in temperature; the other "chiefly from the dry sandy traversia of Mendoza, three thousand feet, more or less." If some of the bushes should grow, but not be healthy, try a slight sprinkling of salt and saltpetre. The plain is saliferous. . . . In the Mendoza bag, there are the seeds or berries of what appears to be a small potatoe plant with a whitish flower. They grow many leagues from where any habitation could ever have existed, owing to the absence of water. Amongst the Chonos dried plants, you will see a fine specimen of the wild potatoe, growing under a most opposite

climate, and unquestionably a true wild potatoe. It must be a distinct species from that of the lower Cordilleras.

1. [Read at a meeting of the Cambridge Philosophical Society, 16 November 1835.] A reprint of the original pamphlet dated Cambridge, 1 December 1835. Privately printed by the Cambridge Philosophical Society in 1960.†
2. A coral.†
3. Rocky islands off the coast of Brazil, 17° to 18° S latitude, north of Rio de Janeiro. Should be Abrolhos.†
4. North of Rio de Janeiro.†
5. Bory de Saint-Vincent, [Jean V.] Audouin, Isid. Bourdon, Ad. Brongniart, de Candolle . . ., *Dictionnaire Classique d'Histoire Naturelle*, 17 vols. (Paris: Rey, Gravier, 1822–31).†
6. A genus of blue green algae.†
7. A group of South American rodents, including guinea pigs.†
8. An order of placental mammals, including anteaters, sloths, and armadillos.†
9. William S. Macleay, author of *Horae Entomologicae*, vol. 1, pts. 1 and 2, 1819, 1821; see Nora Barlow, *Darwin and Henslow* (London: Murray, 1967, p. 62, n. 2).†
10. Two-footed lizard.†
11. Venomous snakes, including copperheads and water moccasins.†
12. Rattlesnakes.†
13. Venomous snakes of the Old World.†
14. A genus of bryozoans; marine, coral-like animals.†
15. A genus of transparent, marine animals of the Tunicata.†
16. Early larval form of certain Crustacea.†
17. A crustacean: Stomatopoda, which includes prawns, shrimp, and so on.†
18. A genus of bivalved, clamlike animals of the phylum Brachiopoda.†
19. A fossil section of a crinoid stem.†
20. Rodents of the genus *Dasyprocta*, about the size of rabbits.†
21. Extinct mollusk, with an intricate, chambered shell.†
22. Philip Parker King commanded the first *Beagle* expedition, 1826–30, and was the author of *Voyages of His Majesty's Ships* Adventure and Beagle, vol.1, Captain Robert FitzRoy, ed. (London: Colburn, 1839).†
23. A delicate branching coral.†
24. See *Dictionnaire Classique*, n. 5.†
25. See John Narborough, J. Tasman, J. Wood, and F. Marten, *An Account of Several Late Voyages and Discoveries to the South and North towards the Streights of Magellan, the South Seas, etc.* (London: Smith, Walford, 1694).†
26. A stag beetle.†
27. [J. F.] Stephens, "Description of Chiasognarthus Grantii: A New Lucanideous Insect Forming the Type of an Undescribed Genus . . .," *Transactions of the Philosophical Society of Cambridge* 4(1833):209–17.†
28. See Alex. Brongniart, "Rapport fait à l'Académie Royale des Sciences, sur les Travaux Géologiques de M. Gay," *Annales des Sciences Naturelles* 28(1833): 394.†
29. Acorn barnacles.†
30. A clam. Should be *concholepas*.†
31. Pass: elevation 12,650 feet; southern foot of Aconcagua, highest peak of western hemisphere, 22,831 feet, between Santiago, Chile, and Mendoza, Argentina.†

32. Of dykes?
33. Peuquenes range: the Western or Chilian range of the Cordillera mountains between Chile and Argentina. See Charles Darwin, *Journal of Researches* . . . (London: Colburn, 1839, p. 389).†
34. Fossil, oysterlike shells.†
35. Black stone high in bituminous substances.†
36. The following is an Extract from the Newspaper referred to by Mr Darwin:

> Besides these labours I employed myself during the great rains in dissecting various reptiles. It must be interesting to know the influence of the climate of Valdivia on the animals of this family. In the greater part of those which I have been able to submit to my scalpel, I have found a truly extraordinary fact, that they were viviparous. Not only the innocent Snake of Valdivia has offered to my notice this singular phenomenon, but also a beautiful and new kind of Iguana which approaches very near to the *Liposoma* of Spix, and to which, on account of its beautiful colours, he has given the name of *Chrysosaurus*. All the species, even those which lay eggs in Santiago, here produce their young alive; and the same thing happens with some *Batrachians*, and particularly with a genus near to the *Rhinella* of Fitzingen, of which the numerous species have the skin pleasingly spotted with green, yellow, and black. I need not dwell on the importance of this last example, in reference to comparative anatomy: an importance which appeared to me still greater when, on analyzing a Tadpole not yet transformed, I satisfied myself that nature has not varied her plan of organization. In these, as in the Tadpoles which live in water, the intestines were of a length very disproportioned to the body: now if this length was necessary to the latter, which live upon vegetable substances, it was altogether useless to those which are to undergo their metamorphosis in the belly of the mother: and thus nature has followed the march prescribed to her by a uniformity of construction, and without deviating from it, has admitted a simple exception, a real hiatus, well worthy the attention of the philosophical naturalist.

Geological Notes Made during a Survey of the East and West Coasts of South America, in the Years 1832, 1833, 1834, and 1835, with an Account of a Transverse Section of the Cordilleras of the Andes between Valparaiso and Mendoza[1]

Prof. Sedgwick began by observing that the notes were extracted from a series of letters (addressed to Professor Henslow), containing a very great mass of information connected with almost every branch of natural history; and that he had selected for the occasion those remarks only which he thought more especially interesting to the Geological Society.

Mr. Darwin's first letter contained some account of St. Jago (one of the Cape Verde Islands), which he visited early in 1832; and he considered that he had good evidence of its recent elevation, as he found on its

surface beds of *recent shells and corals* considerably above the actual level of the sea.

In various portions of the notes he shortly described the vast extent of primary rocks along the shores of Patagonia, the existence of highly crystalline schists in the Falkland Islands, alternating with micaceous slaty sandstone, exhibiting the casts of bivalves (Terebratulae), and encrinital stems,[2] and a rock near Cape Famine containing some sort of Ammonites. On the line of the western coast of South America, from Chiloe to Tres Montes, he found a widely extended formation of mica-slate, traversed and burst through by a grand transverse chain of granite, and penetrated by innumerable dykes of great complexity of mineral structure.

From the position of the tertiary deposits, which exist on both sides of the Southern Andes, he concludes that the primary chain must have had a great elevation anterior to the tertiary period: and he thinks that a rough approximation may be made to the date of the commencement of the volcanic period, by observing the first association of streams of lava, with certain tertiary groups on the Patagonian side.

A considerable portion of the extracts was devoted to a description of the great tertiary groups on both sides of the chain of the Andes. Some of the details respecting the eastern side were derived from observations made on the Rio Negro, and on the line of a transverse section from Rio Santa Cruz to the base of the Cordilleras. These exhibit the structure of what Mr. Darwin calls the great southern tertiary formations of Patagonia.

The lowest of these formations appears to be of great extent and thickness, and in one instance was found to contain a bed of ancient lava, which seemed to mark the commencement of the eruption from the craters of the great chain of the Andes. It is characterized by a great oyster, and by other shells and corals, *some* of which belong to species now living on the neighbouring coasts. Over it is a deposit which Mr. Darwin describes as chiefly composed of rolled porphyry pebbles, which he had himself traced for more than 700 miles. Overlying all the rest, and at a greater elevation above the level of the sea, were beds of recent shells, identical in species with the littoral shells of the neighbouring shores. Among these, he more especially notices a widely extended bed of Muscles, which still retain their blue colour, and emit an animal smell when thrown in the fire. From these facts, he thinks the tertiary deposits of Patagonia may be separated into distinct periods, somewhat similar to those derived by Mr. Lyell from a comparison of the newer deposits of Europe; and in making the transverse section, he thought that he saw traces in the valley of Santa Cruz of an ancient channel, which must have traversed a great portion of the south part of the continent before the elevation of the tertiary groups.

In noticing the groups on the western side of the Andes, he describes an old tertiary deposit (eocine or miocene?) south of Rio Maypo,[3] and abundance of recent shells 1300 feet above the same level. He also describes the association of lava with beds containing recent shells in the island of Chiloe. Among other facts, he notices the appearance of pitchstone among the beds of lava, and the occurrence of a forest growing over a bed of recent oysters 350 feet above the actual level of the sea. All these *recent shells* are the *littoral shells* of the neighbouring shores; from which he concludes that the elevation must have been gradual, or by successive hitches, similar to those by which the coast of Chili and, more recently, the coast of Chiloe have been unquestionably elevated.

In addition to these very remarkable notices, Mr. Darwin mentions other *tertiary deposits* at Chiloe and Conception, composed of beds of sandstone and carbonaceous shale without shells, but containing many silicified trunks of dicotyledonous trees, and alternating with beds of lava.

During the progress of the four years' survey (in addition to the traverse above mentioned), Mr. Darwin crossed from Rio Negro to Buenos Ayres by Sierra de la Ventana, a chain almost unknown to geographers. He found two immense collections of large bones (of Mastodons) near Santa Fé, but in a condition not to admit of their being removed. He also found bones of a species of Mastodon at Fort St. Julian, S. lat. 50°, and more than 600 miles from the former localities. In one instance the bones appear to have been associated with marine shells. In the gravel of Patagonia he also found many bones of the Megatherium and of five or six other species of quadrupeds, among which he has detected the bones of a species of Agouti. He also met with several examples of the polygonal plates of the Megatherium, which at first induced him to regard the animal as a gigantic Armadillo. A very large collection of these fossils has been sent to England, and are in the custody of Professor Henslow till Mr. Darwin's return.

Professor Sedgwick concluded by reading extracts from two letters describing a section transverse to the Andes, extending from Valparaiso to Mendoza. The Cordillera is here composed of two separate and parallel chains. The western chain is composed of sedimentary rocks, distinctly stratified, and resting on granite. The sedimentary rocks (composed of red sandstone, conglomerate, gypsum, &c.) are violently contorted, and dislocated along parallel north and south lines, and as they approach the granite, become so crystalline that they cannot be distinguished from the prophyritic dykes by which they are traversed.

Following the line of section, Mr. Darwin found, at the Pass of Puquenas, elevated 12,000 feet above the sea, that the red sandstone was

replaced by a black rock, like clay slate and pale limestone, containing numerous impressions of shells; a Gryphaea? is the most abundant; but he also found Ostrea, Turritella, Ammonites, and a small bivalve (Terebratula?).

At the Portillo Pass is a conglomerate resting on micaceous sandstone, and traversed by great veins of granite. But at the Uspellata Pass (in the eastern chain), he found highly crystalline and felspathic rocks, regularly bedded, and resting on granite, the peaks of which reach the elevation of 14,000 feet. A wider examination of the overlying groups convinced him, not only that they were more recent than the western chain (being partly made up of its *debris*), but that they were of the same age with certain tertiary formations above noticed. For example, he discovered along the line of section, in the eastern chain, beds of sandstone, with silicified trunks of dicotyledonous trees, and beds of carbonaceous shale, resting on an ancient stream of lava, and surmounted by black augitic lava, 2000 feet thick; over all these were five grand alternations of black volcanic rocks and sedimentary deposits, amounting to several thousand feet in thickness. This series, in its structure and fossils, is considered as identical with certain tertiary deposits of Patagonia, Chiloe and Conception; for it loses its mineral character only where it approaches the granite; in which case it is shattered, contorted, and traversed by great veins rising out of the central mass; and its several beds, as well as the fossils they contain, become entirely crystalline. Mr. Darwin further states, that this singular overlying group contains very numerous veins of copper, silver, arsenic, and gold, which may be traced down to the granite; and as a general conclusion, he expresses his conviction that the granite (now rising into central peaks 14,000 feet in elevation), must have been in a fluid state since the tertiary group, above described, was deposited.

1. [Read 18 November 1835.] *Proceedings of the Geological Society of London* 2(1838):210–12.†

2. Extinct genus of stalked crinoids, of the phylum Echinodermata (starfish are members of this phylum).†

3. Rio Maipo, Chile, near Santiago and Valparaiso.†

A Letter, Containing Remarks on the Moral State of Tahiti, New Zealand, &c.[1]

A very short stay at the Cape of Good Hope is sufficient to convince even a passing stranger, that a strong feeling against the Missionaries in South Africa is there very prevalent.

From what cause a feeling so much to be lamented has arisen, is

probably well known to residents at the Cape. We can only notice the fact: and feel sorrow.

Having lately visited some of the principal islands in the Pacific, and passed time enough in Australia to become acquainted with the opinions of some of the first men in that country respecting Missionaries, and the Missionary system, we were wholly unprepared for such notions as those so predominant in Cape Town.

Before requesting a few minutes' attention to some fact connected with this subject,—let me ask, whether the ideas expressed in the two following extracts from the works of Sir James Mackintosh,[2] are not of higher value than the hastily formed opinions,—I would hardly say prejudices,—of people who think but little?

Speaking of England, Sir James says, "Our scanty information relating to the earliest period of Saxon rule leaves it as dark as it is horrible. But Christianity brought with it some mitigation.

"The arrival of Augustine in Kent, with forty other missionaries, sent by Gregory the Great to convert the Saxons, is described in picturesque and affecting language, by Bede, the venerable historian of the Anglo-Saxon Church.

"It cannot be doubted that the appearance of men who exposed themselves to a cruel death for the sake of teaching *truth*, and inspiring *benevolence*, could not have been altogether without effect, even among the most faithless and ruthless barbarians. Liberty of preaching what they conscientiously believed to be Divine truth, was the only boon for which they prayed."

Again Sir James says, "Let those who consider any tribes of men as *irreclaimable barbarians*, call to mind that the Danes and Saxons, of whose cruelties a small specimen has been given, were the progenitors of those who, in Scandinavia, in Normandy, in Britain, and in America, are *now* among the most *industrious, intelligent, orderly, and humane*, of the dwellers upon earth."

If it is said that the races of men above mentioned always surpassed the Hottentots, the Bushmen, or the Caffers, in natural abilities and disposition, I will ask, are there any tribes of savages in the world, in a state more degraded than those just named? I presume the answer will be "yes, the New-Hollanders, and the natives of Terra del Fuego."[3]

Yet some of those *most* degraded of human beings, four natives of *Terra del Fuego*, were carried to England in the Beagle; were placed under the care of a schoolmaster, in whose house they lived, (one excepted) and there learned to speak English, to use common tools, to plant, and to sow. They were taught the simpler religious truths and duties; and the younger two were beginning to make progress in reading and writing when the time arrived for their return to their own country. I

landed them among their people, by whom they were well received, but very soon plundered of most of the treasures their numerous friends in England had given to them. No dulness of apprehension was shewn by those natives—quite the reverse.

The dispositions of all, especially the younger ones, were so good, although with failings inseparable from a thorough-bred savage, that it was hard to believe that, in the latitude of 54 degrees south, they once went naked, destitute of any covering, except a small piece of seal skin, worn only upon their shoulders; that they had devoured their enemies slain in battle; or that they had smothered, and afterwards eaten, the oldest women of their own tribe, when hard pressed by hunger during a severe winter!

Surely, if three years sufficed to change the natures of such cannibal wretches as Fuegians, and transform them into well behaved, civilized people, who were very much liked by their English friends, there is some cause for thinking that a savage is not irreclaimable, until advanced in life; however repugnant to our ideas have been his early habits.

Humboldt[4] says,—

"If in the great and useful establishment of the American missions, those improvements were gradually made, which have been demanded by several bishops; if, instead of recruiting missionaries at hazard in the Spanish convents, young ecclesiastics were prepared for these functions in seminaries or colleges of missions founded in America, the military expeditions which I propose would become useless.

"Even those Indians, who, proud of their independence and their separate state, refuse to suffer themselves to be governed by the sound of the bell, receive with pleasure the visit of a neighbouring missionary.

"By leaving the Indians to enjoy more of the fruits of their labors, and by governing them less,—that is, by not shackling every instant their natural liberty,—the missionaries would see the sphere of their activity, which ought to be that of civilization, rapidly increase.

"Monastic establishments have diffused in the equinoctial part of the New World, as in the north of Europe, the first germs of social life.

"They still form a vast zone around the European possessions; and, whatever abuses may have crept into institutions, where all power is confounded in one, they would be with difficulty replaced by others, which, without producing more serious inconveniences, would be as little chargeable, and as well adapted to the silent phlegm of the natives.

"I shall recur again to these settlements, the political importance of which is not sufficiently understood in Europe. It will be sufficient here to observe that expeditions of discovery, accompanied by an armed force, would be useless, were the government and the bishops to employ themselves seriously in the melioration of the missions.

"The progress of the missionaries would become rapid, if (after the example of the Jesuits) extraordinary succours were assigned to the most distant missions; and if the most intelligent and courageous ecclesiastics, and those best versed in the Indian languages, were placed in the most advanced posts.

"In both Americas the missionaries arrive every where first, because they find facilities which are wanting to every other traveller.

"'You boast of your journeys beyond Lake Superior,' said an Indian of Canada to some fur-traders of the United States; 'you forget, then, that the *black coats* passed it long before you; and that it was they who showed you the way to the west?'"

But who can hear, or read of the wonderful exertions and effects of missionary zeal in South America, without admitting their important utility? Very many parts of that extensive continent are now almost unknown, and inhabited only by savages; which, before the expulsion of the Jesuits, were the seats of flourishing establishments of Indians, at the least semi-civilized, increasing, and improving yearly.

Yet in how few years had the missionaries effected so much!

Southey[5] informs us that the first six Jesuits who set foot in the New World, landed at Bahia de todos Santos, in April, 1549. "Most distinguished among them was Manoel de Nobrega, the Apostle of Brazil, contemporary of the illustrious Xavier, and his rival in disinterested exertions for the good of his fellow-creatures.

"The obstacles they had to encounter in the works of civilization were most formidable, but their zeal and assiduity rose with the difficulty of the enterprise, and the most salutary effects resulted from their exertions.

"They began by teaching the native children the Portuguese language, and thus, while they fitted them to become interpreters, were also learning the Indian tongue. The greatest obstacle they had to surmount arose from the cannibal propensities of the natives. In feasts of this horrid description, their pride, their religion, their greatest luxury, were all implicated. The missionaries resolved to try to conquer this diabolical habit; but though they succeeded in putting down drunkenness—in healing intestine feuds—in making a man content with one wife; the delight of feasting on the flesh of their enemies was too great to be relinquished; this propensity they could not overcome!!"

A remarkable characteristic of the zealous spirit of those earlier American missionaries was their "entirely gratuitous performance of every religious ceremony."

"Nobrega had a school near the city of Bahia, where he instructed the native children—the orphans sent from Portugal—and the children of mixed breed. Reading, writing, and arithmetic were taught them; they learned to assist at mass, and to sing the church service.

"Frequently they went in procession through the town. The singing

had a great effect, for the natives were passionately fond of music.

"When on an expedition to a strange, and perhaps hostile tribe, Nobrega took with him a few of the little choristers.

"At approaching an inhabited place, one of them carried the crucifix, in advance, the others followed singing the litany.

"Every where the savages received him so joyfully, that Nobrega began to think that the story of Orpheus, however exaggerated, had a better foundation than that of a fable. The pleasure of learning to sing was such a temptation, that the little children sometimes ran away from their parents to put themselves under the care of the Jesuits.

"Nobrega died in the fifty-third year of his age, prematurely worn out by a life of incessant fatigue, consequent on unexampled exertion, and heroic virtue. The day before his death, he took leave of all his friends, as if he were about to undertake a long journey. 'They asked him where he was going?' He replied, 'Home! to my own country!'"

Quitting opinions, and the tale of other times, it may be desirable to see what has been doing at Otaheite (now called Tahiti,) and at New Zealand, towards reclaiming the 'barbarians.' That epithet is, however, inapplicable to the natives of Otaheite, who were semi-civilized when discovered by Wallis,[6] in 1765.

The Beagle passed a part of last November at Otaheite or Tahiti. A more orderly, quiet, inoffensive community I have not seen in any other part of the world. Every one of the Tahitians appeared anxious to oblige, and naturally good tempered and cheerful. They showed great respect for, and a thorough good will towards, the missionaries (of the London Missionary Society); and most deserving of such a feeling did those persons appear to be, with whom I had the sincere pleasure of making acquaintance,—Messrs Pritchard, Nott, and Wilson.

The missionary body have a considerable influence over the Queen of the Society Islands, as well as over her council, being considered the wisest men, and the truest friends, whom the natives of Tahiti can consult; but the Queen and the Chiefs are far from allowing any approach to dictation, or authority, on the part of *any* foreigner; they are tenacious of their own honor and independence; and only yield to advice when their reason is sufficiently convinced of its propriety.

To detail all that occurred, during even our short visit, tending to shew the beneficial effects of missionary exertion in that distant island, would occupy too much of your time; I will only copy a few of the notes which are in our journals, (Mr. Darwin's and my own,) taking them as they occur—without alteration—believing that, in their original language, the feelings excited at the time will be shewn better than by an abridgment.

Monday, 16th Nov. 1835. At Tahiti.—The Beagle was scarcely secured at her anchorage, before a number of canoes had assembled around her.

All could not get alongside—but those whose outriggers obliged them to keep at a distance, contained natives who appeared to be as happy, and as civilly-disposed, as those who patiently waited by the ship's side until leave was given for them to come on board. The necessary work being completed, permission was given, and in a few minutes our deck was thronged by men and boys. No women appeared.

Every one was more or less clothed, excepting a few little boys.

D.[7]—I suppose the number of natives on board the Beagle could not have been less than two hundred. It was the opinion of every one, that it would have been difficult to have selected an equal number of the lower order of any other nation, who would have given so little trouble, or behaved so well.

Mr. Darwin and I landed among a mob of amusing, merry souls, most of them women and children. Mr. Wilson, a missionary who came out in the ship Duff more than thirty years ago, was at the landing place, and welcomed us to his house. The free, cheerful manners of the natives, who gathered about the door, and unceremoniously took possession of vacant seats, either on chairs or on the floor, shewed that they were at home with their instructor, and that churlish seclusion, or affected distance, formed no part of his system.

Two chiefs walked into the room; they shook hands, sat down, and conversed familiarly with Mr. and Mrs. Wilson, in quite an European manner. They were cleanly, and, for the climate, well dressed. Their appearance and manners were prepossessing, and totally different from those of savages.

A proof that the missionary influence is not paramount, I may copy from Mr. Darwin's journal,—"A very unbecoming custom is now almost general. The natives cut their hair so closely, that the heads appear shaven like those of monks, who leave only a small circle of hair. The missionaries have tried to persuade the people to change this habit, but they say 'it is the fashion:' a definite answer at Tahiti, as well as at Paris.[8]

19th November.—Walking towards the house of Mr. Nott, I saw an elderly native writing, in his cottage, and apparently very intent upon his employment. I asked to see what engaged his attention. It was a Tahitian version of the book of Jeremiah, written by Mr. Nott, which he was copying in a good distinct hand. Mr. Nott, the senior missionary upon the island, has almost completed a great work,—the translation of the Old Testament.

18 November.—

D.—Suspended, as it were, on the mountain side, there were glimpses into the dark depths of vallies; and at the highest pinnacles of the central mountains, which, ascending to within sixty degrees of the zenith, hid nearly half the sky. It was a sublime spectacle to watch the shades of night gradually covering the highest summits.

Before we laid ourselves down to sleep, the elder Tahitian fell on his knees, and repeated a long prayer. He seemed to pray as a christian should, with fitting reverence to his God, without ostentatious piety, or fear of ridicule. 19*th*. D. At daylight, after their morning prayer, my companions prepared an excellent breakfast of bananas and fish. Neither of them would taste food without saying a short grace. Those travellers, who hint that a Tahitian prays only when the eyes of the missionary are fixed on him, might have profited by similar evidence.

D.—About two years ago, although the use of the Ava[9] was prohibited, drunkenness, from the introduction of ardent spirits, became very prevalent. The missionaries prevailed on a few good men, who saw their countrymen rapidly working their own ruin, to join with them in a Temperance Society. From good sense and shame, the Queen, and all the chiefs, were at last induced to become members. A law was then immediately passed prohibiting the importation, or sale, of any kind of spirit. With remarkable justice, a certain period was allowed for the sale of stock in hand; but on an appointed day a general search for spirits took place, from which even the closets or trunks of the missionaries were not exempted.

D.—When one reflects on the effects of intemperance on the aboriginals of both Americas, one may estimate the gratitude due from the Tahitians to their missionary counsellors.

20th.—Conversing with a Mr. Middleton about the Low Islands, (those coral islands extending eastward from near Tahiti to beyond the Gambier[10] group,) among which he has spent much time, I was much struck by the personal dislike and jealousy shown by him, when alluding to the missionaries themselves; and by the strong terms in which he mentioned the good *effects* of their intercourse with the Low Islanders; and how

much *more* missionaries were required. His own words, as I have them in a paper written by himself, are: "The inhabitants of those Islands are now familiarized to Europeans, and are becoming partly civilized, owing to the gospel having been preached to them by the missionaries."

In another place he says,—"On this island there are inhabitants enough to require the constant residence of two missionaries."[11]

His own antipathy to the individuals, has arisen, I find, from personal differences.

At the door of a house I saw the owner reading a book attentively. It was the New Testament. I shall not easily forget the expression of that man's countenance, as he read aloud, thinking himself alone. To my mind, such a sight tells more than any descriptions.

21st.—One of the officers slept in a house inhabited by a large family of the middling class of Tahitians. He told me that before sleeping the oldest man said prayers; one of the young men read a short portion of the New Testament; and then a hymn was sung by the whole family. I am informed that this was no more than the general custom in Tahitian families.

22nd, Sunday.—A party of us went to Papiete; others to Mr. Nott's church. Those who could not go far from the ship attended Mr. Wilson's Sabbath meeting, to see the natives at divine service.

At Mr. Pritchard's church in Papiete, we found an orderly, attentive, and decently-dressed congregation. I saw nothing grotesque, nothing ludicrous, (as some late voyagers have seen); nor any thing which had a tendency to depress the spirits, or disappoint expectation. The church was quite full; many were sitting outside. I suppose six hundred people were present, besides children. The fluent delivery of Mr. Pritchard, while preaching in the Tahitian language, surprised, and very much pleased us. Two of them were making notes of the sermon upon paper. A few were inattentive, but very few, compared with the number present.

It was evident the children had not been treated with harshness, for they clustered about their minister so closely, that he could not move without pushing them aside.

D.—Mr. Pritchard was regularly educated at the Mission College. He appears to be a sensible, agreeable gentleman, and a good man. I have already mentioned Mr. Wilson with respect. Mr. Nott, the senior missionary whom we have seen, has resided forty years on the island.

His occupations are now chiefly literary. He bears a very high character. I have said this much of these three persons, because the character of those who labour in the cause to which they are devoted, has been so often attacked.

D.—One of my impressions which I took from Beechey[12] and Kotzebue,[13] was entirely wrong. I thought that the Tahitians had become a gloomy race, and lived in fear of the missionaries. Of the latter feeling I saw no trace. As to discontent, it would be difficult to pick out of an European crowd so many happy, merry faces.

D.—On the whole, it is my opinion that the state of morality and religion in Tahiti is highly creditable. Perhaps those who attack the missionaries, their system, and the effects produced, do not compare the present state of things with that of twenty years ago, nor even with that of Europe at the present day. Looking only to the high standard of gospel perfection, they seem to expect that the missionaries shall effect what the apostles themselves failed in doing. In proportion as the state of things seems to be short of their high and ideal standard, the missionaries are blamed.

Credit due for what *has* been effected, is not allowed. It appears to be forgotten by those persons, that human sacrifices,—the bloodiest warfare,—parricide,—and infanticide,—the power of an idolatrous priesthood,—and a system of profligacy unparalleled in the annals of the world,—have been abolished,—and that dishonesty, licentiousness, and intemperance have been greatly reduced, by the introduction of Christianity. In a voyager it is base ingratitude to forget these things. At the point of shipwreck, how earnestly he will hope that the lesson of the missionary has extended to the place on which he expects to be cast away!

25th, Wednesday.—At day-light this morning I[14] went to Mr. Wilson's school-house, now used also as a chapel, the old church at Matavi[15] having been blown down by a violent gale of wind. (On each Wednesday morning a short service is performed in each of the Tahitian Churches.) This morning a hymn was sung, an extempore prayer followed, and then another hymn. The congregation was numerous and very attentive. I noticed that all the principal men of the district were present. Mr. Wilson's manner pleased me much. It was the sincere and naturally

impressive manner of a kind-hearted, honest man, earnestly performing a sacred duty.

The Queen, and a large party, passed some hours on board the Beagle. Their behaviour was extremely correct, and their manners were inoffensive. Judging from former accounts, and what we witnessed, I should think that they are improving yearly; and that the conduct of the missionaries, and their families, has an influence over them exceeding that of a very differently disposed people by whom, unfortunately, they are often visited.

Thursday, 26th.—At daylight this morning some of our party went to the school at Papiete. As we had heard of 'compulsion' and 'unwilling attendance,' I went early without having said a word to any one which could lead them to expect a visitor. In and about the large church, I found groups of elderly, and even old, people sitting together *helping each other* to read. While one read, the other listened; and if able, corrected him. One man, with spectacles, not less than sixty years of age, was *learning* to read! Some came in, others went out, just as they chose. During about an hour after sun-rise, these elderly people were instructing one another in this manner, previous to beginning their daily employment.

Meanwhile in the school-house, a number of children (about ninety) were occupied in reading aloud, writing on slates, or answering questions, in the usual manner of infant schools.

Mr. Pritchard asked me to desire them to write a sentence. I said 'the captain wishes you much happiness.' Mr. Pritchard having interpreted, they wrote his words instantly, and some of their own accord, added, 'and we wish happiness to the captain.' The hand-writing of many, indeed most of the elder girls and boys, was very good. The questions they answered readily; and though apparently in good discipline, a merrier, or more cheerful looking set of children I never saw.

Returning by way of the (Tahitian) church, I saw Hitote, and several other chiefs, engaged in eager discussion. Mr. Pritchard and I went in. "You are come just in time," said they, "We are disputing about the lightning conductors on board the Beagle; and cannot determine whether they end in the snip's hold, or whether they go through her bottom into the water." Mr. Pritchard interpreted to me, and I tried to give them an explanation.

As to the morality of these islanders, and especially that of the women, which, though reprobated by some, has been defended by no less authorities than Cook[16] and Turnbull,[17] I would scarcely venture to give

a general opinion, after only so short an acquaintance; but I may say that I witnessed no improprieties, neither did I see anything that would not incline me to suppose that their habits of morality are better than those of many civilized nations.

It appears to me that the missionaries have succeeded in carrying attention to religion, and general morality, to a pitch at which it can hardly be maintained in future years, when intercourse with other countries will undermine their influence.

Human nature in Tahiti cannot be supposed superior to erring human nature in other parts of the world.

With respect to those who have severely censured the interference and effects of the missionary system,—I subscribe entirely to the following remark of Mr. Darwin:

D.—I do believe that, disappointed in not finding the field of licentiousness so open as formerly, and as was expected, they will not give credit to a morality which they do not wish to practise, nor to the effects of a religion which is undervalued, if not quite despised.

Is it not a striking fact, and one which ought to be recorded to the lasting honor of missionaries, that, owing to their example and influence, a *Nation* has solemnly rejected the use of ardent spirits?

New Zealand

21st December.—In a conspicuous, solitary position, an English-looking house, without a building, or indeed any object except a flag staff, near it, presented a remarkable contrast to the fortified villages of the natives, and impressed one's mind with a conviction of the great influence already obtained over the wild cannibals of New Zealand.

In that lonely house lives the British Resident, his sole defence our national flag; his interpreters, and only supporters, the missionaries of the Church of England.

From the anchorage, in the Bay of Islands, the view is very pleasing. One of the most conspicuous objects is the new church, now building by voluntary contributions.

22nd.—I walked with Mr. Baker (missionary) about the little village of Paihia. Mr. Henry Williams, who was formerly a lieutenant in the navy, was absent on an exploring and negotiating expedition to the southern parts of the island.

I much regretted having missed seeing him. He is considered the

leading person among the missionary body in New Zealand; and is said by every one, who speaks of him, to be most thoroughly devoted to the great cause, in which he was one of the first, and most daring.

Afterwards we went to Kororadika a village at the side of the harbour opposite to Paihia.

The new church, before mentioned, is a slightly built edifice of bricks, with an abundance of bad glass windows. Placing a church at the head quarters of iniquity, at such a notorious place as Kororadika, is certainly a bold trial.

Notwithstanding the ill-will entertained towards the missionaries by our *spirit-selling* countrymen, and by the evilly-disposed of the native population, not a pane of glass has been broken, nor has the slightest impediment been offered.

In a long list of subscribers to the building, I saw the names of many masters of merchant ships and whalers, placed before very considerable sums.

D.—This little village (Kororadika) is the very strong hold of vice. Although many tribes, in other parts, have embraced christianity, here the greater part of the people are still heathens. By them the missionaries are held in no esteem; but their conduct is innofensive compared with that of our own countrymen. Strange as it appears, I have heard the missionaries say, that the only protection they now need, or on which they securely rely, is that of the native chiefs; and that too against their own countrymen,—English settlers.

23rd.—With Mr. Baker I[18] went to Tapuna, the place where the first missionaries, Mr. King and Mr. Kendal established themselves in 1813. Mr. King was absent. His son told me that he was travelling amongst the natives. He was on horesback, the son said, but quite alone. Mrs. King described the former state of things which she witnessed herself, in very strong terms. She could not look back to those days without a shudder.

Many times they were told that 'before morning their house would be in flames,' and that 'stones were heating in the ovens in which they were to be cooked.' But Mr. King found a trusty friend in a well-known chief named Waripoaki, and to him he always sent for assistance.

Returning, we landed upon an island lately purchased by two Englishmen, not long ago masters of whale ships. The verbal attacks upon the missionaries made by these men, their illiberal aspersions of Mr. Busby's[19] character, and their own manners, and disgusting conversation, prevented our remaining many minutes in their company. Such

men as these,—strongly prejudiced, deaf to reason, and often habitually vicious,—run-away convicts, whose characters may be imagined,—and democratic, untractable natives, cause the principal difficulties against which honest, upright settlers, and the whole missionary body, have to contend.

24th.—I went with Mr. Baker to a native village at some distance, called Cawa-cawa. It was pleasant to witness the cordial greetings which passed between Mr. Baker and the natives whom we occasionally met. He had been asked by them to visit their village in order to settle some disputes which had arisen between their neighbours and themselves. He also wished to gain advocates for the abolition of the use of spirits. By temporising, talking to both parties, and inducing each to go half way, Mr. Baker succeeded in amicably settling the disputed affairs.

Is it not gratifying to find that even in this savage country the missionaries are appealed to, and act as mediators and peacemakers?

In our return I went a little way out of the path to look at two oxen, lately imported.

Near the door-way of a house, in a retired place, a sick woman was reading a book. It was a copy of the Gospel of St. Matthew, printed at Paihia, in the New Zealand language. Now there could have been no affectation nor hypocrisy in the occupation of that woman; her being seen was quite accidental and unexpected.

Enquiring about her afterwards, I found that she was one of a long list of invalids, who depend upon the mission for advice and medicine. Mr. Baker told me that one of the most troublesome, though gratifying duties of the Missionaries, is that of acting, or attempting to act, as medical men. No regular practioner having as yet established himself in the land, every complaint is entrusted to their attention and good will, but slight medical knowledge. How necessary it must be for a missionary to have some knowledge of medicine and surgery!

The successful wanderings of the Jesuit Faulkner[20] would be a sufficient demonstration, if numerous other instances were wanting. Owing to his skill in medicine, Faulkner was enabled to be a solitary instance of a white man living many years in safety among various tribes of wandering South American Indians.

28th.—I went to Waimate, the settlement lately formed by the Mission, with the view of introducing agriculture, and the mechanical arts, among the natives. The thoroughly English appearance of three well designed, respectable houses, surrounded by gardens, out-houses, and well cultivated fields, was surprising and delightful. About twenty acres of land seemed to be worked. Corn was in full ear, and looked well. I was

received by a person whose intelligent, kind, and truly respectable demeanor at once excited a kindly feeling. This was Mr. Davis, the superintendant of the farming establishment. Mr. William Williams and Mr. Clarke, were absent, having gone to the opposite side of the island to attend the funeral of a young missionary of the Wesleyan persuasion. In the gardens all English vegetables seemed to thrive. The farm yard was thoroughly English. A large barn, built entirely by natives, under Mr. Davis's directions; a blacksmith's shop and forge; English carts and farming implements, successively engaged attention. In the barn two natives were thrashing corn; another native was attending to the winnowing machine. A mill, and mill-dam, entirely the work of the natives, were next examined. They were good works of their kind, and would have been interesting, independent of their locality. Mr. Davis told me that when the mill was finished and first put in action, nothing could exceed the surprise and delight of the natives, especially those who had assisted in the work. They called it 'a ship of the land!' 'wonderful white men,' said they, 'fire, water, earth, and air, are made to work for them, by their wisdom!'

I was much struck by the harmony and apparent happiness of the three families of whose society I had too slight a glimpse. Instead of hours, I should have enjoyed passing days with them. An air of honesty, and that outward tranquility which is the result of a clear conscience and inward peace, offered a forcible contrast to the alleged gloom, and even misanthropy, of which some missionaries have been accused by those persons whose own habits, or *associates*, made them perhaps most undesirable acquaintances for an English family.

It was very satisfactory to mark the lively interest taken by them in every detail connected with the Fuegians. Again and again they recurred to the subject. Their anxiety also about the state of other South American Savages, and about other islands in the Pacific, gave me a high opinion of their true missionary spirit. It was striking to find all the members of this isolated society so anxious to hear about and to talk of Fuegians, and other distant tribes of savages, rather than to draw attention to their own doings,—to their troubles, their success, or their wants.

Speaking of the agricultural settlement at Waimate,—Mr. Darwin says, "All this is very surprising, when it is considered that three years ago nothing but fern was seen on the place now occupied by houses, gardens, and corn fields."

D.—Moreover, native workmen effected this change. The lesson of the missionary has acted like an enchanter's wand. Houses have been built, windows framed and glazed; fields ploughed, and even trees grafted by New Zealanders.

At the mill, a native is seen, powdered over with flour, like his brother millers elsewhere.

When I considered this whole scene, I thought it admirable; not only because England was vividly brought to mind; nor solely because of the triumphant feeling excited by seeing such effects of English energy and devotion to a good cause; but chiefly because of the moral effect it must have upon the natives.

D.—The young men and boys employed about the houses seemed to be very merry and good humoured. In the evening, they were playing at cricket with the sons of the missionaries. The young women who attended in the houses had a tidy, healthy look, very different from that of the women about the villages of Kororadika and Paihia.

D.—Late in the evening I went to Mr. W. William's house, where I passed the night. I found there a large party of children (of the missionaries) assembled to pass Christmas day together. They were sitting round a table at tea; a nicer, or more merry group, I never saw. (To think that this sight was in the land of cannibalism, and all atrocity!)

The cordiality and happiness so plainly visible in the faces of the young ones, seemed to be equally prevalent among the older persons of the mission.

D.—When I took leave of these families, it was with thankfulness for their kind welcome, and with feelings of high respect for their evidently superior characters. I think it would be difficult to find men better adapted to discharge the duties of their important office.

D.—As far as I can understand, the greater proportion of the inhabitants of the northern parts of the Island profess Christianity.

It is curious, that the religion of the others has been altered, and is now partly Christian and partly heathen. Moreover, the outward conduct of the unbelievers is said to be improving in consequence of the general spread of some of the Christian doctrines. How far those who profess Christianity are sincere, I have had no opportunity of judging. Mr. Busby, the British Resident, mentioned a pleasing fact:

One of his young men, who had been accustomed to read prayers to the rest of the servants, left him. Some weeks afterwards, happening to pass, late in the evening, by an out-house, he saw and heard another of

his men reading the bible, with difficulty, and by the light of the fire, to the rest of his companions.

Afterwards they knelt down and prayed. In their prayers they mentioned Mr. Busby, and his family, and each of the missionaries.

Dec. 30th.—By all accounts the New Zealanders are improving yearly; so are the natives of other islands which have been visited by missionaries. Those islanders who have been visited only by whalers, or purveyors for Chinese epicures, have in no way profited. On the contrary, they have learned to shew less respect to their own ordinances, and have learned no better ones.

The most abandoned, profligate habits and ideas have been taught, or encouraged, by their visitors. Fire-arms, ammunition, and spirits, have been exchanged for provisions, and for women.

Escaped convicts have done very great harm in the Pacific. Unrestrained by any principle, those abandoned men have been the springs of excessive injuries. The murder of the missionaries at the Friendly Islands was caused by the dark and revengeful intrigues of a convict who had escaped to those Islands from Sydney. Judging from all I have heard on the spot, and since, I should think it difficult to form any moderate estimate of the tumultuous anarchy, and destruction of human life, which has been prevented during the last twenty years by the presence and active exertions of missionaries.

Without estimating the ships of other nations, under the colors of the United States, and of our own country, more than five hundred sail of vessels have been annually employed in the Pacific during late years. For refreshments and supplies, only those islands can, with safety, be now frequented, on which either European or native missionaries have established themselves.

When a merchant ship approaches a remote island in the Pacific, her first object is to ascertain whether it has ever been visited by a missionary. If it has, she knows she may approach with confidence; if it has not, she keeps under sail in the offing, and if she does communicate with the shore, it is with the utmost caution, and with much reluctance.

But even while profiting by the influence of the missionaries, and even assisted by them in intercourse with the natives, many persons have not hesitated to ridicule the means by which the missionary has gained that very influence by which they are profiting; and, in direct opposition to his entreaties, or well-known wishes, encourage the natives in immorality, and in the use of spirits.

Moreover, they abuse, and seek for faults in a system, and in the conduct of individuals, which has a tendency to check, or expose, the impropriety of their own hitherto unrestrained immorality.

If the opponents of missionaries, and of the missionary system, allow no other good character to have been earned in the Pacific, by those hard working men, never can they be deprived of that of Peace-makers.

Surrounded by those who are engaged in commerce, annually increasing; unavoidably involved in local dissensions; referred to on all occasions as interpreters or mediators, and, I may say, as the consular agents of white men of all nations; does it not argue very favourably for the missionaries, that, although sneered at by nominal friends, censured by enemies, and always struggling against opposition, they have as yet upheld the character of their sacred office? Speaking of them as consular agents is, because they now attend to most of the local affairs between natives and foreigners, which would employ the time of a consul, where national agents are established. At Tahiti there is, *nominally*, a British Consul; but he lives at the Sandwich Islands! and might as well be at Kamschatka! At New Zealand there is a British Resident;—an anomalous appointment destitute of authority, yet ostensibly important.

In those places the greater part of the duties to which our government expect their agents to attend, are, in fact executed by the missionaries, not by choice, but because, as Christian men, they cannot tamely withdraw from difficulty, and deny that assistance which *they* are *able* to give, although they see those in official situations set the example of the Levite in the Parable.

After reading these statements, it will not be difficult to form an idea of the secular embarrassments which perplex the South Sea missionaries; after having overcome the primary dangers and difficulties of first establishing themselves among hostile and cannibal savages, yet, although they are *now* able to assist their own countrymen, who have *eagerly profited* by their exertions, and are *now* settling in every direction upon those very lands to which access was attained by their hardy, daring enthusiasm, their own strength is failing! Embarrassments of many kinds are arising. One is a mean jealousy of that very influence, which has enabled those who are jealous, to approach the place where they now revile those to whom they owe gratitude for enabling them to be there.

While their assistance was wanted, no praise of the missionaries was too warm for the adventurers to bestow, who were seeking a settlement. But when once established, and a knowledge of the language attained, "Why should Mr.____ have more influence over the natives than I?" is too frequent a thought. A *few* respectable settlers, such as Mr. Clindon, Mr. Bicknell, Mr. Main, and Mr. Henry, have acted in a very different way: in the most honorable, and truly English manner. But for their support, the few, almost isolated missionaries, would have to contend alone against a host of reprobates.

To me it appears that the steady support, and respectable countenance

of those upholders of the true character of Britons, has, in a quiet, unpretending manner, assisted in a very great degree the progress of incipient civilization and Christianity.

By those who dislike the natural influence of the Missionaries, an outcry has been raised against their alleged attempts to *'monopolize the land.'*

Say those men—"Why should a missionary be allowed to purchase so much land as to prevent those who come after him from obtaining an eligible piece of ground near a frequented part?" or,—"Why should Mr.____ be allowed to prevent Waripoaki and his tribe from selling me that piece of ground, because he thinks that I shall sell spirits, or build a public-house?" "Have not," (say they) "Have not the missionaries already monopolized the best lands, in the finest situations?"

Now, lest it should be thought that undue advantage had been taken by any members, or by the whole of the missionary body, it ought to be here explained that a large extent of land was long ago purchased in New Zealand, by the 'Church Missionary Society'; and that it is not, as supposed by some, the private property of individuals. Other lands from time to time have been purchased by individuals of the missions, for the future maintenance of their families.

And what else could those do who have divided that tie which held them to a country which *could* not be their children's home?

Around them are a group of little ones, who will acknowledge *New Zealand* to be *their* country and *their* home.

Shall the missionaries be debarred from endeavoring to make that home acceptable to those children, and from providing for their future maintenance?

If a missionary, and a recent settler, are each in treaty for a particular piece of ground, and the former obtains it upon easier terms than the latter, in consequence of the good-will of the natives, is it not a natural and legitimate advantage earned in the fairest and the most honourable manner?

Many of the natives understand and appreciate the motives of the missionaries, and are, moreover, personally attached to their little children, whom they like to consider as belonging to *their* country.

If anathemas, indulgences, or excommunications were resorted to by protestant missionaries, one might have a suspicion of *undue* influence; but as such engines of power have not emanated from a British mission, may we not take it for granted that the influence of missionaries appointed by the Church of England or London Missionary Societies, is not *undue?*

The facts, in their simplicity, are these:—As opportunities offered, the missionaries, *always upon the spot,* and watching their opportunity,

have bought lands upon terms more advantageous than those which could be obtained by visitors or recent settlers, strangers to the natives.

Owing to the same natural advantages,—those of local acquaintance, and being always near at hand,—the missionaries have selected the best lands they could afford to purchase.

Ought they to have taken the worst? After all, the property (that island in New Zealand) of the Church Missionary Society, and of individuals of the missions, *taken together*, does not bear a larger proportion to New Zealand than the country of Rutland does to Great Britain and Ireland! Is there then no room left for settlers?

With a remark or two applicable to all missionaries, this letter, already long,—though not too long for such a subject,—shall be concluded.

In the Pacific, not a single avowed disagreement, or misunderstanding, has yet taken place between officers of government and missionaries; but there are plain signs of an increasing and mutual approach towards a kind of jealousy which cannot too soon be prevented.

When authorized agents of government assume active functions in newly-settled, or recently civilized countries, is it not time for the political agency of the missionary to cease? His work, as connected with affairs of policy, or government, is done, and the crowning proof that so great a point in civilization has been gained, in consequence of his energetic exertions, is *the appointment* of such an officer.

From that time ought not the missionary to separate himself from secular affairs?

Should he not reflect, that however he may have been called upon to act during former emergencies, the special duties of his sacred calling *ought* to be separated from politics, or any kind of hostilities or dissensions.

Excepting as peace-maker, his voice should not be heard, neither should his influence be exerted in other than offices of religion and education,—while the authorized officer, or agent of government, can be referred to in secular affairs.

Robt. FitzRoy

On the whole, balancing all that we have heard, and all that we ourselves have seen concerning the missionaries in the Pacific, we are very much satisfied that they thoroughly deserve the warmest support, not only of individuals, but of the British Government.

1. [At sea, 28 June 1836.] R. FitzRoy and C. Darwin. *South African Christian Recorder* 2(1836):221–38.†

2. See James Mackintosh, *The History of England from Earliest Times to the Year 1588* . . . , vol. 1 (1 vol. only) (Philadelphia: Carey, Lea, Blanchard, 1834, pp. 23, 34).†

3. Throughout this article, "I" refers to FitzRoy, except in quotations of Darwin, etc.†

4. See Alexander Humboldt and Aimé Bonpland, *Personal Narrative of Travels to the Equinoctial Regions of the New Continent during the Years 1799–1804*, trans. H. M. Williams, 7 vols. (London: Longman, Hurst, Rees, Orme, and Brown, 1814–29).†

5. Robert Southey, author of *History of Brazil*, 3 pts. (London: Longman, 1810–19).†

6. See Robert Wilson, ed., *Voyages of Discoveries Round the World; Successively Undertaken by Byron in 1764, Captains Wallis and Carteret, in 1766, and Captain Cook . . . 1768–1789 . . .*, 3 vols. (London: James Cundee, 1806).†

7. The letter D. prefixed, denotes an extract from the Journal of Mr. Darwin.

8. This quotation, and the following of Darwin's, are somewhat liberally edited from the original. See Nora Barlow, ed., *Charles Darwin's Diary of the Voyage of H. M. S. "Beagle"* (Cambridge: Cambridge University Press, 1934, pp. 346 ff.).†

9. Obtained from a native root.

10. Mangareva, coral group of islands, South Pacific.†

11. From Tahiti, many natives have gone, as missionaries, to other Islands. Of late years, the natives have opened the way for the European teachers. By their united influence and unabated exertion, Christianity, and consequent civilization, is spreading rapidly amongst the natives of Polynesia.

12. Frederick William Beechey, *Narrative of a Voyage to the Pacific and Beering's Strait . . . in the Years 1825, 26, 27, 28* (London: Colburn, Bentley, 1831).†

13. Otto von Kotzebue, *A Voyage of Discovery, into the South Sea and Beering's Straits . . . 1815–1818*, trans. H. E. Lloyd, 3 vols. (London: Longman, Hurst, Rees, and Brown, 1821).†

14. FitzRoy.†

15. Mr. Pritchard lives at Papiete, near the Queen's usual abode. Matavi is the name of the village in which Mr. Wilson lives, about eight miles from Papiete.

16. James Cook, author of *The Three Voyages of Captain Cook Round the World. With . . . a Memoir of His Life* (London: Limbird, 1824).†

17. John Turnbull, *A Voyage Round the World, in the Years 1800, 1801, 1802, 1803, and 1804 . . .*, 2d ed. (London: Maxwell, 1813).†

18. FitzRoy.†

19. The British Resident.

20. See Thomas Falkner, *A Description of Patagonia and the Adjoining Parts of South America . . .* (London: Lewis, 1774). Note the variation in spelling of "Faulkner" by Darwin.†

Notes upon the Rhea Americana[1]

Mr. Gould[2] brought before the notice of the meeting, from the collection of Mr. Darwin, a new species of *Rhea*[3] from Patagonia, and after offering some observations upon the distribution of the *Struthionidae*,[4] and upon the great interest attending this addition to that family, he remarked that the new species is distinguished from *Rhea Americana* of

authors, in being one-fifth less in size, in having the *bill* shorter than the head, and the *tarsi* reticulated in front instead of scutellated, and in being plumed below the knee for several inches. It has also a more densely plumed wing, the feathers of which are broader, and all terminated by a band of white.

Mr. Gould, in conclusion, adverted to the important accessions to science resulting from the exertions of Mr. Darwin, and to his liberality in presenting the Society with his valuable Zoological Collection; to commemorate which he proposed to designate this interesting species by the name of *Rhea Darwinii*.

Mr. Darwin then read some notes upon the *Rhea Americana*, and upon the newly described species, but principally referring to the former.

This bird abounds over the plains of Northern Patagonia and the United Provinces of La Plata; and though fleet in its paces and shy in its nature, it yet falls an easy prey to the hunters, who confound it by approaching on horseback in a semicircle. When pursued it generally prefers running against the wind, expanding its wings to the full extent. It is not generally known that the *Rhea* is in the habit of swimming, but on two occasions Mr. Darwin witnessed their crossing the Santa Cruz river, where its course was about 400 yards wide and the stream rapid. They make but slow progress, their necks are extended slightly forwards, but little of the body appears above water. At Bahia Blanca,[5] in the months of October and September, an extraordinary number of eggs are found all over the country. The eggs either lie scattered about, or are collected together in a shallow excavation or nest; in the former case they are never hatched, and are termed by the Spaniards *Huachos*. The Gauchos unanimously affirm that the male bird alone hatches the eggs, and for some time afterwards accompanies the young. Mr. Darwin does not doubt the accuracy of this fact, and states that the cock bird sits so closely that he has almost ridden over one in the nest. Mr. Darwin has also been positively informed that several females lay in one nest, and although the fact at first appears strange, he considers the cause sufficiently obvious, for as the number of eggs varies from 20 to 50, and, according to Azara,[6] even 70 or 80, if each hen were obliged to hatch her own before the last was laid, the first probably would have been addled; but if each laid a few eggs at successive periods in different nests, and several hens, as is stated to be the case, combine together, then the eggs in one collection would be nearly of the same age. Mr. Burchell[7] mentions that in Africa two ostriches are believed to lay in one nest.

Mr. Darwin then proceeds to notice the other species of *Rhea*, which he first heard described by the Gauchos, at River Negro, in Northern Patagonia, as a very rare bird, under the name of *Avestruz Petise*. The eggs were smaller than those of the common *Rhea*, of more elongated

form, and with a tinge of pale blue. This species is tolerably abundant about a degree and a half south of the Rio Negro, and the specimen presented to the Society was shot by Mr. Martens at Port Desire in Patagonia, (in latitude 48). It does not expand its wings when running at full speed, and Mr. Darwin learned from a Patagonian Indian that the nest contains fifteen eggs, which are deposited by more than one female. It is stated in conclusion that the *Rhea Americana* inhabits the country of La Plata as far as a little south of the Rio Negro, in lat. 41°, and that the *Petise* takes its place in Southern Patagonia.

1. [Read 14 March 1837.] *Proceedings of the Zoological Society of London* 5(1837):35–36.†
2. John Gould, "Birds," pt. 3, in *The Zoology of H. M. S. Beagle, etc.*, Charles Darwin, ed. (London: Smith, Elder, 1841).†
3. Three-toed ostriches of South America.†
4. Ostriches.†
5. Coast of Argentina, 400 miles south of Buenos Aires.†
6. See Félix d'Azara, *Essais sur l'histoire Naturelle des Quadrupèdes de la Province du Paraguay*, 2 vols. (Paris: Pougens, 1801) and *Voyages dan l'Amérique Méridionale . . . 1781–1801* (Paris: n.p., 1801), both of which are in Charles Darwin's library (H. W. Rutherford, *Catalogue of the Library of Charles Darwin Now in the Botany School* [Cambridge: Cambridge University Press, 1908]).†
7. William John Burchell, author of *Travels in the Interior of Southern Africa*, 2 vols. (London: Longmans, 1822, 1824).†

Remarks upon the Habits of the Genera *Geospiza, Camarhynchus, Cactornis,* and *Certhidea* of Gould[1]

The group of groundfinches, characterised, at a previous meeting, by Mr. Gould, under the generic appellations of *Geospiza, Camarhynchus, Certhidea,* and *Cactornis,* were upon the table; and Mr. Darwin being present, remarked that these birds were exclusively confined to the Gallapagos Islands; but their general resemblance in character, and the circumstance of their indiscriminately associating in large flocks, rendered it almost impossible to study the habits of particular species. In common with nearly all the birds of these islands, they were so tame that the use of the fowling-piece in procuring specimens was quite unnecessary. They appeared to subsist on seeds, deposited on the ground in great abundance by a rich annual crop of herbage.

1. [Read 10 May 1837.] *Proceedings of the Zoological Society of London* 5(1837):49.†

Observations of Proofs of Recent Elevation on the Coast of Chili, Made during the Survey of His Majesty's Ship Beagle, Commanded by Capt. FitzRoy, R.N.[1]

The subject of recent elevations on the coast of Chili being, in the opinion of many, still open to discussion, Mr. Darwin gives, in this memoir, the results of his own observations. The portion of the coast, more particularly examined by the author, extends from the river Rapel, about sixty miles south of Valparaiso, to Conchali, about eighty miles north of it.

Close to the mouth of the Rapel, dead barnacles occur adhering to rocks three or four feet above the highest tidal level; and in the neighbouring country recent marine shells are scattered abundantly to the height of about 100 feet. Ten miles to the north, and at an equal distance from the sea, is the village of Bucalemu, in the neighbourhood of which are very extensive beds of recent shells. At the bottom of the great valley of Maypo, and some miles from the coast, marine shells of existing species are also numerous; and at St. Antonio, near the northern point of that river, are large quarries of shells. Between this point and Valparaiso in the ravine, Quebrada Onda, the remains of a species of shell common on the coast, were noticed by the author. Along the bold granitic coast south of the promontory which forms the bay of Valparaiso, are numerous level and horizontal beds of shells, constituting an almost continuous band, elevated from 60 to 230 feet above the level of the sea. The shells are brittle, but of various kinds, and are all similar and in similar proportional numbers to those on the beach. They are mingled with some earth, though packed closely together, and overlie a partially consolidated breccia of granitic fragments which rests on the solid rock. After a careful examination of these deposits, first by himself, and afterwards with Mr. Alison, guarded by a recent inspection of the heaps of shells accumulated by the natives in Tierra del Fuego, Mr. Darwin was convinced that the shelly beds near Valparaiso, were formed when the sea occupied a different level. The following are the principal circumstances which lead to this conviction. The great number of the shells forming extensive, horizontal beds, whereas the heaps in Tierra del Fuego collected by the inhabitants, always retain a conical figure: their position, at the extremities of headlands inaccessible from the sea, and unfit for strongholds, being without fresh water: the large proportional number of extremely small shells: and lastly, their brittle and decayed condition, the state of decomposition having an evident relation to the comparative heights at which the shells were lying. Comminuted shells were noticed by Mr. Darwin at the heights of 560 and 1300 feet, but the

evidence of their having been part of a beach was not convincing.

At San Lorenzo in the bay of Callao, Mr. Darwin traced a similar process of decay from perfect shells in the lowest beds to a mere layer of calcareous powder in the highest. This phenomenon, he adds, can be observed only in countries where rain never falls.

On the north side of the bay of Valparaiso, near the Viña del Mar, is an abundance of elevated shells. Mr. Alison, by climbing a point of rock about fourteen feet above high water, and removing the dung of sea fowls, discovered Balani adhering to the stone.

With respect to the historical evidence of the earthquake of 1822, Mr. Darwin says that he met with no intelligent person who doubted the rise of the land, or with any of the lower order who doubted that the sea had fallen. He mentions also the altered position of the wreck and of the rock in the bay; and from a part of the fort being invisible from a point on the land before the earthquake, but visible afterwards, he infers that the movement of the land was unequal.[2] A further proof of change, obtained for the author by Mr. Alison, is shown by the remains of a sea-wall built in 1680, and over which, up to 1817, the sea broke during the northerly gales. Mr. John Martin, a ship carpenter of Valparaiso, remembers walking in 1819 on the beach at the foot of this wall, and he has been frequently obliged to climb up to the street to avoid the sea. This wall is now separated from the bay by two rows of houses, but a portion of what appeared to be its base, carefully levelled by a resident engineer, was found to be 11 feet 6 inches above high water mark. Mr. Darwin does not ascribe the whole of this change to the earthquake of 1822, and is of opinion that the alteration then produced was under three feet. The church of San Augustin is believed to have been built in 1634, and the base of its walls is 19 feet 6 inches above high tide level; but there is a tradition that the sea formerly approached very close to its foundations. Allowing, therefore, 4 feet 6 inches for its protection when built, the amount of change in 220 years is only 15 feet. The granite rocks which form the coast are also water-worn and hollowed at about the same height, namely, 14 feet above the present sea level. These data, Mr. Darwin is of opinion, prove, that though the changes in 220 years have been small, yet that they were preceded by a period of comparative rest, during which there was time for any former marks on the rocks to become obliterated.

The author then described the beds of recent shells between Concon and Quintero, about 100 feet above the sea level; the deposits near Plazilla and Catapilco; and in the valley of Longotomo. On the hills to the north of the latter, about 200 feet above the sea, immense quantities of recent shells coat the surface or the sides of the ravines; and hence Mr.

Darwin infers that the action of the sea determined the minor inequalities of the land. Similar deposits, more or less abounding in shells, were noticed by him near Guachen, and in the valley of Quilimap. Close to Conchali, on the south side of the bay, are two very distinct terrace-like plains, the lower being about sixty feet high.

Mr. Darwin then gave a very brief notice respecting the marine origin of the terraces at Coquimbo, described by Capt. Basil Hall[3] and discussed by Mr. Lyell. The proofs of the origin assigned to them rest on the occurrence of recent shells in a friable calcareous rock elevated 250 feet above the sea. This calcareous stratum passes downwards into a shelly mass chiefly composed of fragments of Balanideae, and this again overlies a sandstone abounding with silicified bones of gigantic sharks mingled with extinct species of oysters and Pernae[4] of a great size. The intermediate bed contains some shells in common with the upper, in which all are recent, and with the lowest in which the greater number are extinct. The phenomena of the parallel terraces and the elevated shells occur in a strongly marked manner in the villages of Guasco and Copiapo, the latter being 350 miles to the north of Valparaiso: recent shells also occur at different elevations at an equal distance to the south of it at Concepcion and Imperial. Mr. Darwin believes that the land on the coast of Chili has risen, though insensibly, since 1822. In the Island of Chiloe he is fully convinced, from oral testimony and the state of the coast, that a change effected imperceptibly is now in progress. In support of this gradual rise, independent of earthquakes, he states, that the eastern coast of South America, bordering the Atlantic from the Rio Plata to the Strait of Magellan, presents terraces containing recent shells; yet in the provinces near the mouth of the Plata, earthquakes are never experienced; and it is impossible to suppose that the most violent of the Chilian earthquakes could produce these effects, as the shocks are scarcely transmitted to the plains at the western foot of the Cordilleras. Hence, he concludes, that the earthquakes, volcanic eruptions, and sudden elevations on the coast line of the Pacific, ought to be considered as irregularities of action in some more widely extended phenomenon.

1. [Presented at the meeting of 4 January 1837.] *Proceedings of the Geological Society of London* 2(1838):446–49.†

2. . . . a part of the fort previously invisible is stated to have become visible; but this apparent discrepancy arises from the observations alluded to by Mr. Caldcleugh having been made from the shipping, and those by Mr. Darwin from a point on the land.

3. Basil Hall, author of *Extracts from a Journal Written on the Coasts of Chili, Peru, and Mexico, in the Years 1820, 1821, 1822*, 3d ed., 2 vols. (Edinburgh: Constable, 1824).†

4. Oysterlike clam.†

A Sketch of the Deposits Containing Extinct Mammalia in the Neighbourhood of the Plata[1]

Mr. Darwin premised his account of the geological features of the district in which the remains of the *Toxodon*, described at the meeting on the 19th of April by Mr. Owen, (p. 541)[2] were found, by remarking that as the other mammalia and the fossil shells had not yet been accurately examined, the notice was necessarily imperfect.

To the westward and southward of the great estuary of La Plata, extend those level and almost boundless plains which are known by the name of the Pampas. Their geological constitution over many hundred square miles does not vary. It consists of a reddish argillaceous earth, which generally contains irregular concretions of a white aluminous limestone, or indurated marl, often passing irregularly into a compact calcareous stone, traversed by small linear cavities, similar to those which occur in many of the freshwater limestones of Europe. In the province of Entre Rios, the formation which composes the surface of the Pampas overlies and passes into a series of beds of sand, clay, and crystalline cellular limestone; containing sharks' teeth, gigantic oysters, and other shells belonging to the genera arca, Venus and pecten. These shells, with the exception of the oyster, have a general resemblance to existing species. To the northward and eastward of the Plata, the province of Banda Oriental, though very low and level, consists of gneiss, granite and primary slate. These rocks are generally concealed by a considerable thickness of a reddish earth, which, though at first sight like ordinary detritus, belongs to the same formation with that composing the Pampas. This deposit, extending over so wide an area on both sides of the Plata, abounds with very numerous remains of various extinct mammalia; among which the *Toxodon*, *Megatherium*, *Mastodon*, an animal covered with an armadillo-like case, and as Mr. Darwin believes, the horse, co-existed in the same district.

Proofs of the elevation of the land within a recent period, occur in several parts. Mr. Darwin stated that he had seen in the possession of Sir W. Parish, marine shells which occur near Buenos Ayres in great beds, elevated several yards above the level of the river; and these same species the author had found living on the mud banks on another part of the coast. He, therefore, inferred, that at no very remote period a great bay occupied the area both of the Pampas and of the lower parts of Banda Oriental; and that into this bay the several rivers, which now unite to form the Plata, poured down reddish sediment, resulting, as at the present day, from the decomposition of the granites of Brazil, and charged with carbonate and sulphate of lime, perhaps derived from the Cordillera. On the cliff-formed shores of Entre Rios, the line can be distinguished where the estuary mud first encroached on the deposits of

the ocean. The author also supposed that the ancient rivers, like those of the present day, carried down the carcases of land animals, which thus became entombed in the accumulating sediment. Since that period, by the gradual rising of the land, the bottom of the great bay has been converted into plains, almost as level as the surface of the former sea; and the rivers now hollowing out courses for themselves, have exposed, in many places, the skeletons of those ancient inhabitants of the neighbouring land.

Mr. Darwin then briefly alluded to a small formation of mud and shingle at Bahia Blanca, some hundred miles south of the Plata, in which the remains of several extinct quadrupeds have been discovered. Amongst these he enumerated the *Megatherium Cuvieri*, the remains perhaps of a smaller species of *Megatherium;* a quadruped closely allied to the armadilloes, but nearly as large as a horse; some small rodents, and other animals. These remains are embedded with one species of terrestrial, and several of marine shells, the latter being identical with some existing in the adjoining bay. It is, therefore, certain that the greater number of the above mammalia found at Bahia Blanca lived within a very recent epoch; and from the position of the bed in which they occur, it is equally certain that the form of the land has undergone, since that period, very little change, even of level, with respect to the ocean.

Several hundred miles further southward, Mr. Darwin found the remains of an animal which Mr. Owen says has an affinity with the Llama or Guanaco, but was of a gigantic size: this animal likewise existed since the Atlantic has been peopled by the shells now living.

The author observed in conclusion, that the comparative recentness of the epoch at which the fossil mammalia lived, is shown, first by the shells associated with them; secondly, by the recent tertiary character of the strata underlying the deposit containing those remains; and thirdly, from the little altitude of such beds above the level of the sea; for in this country, according to the author's observations, the movements seem to have been so regular, that the amount of elevation becomes a measure of time.

These facts relating to the former existence of the inhabitants of a part of the globe so remote from Europe, fully confirm the remarkable law, often insisted upon by Mr. Lyell, that "the longevity of the species" among mammalia has been of shorter duration than among molluscs. The author finally remarked, that although several gigantic land animals, which formerly swarmed in South America, have perished, yet that they are now represented by animals, confined to that country; and though of diminutive size, possess the peculiar anatomical structure of their great extinct prototypes.

1. [Presented at the meeting of 3 May 1837.] *Proceedings of the Geological Society of London* 2(1838):542–44.†

2. Reference could not be traced.†

On Certain Areas of Elevation and Subsidence in the Pacific and Indian Oceans, as Deduced from the Study of Coral Formations[1]

The author commenced by observing on some of the most remarkable points in the structure of Lagoon islands. He then proceeded to show that the lamelliform corals, the only efficient agents in forming a reef, do not grow at any great depths; and that beyond twelve fathoms the bottom generally consists of calcareous sand, or of masses of dead coral rock. As long as Lagoon islands were considered the only difficulty to be solved, the belief that corals constructed their habitations (or speaking more correctly, their skeletons), on the crests of submarine craters, was both plausible and very ingenious; although the immense size, sinuous outline, and great number, must have startled any one who adopted this theory. Mr. Darwin remarked that a class of reefs which he calls "encircling" are quite, if not more, extraordinary. These form a ring round mountainous islands, at the distance of two and three miles from the shore; rising on the outside from a profoundly deep ocean, and separated from the land by a channel, frequently about 200 and sometimes 300 feet deep. This structure as observed by Balbi[2] resembles a lagoon, or an atoll, surrounding another island. In this case it is impossible, on account of the nature of the central mass, to consider the reef as based on an external crater, or on any accumulation of sediment; for such reefs encircle the submarine prolongation of islands, as well as the islands themselves. Of this case New Caledonia presents an extraordinary instance, the double line of reef extending 140 miles beyond the island. Again the Barrier reef, running for nearly 1000 miles parallel to the North-East coast of Australia, and including a wide and deep arm of the sea, forms a third class, and is the grandest and most extraordinary coral formation in the world.

The reef itself in the three classes, encircling, barrier and lagoon, is most closely similar; the difference entirely lying in the absence or presence of neighbouring land, and the relative position which the reefs bear to it. The author particularly points out one difficulty in understanding the structure in the barrier and encircling classes, namely, that the reef extends so far from the shore, that a line drawn perpendicularly from its outer edge down to the solid rock on which the reef must be based, very far exceeds that small limit at which corals can grow. A distinct class of reefs however exists, which the author calls "fringing reefs," which extend only so far from the shore, that there is no difficulty in understanding their growth. The theory which Mr. Darwin then offered, so as to include every kind of structure, is simply that as the land with the attached reefs subsides very gradually from the action of subterranean causes, the coral building polypi soon again raise their solid masses to the level of the water; but not

so with the land; each inch lost is irreclaimably gone:—as the whole gradually sinks, the water gains foot by foot on the shore, till the last and highest peak is finally submerged. Before explaining this view in detail, the author offered some considerations on the probability of general subsidences,—such as the small portion of land in the Pacific, where many causes tend to its production, an argument first suggested by Mr. Lyell, and the extreme difficulty (with the knowledge that corals grow at but limited depths) in explaining the existence of a vast number of reefs on one level, without we grant subsidence, so that one mountain top should be submerged after another; the zoophytes always bringing up their stony masses to the surface of the water. Subsidence being thus rendered almost necessary, it was shown by the aid of sections, that a simple fringing reef would thus necessarily be converted by the upward growth of the coral into one of the encircling order, and this finally, by the disappearance through the agency of the same movement of the central land, into a lagoon island. In the same manner a reef skirting a shore would be changed into a barrier extending parallel to, but at some distance from, the mainland.

Mr. Darwin then showed, that there existed every intermediate form between a simple well characterized encircling reef, and a lagoon island; that New Caledonia supplied a link between encircling and barrier reefs; that the different reefs produced by the same order of movement were always in juxtaposition, of which the Australian barrier associated with encircled islets and true lagoons, affords a good example. He then proceeded to show that within the lagoon of Keeling Island, proofs of subsidence might be deduced from many falling trees and a ruined storehouse; these movements appearing to take place at the period of bad earthquakes, which likewise affect Sumatra, 600 miles distant. It was thence inferred as probable, that as Sumatra rises, (of which proofs are well known to exist,) the other end of the lever sinks down; Keeling Island thus acting as an index of the movement of the bottom of the Indian Ocean. Again at Vanikoro,[3] where the structure indicates according to the theory recent subsidence, violent earthquakes are known lately to have occurred.

The author then removed an apparent objection to the theory, namely, that subsidence would form a disc of coral but not a cup-shaped mass or lagoon, by showing that the corals which grow in tranquil water are very different from those on the outside, and less effective; and that as the basin becomes shallower they are subject to various causes of injury. The lagoon nevertheless is constantly filling up to the height of lowest water spring tides, (the utmost possible limit of living coral,) and in that state it long remains, for no means exist to complete the work. Mr. Darwin then proceeded to the main object of the paper, in showing that as continental

elevations act over wide areas, so might we suppose continental subsidences would do, and in conformity to these views, that the Pacific and Indian seas could be divided into symmetrical areas of the two kinds; the one sinking, as deduced from the presence of encircling and barrier reefs, and lagoon islands, and the other rising, as known from uplifted shells and corals, and skirting reefs. The absence of lagoon islands in certain wide tracts, such as in both the West and East Indies, Red Sea, &c., was thus easily explained, for proofs of recent elevation are there abundant. In a like manner, in very many cases where islands are only fringed with reefs, which according to the theory had not been subsiding, actual proofs of elevation were adduced. Mr. Darwin remarked that, excepting on the theory of the configuration of reefs being determined by the order of movement, the circumstance that certain classes which are characteristic and universal in some parts of the sea, being never found in others, is quite anomalous, and has never been attempted to be explained.

Mr. Darwin then pointed out the above areas both in the Pacific and Indian Oceans, and deduced the following as the principal results. 1st. That linear spaces of great extent are undergoing movements of an astonishing uniformity, and that the bands of elevation and subsidence alternate. 2. From an extended examination, that the points of eruption all fall on the areas of elevation. The author insisted on the importance of this law, as thus affording some means of speculating, wherever volcanic rocks occur, on the changes of level even during ancient geological periods. 3. That certain coral formations acting as monuments over subsided land, the geographical distribution of organic beings (as consequent on geological changes as laid down by Mr. Lyell) is elucidated, by the discovery of former centres whence the germs could be disseminated. 4. That some degree of light might thus be thrown on the question, whether certain groups of living beings peculiar to small spots are the remnants of a former large population, or a new one springing into existence.[4] Lastly, when beholding more than a hemisphere, divided into symmetrical areas, which within a limited period of time have undergone certain known movements, we obtain some insight into the system by which the crust of the globe is modified during the endless cycle of changes.

1. [Presented at the meeting of 31 May 1837.] *Proceedings of the Geological Society of London* 2(1838):552–54.†

2. Gasparo Balbi, "Gasparo Balbi's Voyage to Pegu, and Observations There Gathered from His Own Italian Relation," in John Pinkerton, ed. *A General Collection of the Best and Most Interesting Voyages and Travels . . .*, 17 vols., vol. 9, pp. 395–405 (1811) (London: Longman, Hurst, Rees, and Orme, 1808–14).†

3. Solomon Islands, east of Guadalcanal.†

4. Darwin's first published hint of his belief in evolution. See transcriptions of

his 1837–39 transmutation and metaphysics notebooks in Howard E. Gruber and Paul H. Barrett, *Darwin on Man* (New York: Dutton, 1974).†

On the Formation of Mould[1]

The formation of the superficial layer of earth, commonly called vegetable mould, offers some difficulties in being fully understood, which apparently have been overlooked. In old pasture lands, the mould, to the depth of a few inches, differs but slightly, although resting upon various kinds of sub-soil. The uniform fineness of its particles is one of its chief distinguishing characters; and this may be well observed in a gravelly country, where a recently ploughed field immediately adjoins another, which has long remained undisturbed for grazing. In the latter, not a pebble will be seen, either on the surface or immediately below it; although in the ploughed field, a large proportion of the soil may be composed of small stones. From the prevailing use of the expression "vegetable mould," it would appear that its origin is generally attributed to some effect of vegetation; yet it is scarcely conceivable that the turf, in the case of the two adjoining fields, can have produced so remarkable a difference as that alluded to.

My attention was called to this subject by Mr. Wedgwood,[2] who showed me, whilst I was staying at Maer Hall, in Staffordshire, several fields, some of which a few years previously had been covered with lime, and others with burnt marl and cinders. These substances, in every case, were buried some inches beneath the turf. In several parts of three grazing fields, I dug square holes, and obtained the following results:— 1st. In some good pasture land which had been limed, without having been ploughed, about ten years before, the turf, or the layer in which the roots of the grasses are closely woven together, was about half an inch thick. At two inches and a half beneath this, or about three from the surface, a layer of lime, or a row of small aggregated lumps of it, formed a well-marked white line around the holes. The soil beneath this layer of lime was gravelly, or of a coarse sandy nature, and differed considerably from the mould nearer the surface. About three years ago cinders also had been spread on this field; but when I examined it, they were buried at the depth of one inch. They were not sufficiently numerous to form a layer, though a line of black spots could clearly be traced parallel to and above the white one of lime. Some other cinders, which had been scattered in another part of this same field, only about half a year before, lay either on the surface or were entangled in the roots of the grass.

The second field, I mention only from the fact of the cinders being buried in such quantities, about three inches deep, as to form a stratum nearly one inch in thickness. The layer in some parts was so continuous,

that the upper soil was united to the lower only by the longer roots of the grasses. The sub-soil was a red clay, and it occurred a little below the cinders.

The third case which I shall describe, is that of a field which, Mr. Wedgwood informed me, was waste land fifteen years ago. It was at that time drained, ploughed, harrowed, and well covered with burnt marl and cinders. It has not been disturbed since, and now supports a tolerably good but rather coarse pasture. The section in this field,

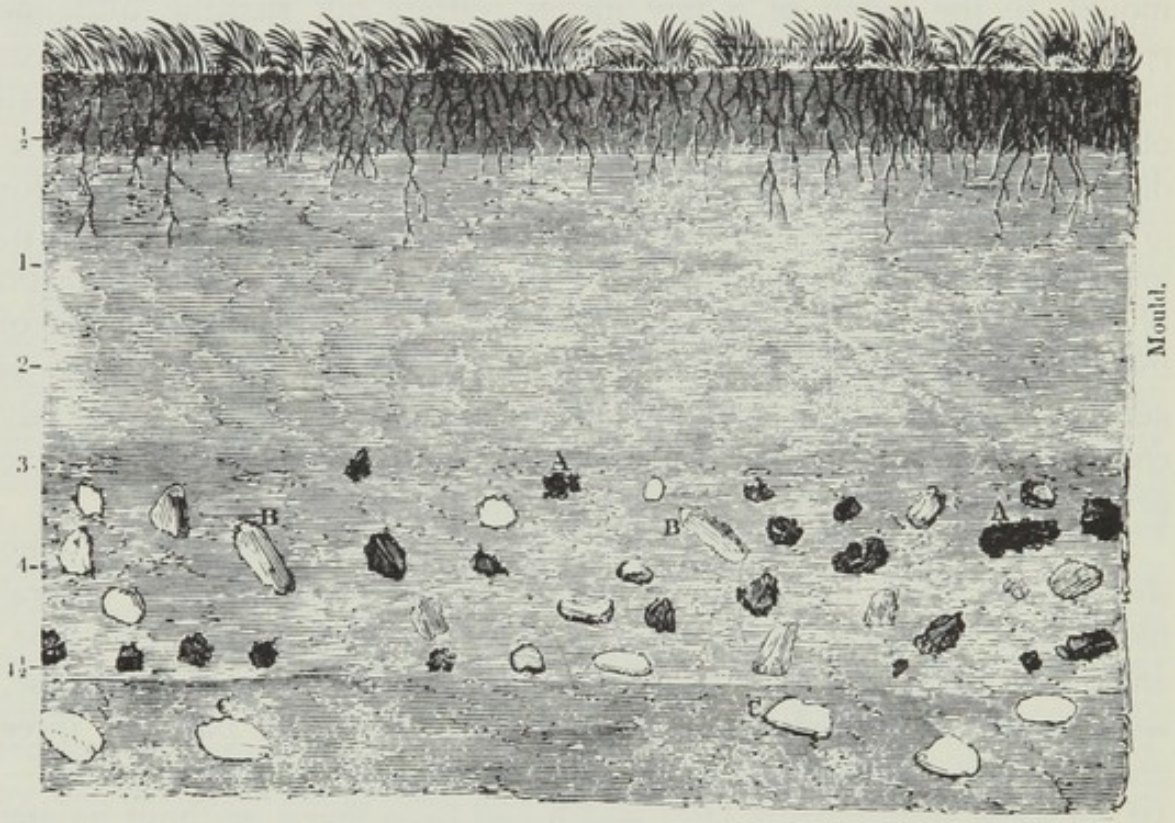


Fig. 1. Section of field: A, cinders; B, burnt marl; C, quartz pebbles.

as represented in the wood-cut, was, turf half an inch; vegetable mould two inches and a half; a layer, one and a half inch thick, of fragments of burnt marl, (conspicuous from their bright red colour), of cinders, and a few quartz pebbles, mingled with earth. One of the angular fragments of burnt marl lying near the bottom, measured one inch in length by half an inch in breadth, and a quarter in thickness. Lastly, about four inches and a half below the surface, was the original black peaty soil. We thus find, beneath a layer, nearly four inches thick, composed of fine particles of earth mixed with decayed vegetable matter, those substances which had been spread on the *surface* fifteen years before.

The appearance in all the above cases was, as if (in the language of the farmers, who are acquainted with these facts) the fragments had worked themselves down. It is, however, scarcely possible that cinders or

pebbles, and still less powdered quick-lime, could sink through compact earth and a layer of matted roots of vegetables, to a depth of some inches; nor is it at all probable that the decay of the grass, although adding to the surface some of the constituent parts of the mould, should separate in so short a time the fine from the coarse earth, and accumulate the former on those objects, which so lately had been on the surface. I may add, that I have repeatedly observed fragments of pottery and bones buried beneath the turf, in fields near towns, (on which such substances are often thrown with manure); and as these fields did not appear to have been ploughed, the circumstance often surprised me. On the contrary, I have noticed in gardens lately dug, that the rain, by washing away the finer particles, leaves stones and other hard bodies accumulated on the surface.

The explanation of these facts, which occurred to Mr. Wedgwood, although it may appear trivial at first, I have not the least doubt is the correct one, namely, that the whole operation is due to the digestive process of the common earth-worm. On carefully examining between the blades of grass in the fields above described, I found scarcely a space of two inches square without a little heap of the cylindrical castings of worms. It is well known, that worms, in their excavations, swallow earthy matter, and that, having separated the portion which serves for their nutriment, they eject at the mouth of their burrows the remainder in little, intestine-shaped heaps. These partly retain their form until the rain and thaws of winter, as I have observed, spread the matter uniformly over the surface. The worm is unable to swallow coarse particles, and as it would naturally avoid pure or caustic lime, the finer earth lying beneath the cinders, burnt marl, or lime, would be removed, by a slow process, to the surface. This supposition is not imaginary; for in the field in which cinders had been spread out only half a year before, I actually saw the castings of the worms heaped on the smaller fragments. Nor, I repeat, is the agency so trivial as at first it might be thought: the great number of earth-worms, as every one must be aware who has ever dug in a grass field, making up for the insignificant quantity of the work which each performs. On the idea of the superficial mould having been thus prepared, the advantage of old pasture land, which it is well known farmers in England are particularly averse to break up, is explained; for the length of time required to form a thick stratum must be considerable. In the peaty field, in the course of fifteen years, about three inches and a half had been well prepared; but it is probable that the process is continued, though at a very slow rate, to a much greater depth. Every time a worm is driven, by dry weather or any other cause, to descend deep, it must bring to the surface, when it empties the contents of its body, a few particles of fresh earth.[3] Thus, the manures added by man,

as well as the original constituent parts of the soil, become thoroughly mingled, and a nearly homogeneous character is given to the whole.[4]

Although the conclusion may appear at first startling, it will be difficult to deny the probability, that every particle of earth forming the bed from which the turf in old pasture land springs, has passed through the intestines of worms; and hence the term "animal mould" would in some respects be more appropriate than that of "vegetable mould."

I may conclude by remarking, that the agriculturist in ploughing the ground follows a method strictly natural; he only imitates in a rude manner, without being able either to bury the pebbles or to sift the fine from the coarse earth, the work which nature is daily performing by the agency of the earthworm.

Note.—Since my communication on the "formation of mould," read on the 1st of November, I have received from Staffordshire an account which corroborates the statements then made, on the apparent sinking of objects placed on the surface of turf land. The first case I mention only because the substance is different from those previously described. In the spring of 1835 a boggy field, which had long remained as grass land, was so thickly covered with sand that the whole surface appeared of a red colour. At the present time, namely about two years and a half afterwards, the sand forms a layer three-fourths of an inch below the surface, that thickness consisting of peaty soil.

The second case is more interesting. It has been ascertained that a field, which has since been ploughed, was covered about eighty years ago with marl; an imperfect layer of it, but sufficiently distinct to be traced, is now found at a depth, very carefully measured from the surface, of twelve inches in some parts and fourteen in others: the difference corresponding to the top and hollow of the ridges produced by ploughing. It is certain, the marl must have sunk or been buried before the field was ploughed, for otherwise the fragments would have been scattered in the soil: this conclusion, moreover, explains the circumstance of the layer being horizontal, whilst the surface is undulating. At the present time no plough could possibly touch the marl, as the land in this country is never turned up to a greater depth than eight inches. In the preceding communication, I have shown, that in a field lately reclaimed from being waste land, three inches of mould had been prepared by the worms in the course of fifteen years. We now find, that within a period of less than eighty years, (but how much less cannot be told, unless the date when the field was first ploughed were known) the earth-worms have covered the marl, which was originally strewed on the surface, with a bed of earth of an average thickness of no less than twelve or thirteen inches.

November 14, 1837

1. [Read 1 November 1837.] *Transactions of the Geological Society of London*, 2d ser., pt. 3, 5 (1840):505–9.†

2. Josiah Wedgwood of Maer, the brother of Darwin's mother, and the father of Darwin's wife Emma.†

3. Mr. W. Lindsay Carnagie of Kimblethment, writing from Scotland to Mr. Lyell on the subject of this paper, as it is given in the Proceedings, states, that in clearing away a stiff clayey soil above a stone quarry, he has seen worms in small chambered passages between seven and eight feet below the surface. The black mould on the clay was there two feet thick. Mr. Carnagie observes, also, in his letter, that the Scotch farmers, from a belief that the lime itself has some tendency to sink, are afraid of putting it on ploughed land until just before it is laid down for pasture. He then adds, "Some years since, in autumn, I laid lime on an oat-stubble and ploughed it down; thus bringing it into immediate contact with the dead vegetable matter, and securing its thorough mixture through the means of all subsequent operations of fallow; I was considered, in consequence of the above prejudice, to have committed a great fault, but the result was eminently successful, and the practice *partially* followed. By means of Mr. Darwin's observations, I think the prejudice will be entirely removed."—June 1838

4. In an earlier version of this same paper (*Proceedings of the Geological Society* 2(1838):574–76), the following is appended at the end of this paragraph. "The author [Darwin] observed, that the digestive process of animals is a geological power which acts in another region on a greater scale. In recent coral formations, the quantity of stone converted into the most impalpable mud, by the excavations of boring shells and of nereidous animals, is very great. Numerous large fishes (of the genus *Sparus*) likewise subsist by browsing on the living branches of coral. Mr. Darwin believes, that a large portion of the chalk of Europe was produced from coral, by the digestive action of marine animals, in the same manner as mould has been prepared by the earth-worm on disintegrated rock."†

On the Connexion of Certain Volcanic Phenomena in South America; and on the Formation of Mountain Chains and Volcanos, as the Effect of the Same Power by which Continents Are Elevated[1]

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Introduction

The object of the present memoir is to describe the principal phenomena generally accompanying the earthquakes on the west coast of South America; and more especially those which attended the shock that overthrew the city of Concepcion on the morning of the 20th of February, 1835. These phenomena evince, in a remarkable manner, the intimate connexion between the volcanic and elevatory forces; and it will be attempted to deduce from this connexion, certain inferences regarding the slow formation of mountain chains.

Observations on the Earthquake in Chile of Feb. 20th, 1835

This earthquake has been the subject of several published memoirs: the sixth volume of the Geographical Journal[2] contains an admirable account of it by Capt. FitzRoy, R.N., in which many interesting facts are detailed, and the elevation of a large extent of coast is incontestably proved. The Philosophical Transactions for 1836, also, contains a memoir on this subject by Mr. Caldcleugh.[3] I must, therefore, refer to these authors, whose statements, as far as I had an opportunity of observing, I can fully corroborate, for a particular description of the earthquake itself, and of the changes of level which accompanied it in the neighbourhood of Concepcion. I will add only a few details, and will then proceed to describe the manner in which the southern volcanos of Chile were affected during the shock.

The island of Juan Fernandez, situated 360 geographical miles N.E. of Concepcion, seems to have been more violently shaken than the opposite shore of the mainland, and at the same time a submarine volcano, which continued in action during the day and part of the following night, burst forth near Bacalao Head, where the depth was afterwards ascertained to be sixty-nine fathoms. This fact possesses a peculiar interest, inasmuch as during the earthquake of 1751, which utterly overthrew Concepcion, this island was likewise affected in a remarkable manner, considering its great distance from the chief seat of disturbance. If any exact record had been kept of that event, many other points of resemblance would probably have been discovered. There is a tradition, that the land was then permanently elevated, and the area affected appears to have been very much the same with that disturbed in Feb. 1835. Molina[4] also states, that the undulation travelled from the southward; and in this second catastrophe the inhabitants agreed in thinking that it came from S.W., or even more southerly. After an interval of only eighty-four years, it is not at all improbable that the subterranean forces should be directed towards the same identical points.

Being anxious to trace the effects of the earthquake to the south, I wrote, shortly after visiting Concepcion, to Mr. Douglas, a very intelligent man, with whom I had become acquainted in the island of Chiloe; and the answer, which I have received since my return to England, is full of curious information.

He describes the earthquake, which appears to have been felt over the whole area at almost the same minute, (as far as the clocks of the country can be relied on,) as being very violent. He says, that twenty minutes before the great shock a trifling one was felt, a circumstance which I did not hear of in any other part. He was at the time on the island of Caucahue, (one of the many islets on the inland shore of Chiloe) and at the time wrote down the following remarks in his pocket-book:

> Felt an earthquake at half-past eleven o'clock, motion horizontal and slow, similar to that of a ship at sea going before a high regular swell, with three to five shocks in a minute, somewhat stronger than the continued motion; direction from N.E. to S.W. Forest trees nearly touched the ground in these directions, but none fell in our vicinity;—pocket compass placed level on the ground, N. point set to lubbers' point; remarked that it vibrated during the violent shocks two points to westward and only half a point to eastward; stood at N. when the motion was less violent. Four minutes afterwards, a shock more violent than any of the preceding ones, affecting the compass as before: another violent shock, and then the movements became gradually less distinct, and eight minutes after the first commencement, they entirely ceased.

I have quoted Mr. Douglas's statement with regard to the compass, although it is not clear how any movement could have forced it to oscillate towards one side more than to another. I presume, however, if the needle with its card had not been acted on by the magnetic force, it would have been thrown in the trough (if such an expression may be used,) of the undulation, that is, in a N.W. and S.E. line, and, therefore, that the *recurrence* of this tendency, acting against the polar attraction, caused the unequal oscillations, as described. In my Journal of Researches,[5] I have endeavoured to show, that the vorticose movement, which in several earthquakes appears to have affected the stones in buildings, possibly may be explained on the same principle, namely, that the stones are so shaken that they arrange themselves according to their forms, in the line of vibration, as the compass would have done, had it not been acted on by the magnetic force. That the movement of the surface was undulatory, is shown by the fact, that at Concepcion the walls which had their extremities directed towards the chief point of disturbance generally remained erect, although much fractured; whilst those extending at right angles to these first lines, were hurled to the

ground; for in the latter case we must suppose, that the whole wall was thrown at the same moment out of its perpendicular by coinciding with an undulation.

The fact mentioned by Mr. Douglas of the trees almost touching the ground from the effects of the movement, though very extraordinary, has been noticed by eye-witnesses of earthquakes in other parts of the world.[6] The circumstance (even supposing it somewhat exaggerated) is the more remarkable, since at Valdivia, which is situated on the coast between this island and the centre of the disturbance at Concepcion, the shock produced no such effects. I was seated in a thick wood there, during the earthquake, and the trees were only slightly shaken.

The range of the Cordillera opposite Chiloe, a narrow island ninety miles in length, is not nearly so lofty as in Central Chile, and a few only of the culminate peaks, which are all active volcanos, exceed 7000 feet in height. Mr. Douglas has given me a detailed account of the effect produced on them by the shock.

The volcano of Osorno had been in a state of moderate activity for at least forty-eight hours previously; Minchinmadom in much the same gentle action as for the last thirty years; and the Corcovado had been quiet during the whole previous twelve months.

> At the moment of the shock, Osorno threw up a thick column of dark blue smoke, and directly that passed, a large crater was seen forming on the S.S.E. side of the mountain; it boiled up lava, and threw up burning stones to some height, but the smoke soon hid the mountain. When seen again a few days afterwards, it showed very little smoke by day, but by night, the new crater, as well as the old one on its truncated summit, shone with a steady light. This volcano appears to have remained in activity throughout the year. The action of Minchinmadom was similar to that of Osorno: two curling pillars of white smoke had been observed all the morning; but during the shock, numerous small chimneys seemed to be smoking within the great crater, and lava was thrown out of a small one just above the lower verge of the snow. Eight days afterwards this little crater was extinct; but at night five small red flames were seen in a line, equidistant from each other, like those in the streets of a village. By the 1st of March its activity was much diminished; but on the 26th there was a smart earthquake, and at night the five fires were again seen. A fortnight afterwards the tops of fifteen conical hills could be seen within the wall of the great crater, and at night nine steady fires, of which seven were in a line, and two straggling.

At the time of the great shock, the Corcovado showed no signs of activity, nor was it heard in action after the Cordillera were hidden in the clouds. Mr. Douglas, however, states that when that volcano was visible a week afterwards, the snow was seen to have been melted around

the N.W. crater. On Yantales, a lofty mountain south of the Corcovado, three black patches having the appearance of craters were observed above the snow-line; and Mr. Douglas did not recollect having seen them before the earthquake. Bearing in mind, that on many occasions, the melting of the snow on a volcano has been the first indication of a fresh period of activity, and that, as I shall presently show, the eruptions of the Corcovado and Osorno are sometimes co-instantaneous, I think there can be little doubt that these appearances prove the effects of the great convulsion of the 20th of February to have been felt by these, the most southern volcanos in America.

Mr. Douglas states, that on the night of November 11th (ten months after the overthrow of Concepcion), Osorno and Corcovado both burst out in violent action, throwing up stones to a great height, and making much noise. He subsequently heard, that on the same day, Talcahuano, the port of Concepcion, little less than 400 miles distant, was shaken by a severe earthquake. This latter statement has since been confirmed to me by a gentleman, who was at the time resident in Chile. Here, then, we have a repetition of the same connected action, which was displayed in so remarkable a manner on the 20th of February. Mr. Douglas in conclusion adds, that on December the 5th his "attention was arrested by the grandest volcanic spectacle he had ever beheld; the S.S.E. side of Osorno had fallen in, thus uniting the two craters, which appeared like one great river of fire. Enormous quantities of ashes and smoke were erupted during the succeeding fortnight."

It is therefore evident, that the volcanic chain from Osorno to Yantales (a length of nearly 150 miles) was affected not only at the moment of the great shock of February 20, 1835, but remained in very unusual activity during many subsequent months.

Again, on November 7, 1837, two years and three quarters after the overthrow of Concepcion, both Valdivia and San Carlos, the capital of Chiloe, were violently convulsed, even more so, according to M. Gay,[7] than in 1835, or on any former recorded period; this shock was sufficiently strong (*bastante recio*[8]) at Talcahuano; and it appears from the evidence of Captain Coste, published in the Comptes Rendus,[9] that the island of Lemus in the Chonos Archipelago, 200 miles south of San Carlos, was, by this same earthquake, upraised more than eight feet: describing the present state of the island, M. Coste says "des roches jadis toujours couvertes par la mer, restant aujourd'hui constamment découvertes."

We see, therefore, that, in 1835,—the earthquake of Chiloe,—the activity of the train of neighbouring volcanos,—the elevation of the land around Concepcion,— and the submarine eruption at Juan Fernandez, took place simultaneously, and were parts of *one and the same* great phenomenon. Again in 1837 a large part of the same area was violently

affected, whilst a district, 200 miles southward of San Carlos in Chiloe, instead, as in 1835, of 300 northward of it, was permanently upraised. We must therefore believe, that these two elevations of the land, although not simultaneous, were effects of the same motive power intimately connected together.

Although the earthquake of February 1835 was so severe in Chiloe, yet at Calbuco, a village situated on the mainland opposite the northern extremity of the island, it was felt with much diminished violence, and on the neighbouring Cordillera (near Mellipulli) not at all. Some men who had been employed in the mountains splitting fir-planks, when they returned in the evening to Calbuco and were told of the shock, said, that "about the time mentioned, they recollected that they had not been able to strike fair with the axe, and that they had spoilt a board or two, by cutting too deep." This probably is not so fanciful as it at first appears; at least it shows that, if there were any motion, it was of an exceedingly gentle kind. It is a most interesting circumstance thus to find, that the great columns of smoke shot forth from the tall chimneys of the Andes, relieved the trembling ground, which at that moment was convulsed over the whole surrounding country.

Mr. Caldcleugh,[10] has stated in his Memoir, that several volcanos in the Cordillera northward of Concepcion were in a state of great activity after the earthquake. It is therefore remarkable that Villarica (near Valdivia), a volcano which is more frequently in eruption than almost any other in the range, although situated in an intermediate position, between those of central Chile and those in front of Chiloe, was not in the least affected. The day was very clear, and although not at the moment of the shock, yet within two hours after it, I attentively watched its truncated summit, but did not perceive the least signs of action. This circumstance probably has an intimate relation with the less force of the earthquake in the same intermediate district. In 1837, however, it suffered similarly with Chiloe. Although Villarica was passed over in 1835, yet in the account of the earthquake of 1822 at Valparaiso, it is said, "at the moment the shock was felt, two volcanos in the neighbourhood of Valdivia (where the earthquake was pretty sharp) burst out suddenly with great noise, illuminated the heavens and the surrounding country for a few seconds, and as suddenly subsided into their quiescent state."[11] The vents in Central Chile, nearer the chief focus of disturbance, were not at the time of that earthquake affected; but according to the information received by Dr. Gillies[12] in 1836 [1830], from a miner who had resided many years in sight of the volcano of Maypu, its eruptions were very frequent during the four years immediately subsequent to it. Many other instances are on record of earthquakes having passed over certain districts, in the same manner as we see the eruptive force acted

with respect to Villarica. Humboldt[13] remarks, that the inhabitants of the Andes, speaking of an intermediary ground, which is not affected by the general motion, say with simplicity, "that it forms a bridge" (*que hace puente*); and he adds, "as if they meant to indicate by this expression, that the undulations were propagated at an immense depth under an inert rock."[14]

On the Identity of the Force which Elevates Continents, with that which Causes Volcanic Outbursts

It has frequently happened, that during the same convulsion large areas of the globe have been agitated, and strange noises propagated to countries many hundred miles apart[15]; but in these cases, it is not possible to form any conjecture over how wide an extent, any actual change has taken place in the subterranean regions. It is different, when we hear from Humboldt, that at the moment when the volcano of Pasto ceased to eject a column of smoke, the city of Riobamba, sixty leagues to the southward, was overwhelmed by an earthquake; for the effect here produced certainly cannot be explained by the mere transmission of a vibration.[16] During the Concepcion earthquake, at one extremity of the area affected, the snow was melted on Yantales and the neighbouring vents renewed their activity; whilst at Juan Fernandez, at the distance of no less than 720 geographical miles from Yantales, an eruption took place beneath the sea; and soon afterwards the volcanos in the Cordillera, 400 miles to the eastward of that island, burst forth in action,—a large extent also of country, intermediate between these extreme points, being permanently upraised. To form a just idea of the scale of this phenomenon, we must suppose, during the same hour, Europe to be shaken from the North Sea to the Mediterranean,—a large tract of the eastern coast of England to be permanently elevated,—a train of volcanos on the northern coast of Holland to burst forth in action,—an eruption to take place at the bottom of the sea, near the northern extremity of Ireland,—and the ancient vents of Auvergne, Cantal, Mont d'Or, and others, so long extinct, each to send up to the sky a dark column of smoke. Moreover, as, in Chile, a large part of the same area was two years afterwards most violently shaken, at the same time that Lemus was upraised, so must we imagine that, subsequently also, in Europe, whilst France, from the English Channel to the central provinces, where the volcanos had been excited into long and fierce action, was desolated by an earthquake, an island in the Mediterranean was permanently elevated;—then should we have the subterranean movements which shook South America on the 20th of February, 1835, and on the 7th of November, 1837, acted in countries with which we are familiar.

When first considering these phenomena, which prove that an actual movement in the subterranean volcanic matter occurred almost at the same instant of time at very distant places, the idea of water splashing up through holes in the ice of a frozen pool, when a person stamps on the surface, came irresistibly before my mind. The inference from it was obvious, namely, that the land in Chile floated on a lake of molten stone, of which the area, as known from the various points in eruption on the day of the earthquake, would be nearly double that of the Black Sea. If this inference be denied, the only alternative is, that channels from the various points of eruption unite in some deep-seated focus, like the arteries of the body in the heart, whence an impulse can be transmitted to distant parts of the surface, with nearly equal force. But according to this view, if two separate trains of volcanos in the Andes have any connexion whatever, which seems highly probable from the symmetry of the Cordillera, (and possibly an intimate one, as will presently be discussed,) then the common focus, from which the two main branches are sent off, must be seated at an enormous depth. But all the calculations regarding the depth at which molten rocks must necessarily be met with, if they can be at all trusted,[17] tend to prove, that the earth's crust is not much more, and perhaps less, than twenty miles in thickness; and if this be so, the crust may, indeed, be well compared with a thin sheet of ice over a frozen pool.

These considerations are, perhaps, of little weight, but we must bear in mind, that the elevation of many hundred square miles of territory near Concepcion is part of the same phenomenon, with that splashing up, if I may so call it, of volcanic matter through the orifices in the Cordillera at the moment of the shock; and as this elevation is only one of a long series, by which the whole coast of Chile and Peru, even for more than a thousand miles, has been upraised several hundred feet within the recent period, (as I endeavoured to show in a paper formerly read to the Society,[18] and I hope hereafter to prove more fully,) the body of matter added below must have been enormous. When we reflect on this, it is obvious, that the term *channel* cannot be applied to a means of communication extending beneath a large portion of a continent, and from the interior of the globe to the superficial crust.[19] The facts appear to me clearly to indicate some slow, but in its effects great, change in the form of the surface of the fluid on which the land rests.

In a geological point of view, it is of the highest importance thus to find three great phenomena,—a submarine outburst, a period of renewed activity through many habitual vents, and a permanent elevation of the land,—forming parts of one action, and being the effects of one great cause, modified only by local circumstances. When we consider, that the southern volcanos were in eruption some days before the earthquake, and that one of them, Minchinmadom, has seldom been dormant for the

last thirty years, and that they all remained active for many months afterwards, we must conclude that the impulse given to them at that moment, was of the same nature with the force which has kept up their activity during the many ages necessary to accumulate the volcanic matter into great snow-clad cones, and which force still continues to add to their height. If the earthquake or trembling of the ground (which, however, we have seen was less near these volcanos than elsewhere) had acted in no other way, than in merely breaking the crust over the lava within the craters, a few jets of smoke might have been emitted, but it could not have given rise to a prolonged and vigorous period of activity.

But the power which manifested itself in this renewed action, and to which same power, acting at former periods, the entire formation of these several volcanos has evidently been due, was likewise the cause of the permanent elevation of the land;—a power, I may remark, which acts in paroxysmal upheavals like that of Concepcion, and in great volcanic eruptions, in precisely the same manner, for both these phenomena occur only after long intervals of rest, during which the volcano merely casts out, perhaps, a few showers of scoriae, and the land rises with so slow a movement that it is called insensible;—therefore no theory of the cause of volcanos which is not applicable to continental elevations can be considered as well-grounded. Those who believe that volcanos are caused by the percolation of water to the metallic bases of the earth, or simply to intensely heated rock, must be prepared either to give up this view, or to extend it[20] to the elevation of such vast continents as that of South America.

On Periods of Increased Volcanic Action Affecting Large Areas

Humboldt, when describing certain volcanic phenomena in that part of South America which borders the West-Indian sea, seems to consider, that periods of increased activity affect large portions of the surface of the earth. He has drawn up the two following tables,[21] to which I have added a third, containing the remarkable events that happened during the years 1834 and 1835:

1st. Table of Volcanic Phenomena

1796.	November	The volcano of Pasto began to emit smoke.
1797.	February 4th	Destruction of Riobamba.
——	September 27th	Eruption in the West-Indian Islands. Volcano of Guadaloupe.
——	December 14th	Destruction of Cumana.

2nd Table

1811.	May	Beginning of the earthquakes in the Island of St. Vincent, which lasted till May 12th.
——	December 16th	Beginning of the commotions in the Valley of the Mississippi and the Ohio, which lasted till 1813.
——	December	Earthquake of Caraccas.
1812.	March 26th	Destruction of Caraccas, earthquakes, which continued till 1813.
——	April 30th	Eruption of the volcano in St. Vincent's, and on the same day, subterranean noises at Caraccas and on the bank of the Apure.

3rd Table

1834.	January 20th	Sabiondoy, lat. 1° 15′ N. (near Pasto), dreadful earthquake; eighty persons perished; town of Santiago swallowed up.
——	May 22nd	Santa Martha, lat. 11° 30′ N.; two-thirds of the town thrown down; in course of a few days, sixty bad shocks.
——	September 7th	Jamaica,—violent earthquake, town not much damaged.
1835.	January 20th	Osorno, lat. 40° 31′ S. in eruption.
	Before day-light	Aconcagua, lat. 32° 30′ S. in eruption.
	in the morning	Coseguina, lat. 13° N. in terrific eruption, continuing in activity during the two ensuing months.
——	February 12th	Earthquake at sea, very strong off the coast of Guyana.
——	February 20th	Juan Fernandez, lat. 33° 30′ S., submarine eruption.
	11½ A.M.	Concepcion, (lat. 36° 40′ S.), and all the neighbouring towns destroyed by an earthquake; the coast permanently elevated. Volcanos along the whole length of the Cordillera of Chile in eruption.

		N.B. These volcanos remained in activity for some months subsequently, and many earthquakes were felt.
——	November 11th	Concepcion, severe earthquake; Osorno and Corcovado in violent action.
——	December 5th	Osorno fell in with a grand explosion.

With respect to these tables, it must be observed, that we can never feel sure that the connexion of volcanic phenomena at very distant points is real, until some strongly marked event takes place during the same moment at those points, the intermediate country being likewise affected to a certain degree. In the first two tables, the connexion of the West-Indian vents and the coast of Venezuela may be admitted as almost certain,[22] nor is the distance very great, being at most only 400 miles. But when, on the one hand, we include Quito, distant from the above area more than 1200 miles, and, on the other, the Valley of the Mississippi, the case is very much more doubtful. The coincidence certainly is very remarkable, both in regard to the commencement and the cessation of the long series of earthquakes which affected South Carolina, the basin of the Mississippi, the Leeward Islands, and Venezuela: yet New Madrid is more than 2000 miles from the latter. A repetition alone of such coincidences can determine how far the increased activity of the subterranean powers, at points so remote, is the effect of some general law, or of accident.

We now come to the third table, with which we are more particularly concerned. I have already described in detail the remarkable volcanic phenomena which happened, in connexion with each other, on the morning of February 20th, 1835, and likewise during the subsequent year.

On January 20th, one month previously, three eruptions, as stated in the table, occurred almost at the same hour in very distant points of the Cordillera. Near midnight on the 19th, the summit of Osorno shone like a great star in the horizon; and this appearance soon increased into a magnificent glare of light, in the midst of which, by the aid of a telescope, great dark bodies were seen to shoot upward and to fall down in endless succession. When I was at Valparaiso some time afterwards, Mr. Byerbache, a resident merchant, informed me, that sailing out of the harbour one night very late, he was awakened by the captain to see the volcano of Aconcagua in activity. As this is a most rare event I recorded the date. Some time afterwards papers arrived from Central America giving an account of one of the most fearful eruptions of modern times.[23]

Pl. 1. Part of the western coast of South America.

"On the 19th of January, after twenty-six years' repose, a slight noise, attended with smoke, proceeded from the mountain of Coseguina. On the following morning (the 20th) about half-past six o'clock, a cloud of very unusual size and shape was observed by the inhabitants to rise in the direction of this volcano." Enormous quantities of ashes and pumice were then ejected, and the air was darkened, and the ground convulsed, during the three succeeding days. Nearly two months afterwards the volcano was in action. Mr. Caldcleugh observes, that perhaps the only parallel case on record is the well-known explosion of Sumbawa in 1815.

When I compared the dates of these three events, I was astonished to find that they agreed within less than six hours of each other. Aconcagua is only 480 miles north of Osorno, but Coseguina is about 2700 north of Aconcagua. It may be asked, were these three eruptions, which burst through the same chain of mountains, in any respect connected, or was the coincidence accidental? We cannot be too cautious in guarding against the assumption that phenomena are connected, because they happen at periods bearing some determined relation to each other. If we wished to show that the subterranean forces acted after periods of a century, as has sometimes been believed, we might adduce the case of Lima, violently shaken by an earthquake on the 17th of June, 1578, and again on the very same day in 1678; or the eruptions of Coseguina in the years 1709 and 1809, which are the only two on record previous to that of 1835. Again, we might urge, on such grounds, that the Guatimala convulsions follow, at the interval of one year, those near Pasto; for a district in the neighbourhood of the latter place was overthrown by a violent shock precisely one year before the explosion of Coseguina; both having occurred on the 20th of January. Cosme Bueno imagined that this relation actually did exist between the subterranean movements in Guatimala and Peru, and this case makes one more to the list which I have subjoined[24] as extracted from Humboldt. With respect to the simultaneous eruptions of Aconcagua and Osorno, there is little difficulty in admitting that they may have been connected, because in this same region, and only a month subsequently, volcanos further apart were affected by the same impulse. There is nevertheless this remarkable difference in the two cases;—the last, or that of February the 20th, was a period of commotion throughout the kingdom of Chile, while the simultaneous eruption of Aconcagua and Osorno appears to have been unaccompanied by any *general* movement in the subterranean regions. This eruption, probably, was the first indication of those great volcanic disturbances which ensued exactly one month afterwards; for it seems to be a very general occurrence in earthquakes, that weak spasms precede the worst convulsions. Thus, in 1822, on the 4th of November, Copiapó (lat. 27° 10′) was visited by a severe shock, which damaged many

houses; and was followed the next day by a much more violent earthquake, which nearly destroyed the town, and did considerable injury to that of Coquimbo, in lat. 29° 50'.[25] On the 19th of the same month Valparaiso was almost destroyed. Other instances[26] might be brought forward to show that most earthquakes, though appearing sudden, are in truth parts of a prolonged action, as evinced both by the events which precede and those which follow it.

Although, possibly, we may allow that the eruptions of Aconcagua and Osorno, occurring in the middle of the same night, were connected together, and formed a part of the great subsequent disturbances,—yet what must we conclude respecting their coincidence with Coseguina, so immensely remote? The case is rendered far more extraordinary by two of the three volcanos being generally quiescent. Coseguina, according to Mr. Caldcleugh, burst forth after twenty-six years of repose; and Aconcagua so seldom manifests any signs of activity, that it had even been doubted whether any part of this gigantic mass, with an altitude of more than 23,000 feet, is of volcanic origin. To illustrate the case: if we suppose Stromboli and Vesuvius to be in violent eruption on the same hour of the night, little would be thought of the coincidence; but it would be otherwise if this should happen with Vesuvius and Etna; and our surprise would be greatly increased if we afterwards heard that Hekla, after twenty-six years' repose, had burst forth at the same time with tremendous explosions. Nevertheless, if such a coincidence had occurred in Europe, a country possessing no unity of character, and the two points not being more than 2000 miles apart, it is very doubtful how far the phenomenon would have been worthy of consideration. But the case is different in America, where the volcanic orifices all fall on one great wall or fissure, (for the Andes may be indifferently so called,) and where the immensity of the level area on the eastern side, proves with what wonderful equability the subterranean forces have acted on this portion of the globe. Moreover, when a line of coast more than two thousand geographical miles in length has been elevated (as I hope hereafter to prove) within a period so recent, that, as compared to the countless past ages of which we possess records in the *works* of nature, it may be reckoned as unity; on such a coast it ceases to be improbable, in any excessive degree, that the many impulses which together have produced the one grand effect, should sometimes have been absolutely simultaneous.

It has long been remarked, that the vents throughout the Cordillera may be grouped into several systems. Thus we have already shown, that the extreme southern volcanos are connected with those of Central Chile; and I was informed by an intelligent resident that he had seen Aconcagua and two volcanos northward of it, in great activity together:—we thus

have a portion of the Andes 780 geographical miles in length (about the distance from the south of England to Vesuvius) forming one connected system. Ulloa[27] states, that when Lima was overthrown in 1746, three volcanos near Patas and one near Lucanas burst forth; these places being 480 miles apart from each other. Moreover, Arequipa, to the south, has twice (1582 and 1687) been affected by severe earthquakes simultaneously with Lima. The distance between Arequipa (where there is an active volcano) and Patas is rather more than 600 miles; and this perhaps may form a second system.

Humboldt[28] says, "it appears probable that the higher part of the kingdom of Quito, and the neighbouring Cordillera, far from being a group of distinct volcanos, constitute a single swollen mass, an enormous volcanic wall stretching from north to south, and the crest of which exhibits a surface of more than six hundred square leagues. Cotapaxi, Tunguragua, Antisana, and Pichincha, are placed in this same vault, on this raised ground." He afterwards shows, from the phenomenon already alluded to, of the cessation of the column of smoke at the moment when Riobamba was overthrown, the connexion of these volcanos with those of Pasto and Popayan. This joint system is rather less than 300 miles in length. Again, to the north at Guatimala, Mexico, and California, we have three groups of volcanos, each system being a few hundred miles apart.

The connexion between the vents in each separate system has been, in some places, plainly shown, and is extremely probable in all; but what relation the different systems bear to each other is more doubtful. I am not aware of any fact on record, similar to the contemporaneous eruption of Osorno and Aconcagua with Coseguina. It must not, however, be overlooked, that such events may have happened every year since the Spanish conquest, without the coincidence having once been detected. Excepting from the concurrence of two accidents, I should never have known of this case. On that same night every vent in the Cordillera might have shown transient signs of activity, and six months afterwards it would have been as impossible to have discovered that such had happened, as to have ascertained whether the next day were bright or clouded. There are some active and some nearly extinct craters, in the interval between the Chilian and Peruvian systems, (which is the longest of any, being 900 miles,) but they are situated in countries very thinly peopled, and in some parts entirely desert; and who is there in such cases to record phenomena, which, even if beheld, are thought of little consequence?

Returning to the third table, I feel no doubt that the volcanic phenomena which occurred in S. America sometime previously as well as subsequently to the months of January and February 1835, were far

more numerous than the average proportion during an equal length of time. This remark applies to the two tables copied from Humboldt. In looking at the dates of these events, it must be remembered that each date represents only the moment when the crust of the earth has given way beneath the force, which in some cases has already shown its action, and invariably continues to do so during a period, often of considerable length. Under this point of view, the earthquakes of Caraccas and New Madrid, of Coseguina and Concepcion, may be considered as actually contemporaneous.

From these various circumstances, I am strongly inclined to believe, that the subterranean forces manifest their action beneath a large portion of the South American continent, in the same intermittant manner as, in accordance with all observation, they do beneath isolated volcanos,—that is, remaining for a period dormant, and then bursting forth throughout considerable districts with renewed vigour.

Nature of the Earthquakes on the Coasts of South America

I will now more particularly consider the nature of the earthquakes which occur at irregular intervals on the coast of South America. It cannot be otherwise than difficult to trace their precise origin, but the following considerations, as it appears to me, lead to one conclusion alone—namely, that they are caused by the interjection of liquefied rock between masses of strata. Ulloa, in his travels,[29] says,

> Experience has sufficiently shown, especially in this country (South America), by the many volcanos in the Cordillera which pass through it, that the bursting of a new burning mountain causes a violent earthquake, so as totally to destroy all the towns within its reach, as happened at the opening of the volcano in the desert of Carguagoazo. This tremulous motion, which we may properly call an earthquake, does not so usually happen in case of a second eruption, when an aperture has been before made, or, at least, the motion it causes in the earth is comparatively but small.[30]

Although the bursting forth of a *new* vent may invariably be accompanied by an earthquake, the converse is not true; for if it were, at Valparaiso, Concepcion, Lima, Caraccas, and other places, in the *immediate neighbourhood* of the part most violently shaken, an eruption must always have taken place, which, even if we suppose it to have occurred beneath the sea, is improbable in the highest degree. But we may suppose that these earthquakes are owing to some phenomenon analogous to volcanic eruptions. This opinion is much strengthened by the fact, that great earthquakes, like great eruptions, generally recur only after long intervals of repose, and they thus lead us to believe, that the

subterranean force is relieved by either in the same manner. This, indeed, is the direct opinion of the inhabitants of the whole west coast of South America, who are firmly convinced of an intimate relation between the suppressed activity of the volcanos in the Andes and the tremblings of the ground. We have, also, seen that, when the island of Chiloe was strongly shaken, some men at work on the flanks of the Cordillera, between the volcanos of Osorno and Minchinmadom, (which both sent up dark columns of smoke, like signals to mark the new period of violence,) were quite unaware of the great convulsion, which then caused the shores of the Pacific to vibrate throughout a space of more than a thousand miles.

There is, however, one difference, although more apparent than real, between earthquakes like that of Concepcion, and those alluded to by Ulloa. In the former, it has almost invariably happened, at least in those on the South American coast, that a vast number of shocks have followed the first great convulsion,[31] and these, as well as the accompanying subterranean noises, having proceeded from the same quarter with the first shock, are therefore undoubtedly due to the very same cause, only acting with somewhat less intensity. Thus, even in the first twenty-four hours after the earthquake of 1746 at Lima, no less than 200 horrible (I use the language of its historian) shocks were counted. Now in the other case, Ulloa says, that when the orifice of eruption is once formed, the earth becomes nearly tranquil; yet we well know, that the volcano itself almost invariably continues in great activity for many weeks afterwards. Had Ulloa, however, stood near the crater itself, he would undoubtedly have felt those small tremors, which accompany each fresh explosion, as described by others who have been so circumstanced. The tremors, therefore, seem analogous to the secondary shocks; and, this being so, the phenomena in the two cases are in every respect closely similar. In a primary volcanic outburst, we know the cause to be the explosion of liquid and aeriform matter, first through solid strata, and afterwards through a nearly open passage; hence we are led to conclude, that the cause of the simple earthquake, with its secondary shocks, are explosions of a similar nature, which, however, do not open a passage, but rend successively portions of the superincumbent masses.

At Concepcion, where the streets run in two series, at right angles to each other, the walls were affected, as already observed, according to their direction. This was strikingly exemplified in the cathedral, where the great buttresses, built of solid brickwork, were cut off as if by a chisel, and hurled to the ground; whilst the wall, to support which they had been vainly built, though much shattered, stood erect,—for the latter had its extremity directed towards the point whence the vibration

travelled, but the buttresses were in lines parallel to the undulation. Nearly similar circumstances were observed[32] in 1822 at Valparaiso. At the great earthquake of Caraccas the direction of the vibration was E.N.E. and W.S.W., and some definite direction appears to have been observed in almost every violent earthquake. Now, it may be asked, could a vibration, which had travelled upwards through the earth from a profound depth, be felt on the surface, as if it had come from a given point of the compass, and could it likewise determine the overthrow of walls according to their direction with respect to any such point? It appears to me clearly not; but that a vibration to produce such effects must be transmitted from the rending of strata, at a point not very deep below the surface of the earth.

Earthquakes generally affect elongated areas. In the shock of 1837, in Syria, the vibration was felt "on a line 500 miles in length by 90 in breadth."[33] Humboldt[34] remarks, that earthquakes follow the coast of New Andalusia in the same manner as they do that of Peru and Chile. Thus, at Valparaiso in 1822, the movement was felt along 880 miles of the shore of the Pacific; and at Concepcion, in 1835, for the greater length of more than 1000 miles; but on no occasion has the shock been transmitted across the Cordillera to a nearly equal distance. In 1835 the rocking of the ground was so gentle at Mendoza, that an old man, one of the inhabitants, (and every one in these countries is possessed with an almost instinctive power of perceiving the slightest tremor,) told me, that for some time he mistook the movement of the ground for a giddiness in his head, and that he called out to his friends that he was going to die. At Concepcion, Valparaiso, Lima, and Acapulco,[35] the residents believe that the disturbance generally proceeds from the bottom of the neighbouring sea; and thus they explain the unquestionable fact,[36] that the inland towns are generally much less injured than those near the coast. It does not appear, that the disturbance proceeds from any one point, but from many ranged in a band; otherwise the fact of the linear and unequal extension of earthquakes would be unintelligible. Thus, in 1835, the island of Chiloe, the neighbourhood of Concepcion, and Juan Fernandez, were all violently affected at the same time, and more so than the intermediate districts. In mountainous countries, such as New Andalusia, Peru, and Chile, when earthquakes follow coast lines, they may be said to extend parallel to the littoral chain of mountains.

The last consideration I shall enter on, as indicating the cause of earthquakes, is, that in South America they have sometimes (if not, as I believe, generally)[37] been accompanied by elevations of the land; but this, judging from the Lima shock of 1746, does not appear to be a necessary concomitant, at least to a *perceptible* amount. It might at first be thought that, at Concepcion, the uplifting of the ground, which

accompanied the first and great shock, would by itself have accounted for the whole phenomenon of the earthquake. The great shock, however, during the few succeeding days, was followed by some hundred minor ones (though of no inconsiderable violence), which seemed to come from the same quarter from which the first had proceeded; whilst, on the other hand, the level of the ground certainly was not raised by them; but on the contrary, after an interval of some weeks, it stood rather lower than it did immediately after the great convulsion,—a consequence, perhaps, of the settling down of the shaken ground. In the same manner, in 1822, at Valparaiso, the permanent change of level in the rocks on the coast was observed the morning next after the great shock; though the earth continued to tremble at intervals for many days. In these instances of change of level we have, then, a clear indication of some cause of disturbance, super-added to that which produced the vibrations, and which, it is highly probable, would accompany the simple elevation of the coast in mass.

From these considerations, we may, I think, fairly conclude, with regard to the earthquakes on the west coast of South America,

1st. That the primary shock is caused by a violent rending of the strata, which seems generally to occur at the bottom of the neighbouring sea.

2nd. That this is followed by many minor fractures, which, though extending upwards nearly to the surface, do not (excepting in the comparatively rare case of a submarine eruption) actually reach it.

3rd. That the area thus fissured extends parallel, or approximately so, to the neighbouring coast mountains.

4th. That when the earthquake is accompanied by an elevation of the land in mass, there is some additional cause of disturbance.

And lastly, That an earthquake, or rather the action indicated by it, relieves the subterranean force, in the same manner as an eruption through an ordinary volcano.

Now, what constitutes the axis, where visible, of most great mountain-chains? Is it not a wedge-formed linear mass of rock, which scarcely any geologist disputes was once fluid, and has since cooled under pressure? Must not the interjection of such matter between masses of strata have relieved the subterranean pressure in the same manner, as an ejection of lava and scoriae through a volcanic orifice? The dislocation having been effected in that portion of the upper crust of the earth, now forming a mountain, must not superficial vibrations, proceeding from a focus not deeply seated, have been propagated over the surrounding country? And, whatever direction these dislocations took, would not an area, elongated in the same line, have been affected by the vibration?

In drawing this parallel, I state my belief, that those earthquakes, with

their secondary shocks, which are attended by such phenomena as accompanied the earthquake of Concepcion in 1835, are caused by the rending of great masses of strata, and their interjection by fluid rock;—a process which must have formed one step in a line of elevation.

The inhabitants of Concepcion believed, that the vibrations proceeded from the south-west, in which quarter subterranean noises were likewise frequently heard. It is, therefore, a most interesting circumstance, that the island of Santa Maria, situated 35 miles distant in this direction, was found by Captain Fitz-Roy to have been elevated to nearly three times the height that the coast near Concepcion was upraised. At Tubul, S. by E. of Santa Maria, the land was raised 6 feet; at the southern extremity of the latter island, 8 feet; in its middle, 9 feet; and at its northern extremity, upwards of 10 feet.[38] These measurements, which were made with extreme care by Captain Fitz-Roy, seem to point out an axis of elevation in the sea off the northern end of Santa Maria.

There is one remark, which I must introduce here. The motion of the earth, on February 20th, 1835, at Valdivia, appeared to me like that of a crust, spread over an undulating fluid; and in my Journal, I have compared the motion to the bending of thin ice, beneath a moving weight. Afterwards, when I became convinced that the crust there does rest upon a sea of molten rock, my first impression regarding the movement was strongly confirmed. Michell long since observed, (Phil. Trans., 1760, p. 8) that "the motion of the earth in earthquakes is partly tremulous and partly propagated by waves, which succeed one another, sometimes at larger and sometimes at smaller distances; and this latter motion is generally propagated much further than the former." This distinction, I believe, is strictly true. Professor Phillips[39] argues that rocks, although elastic in their parts, are "very imperfectly so in their mass, owing to the numerous divisions which intersect them. Composed of such materials," he says, "the crust of the earth does not, and in fact hardly can, *vibrate*, in the ordinary sense of this term; the motion observed is more similar to the undulation of a flexible lamina over an agitated liquid." The result arrived at by this reasoning thus coincides with mine, drawn from the impression on my senses; and it, at first, appears to explain, in a very satisfactory manner, the propagation to greater distances of the long and gentle undulations than of the vibrations, by the transmission of the former in the subterranean fluid, and of the latter in the crust of the earth. With respect, however, to the supposed want of elasticity in the crust of the earth, taken in mass, I cannot agree with Professor Phillips. Michell, (Phil. Trans., 1760, p. 35,) when he adduces the fact of the great vibration, or rather oscillation, during gales of wind, of steeples, and even towers,[40] which may be said to be composed of a vast number of strata of different densities, and

which are frequently traversed by fissures or faults, leaves scarcely any doubt on the mind that a similar and much greater vibration might be transmitted from the depths of the earth, where the parts must be pressed together with incomparably greater force than in any building. Plausible as is the foregoing explanation of the two kinds of movements, I do not believe it to be the correct one; for if an undulation be ever produced in the subterranean fluid expanse, we can hardly conceive a more powerful cause of it, than the upward rush of a great body of molten rock and aëriform matter from the lowest abyss of a volcano: but we know that eruptions on an enormous scale have happened through old vents, even in areas subject to far-extended and undulating earthquakes, without such movements having been produced. From this consideration, and from the fact that the force of earthquakes appears to have a definite relation to the thickness of crust ruptured, as we may conclude from the great difference in the effects caused by an eruption through an old, and one through a new orifice, I do not conceive we are justified in admitting the hypothesis of an undulating fluid. The two kinds of movements may, possibly, be explained, by considering that when the crust yields to the tension, caused by its gradual elevation, there is a jar at the moment of rupture, and a greater movement may be produced by the tilting up of the edges of the strata and by the passage of the fluid rock between them. In breaking a long bar of steel, would not a jar be caused by the fracture, as well as a vibration of the two ends when separate?

Mr. Hopkins,[41] in his Researches on Physical Geology, has demonstrated, that when an elongated area is elevated by a force acting equally beneath all parts, if the strata yield, fissures must be formed parallel to its longer axis, and other minor ones transverse to it. Knowing then with certainty, that the coast of Chile, near Concepcion, was elevated on the 20th of February, and likewise that the area affected by the earthquake was elongated;—bearing also in mind, that several of these elevations have occurred, as attested both historically and by the extensive beds of recent species of shells, at the altitude of some hundred feet, we are absolutely compelled to believe, that the area (without we assume that the strata possessed extraordinary powers of extension) was at that time fissured in lines, the principal of which were parallel to its longer axis. If, however, the elevatory force acted unequally in different parts, as was the case in Chile, we can understand, from the admirable generalization of the same author, that separate fissures might be formed, which would produce at the same instant, in distant places, separate shocks, perhaps of different intensities. Hence we need not suppose, that the shocks felt more strongly at Juan Fernandez, Concepcion, and Chiloe, than at intermediate points, proceeded from any one focus, but that they were generated in each separate district,—the vibrations probably having, in

each case, different directions.[42] This explanation is, I think, far more satisfactory than that offered by Humboldt, of the supposed inertness of an intermediary mass of rock, in transmitting to the surface vibrations from a deeply-seated focus.

On Different Kinds of Earthquakes; and Conclusions Regarding those which Accompany Elevatory Movements

I confine the foregoing observations to the earthquakes on the coast of South America, or to similar ones, which seem generally to have been accompanied by elevation of the land. But, as we know that subsidence has gone on in other quarters of the world, fissures must there have been formed, and therefore earthquakes. I think, it would be highly advantageous to geology, if the author who has followed out the effects of an elevatory force, would consider those produced by the failure of support in the arched surface of the globe. The earthquakes of Calabria, and perhaps of Syria, and of some other countries, have a very different character from those on the American coast. When Molina, the historian of Chile was in Italy, he was much struck with this difference; he says,[43] in Chile even the smaller shocks extend over the whole kingdom, and are propagated horizontally, whilst those which he felt at Bologna, were of small extension, but instantaneous, and commonly explosive.

I will add, that in the accounts collected by Mr. Lyell[44] of the earthquakes of Calabria, Lisbon, and some other places, portions of the surface are described as having been absolutely engulphed, and seen no more: but this does not appear to have happened in any of the earthquakes on the west coast of South America. If the fluid matter, on which I suppose the crust to rest, should gradually sink instead of rising, there would be a tendency to leave hollows, and therefore a suction exerted downwards; or hollows would be actually left, into which the unsupported masses might be precipitated with the violence of an explosion. Such earthquakes, we may conclude, from what has been shown in the foregoing part of this paper, would seldom be accompanied by eruptions, and never, probably, by *periods* of renewed volcanic energy. According to M. Boussingault,[45] those earthquakes in South America which have been most destructive to human life, that is, which have been most sudden and violent, have not coincided with volcanic eruptions. He adduces several instances, including the shocks felt at Caraccas in 1812; but, according to Humboldt,[46] the connexion between the subterranean disturbances at that place and the West Indian vents can hardly be doubted. M. Boussingault's remark, indeed, although perhaps generally true, should be taken with some reserve; for had the

earthquake of Concepcion happened at night, thousands of persons must inevitably have perished.

In a line of fracture, produced by subsidence, the distortion and overthrow of the strata would probably be even greater than in one of elevation, from the circumstance, that as soon as the weight of the mass overcame its cohesion, and it began to sink, there would be no counterbalancing power, like gravity during elevation, to check the movement, excepting, indeed, the lateral pressure of the masses together, and this would only add to the disturbance. There would be, in this case, no axis of injected plutonic rock, or at least not one protuberant above the general surface; and thus we may explain the extreme disturbance in the strata of countries which are only hilly, like parts of Great Britain; and the occurrence there of such axes of elevation, as they are generally called, but which probably, in most cases, would be more appropriately termed axes of subsidence.

If the theory which I have given of the cause of the earthquakes on the west coast of South America be true, we might naturally expect on the same principle to find proofs of successive formation in the many parallel ridges, of which the Cordillera is composed. In the parts of Central Chile which I examined, this is true, even with regard to the two main lines; of which one is partly formed of inclined beds of conglomerate, consisting of pebbles derived from the rocks of the other. I have also evidence, but of a less satisfactory kind, that some of the exterior lines of mountains are altogether of subsequent date to the more central ridges. Moreover, in all parts of the Cordillera, there are proofs of an equable elevation *in mass* to a very great altitude. I was so much struck with this latter fact, connected with what I imagined must have taken place during the Concepcion earthquake, that I came to nearly the same conclusion, which Mr. Hopkins has demonstrated by his mathematical researches, namely, that mountain-chains are only subsidiary and attendant phenomena on continental elevations. If this be so, and few, after having read Mr. Hopkins's memoir, will dispute it; then, as it is certain continental elevations have certainly taken place on a great scale within the recent period, so, as certainly, must masses on the lines of fracture have been unequally lifted up and let down;—that is, some steps in the formation of a mountain-chain have been produced.

I may here ask, when Mr. Hopkins[47] says, he "can in no way conceive the successive formation of parallel fissures, without hypotheses respecting the mode of action of the elevatory force, which are infinitely too arbitrary to be admitted for an instant," has he considered the effects of long intervals of rest, during which the injected rock might become solid? Would not the crust in such case yield more readily on either flank, as I believe it must have done in the Cordillera, than on the line of an axis

composed of solidified rocks, such as granite or porphyry?[48] An extremely slow elevation of the land, with long intervals of rest, being the only kind of movement of which we have any knowledge, the slow cooling of that portion of the liquefied rock which is propelled into the upper parts of the crust, cannot be considered an arbitrary assumption.

From the facts stated in this paper, we may safely conclude, that volcanic action, even on a *very grand scale*, as in the Andes, is only one effect of the power which elevates continents, at the slow rate at which the South American coast is now rising. In looking back to the past history of the world, we learn from Mr. Lyell,[49] that there have been volcanic eruptions during every epoch, from that of the Cambrian formations to the present day. The ancient eruptions seem to have been accompanied by all the circumstances which attend modern ones; nor is there any evidence, as remarked by the same author, that the quantity of matter ejected, in the greater number of ancient cases, was excessive. Therefore, we must conclude that continental elevations, one of the effects of the same motive power which keeps the volcano in action, has ordinarily gone on, since those ancient days, at the same slow rate as at present, and, consequently, as above inferred, the step-like formation of mountain-chains. It may, therefore, be questioned, whether we are justified in admitting the hypothesis of a paroxysmal elevation of any mountain-chain, without distinct proofs in each particular case, that a series of impulses, like those, which now acting frequently on the same lines, rend the earth's crust, and elevate unequally portions of it, could not have effected the observed effects. It is, however, a subordinate question, whether there exist proofs of paroxysmal violence in some mountain-chains; the important fact which appears to me proved, is, that there is a power now in action, and which has been in action with the same average intensity (volcanic eruptions being the index) since the remotest periods, not only sufficient to produce, but which almost inevitably must have produced, unequal elevation on the lines of fracture.

Theoretical Considerations on the Slow Elevation of Mountain Chains

The conclusion that mountain-chains are formed by a long succession of small movements, may, as it appears to me, be rendered also probable by simple theoretical reasoning. Mr. Hopkins has demonstrated, that the first effect of equably elevating a *longitudinal* portion of the crust of the earth, is to form fissures, parallel to the longer axis (with others transverse to them, which may here be neglected) of the kinds represented in the accompanying diagram (No. 1.), copied from that published in the Cambridge Philosophical Transactions. But he further

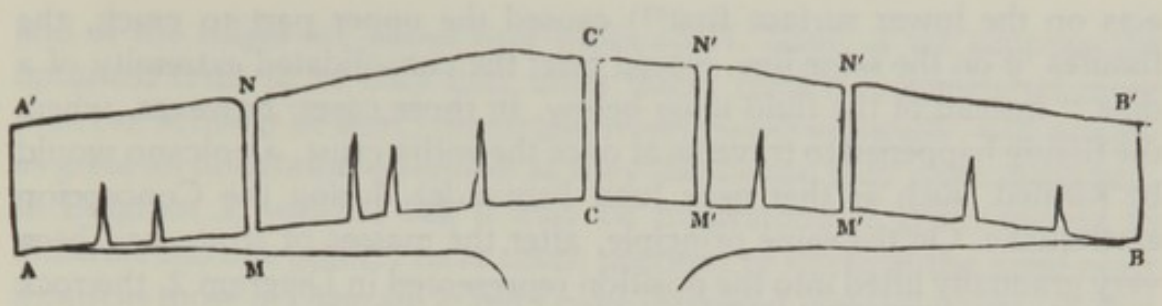


Fig. 1. Uplifted square masses.

shows, that the square masses, now disjointed, will,—from the extreme improbability of the force uplifting them, when separate, equably, or from their settling down afterwards,—assume some position as that given in Diagram No. 2. In the Cordillera, which may be taken as a good example of the structure of a great mountain-chain, the strata in the central parts are inclined more commonly at an angle above 45°, than beneath it; and very often they are absolutely vertical. The axis of the lines of dislocation is formed of syenitic[50] and porphyritic masses, which, from the number of dikes branching from them, must have been fluid when propelled against the lower strata.[51] If then we suppose Diagram 2

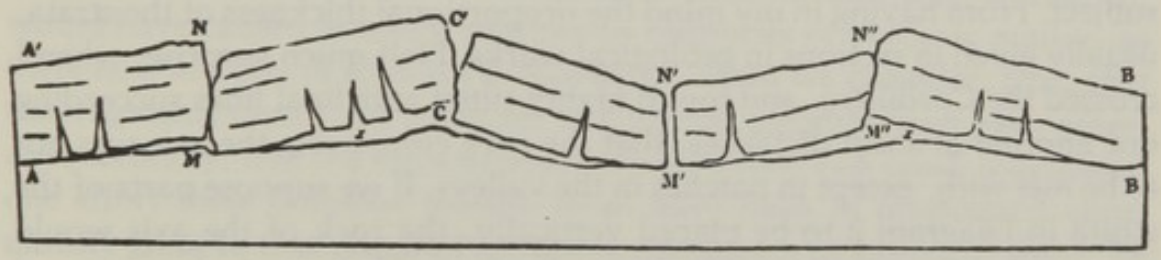


Fig. 2. Dislocated square masses.

to represent the section of the Cordillera before its final elevation, I may ask, how is it possible, that some of the masses of strata should be placed vertical, and others absolutely overturned, by the action of the *fluid* rock, without the very bowels of the earth gushing out? Should we not have one enormous deluge of volcanic matter, instead of wedge-formed, injected masses of solid crystalline rock? On the other hand, if we suppose the loftiest chain of mountains to be formed by a succession of shocks similar to those of Concepcion,—a few stronger and many slighter ones, separated from each other by long intervals of time,—then we may believe, that the formation of a fissure through the whole thickness of the crust would be the effect of many efforts on the same line, and that during the intervals, the rock first injected would become cooled. When, therefore, the tension (which, according to Mr. Hopkins,

acts on the lower surface first[52]) caused the upper part to crack, the fissures, if on the same line, would meet the consolidated extremity of a dike,[53] instead of the fluid mass below. In those cases, however, where the fissure happened to traverse at once the entire crust, a volcano would be formed, such as that near Juan Fernandez during the Concepcion earthquake. On the same principle, after the masses of strata had been very gradually lifted into the position represented in Diagram 2, the rock beneath the anticlinal axes, from having been propelled beyond its former subterranean isothermal line, would be cooled, and, if sufficient time were allowed, it would be consolidated. In this manner the strata, each new fracture being firmly cemented by the cooling of the injected rock, might be overturned into any possible position, and yet, from a gradually thickening crust being formed over the fluid mass, on which the whole is believed to rest, the earth would be protected from a deluge of lava. If this reasoning be sound, we may deduce this remarkable conclusion, that in a mountain-chain, having an axis of plutonic rock, which was propelled upwards in a fluid state, where the strata betray the effects of the most violent action, although it be on a gigantic scale, there we have the best evidence of an almost infinite series of small movements.[54]

I will enter on only one other consideration connected with this subject. From having in my mind the proportional thickness of the strata, usually given in sections in geological works, I felt much surprise, when I crossed the Cordillera, and found highly-tilted anticlinal lines succeeding one another at short distances, that the rock composing the axis was not to be met with, except in patches in the valleys. If we suppose parts of the strata in Diagram 2 to be placed vertically, the rock of the axis would necessarily be exposed in wide spaces; but here, I believe, is the source of error,—geologists have not always sufficiently considered the thickness of the mass upturned, in relation to the distance of the parallel anticlinal lines one from another. In the Cordillera, in a width of about sixty miles, there are eight or more anticlinal lines; and thus the centres of the troughs

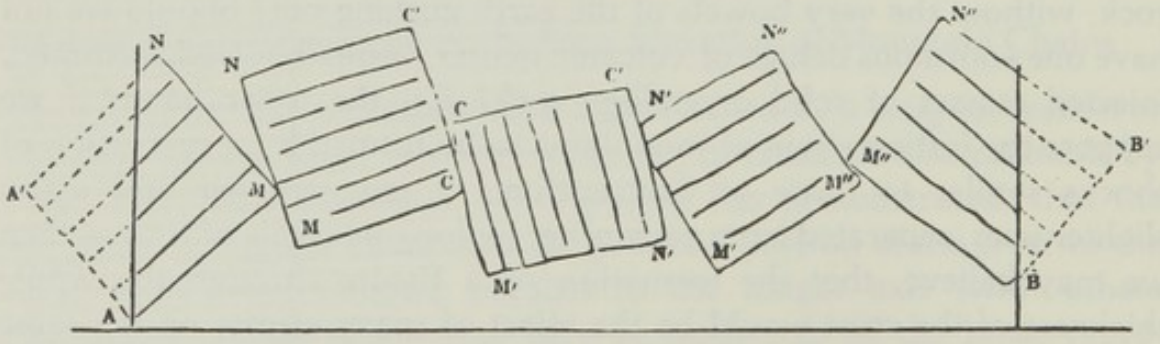


Fig. 3. Overthrown crustal masses.

and of the ridges are about four miles apart. Now, if we suppose the upturned crust to be only four miles thick, (which is somewhat more than can actually be seen,) the strata, when placed vertically, will occupy as great an horizontal extension as they did before they were disturbed. In Diagram 3, which I beg it may be understood is given merely to illustrate this *one* point, I have taken portions of strata of the same exact length as those in Diagram 2; but I have increased their thickness, so that it equals the distance of the anticlinal lines from each other;—we shall now see that not only the whole axis is covered, but that the masses cannot be forced into their former horizontal limits. I have not, however, allowed for the immense abrasion which, under such circumstances, the lower angles would suffer, nor for the denudation and rounding of the upper ones. This supposed crushing together of such gigantic fragments will, perhaps, explain the utter confusion, which must be familiar to every geologist who has examined any great mountain-chain.[55] I must here add, that according to these views, which I believe are correct, the theoretical part of the foregoing argument, namely the difficulty of confining during any paroxysmal movement the fluid matter within the crust, is weakened; yet I believe the principle holds good, for, in order to break up and throw over portions of *very thick* crust, as in Diagram 3, there must have been great horizontal extension, and this, if sudden, would have caused as many continuous outbursts of volcanic matter, as there now are axes of solid rock. Moreover, when we consider, first, that the fragments must have stood for one instant separate from each other, and, secondly, that the force necessary to turn over and crush together at one effort these immense masses, must have been in proportion enormously great to that required merely to lift them,—it cannot, I think, be doubted for a moment, that if the force had acted *suddenly*, these portions of the earth's crust would have been absolutely blown off, like fragments of rock by gunpowder; but this has not happened, and, therefore, the force did not act suddenly.[56]

If we grant that the earthquake of Concepcion on the 20th of February marked one step in the elevation of a mountain-chain; then, as during the twelve succeeding days there were counted upwards of three hundred shocks, which proceeded from the same quarter with the great shock, so the fluid stone must have been pumped into the axis by as many separate strokes; nor did the process cease for many subsequent months.[57] In the central ridges of the Cordillera, there are masses of compact unstratified rocks, half again as lofty as Etna, and I believe, from the reasoning above given, that they were formed by steps nearly as slow as those indicated by the innumerable layers of volcanic matter accumulated on the flanks of the Sicilian mountain. In the volcano, that is, a mountain which has been ruptured in its incipient state, the fluid stone being brought to the

surface is rapidly cooled, and hence successive layers are formed; but in the axis of plutonic formation (or subterranean volcano, if it may be so called), the injected matter, not being rapidly cooled, is blended into one huge conical pile. This whole view is nothing more than an application of Hutton's doctrine of the repetition of small causes to produce great effects; and which Mr. Lyell has already brought distinctly to bear on this particular subject.

The action of the elevatory force, as known by beds of littoral shells, successive lines of aqueous erosion on cliffs of solid rock, and terraces rising one above another, seems everywhere to have been prolonged, although intermittently: in the volcano, the structure of the mountain, as well as all history, bespeaks the same fact with respect to the eruptive force. During the Concepcion earthquake, we have seen that these powers, so analogous in their action, were absolutely parts of one common phenomenon. Bearing in mind Mr. Hopkins's demonstration, if there be considerable elevation, there must be fissures, and, if fissures, almost certainly unequal upheaval, or subsequent sinking down,—the argument may be finally thus put:—mountain-chains are the effects of continental elevations; continental elevations and the eruptive force of volcanos are due to one great motive power, now in progressive action; therefore the formation of mountain-chains is likewise in progress, and at a rate which may be judged of by either phenomenon, but most nearly by the growth of volcanos.

Concluding Remarks

With these views, if we look at a map of America, and observe the continuity of the great chain of the Andes, and its lesser parallel ones, in which from lat. 55° 40′ South to 60° North, a space of little less than 7000 miles, the volcanic forces either now are, or recently have been, in action,—and likewise the symmetry of the whole,—we shall be deeply impressed with the grandeur of the one motive power, which, causing the elevation of the continent, has produced, as secondary effects, mountain-chains and volcanos. The same reasons which led me to the conviction, that the train of connected volcanos in Chile and the recently uplifted coast, together more than 800 geographical miles in length, rested on a sheet of fluid matter, are applicable with nearly equal force to the areas beneath the other trains. We see that these areas are connected by one uniform chain of mountains, from many distant points of which fluid rock is yearly ejected; and as there are proofs that nearly the whole west coast of South America has been elevated within a period geologically modern, and that this movement, in some parts at least, has extended across the continent,—keeping, also, in mind the probability, that during

periods of increased subterranean action, such as those indicated in the foregoing tables, the whole western part of the continent has been almost simultaneously affected, it appears to me, that there is little hazard in assuming, that this large portion of the earth's crust floats in a like manner on a sea of molten rock. Moreover,—when we think of the increasing temperature of the strata, as we penetrate downwards in all parts of the world, and of the certainty that every portion of the surface rests on rocks which have once been liquefied;—when we consider the multitude of points from which fluid rock is anually emitted, and the still greater number of points from which it has been emitted during the few last geological periods inclusive, which, as far as regards the cooling of the rock in the lowest abysses, may probably be considered as one, from the extreme slowness with which heat can escape from such depths;—when we reflect how many and wide areas in all parts of the world are certainly known, some to have been rising and others sinking during the recent aera, even to the present day, and do not forget the intimate connexion which has been shown to exist between these movements and the propulsion of liquefied rock to the surface in the volcano;—we are urged to include the entire globe in the foregoing hypothesis.

To the belief in these large seas of molten rock, not to speak of an entire concentric layer so constituted, it has been objected, that if its fluidity be tolerably perfect, (which there is good reason to think is the case from what we see of the junction of the plutonic with the metamorphic formations,) the lava ought to stand (supposing a comparative examination possible) at nearly equal heights, within neighbouring volcanic orifices. To this I may answer, if it be permitted me to assume that the subsiding as well as the rising areas rest on a fluid surface, that whatever the power is which causes one to rise and another to sink, acts with unequal force (greatly modified, also, by unequal resistance) in different parts of even a very limited area. The main strength of the earthquake of February 20th, 1835, passed over Valdivia, but affected the districts north and south of it; and it appears that this town, until November 1837, had been less injured by the innumerable shocks which devastated Chile than any other; yet the subterranean abysses directly beneath it are in connexion (as shown by the action of Villarica in 1822) with the district to the North, which has been so often convulsed; and in November 1837, at the same time that an island far southward was upraised eight feet, it was shaken by an earthquake so violent that it escaped utter ruin only from the houses being built of wood. The comparative freedom from disturbance of Valdivia on the 20th of February, cannot be attributed to the action of Villarica, for we have seen that this volcano was quiet; nor indeed is there any reason why such an effect should be attributed to its action, since the eruptions of Osorno

and Minchinmadom did not save the northern parts of Chiloe, though they occupy the same relative situation with regard to them, which Valdivia does to Villarica. Shall we then say, that Valdivia escaped so long the subterranean disturbances, some of which affected simultaneously regions north and south of it, solely on account of the greater strength of the crust in that part? This appears to me a cause quite inadequate; and the direct supposition is better, that as within the same period one part of the continent has been elevated more than another, so the lava has been propelled by the action of this force more powerfully towards some, than towards others, of the volcanic orifices which penetrate it.

The secular shrinking of the earth's crust has been considered by many geologists a sufficient cause to account for the primary motive power of these subterranean disturbances; but how it can explain the slow *elevation*, not only of linear spaces, but of great continents, I cannot understand. With the same view, some highly important speculations have recently been advanced,—such as changes of pressure on the internal fluid mass, from the deposition of fresh sedimentary beds, and even the attraction of the planetary bodies on a sphere not solid throughout; but we can see that there must be many agents, modifying all such primary powers; and the furthest generalization, which the consideration of the volcanic phenomena described in this paper appears to lead to, is, that the configuration of the fluid surface of the earth's nucleus is subject to some change,—its cause completely unknown,—its action slow, intermittent, but irresistible.

1. [Read 7 March 1838.] *Transactions of the Geological Society of London*, 2d ser., pt. 3, 5 (1840):601–31.†

2. "Sketch of the Surveying Voyage of His Majesty's ships Adventure and Beagle." Vol. vi. Part II. p. 311.

3. Alexander Caldcleugh, "An Account of the Great Earthquake Experienced in Chile on the 20th of February, 1835, with a Map," *Philosophical Transactions of the Royal Society of London* 126 (1836):21–26.†

4. Compendio de la Historia del Reyno de Chile, vol. i. p. 33.

5. Journal of Researches during the Voyage of the Beagle, p. 376.

6. This is mentioned by Dolomeu as a well-known fact during the Calabrian earthquake of 1783. Lyell's Principles of Geology (5th edition), vol. ii. p. 217.

7. Comptes Rendus, 1838. Séance Juin 11.

8. Voyages of the Adventure and Beagle, vol. ii. p. 418.

9. Comptes Rendus, October, 1838, p. 706.

10. Phil. Transact. for 1836. I likewise was informed by an intelligent person, that he had seen, from the plain near Talca, a volcano in the Cordillera in great activity on the night subsequent to the earthquake.

11. Journal of Science, Vol. xvii.

12. The Edinburgh Journal of Natural and Geographical Science, August 1830, p. 317.

13. Humboldt's Personal Narrative, Vol. iv. p. 21. English Translation.

14. Another instance of earthquakes, violently affecting distant regions and passing over the intermediate country, is mentioned in the "True relation of the Earthquake of Lima, 1746." It is there said (p. 192) that the shock was most violent at Lima and Callao, becoming gradually less along the coast, but that at Guancavelica excessive shocks were felt and noises heard. The editor believes, there is no other place called Guancavelica except the famous quicksilver mines of that name, situated 155 miles to the S.E. of Lima. MacClelland (*Report on the Coal Mines of India*, p. 43,) mentions some cases of intermediate places being little shaken during great earthquakes.

15. As examples of the first case, may be adduced the trembling of the ground on the coast of Chile along a space of more than one thousand miles; and during the Lisbon earthquake in 1755, countries about 3000 miles apart were affected (see Michell on Earthquakes: Phil. Trans. 1760.). With respect to the second case, Humboldt states, that during the eruption at St. Vincent's subterranean noises were heard on the banks of the Apure, a distance of two hundred and ten leagues. (Person. Narr. Vol. iv. p. 27.) During the eruption of Coseguina in 1835, it is said, that noises were heard at Jamaica, 660 miles distant.

16. As other instances of the same kind, I may mention the outburst in 1822, of the volcanos near Valdivia at the same moment that Valparaiso, nearly 400 miles distant, was levelled to the ground. Again, in 1746, when Lima was overthrown, three volcanos near Patas and one near Lucanas, the two places being 480 miles apart from each other, burst forth during the same night. (Ulloa's Voyage, Vol. ii. p. 84.) I allude to these cases more particularly, because that distinguished philosopher, M. Boussingault (*Bulletin de la Soc. Géolog.* Vol. vi. p. 54.), having been much struck with the fact, that the earthquakes which have been most destructive to human life have been unaccompanied by volcanic outbursts, has, I think, generalized the remark too far. The earthquake of Concepcion in 1835 undoubtedly was one of extreme violence, although, from happening in the day, and from *commencing gradually*, it caused but few deaths (probably in the whole province not more than 70); nevertheless we have seen, that it was accompanied by co-instantaneous eruptions from several and very distant points.

17. M. Parrot, however, (Mémoires de l'Acad. Imp. des Sciences de St. Pétersbourg, Tom. i. 1831. Science. Math. Phys. et Naturelles) altogether denies that the data are sufficient to form any judgment on this subject.

18. Proceedings Geol. Soc., Vol. ii. p. 446, Jan. 1837.

19. Professor Bischoff (Edinburgh New Philosophical Journal, Vol. xxvi. p. 59, 1838.) has even argued that "the immense masses of lava ejected from a single volcano, and the enormous extent in which volcanic actions are felt at the same time, scarcely leave room to doubt that every active volcano is in immediate communication with the whole melted matter in the interior." How incomparably stronger this argument is, if applied to the plutonic as well as volcanic rocks, composing the great masses of the Cordillera! but now that we know, that continental elevations are caused by the very same impulses with those which eject lava and scoriae through the mouths of volcanos, the argument from the bulk of matter observable in ejected or interjected masses of rock, may be passed over, since the matter added below, when a whole kingdom is permanently elevated, must far exceed that composing either a volcanic hill or the axis of a mountain-chain; and therefore we are so much the more strongly urged to look for its source in "the whole melted matter of the interior," and not in any local receptacle.

20. The arguments in favour of the theory, that steam, produced by the percolation of waters to the interior of the cooling planet, is the motive power in volcanic action, has been lately strongly put by Prof. Bischoff in his paper in the Edinburgh Journal (Vol. xxvi. p. 25.). That it must be a modifying cause of great importance seems highly probable; but that it is the primary one of continental elevation, I cannot admit. The phenomenon, as it appears to me, is on far too grand a scale to harmonize with such an explanation. Can the *rising* of the whole west coast of South America, and of the whole width, at least of the southern portion of it, be explained by the lateral force exerted during the general shrinking of the earth's crust, modified only by the formation of steam under high pressure, in those parts where water has percolated to the heated interior? Such an explanation surely is inadmissible.

21. Personal Narrative, Vol. iv. p. 36. I have altered some of the dates in these tables, as they did not agree with the text or with the well-known period of the events.

22. Humboldt's Personal Narrative, vol. ii. p. 226., and vol. iv. p. 36.

23. Caldcleugh on the volcanic eruption of Coseguina. Philosophical Transactions, 1836, p. 27.

24.

Mexico	*Peru*	*Difference of time*
(Lat. 13° 32′ North)	(Lat. 12° 2′ South)	
30th of November, 1577	17th of June, 1578	Six and a half months subsequent
4th of March, 1679	17th of June, 1678	Eight months in advance
12th of February, 1689	10th of October, 1688	Four months in advance
27th of September, 1717	8th of February, 1716	Seven and a half months in advance

Humboldt's Personal Narrative, Vol. ii. p. 227. These facts perhaps tend to show that periods of increased volcanic energy are common to remote parts of the continent; but as the order of priority is not constant, I cannot believe any other law is indicated.

25. Journal of Science, Vol. xvii.

26. Several distinct cases are known in which springs and wells have been affected, their water rendered turbid, and altered in quantity, previously to bad earthquakes. This was observed at Lisbon in 1755; and in New England during two or three days before a shock, "the waters of some wells were rendered muddy and stank intolerably." (Michell, Philosophical Transactions, 1760, p. 44.) Humboldt and others have noticed, that the wells in the neighbourhood of Vesuvius are affected previously to its bad eruptions. These facts appear explicable, on the idea of a slight stretching or movement taking place in the crust, before its tension is overcome, a fissure formed, and, as a consequence, an earthquake or eruption caused. Courrejolles, also, has remarked in his memoir on earthquakes (Journal de Phys., Tom. lxiv. p. 106.), that great earthquakes are almost always preceded by lesser ones.

27. Ulloa's Voyage, English Translation, Vol. ii. p. 84.

28. Personal Narrative, Vol. iv. p. 29.

29. Ulloa's Voyage, Vol. ii. p. 85.

30. Michell, in his remarkable paper on Earthquakes in the Philosophical Transactions for 1760, (p. 580,) has quoted this same passage in confirmation of

his view, that "the eruptions of volcanos which happen at the same time with earthquakes may, with more probability, be ascribed to those earthquakes, than the earthquakes to the eruptions, whenever at least the earthquakes are of considerable extent." The term earthquake is here used to express the cause of the trembling of the ground. Sir James Hall, in his celebrated memoir on "Heat modified by compression," (Edin. Phil. Trans., Vol. vi. p. 166,) distinctly states "that the earthquakes which desolate countries not externally volcanic, indicate the protrusion from below of matter in liquid fusion, penetrating the mass of rocks;" but he does not extend this view, which is the same which I hold, to any comprehensive generalization, or restrict it to any particular class of earthquakes.

31. Courrejolles, in his Memoir on Earthquakes, (Journal de Physique, Tom. liv. p. 106,) says, "Les grands tremblemens de terre sont presque toujours précédés et suivis quelque temps avant et après par de petites secousses." Michell (Philosophical Transactions, 1760, p. 10) has given some instances of successive minor shocks, which appeared to travel from the same point, whence the previous more violent ones had come.

32. See Miers's Travels in Chile, Vol. i. p. 392.

33. Proceedings of Geological Society, p. 540. April 5th, 1837.

34. Personal Narrative, Vol. ii., p. 224.

35. At Acapulco, Humboldt says, the shocks come from three different quarters, the west, north-west, and south. (Polit. Essay on the Kingdom of New Spain; English Translation, Vol. iv. p. 58.)

36. Almost every author, from the time of Molina, makes this observation. See Molina's *Compendio de la Hist. del Reyno de Chile*, Vol. i. p. 32.

37. My belief is grounded on the fact that, on the same coasts, and within the same period, in which a vast number of earthquakes are recorded, there exist proofs of an elevation of the land; although the rise is not known to have been connected with any particular earthquake.

38. Geographical Journal, Vol. vi. p. 327.

39. Lardner's Encyclopaedia, Geol., Vol. ii. p. 209.

40. Lardner's Encyclopaedia, Geol., Vol. ii. p. 209.

41. Transactions of the Cambridge Philosophical Society, Vol. vi. Part I.

42. At Concepcion the line of vibration appears to have been N.W. and S.E., coming from S.W. At Mocha, (an island between Concepcion and Valdivia), from the manner in which water oscillated in the bottom of a boat drawn up on shore, the vibration must have been N. and S. coming from either E. or W. For the facts alluded to, see Capt. FitzRoy's account of the Voyages of the Adventure and Beagle, volume ii. p. 414.

43. Compendio de la Historia del Reyno de Chile, Vol. i. p. 36.

44. Principles of Geology, 5th edit. Vol. ii. Book ii. Chap. xiv.

45. Bulletin de la Soc. Geol., Vol. vi. p. 54.

46. Personal Narrative, Vol. ii. p. 226, and Vol. iv. p. 6, English Translation.

47. Abstract of a Memoir on Physical Geology, by W. Hopkins, Esq., M.A., p. 31.

48. Igneous rock composed of distinct crystals embedded in a fine-grained base.†

49. Elements of Geology. In the 24th chapter, Mr. Lyell has collected instances of volcanic eruptions in each of the great epochs of the geological history of Europe. The argument, which follows in the text, is the same with that advanced by this author in the Principles of Geology, (Book I. Chap. v.) but Mr.

Lyell more particularly applies it to the earthquakes and convulsions, "caused by subterranean movements, which seem to be merely another portion of the volcanic phenomena."

50. Any igneous rock that contains much feldspar and little quartz, and is visibly crystalline.†

51. According to M. Boussingault (Bulletin de la Soc. Geol., Tom. vi. p. 55), this is not the case in the Cordillera of the Equatorial regions. He states that trachyte there forms the base of the mountains, and that it has been protruded in a consolidated form. But can the *deep-seated* axis of a gigantic mountain-chain be composed of trachyte,—a rock essentially *volcanic?* If we could penetrate to greater depths, it cannot be doubted we should find the trachyte graduating into some plutonic rock; and one may be allowed to suspect that its junction with the superincumbent strata would present very different appearances from that of the trachyte;—the trachyte, indeed, we may well imagine to be the crust of such plutonic rocks cooled under little pressure, and forced upwards on the surface of the molten mass, in a solid form.

52. Cambridge Philosophical Transactions, Vol. vi. pp. 43–45.

53. Vein of intrusive igneous rock embedded vertically, or at an angle to intruded rock.†

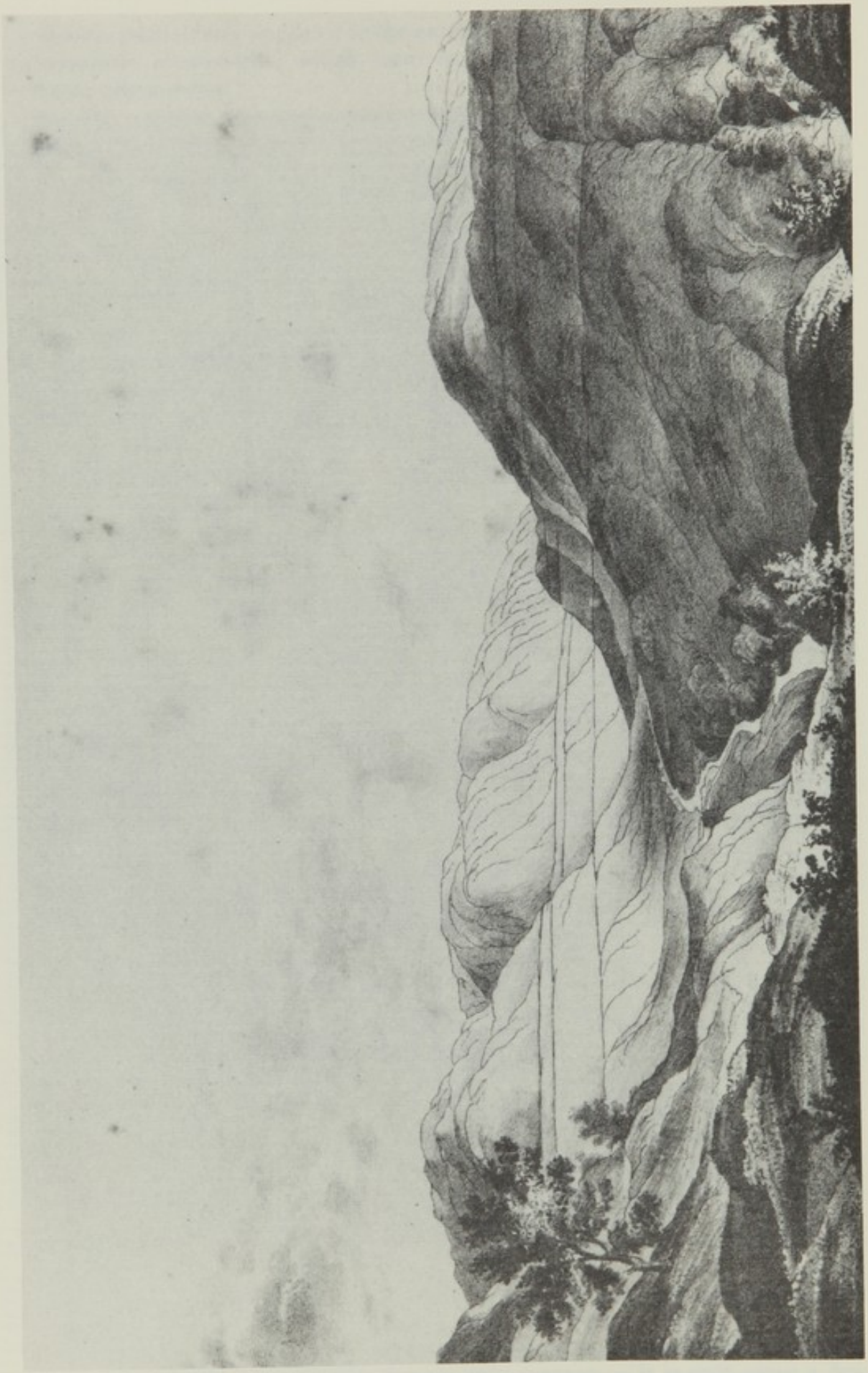
54. Humboldt has insisted on the fact, that in double chains of mountains, such as form large portions of the Andes, the lofty parts of one line correspond with the lower parts of the other. Such symmetry of structure is hardly conceivable on the idea of mountains having been formed by paroxysmal violence; but if we consider the whole range as the effect of a widely-extended elevation, prolonged during many ages, it is easy to understand, that if one line be weak, and consequently be subjected, for a long time, to disturbance from the subterranean force, it is probable that during so much the less time will the parallel and approximate one be affected.

55. In the Cordillera, the axis of plutonic rock is less exposed in the principal, than in the subordinate lines; some strongly marked exceptions, however, occur. In the former, also, the strata are most inclined. As, according to the views here advocated, the formation of a mountain-chain is due to innumerable impulses, the highest part must generally have felt the greatest number of impulses, and therefore its stratification would generally be most disturbed. And if a great part of the disturbance be due to the lateral force resulting from the compression of the great *thick* portions of the earth's crust, then the central lines, or those which have several ridges on both sides of them, would be most crushed together, and consequently the strata would be most closely packed over them. I can understand on no other principle, the circumstance of the rock of the axis being visible not on the loftiest, but on the secondary lines of a mountain-range, which very frequently occurs.

56. Mr. Hopkins moreover argues, (Abstract of a Memoir on Physical Geology, p. 15,) that if the elevatory force had the character of an *impulsive* action, it "would produce the most irregular phenomena, and such as would be altogether without the sphere of calculation. I exclude, therefore, the hypothesis of this kind of action, not as involving in itself any manifest improbability, but as inconsistent with the existence of distinct approximations to general laws in the resulting phenomena." In other parts the author shows that such approximations do exist in nature.—See also Phil. Mag. 1836, Vol. viii. p. 234.

57. In an extract from a letter, dated Concepcion, May 6th, that is seventy-six days after the great earthquake, there is this passage:—"It is only since a few days, that a day has passed without a shock, and even yesterday we had one."

Glen Spean
Loch Lochy
Glen Spean
Glen Spean
Glen Spean
Loch Lochy
Glen Roy
Glen Roy
Glen Ro



Pl. 2. View of the parallel roads of Glen Roy.

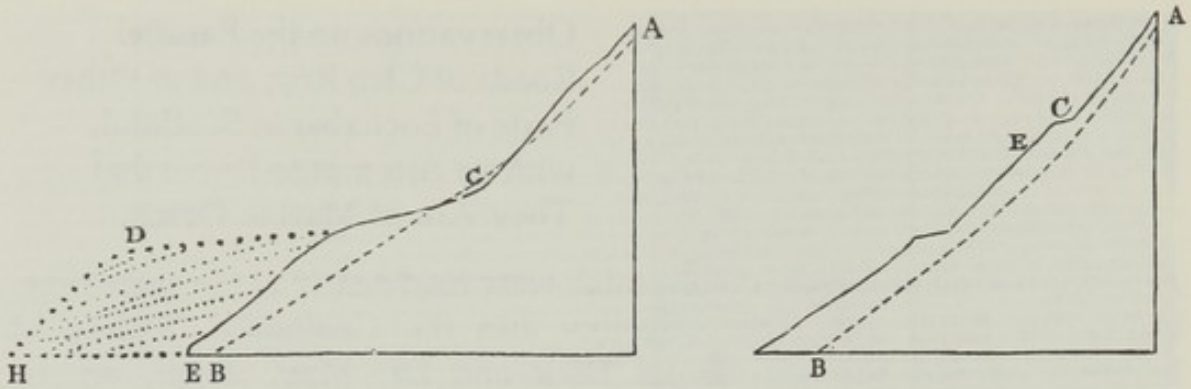
Observations on the Parallel Roads of Glen Roy, and of Other Parts of Lochaber in Scotland, with an Attempt to Prove that They Are of Marine Origin[1]

After the two elaborate memoirs which were read nearly at the same time, before the Edinburgh Royal Society and the Geological Society of London, by Sir Thomas Lauder Dick and Dr. MacCulloch, on the parallel roads of Glen Roy[2] and the neighbouring valleys, any detailed account of the physical structure of that remarkable district would be superfluous. But from the excellence of these papers and the high authority of their authors, it is necessary carefully to consider the theories they have advanced,—a necessity I feel the more strongly, from having been convinced during the few first days of my examination of the district, that their conclusions were impregnable. Moreover the results to which I have arrived, if proved, are of so much greater geological importance than the mere explaining the origin of the *roads*, that I must beg to be permitted to enter into the subject in detail.

Section I.—Description of the Shelves

The parallel roads, shelves, or lines, as they have been indifferently called, are most plainly developed in Glen Roy. They extend in lines, absolutely horizontal, along the steep grassy sides of the mountains, which are covered with a mantle, unusually thick, of slightly argillaceous alluvium. They consist of narrow terraces, which, however, are never quite flat like artificial ones, but gently slope towards the valley, with an average breadth of about sixty feet. There are only four shelves which are *plainly* marked for any considerable length; the lowest one according to MacCulloch is 972 feet above the sea; the next above it is 212 feet higher, and the third, eighty-two above the second, or 1266[3] above the sea; the fourth occurs only in Glen Gluoy; it is twelve feet higher than the third. I shall refer to them either by their absolute altitude, or as being the upper or lower one in the part under description, and not as first, second, or third; for it will be hereafter seen that others occur in every respect similar, only less plainly developed.

It is admitted by every one, that no other cause, except water acting for some period on the steep side of the mountains, could have traced these lines over an extensive district. The dark line in the accompanying wood-cut (No. 1.) represents the real profile of a shelf, and is copied from MacCulloch. To this I have added two imaginary lines, of which the broken one gives the supposed original form of the underlying rock. The formation of the shelf, as may be here seen, is chiefly due to the



Figs. 1 and 2. Original and present shelf profiles. AB, supposed original surface of rock; CE, line of shelf; CDH, line of shelf when expanded into a buttress or terrace.

accumulation of matter in the form of a mound, only very slightly projecting beyond the general slope of the mountain, and partly to the removal or corrosion of the solid rock. The latter effect, although well marked in some particular spots, cannot generally be distinguished, and the shelves no doubt are chiefly due to the accumulation, and not to the removal of matter. In this same diagram (1.) the covering of alluvium is represented as thicker some way below the shelf, than at the same distance above it. I believe this is generally the case, and hence the projection of the shelf is often very obscure; and when two or three occur, one below the other, their outline closely approaches to that represented in wood-cut (2.). MacCulloch will scarcely even allow that a shelf in any case forms a projecting mound; but this certainly is incorrect, and is indeed contradicted by his own statements, and by that implied in the comparison of the shelves with the beaches of lakes, which have been suddenly drained. The shelves entirely disappear, where crossing any part of the mountains in which the bare rock is exposed; for loose matter cannot accumulate there, and the rocks themselves from their laminated structure do not readily become worn into any regular form. They likewise disappear where crossing any part which is gently inclined; for their own slope then coincides with that of the alluvial covering, and cannot be distinguished from it.

The *dotted* line in the wood-cut (No. 1.) is supposed to represent the broader terraces, or even plains, of stratified shingle, sand, and mud, with which the shelves often become united. These terraces do not differ from the shelves in any one essential point of structure, but are much broader; and as the matter of which they are composed is in much larger quantity, a rude kind of stratification may be generally observed. They occur only where the bottom of the valley in its gradual ascent rises nearly to the level of a shelf, or at points on the hill-sides, where it is probable that the streamlets formerly brought down much detritus to the ancient beaches. Sir Lauder Dick has observed[4] that the shelf infallibly

intersects the head of such terraces or buttresses: this certainly is the case (as in diagram 1.) with all the smaller ones; and, therefore, we may infer that their formation dates from the period when the shelf was a beach. But at the head of the greater valleys, where the supply of matter must have been more abundant, and where the slope of the land was highly favourable to its accumulation, the line of shelf sweeps across and blends into a plain, which has an uniform slope upwards and downwards above and below that level. Therefore, when the water stood at any one of the shelves, there were many little deltas which did not rise above its level, but some greater ones that were continuous with an upward slope of shingle, filling the bottoms of the main valleys.[5]

The shelves are chiefly composed of the same kind of alluvium with that covering the whole surface of the mountain; and they seem to have been formed, as suggested by MacCulloch, by the check given to the downward descent of ordinary detritus, and that transported by torrents, at the level of the ancient waters; I could perceive no difference in the nature of the alluvium above and below the upper shelf, as stated to be the case by Sir Lauder.[6] It contains fewer well-rounded pebbles at the greater heights than would have been expected on any theory of the origin of the shelves; but they are abundant in the lower and broader parts of the valleys. Nevertheless where there is any level spot at the height of the upper shelves, well-rounded pebbles may generally be found, as on the summit of a rounded hill, or a flat little strait separating some hillock from a line of shelf (for instance near Craigdhu, on the summit of Meal Roy, and between Upper and Lower Glen Roy). In these cases the pebbles must have been almost exclusively formed by the action of the currents and waves of the former expanse of water. They are frequently derived from rocks not found in the immediate vicinity: erratic boulders also are scattered over these mountains. I state these facts distinctly, because MacCulloch says[7] that the composition of the alluvium of the upper shelves is *wholly* different from that covering the sides of the broad valleys; whereas the difference is only one of degree, for which many causes might be assigned.

I have already observed, that the quantity of solid rock worn away on the line of any shelf is not usually great. At the narrow entrance, however, of Loch Treig (of which a drawing is given by Sir Lauder Dick), on the west side, which is very steep, the gneiss[8] is worn into smooth concave hollows, the peculiar curves of which, though they cannot be described, may be readily imagined by calling to mind the form of rocks washed by a water-fall. This was the only one spot where I could observe this appearance in an unequivocal manner; but this one point of rock would to my mind carry demonstration with it, even if there were not innumerable other proofs, that the water had remained at the level of the

972 feet shelf for a very long period.[9] On the opposite side of the entrance, or gorge, which here slightly bends before entering Loch Treig, the shelf expands into a line of terrace. Standing on the precipitous and waterworn rocks, it required little imagination to go back to former ages, and to behold the water eddying and splashing against the steep rocks on one side of the channel, whilst on the other it was flowing quietly over a shelving spit of sand and gravel. The only other and rather different case of waterworn rock, which I noticed, was at the head of Lower Glen Roy (pointed out by Sir Lauder Dick), where the summits of some irregular hummocks of gneiss on a level with the upper shelf were obliquely truncated by a smooth surface. I have frequently observed a similar structure on the rocky shores of protected harbours. Large fragments of rock are scattered on most of the shelves, of which many are of granite, and have come from a distance, as will presently be described; the greater number, however, have merely rolled down from the heights above. Of the latter, some have fallen recently, whilst others are waterworn, as if they had lain for centuries on a sea coast; and it was in many cases easy to point out, whilst walking along the level shelf, which fragments had been washed by the ancient waves, and which had fallen since.

Sir Lauder Dick has observed, and the fact is very important, that the head of Glen Gluoy is separated from the head of a branch of Glen Roy by a flat land-strait, with which the shelf in the former glen is exactly on a level; so that if Glen Gluoy were filled with water to the *full* level of its shelf, or a few inches above it, besides a great barrier at the lower end, a little mound, perhaps a foot or two in height, would be required to prevent the water flowing into Glen Roy. In the same manner if Glen Roy were closed at its lower end, and if water stood at the level of the upper shelf, it would trickle into the valley of the Spey. The same thing would happen with the lower shelf at the head of the valley of the Spean; and lastly, a short shelf, which I discovered in a gully, which enters the Caledonian Canal near Kilfinnin,[10] between Loch Oich and Loch Lochy, is in a similar manner on a line with a peat moss, forming the watershed between it and another small valley. These four cases are so remarkable, that the coincidence of level must be intimately connected with the origin of the shelves; although such relation is not absolutely necessary, in as much as the middle shelf of Glen Roy is not on a level with any watershed. Sir Lauder endeavours to explain this fact by supposing that when the imaginary barriers of his separate lakes were perfect, the water flowed from that end of the glen, which is now highest, in other words, that the drainage of the supposed lakes was in each case in a reverse direction to that of the streams now occupying their beds. This view implies, moreover, the strange accident, that, during the breaking down

of the barriers, the part that was originally lowest always remained standing, whilst a higher part gave way; and thus the removal of the barrier must be supposed to have happened from the effects of some causes no ways analogous to the wearing down of the mouths of lakes as they ordinarily exist.

The structure of these land-straits must be now described. This has already been minutely done by Sir Lauder with respect to that one which connects the sources of Glen Gluoy with those of Glen Turet, one of the arms of Glen Roy. The only additional observation which I have to make, is that the strait is broad and very level, and that on one side I noticed a beach, like that on a sea shore, of well-rounded pebbles. The accounts given by MacCulloch and Sir Lauder of the division of the waters of Glen Roy and the Spey differ in some essential points. The latter author states that the upper shelf of Glen Roy is on a level (excluding the peat-moss) with the flat where the waters divide. This appears to be accurately the case, as far as the mountain barometer (which stood at the same thousandth of an inch on the two stations) and my eye could be trusted. But on the north side of the watershed there are patches of little terraces about fifteen feet above this level, resembling those which in other parts are connected with shelves, and hence probably having a similar origin with them. On the hill-side higher up, other obscure patches of alluvium occur with somewhat analogous forms. The water of the Spey first flows down a gentle mossy slope eastward, and is then collected in Loch Spey. On the south side of this loch, there is an obscure line of terrace, which appears to be about sixty feet above the loch, and which doubtless led MacCulloch to suppose the upper shelf of Glen Roy was that number of feet above the division of water. The terrace above Loch Spey, as far as I could judge by the eye without a levelling instrument, is horizontal, and may perhaps be traced along the south side of the watershed, even a short distance within Upper Glen Roy, where certainly there occurs a mound parallel to and above the upper shelf. I much regret I was unavoidably prevented from examining this locality with all the attention it deserved. But from the structure of the small terraces, it appeared to me certain that water must for a period have occupied a level above that of the highest shelf of Glen Roy; and likewise that fragments of a shelf, or line of terrace, which as far as the eye could judge was horizontal, extended within the basin of the Spey, and therefore beyond the limits of the supposed lake of Roy. This latter fact is, at least, certain, for I have since learned, through the kindness of Sir David Brewster, that he has seen, as will be hereafter mentioned, shelves resembling those of Glen Roy at two points, at a distance of several miles down the valley of the Spey. The watershed at the head of the valley of Kilfinnin, has precisely the same character with

the foregoing cases: here also a flat-topped buttress projects on one side above the level of the shelf, and this seems to indicate, as in the former case, the presence of water at a level rather above that of the shelf itself.[11]

The division of the waters between most of the glens and ravines in this district, in situations where no shelves occur, does not take place on a sharp ridge, but on level, and often broad land-straits, similar to those just described. I may instance a long one (at an elevation of between 1400 and 1500 feet above the sea,) separating two branches of the water which flows by Habercalder into the Great Glen, and one branch of the Tarf Water. Again another one nearer Fort Augustus, separating the two lowest and nearest branches of the same two rivers; here also there were obscure buttresses on each side above the level of the watershed. An intelligent shepherd who accompanied me, remarked that this form of the land was common wherever the waters in this mountainous country divided; and I observed several instances of it. Finally, I may remark, without wishing to lay any great stress on the argument, that these *land-straits*, whether connected with the shelves, or not, are precisely what might be expected from *straits*, properly so called, between arms of the sea being laid dry.

The discovery of any shelf, beyond the limits where they had been hitherto observed, being evidently an important point with regard to the theory of their origin, I shall fully describe the following case. At the head of a small stream which joins the Caledonian Canal, near Kilfinnin, and which is divided from the waters of the Habercalder by a flat mossy watershed already alluded to, some fragments of a shelf occur on the northern side. This shelf resembles in every respect those in Glen Roy; it seemed, as I walked along it, perfectly level, as it likewise did, when I viewed it from either end, and when I crossed the valley. I then took several measurements at the most distant points with the mountain barometer, and the mercury stood within the same hundredth of an inch. On the northern side of the valley, the shelf, which commences on a level with the mossy plain dividing the waters, extends for about a quarter of a mile almost continuously; it is then lost from the number of fragments of rock which have fallen down the hill, but reappears at the distance of more than half a mile from its commencement under the form of two or three little buttresses. These I ascertained by the barometer to be on a perfect level with the commencement of the shelf, or the watershed, a circumstance which was also apparent by the eye alone. The line further on disappears from the rockiness of the sides of the valley. On the south and opposite side of the valley, a broad sloping terrace extends at a corresponding level for about three quarters of a mile, but is indistinct owing to the gentle slope of the mountain. Further on it seems modelled into more than one terrace: and these, though obscure, appear to a person standing on them perfectly horizontal. Although the terraces are

not plainly developed on this side, yet it is certain, that horizontal mounds, at nearly the level of the watershed, extend about two miles on the face of the mountain. With respect to the absolute elevation of this shelf, I made it about forty feet above the upper one of Glen Roy, and 1120 above Loch Lochy, or 1202 above the sea: but my barometrical observations have no pretensions to accuracy. After having observed this shelf from so many points of view, I am prepared positively to assert that it is in every respect as characteristic a shelf as any in Glen Roy; and although the fragments of it do not extend over more than, perhaps, half a mile in length, its origin must be as carefully attended to in any general theory of the formation of the shelves, as if its length had been twenty times as great. Its want of continuity and shortness possess, indeed, in themselves much interest, because we thus know that those causes which have marked with horizontal lines the sides of the mountains of Glen Roy in so wonderful a manner, have been in action here, though they have produced but little effect. Moreover, we see that if the surface had been originally rather more rocky, or had been less steeply inclined than at present, or had been subjected to a very little more alluvial action, all evidence would have been obliterated of the extension thus far of the action of these causes.

I have already alluded to the important fact communicated to me by Sir David Brewster, namely, that he has seen shelves in the valley of the Spey. At Phones, which is situated about a mile from the Truim, and about five above its confluence with the Spey, one broad and well-marked shelf occurs, along which a carriage can be driven. On the banks of the Spey, about twenty-five miles below its source, two shelves occur in an elevated angle between the Burns of Belleville and the river. They are small; the upper one, however, is very broad; and their elevation is about 800 feet above the sea. Sir David Brewster says, that the shelves in both places appear horizontal, and that they resemble those of Glen Roy, though possessing far less grandeur and symmetry. The fact of their occurrence at these distant points is, as we shall hereafter see, highly important.

Section II.—The Theories of Sir Lauder Dick and Dr. MacCulloch Considered

Sir Lauder believes that a separate lake existed in each valley, where we now see a shelf, and was separately drained. In Glen Roy, where three shelves occur, all plainly developed, (with the exception of Belleville, this is the only place where more than one has been observed,) the arguments in favour of a separate lake possess the greatest force. Without entering into any description of the physical features of Glen Roy, inspection of

the accompanying map, taken with some few alterations from that of Sir Lauder Dick, will show the course of the shelves; although they cannot of course be followed nearly so continuously in nature as here represented. The lower one (972 feet above the sea) is common to nearly the whole line of the Spean and Glen Roy. The two upper shelves are confined to Glen Roy, with the exception of those short portions extending into Glen Collarig. It will be seen that both these lines, if continued round the hill of Bohuntine (at the eastern entrance of Glen Roy), would insulate it, whilst the lower shelf only forms it into a peninsula. From this structure it will be evident, that in order to form Glen Roy into a lake at either of the two upper levels, it would be necessary to erect two barriers, one across Glen Collarig, and the other principal one across the mouth of the Roy.

The lines are here represented as if abruptly cut off, but this is not so; and the following remark holds good in other cases, namely, that where a shelf terminates without any visible change in the nature of the slope, such as being rocky, &c., its disappearance is so extremely gradual, that it can be traced, sometimes to a further and sometimes to a lesser distance, according to the point from which it is viewed. Of this fact the shelves on the south-east side of Glen Collarig offer an excellent example. In the map, the extremities of the lower of the two upper shelves are represented at the four places where they terminate, as extending beyond those of the upper one. I state this on the authority of Sir Lauder Dick with respect to those in Glen Roy, and it is conspicuously the case with that pair in Glen Collarig which I have described as disappearing in so insensible a manner. The lower line can there be traced, though faintly, to a point below the houses of the glen opposite a small tributary torrent, and therefore considerably beyond (or nearer the mouth) than the point where the 972 feet shelf crosses the bottom of the valley. Observing in Glen Collarig the gradual disappearance of either set of lines, and that there is not the smallest apparent cause for it in the nature of the ground, the first and obvious supposition is that a sheet of water extended from the Spean into Glen Roy and Collarig, and that the mere widening of the mouths of the latter, as they approached the less protected expanse of the Spean, gradually became unfavourable to the accumulation of detritus, and therefore to the formation of the shelves. This view is greatly strengthened by the extension of the lower line in each case beyond the upper; for of course the supposed unfavourable condition for their formation, that is, the too great breadth and exposure of the sheet of water of which they formed the beach, would affect the line when the water stood at the higher level to a greater distance from the main expanse, or further up the valley, than when it occupied a lower level. It may, however, be argued (and on the hypothesis of Glen Roy having existed as a lake it must be so argued), that as the higher line is the oldest,

so its terminal portion may soonest have yielded to those causes which modify the surface of the land. This view, however, receives little support from an examination of the rest of the glen, inasmuch as the two shelves through its whole course are in a state of equal preservation. We must therefore conclude, either that we now behold the shelves precisely as they were left by the sheet of water, or that if the two upper shelves did originally extend for an equal length on each side of the two glens, that the causes which tend in a small degree (for the existence of the shelves proves that no great changes have taken place) to smooth the surface, have acted over this district with the most *perfect uniformity*. Moreover, it may be remarked, that wherever a streamlet crosses a shelf, and it is probable from its size that it formerly delivered detritus to the ancient expanse of water, either a greater breadth of shelf or a small buttress there, attests that it was so; and in doing this, likewise attests how perfectly the surface of the land has been preserved. Now I paid particular attention to the following observation, namely, that on both sides of the hill of Bohuntine, and on the opposed mountains, where the shelves terminate, there was not the smallest change in the composition or in the outline of the smooth rounded surfaces. Yet it is in this very spot, where the lines insensibly disappear,—on these very hills, where the little deltas of the ancient streamlets are still preserved,—within this very district, where in the extension of the lower shelf beyond the upper one in the four cases, we have the most satisfactory proof of the action of absolutely uniform causes, either in their formation or in their obliteration; it is here, where the slope of the turf-covered hills is unbroken, where there is not a remnant of any projecting mass, that we are compelled by the theory to believe that the two enormous barriers stood, which formed Glen Roy into the imaginary Loch Roy.

But as it is highly important to show that such a Loch could not have existed, we must for a time, in the face of these great difficulties, suppose the two barriers to have been erected. It may be first remarked, that from the extension of the middle shelf, the barrier in Glen Collarig could not have occupied the only one place, which the structure of the ground indicates, even in the smallest degree, as probable, namely, at the Gap, where the waters divide; but it is necessary to suppose that it crossed the glen at a point some way distant from the Gap, and where the valley has a depth, below the upper shelf, of more than 300 feet. Glen Roy being now converted into a lake, with its drainage reversed, that is, with the water flowing from it by the Spey to the east coast of Scotland, let one of the two barriers, we will say the smaller one in Glen Collarig, give way from the effects of an earthquake or other cause. The lake will now stand at the level of the middle shelf, the barrier having given way eighty-two feet vertically. Again let it burst, and this time rather more than 212 feet vertical must be swept away, so that the larger lake, supposed by Sir

Lauder's hypothesis to occupy the valley of the Spean at the level of the 972 feet shelf, might send an arm a little way up the glen (as shown by the shelf now existing there) above the point where the barrier stood. Let all this have taken place, but still a barrier nearly a mile long, and 800 feet in height, is left standing across the mouth of the Roy. Must we suppose that each time the barrier in Glen Collarig failed, the one in Glen Roy gave way *the same number of feet* through some strange coincidence? or are we to conclude that some awful catastrophe at subsequent times, unconnected with the drainage of the lake, which must have passed through the breach already opened, removed the second barrier (either part or all of it) when *above water*, without having left the smallest remnant of it, or having disturbed the smooth alluvial covering of the steep slopes? The 972-feet shelf is common to the valley of the Spean and Glen Roy, and is supposed to have been formed by a lake, the barrier of which, some miles in length, extended near Highbridge across the mouth of the Spean. This shelf passes uninterruptedly, and with its usual breadth, on both sides of Glen Roy and of Glen Collarig, in the very part where the barriers of Loch Roy, if they existed, *must* have crossed the valley; therefore the whole, or part of the great base of those enormous barriers, must have been swept away when submerged within the bosom of the imaginary Loch Spean; and this must have been so perfectly effected, that no trace of them is left on the smooth slope of the hill, not even by a greater breadth of the shelf, any more than in the part of the second barrier, which must have been removed when above water.[12] And all this is supposed to have taken place on the hills, where I have shown how wonderfully the features of the land have been preserved, and where the boulders which were washed by the waves of the ancient water can be distinguished from those which have fallen since. In conclusion, therefore, I do not hesitate to affirm, that more convincing proofs of the non-existence of the imaginary Loch Roy could scarcely have been invented, with full play given to the imagination, than those which are marked in legible characters on the face of these hills.[13]

The same reasons which render the existence of a separate lake in Glen Roy so excessively improbable, apply with only little less force to each of the imaginary lakes in the other glens. We are, therefore, in giving up *Loch* Roy, involuntarily driven to the theory advanced by MacCulloch, namely, that all the valleys in which shelves occur were included in one large lake; but we shall thus run headlong even into greater difficulties. First, from the structure of the mountains, four immense barriers are required to form the lake,[14] namely, one low down across the valley of the Spey, two at distant points across the Great Glen of Scotland, and a fourth across the mouth of Loch Eil, the last being necessary, as MacCulloch shows,[15] from the structure of the Great Glen in that part. It

may be safely asserted that more improbable situations could hardly be imagined in the whole of Scotland. It is perhaps useless to ask, were the barriers composed of rock or alluvium? if of the former, they were transverse to every line of hill in this part of the country; if of alluvium, we must assume an unexampled case; for where in the whole world shall we find even one barrier a mile and upward in length, and 1200 feet high, composed of loose waterworn materials? Secondly, the theory of one large lake does not explain in a satisfactory manner the remarkable coincidence between the shelves and the watersheds. Thirdly, when by the bursting of any one of the barriers, the level of the lake had fallen from one shelf to another, the hypothesis requires (as with *Loch* Roy) that the three other barriers, now high and dry, and distant many leagues from each other, should have been swept away by some unknown power, acting by some unknown and scarcely conceivable means, from the smooth sides of the mountains, without a remnant of them having been left; so that MacCulloch even frankly confesses one part is almost as probable (I would say improbable) as another for the position of the barriers. And it should be borne in mind, that these extraordinary forces are supposed to have acted on the outskirts of that large area, throughout which we have proofs, most wonderful and unequivocal, of the entire preservation of the surface of the land, as it was left at a period long anterior to the removal (if such removal ever did take place) of the barriers of the lower lakes. I do not hesitate to assert that this one difficulty, even by itself, would be sufficient to refute the theory of one great lake: Sir Lauder's theory has been shown to be equally untenable. It is perhaps here almost superfluous to add, that the discovery of the shelf at Kilfinnin (and probably likewise of those in the valley of the Spey) increases every difficulty manifold; for the valley of Kilfinnin is almost as wide as it is long, which affects one theory, as the lowness of the opposite side of the Great Glen does equally the other. Finally, then, in giving up both, the conclusion is inevitable, that no hypothesis founded on the supposed existence of a sheet of water confined by *barriers*, that is, a lake, can be admitted as solving the problematical origin of the "parallel roads of Lochaber."

Section III.—Proofs of the Retreat of a Body of Water from the Central Parts of Scotland, and that this Water Was that of the Sea

Having now discussed these views which cannot be admitted,—a method of reasoning always most unsatisfactory, but necessary in this instance from the high authority of those who have advanced them,—I will consider some other appearances, which will perhaps throw light on the origin of the shelves. The valley of the Spean, from the point where it

joins the Great Glen of Scotland to where it receives the Roy, is broad, and its bottom moderately level. The solid rock is concealed in almost every part, excepting where the river has cut itself a gorge, by irregularly horizontal strata of gravel, sand, and mud. Large portions of these beds have been removed along the centre of the valley, yet it is quite evident from the fringe or line of terraces which skirt each side, that the bottom must originally have formed a smooth concave surface inclined towards the mouth of the valley. Portions more or less perfect of this same deposit can be followed up the course of the Roy, and up the higher parts of the Spean, where the valley is not too rocky or narrow, to near Loch Laggan. This loch is but little below the 972-feet shelf; and at present I wish, for the sake of the independence of the argument derived from the facts to be stated, to consider only that part of the country which is below the level of that shelf. These irregularly stratified beds, near the mouth of the Spean, attain a thickness of several hundred feet, and they consist of sand and pebbles, many of the latter being perfectly waterworn. Higher up the valley, near the bridge of Roy, the thickness before the central portions were removed appears to have been about sixty feet, but of course the thickness varies according to the original irregularities of the rocky bottom of the valley. Now it may be asked by what agency has this sloping sheet of waterworn materials been deposited along the course of the valley? From the presence of the horizontal shelves we know that there has been no change in the relative level or inclination of the country since this district was last covered with water, and therefore we may argue with safety, that the action of the rivers, as far as it is determined by their inclination, must have been the same since that period as it now is, with the exception of that amount of change which they may have effected in their own beds. Our knowledge that there has been here no axis of elevation, with one part always rising a foot, and another a few inches less; but that the entire system of drainage has remained undisturbed and subject only to its own laws of change, is a circumstance which gives a singular degree of interest to the examination of this district. Now if we look at any portion of these rivers, for instance the Roy above its junction with the Spean, we find it has cut a narrow steep-sided gorge through the solid rock, which is in many parts between twenty and thirty feet deep, whilst on each side there are remnants, as above stated, of a continuous bed of gravel, at least sixty feet in thickness. These beds have certainly been deposited by rapid currents of water, but not by any overwhelming debacle, as may be inferred from the presence of cross layers, and the alternate ones of fine and coarse matter. Seeing also the evident relation of dimension and materials which exists between these deposits and the valleys in which they occur, it can scarcely be doubted that the detritus of which they are composed was

transported by the existing rivers. But are we to suppose that the river, as in the case of the Roy, first deposited along its whole course these layers one over another, thus raising its bed sixty feet above the solid rock, and then suddenly commenced, without the smallest change in the inclination of the country, not only to remove the matter before deposited, but when having gained its former level, to act in a directly opposite manner, and to cut a deep channel in the living rock? Assuredly such a supposition will not be received; and whatever the river had in the accumulation of these waterworn materials, from the very moment (neglecting the annual oscillations of action from the changing seasons) it ceased to add and began to remove, its power must have undergone some most important modification.

It will perhaps be thought that the mere deepening of the bed of the stream, near the mouth of the valley (the effect being slowly propagated upwards), could have caused the difference between the present and the former action of the river. But it is not difficult to replace in imagination the solid rock in the course of the Spean; and although a few small lakes will be thus formed, the average slope will not differ greatly from the present inclination, and this inclination we see is sufficient to cause the river to wear a deep gorge in the solid rock, and therefore it is evident (although I am aware that without actual measurement of the inclination this argument must rest upon eyesight, which cannot generally be trusted) that a change of this nature would be wholly insufficient to reverse the action of the river, as has here been the case. We must not, of course, at the same time replace in imagination those unconsolidated deposits, the origin of which we are considering; otherwise no doubt the inclination of the bed of the river would be greatly altered; although even in that case I by no means believe that the river would be so much retarded as to deposit matter at the heights where it is now left. Some check, therefore, to the transporting power of the stream seems to have acted at many, or at every successive level. If we reflect on what would result, as an hypothesis, from a river delivering during a long period detritus into a lake, the level of which was gradually sinking from the wearing down of its mouth, a gently sloping surface would be formed at its head. But as the barrier was cut deeper and deeper, and the lake sank, the stream in the part where it was once checked by meeting with the still water would gain velocity, and hence would cut through the beds which it had originally deposited. The fringe, of rudely stratified alluvium, the origin of which we are considering, resembles both in structure and composition such beds of detritus as would have accumulated on the shores of a lake, had one existed in these valleys. If, then, we suppose that a subsiding sheet of water did actually fill this valley, either of one or more lakes, with their barrier gradually wearing down, or of an arm of

the sea, the general level of the ocean being stationary during a slow elevation of the land (as now is the case with the fiords of Scandinavia), every appearance on the sides of the valley of the Spean and Roy will be explained; and as there is no other way, that I can see, of accounting for them, the hypothesis is so far worthy of admission.

I ought, perhaps, to have previously observed, that these deposits could not have been formed when the valley was filled with water to the level of the shelves, for the detritus has the character of matter accumulated in shoal water, and the beds abut abruptly against the bases of the mountains, instead of blending with the alluvium on their surface, as would necessarily have happened had the whole been deposited at the same time at the bottom of one basin.

The conclusion, that these valleys have been occupied by a sheet of subsiding water, follows more plainly from a somewhat different class of facts. I have before remarked, that where a streamlet crosses a shelf, especially if it be the lower one, an obliquely truncated buttress, the form of which was represented by dotted lines in the wood-cut No. 1, projects from the side of the hill. It is quite evident that these were accumulated when the shelf existed as a beach, and the streamlet at present only acts in removing those portions with which it comes in contact. Now in some points where the buttresses have been somewhat largely developed, smaller ones at a lower level, composed of the same irregularly stratified waterworn materials, having nearly the same outline, although unconnected with any shelf, may be observed adhering to the slope of the hill. Instances of this structure occur on the east side of Glen Roy; on the south side of the Spean, and between Loch Treig and the bridge of Roy, the accumulation of perfectly rounded shingle, like that on a sea-beach, was enormous. The internal structure in this instance corresponded to the external form, as is shown in the accompanying diagram, where highly inclined beds of sand and coarse gravel are capped by other irregular ones of the same composition, only slightly inclined. In all these cases, where the flat-topped buttresses occur on steep slopes, it is certain (as might have been expected) that the streamlet is steadily at work in removing matter, and does not add one pebble to the mound. No one

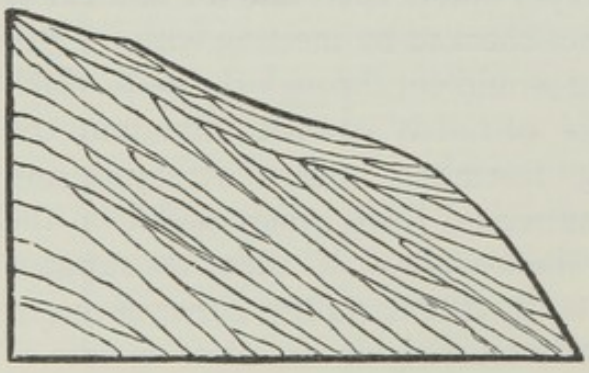


Fig. 3. Internal structure of small buttresses.

will dispute, that those buttresses, which are mere extensions of a line of shelf, were formed at the edge of an expanse of water (of which the shelf was the beach), and it is therefore by itself probable that the other buttresses, of similar external form and composition, though occurring at a different level, had a similar origin. But the argument may be put in a stronger point of view: taking the course of one of these streamlets, and observing the size and position relative to it of the buttresses one above the other, it becomes evident that the materials of which they are formed were accumulated through the agency of this stream, although it is at the same time inconceivable that they were left (especially in such a case as that represented in diagram 3.) on the steep slope by a power which, as it now acts, is steadily at work, tearing away matter in its whole downward course. Therefore, it is absolutely necessary to bring into play some intervening or modifying cause in the action of the streamlet; in the case of the buttresses which are connected with the shelves, no one can doubt what this intervening cause has been; shall we, then, rejecting a *vera causa*, seek some other one, if indeed such other can be found? Certainly not; and the conclusion is inevitable, namely, that a sheet of water must have stood at as many levels as there are buttresses, and this will include by short steps the whole space between the bottom of the valley and the lower shelf. Judging also from the amount of matter accumulated, we must infer that the water remained at these levels for no inconsiderable periods, although for a lesser time at each than at the level of the 972-feet shelf.

I would even further add, that in any valley (the relative level of the country, one part with another, having remained constant) a single buttress, if composed of such materials as could not have slided down the face of the hill in mass, or could not, judging by the presence of cross layers and alternations of fine and coarse beds, have been deposited by a debacle, indicates that the valley was once partly or entirely filled up to that height by such matter; and if the mass be too thick, or at too great an elevation on the sides of the valley, to allow the supposition that it was deposited by the streams now flowing in the valley, subject to such changes in its velocity as by the corrosion of its own bed it could effect, then the formation of such buttresses can be accounted for only by the supposed permanence of a sheet of water, whether of a temporary lake or of the arm of the sea, at their levels. Now such projecting masses are extremely common in the sides of most of the tributary streams of the valleys. I conclude, therefore, from the consideration both of the beds of stratified alluvium at the bottom of the main valleys, which there is the greatest difficulty in believing could have been deposited by the rivers under the existing conditions; and of the buttresses on the sides of the hills, which similarly could not have been formed by the present

streamlets, that it is satisfactorily proved that the valleys of the Spean and Roy have been occupied by a sheet of water which has slowly and very gradually retired, leaving in almost every part unequivocal evidence of the check which matter drifted by a current meets with, when it arrives at or near to the surface of still water.

I have as yet confined my argument to the valley of the Spean and its tributaries, and to that portion of it which is below the lower shelf; but I may here add, that it may be inferred from the same kind of evidence already used, (I allude more particularly to some buttresses above the 972-feet shelf to the north-east of the houses of Glen Turet, and to a shelf intermediate between the two upper ones, Tombhran,) that water long remained in Glen Roy at an altitude above that which we have as yet been considering, and at other levels besides those indicated by the three shelves themselves. If, also, we look to other valleys in this part of the country, we find similar appearances. For instance, on the flanks of the valley of the Tarf Water, which flows into Loch Ness (at the elevation of about 1000 feet, near the bridge, where the road to Garviemore crosses the river), there are large conical piles, with their summits truncated by a rude terrace, composed of well-rounded pebbles, sand, and an argillaceous earth in irregular beds. Some of the layers of sand and fine gravel were in curves, but slightly inclined; and this structure, together with their composition, made it at once evident that they must have been drifted into their present position by currents of water. Again, near Fort Augustus, the Great Glen, with the exception of the central part, where the river has worn for itself a broad course, is filled with irregular strata about thirty feet in thickness of sand, gravel, and coarse shingle. In the sand some of the layers are most regularly waved, as if by a tide ripple. These beds are about seventy feet above the sea; fringes of similar deposits skirt at intervals both sides of the Great Glen, but where they are present they do not occur, as far as I was enabled to observe, at a greater height than about 100 feet, that is, than the water-shed of this great valley,—a fact somewhat analogous to the coincidence in level between the true shelves or roads and the heads of the valleys in which they occur. At the south-west end of the Great Glen, nearly opposite to Loch Leven, there are some extensive flats, which from a distance appear to be similarly composed, and which in one part have been modeled into two nearly regular terraces, one rising above the other. A somewhat similar structure may be observed in a part between Loch Eil and Loch Lochy; and this structure can only be explained by water having successively occupied for long periods different levels.

Referring now to more distant points, we find in the broad valley below Loch Tulla (a tributary of Loch Awe, and the stream flowing thence enters Loch Etive,) there are some appearances, although obscure,

of the bottom of the valley having been once filled up with stratified alluvium. On the river Tay, however, near Loch Dochart, the phenomenon is clearly developed. On the south side there is a long mound or terrace, about 150 feet high, entirely composed of well-rounded pebbles mingled in layers with a yellow sandy clay. From this point to Tyndrum (at an elevation of between 400 and 500 feet above the sea) there are similar banks of waterworn materials, and in more than one part I observed a fine white sand, like that on the seashore. On each side of the valley where it divides, near Tyndrum, a broad expanse is scattered over with low ridges and flat-topped hills of equal height, from which it would appear that the whole space had once been covered up with these deposits. Towards the mouth of the Tay the terraces and platforms of Strathmore have been remarked by many observers, on the sides of the small neighbouring valley of the Dighty. Mr. Blackadder, in a letter to Mr. Lyell, says, "A narrow track of gravel, sometimes in the shape of platforms, at others in small hillocks, very similar in appearance to those of Strathmore, extends to the height of about 600 feet; and some isolated patches on the southern face of the Sidlaw Hills occur at a greater elevation." From expressions used by MacCulloch and other writers, I am led to believe that beds of similar matter irregularly superimposed over each other, occur on the sides of almost all the valleys of Scotland. In such cases as in that of Loch Dochart, we have no proofs, as horizontal shelves or ancient beaches have not been preserved, that the relative level of the country has remained the same, since the period when it was first traversed by running streams; and therefore it is not absolutely certain that the present rivers, with a very different inclination, might not have deposited the rudely stratified beds in the lower part of their courses, and afterwards with an altered velocity have cut through them. But as we do know that no such change has affected a large neighbouring region, and as such movements could hardly thus have influenced the drainage of valleys directed towards different quarters, such doubts may be overruled. This being the case, the same argument as before used may be repeated, namely, that the waterworn materials appear to have been transported by the present rivers, and yet that they are so deposited as could not have happened without some intervening cause. The phenomenon demands an explanation; and the only obvious solution is that which from several and nearly independent considerations was proved to have been the case with the Spean, namely, that it had been occupied by an expanse of gradually subsiding water, either of a lake or of an arm of the sea. This conclusion, therefore, may be urged with only little less force regarding many, if not all, of the valleys in this part of Scotland.

It may be asked, of what nature was this sheet of water? If we suppose

a barrier erected across the mouth of each valley, and a lake to be thus formed, which sunk from the gradual deepening of its mouth, all the appearances above described would be explained. It is a startling assumption to close up the mouth of even one valley by an enormous imaginary barrier; to do this with all would be monstrous. Of such barriers in the district we are considering I need not say there does not exist any trace, nor need I repeat what I have already said against so vain a supposition as that they could have been swept away by any great debacle from the sides of those hills, of which the whole alluvial covering has been preserved since the period when the upper shelves formed beaches, without even a remnant of them being left; and I may add, that it will hereafter be shown by the clearest proofs, that the ordinary alluvial action, and likewise that of running water, even under the most favourable circumstances of a waterfall, has been far less efficient than could have been anticipated.

But it may be asked, would not the hypothesis of a succession of lakes explain the appearance, the matter accumulated above each delta sloping upwards from one level to another. I can only answer this with respect to those valleys which I have myself seen: in the Spean, Roy, Tarf Water, and some others, it is easy, as before stated, to replace in imagination the solid rock; and although some small lakes[16] would be thus formed by the replaced barriers (as probably would be the case in every valley), the fringe of stratified alluvium we are now speaking of skirts the valley at an elevation above them. To assume that these rocky barriers were formerly much higher, and were demolished by some means independent of the action of the river (for this action tends only to form a narrow wall-sided gorge, as may be seen in those barriers which certainly did exist), would be as gratuitous as the imaginary erection of one great barrier across the mouth of the valley, and would explain, from the continuity of the slope, the appearances far less perfectly. Moreover, if the origin of the sloping fringes could be explained by the assumed former existence of a chain of lakes, the buttresses high up on the sides of the valleys clearly could not be so. Nor will any one pretend that any lake-theory can be applicable to the deposits on the sides of the great valleys, such as Strathmore, and the Great Glen of Scotland, which terminate in deep and open friths. Therefore it has not been the water of several lakes any more than of one lake, which slowly retiring from these valleys, determined the accumulation of the beds, where we now see them. There is, then, as we have conclusive evidence that an expanse of slowly subsiding water did occupy these spaces, but one alternative, which we are compelled to admit, and this without any consideration of the *shelves* themselves, excepting so far as they serve as artificial levels to show that the country has not been unequally elevated, namely, that the waters of the sea, in

the form of narrow arms or lochs, such as those now deeply penetrating the western coast, once entered and gradually retired from these several valleys.

Section IV.—Proofs from Organic Remains of a Change of Level between the Land and the Sea in Scotland. The Effects of Elevation Traced in Hypothesis

Another question immediately arises; did the waters of the sea slowly subside, or the land slowly rise, the effect in each case being similar? But first it will be proper to show, from the more ordinary kind of evidence, that there has been some change of level between land and water affecting Scotland within recent times, although not to the amount inferred from the arguments above advanced. Mr. Smith of Jordanhill, in an excellent paper,[17] has lately shown from the presence of elevated organic remains, that within a period geologically extremely recent, both the east and west coast of Scotland has been raised some hundred feet; namely, at Banff and near Glasgow[18] about 350 feet. Considering the facts given in this paper, it can scarcely be doubted, without making the most improbable assumptions, that the Great Glen of Scotland, of which the highest point is only ninety-three feet above the sea, was within this recent period an open strait; and, I may add, it must then have strikingly resembled the Beagle Channel in Tierra del Fuego, an arm of the sea narrower, longer, and straighter, which intersects the extreme southern part of South America. In accordance with this fact, I was informed by the person who now has the charge of the locks on the canal, that when they were cutting through the gravel at the head of Loch Ness many broken sea shells were found in the *lower* part, which appeared to him like those on the sea-coast. When exposed to the atmosphere they soon decayed. This point must be between forty and fifty feet above the level of the sea. There are remnants, as before stated, in this part of the Great Glen, as well as at the south-west extremity, of coarse sublittoral formations, which, I suppose scarcely any one would dispute, were accumulated before that small change of level took place, which is indicated by the elevated marine remains. That the movement must have been exceedingly slow, may be inferred from the existence of so many beaches, each requiring time for its formation, which rise one above another on both coasts of Scotland. Mr. Malcolmson[19] mentions no less than eleven in Elgin, from the lower one of which he procured twelve species of existing marine Testacea. On the opposite coast also, Mr. Smith has described[20] several ancient beaches between the present one, and the great terrace, between thirty and forty feet high, which "forms a marked feature in the scenery of the west of Scotland." It is also

important to observe here, that the supposed greater movement deduced from the nature of the superficial deposits, is of precisely the same slow kind, and interrupted (as will presently be shown) by periods of rest, as this lesser movement, attested by the presence of sea shells and step-formed beaches. If, then, the Great Glen was for a long period occupied by an arm of the sea, which very slowly retired from it, deposits must have accumulated on its shores, and likewise for some little distance within the mouths of the valleys which entered it. If we suppose that the sea stood at the same level in the Great Glen as it lately did both on the east and west coast, then the salt water would have almost entered Glen Roy, and would have wholly covered that sloping fringe of gravel, which has been so often mentioned as skirting the course of the Spean. Whether this be granted or not, after what has been stated it can hardly be disputed, that within recent geological periods an arm of the sea entered at least the mouth of the Spean, and very slowly retreated from it. Remembering that the conclusion was forced on us by distinct lines of arguments, that a body of water must have slowly retired from these valleys, and that lakes sufficiently large to have produced the observed effects could not have existed in them, may we not, with the additional consideration that some parts of the deposits here *must* be of marine origin, deliberately affirm it proved, that it was the waters of the sea that, even at great heights, checked and banked up at successive levels, the detritus brought down by the ancient rivers and streamlets? I am aware that the argument would have had a greater *appearance* of strength had I commenced with the inference deduced from the presence of recent shells at considerable elevations on both coasts of this kingdom, but I preferred the method I have followed, because I believe it is equally legitimate, and of more general application, although at first not so obvious.

From these facts it is certain that there has been a change of level affecting within recent times the whole central part of Scotland, and of a kind very similar to that which has been the subject of so much attention in Sweden, where, according to Mr. Lyell, remains of existing marine animals have been raised to the height of between 500 and 600 feet above the sea. The change of level in the case of Sweden is as certainly known to be due to a slow movement of the land, and not of the water, as it is on the coast of Chile, where a small tract is violently upraised during an earthquake, the distant parts of the same coast being unmoved. It would, however, be quite superfluous here to enter into this question at length, as it has almost ceased to be debateable ground.[21] It may then be concluded that the supposed great change of level in Scotland, deduced from the foregoing arguments, as well as that smaller fraction of it attested by marine remains and ancient sea-beaches, is due to the rising of the land, and not to the sinking of the waters.

We will now endeavour to trace in hypothesis the effects which would be produced by an arm of the sea slowly retiring from inlets during an *equably progressive* elevation of the land. In a deserted sound or flat-bottomed valley, surrounded by mountains, curved lines crossing the river would mark the ancient beaches. Each of these lines would be higher than its neighbour on the sea-side, owing to the rising of the land in the interval of their formation, and would be more distant from the head of the valley, chiefly on account of the matter brought down by the river, and in some parts from the natural slope of the fundamental rock. When the upper line formed a beach, it is evident that the whole of the lower part of the valley must have been under water, and that the prolongation of the beach would stretch along the flanks of the adjoining mountains some way inland from the present shore. In like manner each successive and lower beach-line would wind along the steep sides of the hills, and cross the valley further and further from its head. It should be observed, that although I have spoken of successive beach-lines, yet as the land is supposed by the hypothesis to rise at a perfectly equal rate, every part of the valley will have successively formed, during an equal period, a beach; so that each part having been similarly exposed, the slope will be uniform; nor will it be possible to distinguish any one line of beach. Again, if we suppose matter to be removed from the valley by the action of the tides, instead of being added to it by the river, yet as an equal quantity (or a quantity insensibly varying from the varying degree of exposure, as the form of the land slowly changes during its rise) would be removed at each level, the slope in this case also would be uniform. In that part of each successive beach, which winds along the steep flanks of the mountains, it is not probable that much matter would be added, but the downward descent of some portion of the detritus, which is formed on all land by meteoric agency, would be checked; but as it would be equally checked at each successive level, the outline of the mountain would remain unbroken. These same lines, however, although protected in the more inland parts, might suffer degradation where exposed to the greater force of the waves near the mouth of the sound; but the parts differently affected would blend into each other, and so would it be with each successive beach-line; and the slope therefore, whether added to or corroded, or left untouched, would never show the traces of action on any one defined horizontal line. A little reflection will indeed show that when the water stood at the highest level, any part or point which happened to be most exposed would, from the natural slope of all mountains, be some way inland compared with the same relative point on the present coast; at all intermediate levels the waves would attack an intermediate part, either high up and more inland, or lower down and nearer the coast, so that the line (or rather zone) of greatest littoral action, joining the parts which were successively most affected, would, under

the conditions of the hypothesis, be inclined with the horizon either more or less, according to the original inclination of the land. Lastly, the river in the valley, as it gained power from the sinking of the sea, would generally remove the central portions, and leave only a fringe of the littoral and sublittoral deposits. This fringe, although formed by successive horizontal beach-lines, would *slope* upwards, as the whole bottom of the valley would have done if no part had been removed. I allude to this structure more particularly, because it is not at first obvious that matter accumulated on a sea-shore would in any case form a fringe of this kind.

In the hypothesis I have supposed the upward movement of the earth to have been absolutely uniform during equal periods. But this probably has seldom been the course of nature. There is clear evidence that the action of volcanos is intermittent; and the force which keeps volcanos in action being absolutely the same with that which elevates continents (as I endeavoured to prove in a paper read not long since, March 7th, 1838, before the Geological Society[22]), so we must suppose that the elevation of continents is likewise intermittent,—a conclusion which receives ample confirmation from the occurrence in nature of successive lines of escarpment, rising one above another, which mark those periods of rest when the sea wore deeply into the former coast. Let us then suppose that the water stood for a longer time at some one level than at any other. The first effect would be, that the beach or delta at the head of the sound, where the river is constantly bringing down detritus, would be broader there, owing to the greater accumulation of matter during this longer period, than in any other part; and therefore when the bottom of the whole valley was converted into land, the slope, which is everywhere gentle, would in that part approach nearer to horizontality; but in other respects there would be scarcely any difference. In like manner, in those portions of the mountains, on each side of the valley, where from the protected nature of the site matter did during the whole rise accumulate, though very slowly, the line would, from the greater quantity of matter added during the longer period of rest, slightly project beyond the general slope of the surface; and where any rivulet came down a very little delta would be formed. Also on any projecting or exposed point, the solid rock would be more deeply cut into than in the other lines. But as the land rose, the little deltas gently sloping from the line of ancient beach, with their front part cut off by the action of the subsiding waters, would project from the hill sides in the form of obliquely truncated buttresses; to the heads of which the horizontal lines of beach will exactly coincide, as indeed they likewise will with the broader ones, where crossing the bottom of the main valleys; but the slope in the latter case will blend both above and below with the inclined surface formed by the

matter rapidly accumulated at every successive level. Now it has been shown that Scotland within modern times has undergone a great elevation; it has been shown to be extremely improbable that such movements should be equally progressive; the effects of aqueous action on the surface of the land during the intermittent periods of rest in the elevatory forces have been traced; and it will have been perceived by those who have read the early part of this paper, or the memoirs of Sir Lauder Dick and Dr. MacCulloch, that the results anticipated in the hypothesis are the characteristic features, even in detail, of the "parallel roads of Lochaber": I believe, then, that the hypothetical case gives the true theory of their origin.

Section V.—Objections to the Theory from the Non-extension of the Shelves, and the Absence of Organic Remains at Great Heights, Answered

Several objections to this view, which implies that the whole country has been slowly elevated, the movements having been interrupted by as many periods of rest as there are shelves, will occur to every one. Perhaps the most important of these is, that, as the upward movement probably affected a considerable area, or at least as it cannot be supposed to have been confined within a defined line, so ought the shelves to be continuous over an equal space. I believe, however, from what I have seen in South America, that it would be more proper to consider the preservation of these ancient beaches as the anomaly, and their obliteration from meteoric agency the ordinary course of nature. Some contingencies seem absolutely necessary for the formation of the shelves, such as a sufficient height in the land, a steep slope, and that the country should be formed of rocks which afforded an abundance of somewhat adhesive detritus; we may conclude, moreover, that the surface must have been covered with turf, *immediately* after the waters subsided; for otherwise the loose matter would infallibly have been washed from the hills, and this contingency implies a protected, and hence, perhaps, an *inland* situation, which, at the period, when the water stood at the upper shelves, would leave but a small area. The abundance of detritus no doubt is quite necessary; for although the solid rock is in some parts notched, I do not believe the shelf would anywhere be distinguishable if the soil and detritus were entirely removed from it. It would also appear to be necessary that the valley should either have been originally closed at its upper end, or that during the period of rest some shallow part in it should have become so from the accumulation of sediment, or from any other cause, so that no stream set through it. Thus the two upper shelves of Glen Roy die away as soon as they enter the valley of the Spean, which must at the period when the waters stood at their levels, have

formed an open channel connecting opposite seas. That the ancient beaches in this case extended to that point, beyond which the accumulation of matter was prevented by too much exposure, seems clearly indicated, in a manner before explained, by the extremities of the lower shelf stretching beyond those of the upper. When, however, the 972-feet shelf existed as a beach, the channel of the Spean was converted by the closing of the pass of Muckul into a sound; and the shelf, apparently in consequence, winds along the sides of the valley both of the Spean and Roy. Besides the requisites here mentioned, the shelves appear to be more plainly marked where the valley is narrow, and, perhaps, likewise where it is tortuous. Now from the little I have seen in Scotland, I very much doubt whether these several contingencies occur frequently together; they certainly did not in several valleys which I visited. It must also be borne in mind, that as Sir Lauder Dick traced the lower shelf very much further than MacCulloch had done, and as I found a remnant of one in a distinct valley, and especially as Sir David Brewster has seen shelves in two places on the Spey, the probability is that others, though perhaps obscurely developed, will yet be discovered. The irregularly shaped area, in which shelves have already been found, measures in one line twenty British miles, and in another twenty-five.

Notwithstanding what I have now said, the presence of the shelves in some of the glens and their absence in others, in the district of Lochaber itself, is a very extraordinary circumstance. Thus in Glen Roy three lines are perfectly developed, whilst in the neighbouring one of Glen Gluoy it appears that only one exists. It is useless without data to speculate on the nature and force of the tides, currents, and winds of former periods, or on the kind of vegetation with which the land was then covered; all circumstances, perhaps, sufficient to determine the formation or preservation of a mere narrow mound of soft matter on the steep side of a mountain. But the following case proves, and it deserves particular attention, that the limits of the ancient waters cannot even approximately be inferred from the present extension of the ancient beach-lines. MacCulloch has drawn in his map a shelf intermediate between the two upper ones, on the face of the mountain (Tombhran) opposite to where Glen Turet joins Glen Roy: Sir Lauder Dick has not noticed this shelf.[23] Perceiving its importance I examined it with scrupulous care. It occurs rather nearer the lower than upper shelf, and as these two are only eighty-two feet apart, and are here strongly marked, it was scarcely possible (especially as I purposely looked at it from *every* point of view,) to make any mistake in the absolute parallelism of this intermediate shelf. It can be traced for nearly three quarters of a mile; at the west end disappearing quite insensibly, like the lines in Glen Collarig, but at the other end rather more abruptly in a water course. I walked along its

whole length, and its structure is perfectly characteristic; I refer to the materials of which it is composed, its breadth and inclination. The two regular shelves are, perhaps, more plainly marked here than in any other part of the whole glen; and it would appear probable that this is owing to that portion having been exposed to a longer space of open water, by which means the ancient waves acquired a greater than ordinary power in heaping up detritus. In the mouth, however, of Glen Collarig and of Glen Roy, an exposure to a wider channel, but at the same time to one open at both ends, and therefore probably a tide-way, has entirely prevented the accumulation of matter; and hence the beaches gradually disappear there. This view, if correct, as I fully believe it to be, shows by what a slight difference of circumstances, either a remarkable development or an entire obliteration of the ancient beaches has been determined. The intermediate shelf clearly owes its existence to the same causes which have in this part so strongly marked the upper and lower one; and though it is less strongly marked than these two in this immediate neighbourhood, yet it differs but little from them as they ordinarily occur, and is, I think, fully as plain as the lower shelf throughout Glen Spean. I assert, then, that it is an incontestable fact, that water must have remained at the level of this intermediate shelf for a long period, and only a little less long than at the other lines; yet in no other part of Glen Roy, the valley where circumstances have been so pre-eminently favourable for the formation and preservation of these beaches, a trace of this intermediate shelf has been observed. It has likewise been most clearly shown, that barriers could not have existed at the double mouth of Glen Roy, and we have seen that the surface of the land has been preserved in that neighbourhood in a manner quite extraordinary; yet it is known on the authority of Sir Lauder Dick, who appears to have examined the whole course of the Spean and its tributaries with great care, that not a vestige of either of these upper shelves can be discovered beyond the mouths of Glen Roy. Any argument, therefore, whatever, from the non-existence of the shelves or beaches bearing on the former limits of the ocean over this part of Scotland, during the period of rest in the subterranean movements, is valueless.

In the valleys of the Spean and the Roy, I attentively examined, with the expectation of finding fragments of sea shells, the matter accumulated on the shelves, and more especially the thicker beds of gravel and sand which occur at lower levels; but I could not discover a particle, and the quarrymen assured me they had never observed any. This may at first be thought a strong objection against the theory of the marine origin of these deposits. But having been led in consequence of Mr. Murchison's remarkable discovery of recent sea shells in the inland counties of

Shropshire and Staffordshire, to examine many gravel pits there, and having observed how frequently it happens, that not the smallest particle can be discovered in vast accumulations of the rudely stratified matter, and that when found, the fragments are generally exceedingly few in number and partially decayed, I feel convinced that their preservation may be considered as a remarkable and not as an ordinary circumstance. After a longer interval of time, or under some slightly less favourable conditions, all the gravel beds of Shropshire, which no one can doubt were accumulated beneath the sea, would be as destitute of organic remains as those of Lochaber. In some parts of South America I have found beds of gravel which did not contain a fragment of shell, and yet on the bare surface, nearly perfect ones were strewed in numbers. Mr. Smith describes[24] beds on the west coast of Scotland, and Mr. Lyell[25] others in Sweden, undoubtedly of marine origin, but wholly destitute of organic remains. On the coast of Forfarshire also Mr. Lyell, as I am informed by him, found shells in gravel beds extending to the height of between fifty and sixty feet; but at greater altitudes similar beds occur which do not contain any: he has observed the same kind of fact strikingly illustrated in Norway.[26] It is easy to imagine several circumstances which might determine the preservation or decay of the shells; even on the assumption, which is not necessary, that they have in all such cases been imbedded. Thus in Shropshire, the gravel is covered in most parts by an earthy deposit, which contains a small proportion of lime; hence the rain water having absorbed carbonic acid gas in its descent, would find matter to dissolve before it reached the layers containing shells; whereas in Lochaber the gravel and sand, being derived entirely from granite rocks, does not, as I ascertained, usually contain any free carbonate of lime, and consequently the fragments of shells would more readily be dissolved. I do not wish to assign this circumstance[27] as the real cause of their disappearance, but merely to indicate it, and other similar ones, as quite sufficient to show that the marine origin of the shelves cannot be controverted from the absence of organic remains.

Section VI.—Application of the Theory to Some Less Important Points of Structure in the District of Lochaber, and Recapitulation

By considering the hypothetical case above given, I think it was shown that the proposed theory explains every essential point in the phenomenon of the parallel roads. And I will now endeavour to show how far it applies to some minor points of detail. For instance, I have described a horizontal band of rock on one side of the narrow mouth of Loch Treig, with its face worn into smooth concave forms, like those over which a water-fall rushes; and on the other side, a great spit or bank of sand and

gravel. Now on the belief, that a sheet of water seven or eight miles long, and two or three broad, was drained during each ebb-tide to the depth of several feet through a narrow curved channel, and then again raised by the following tide to its former level, the effects there produced are quite intelligible. It is also easy to perceive, that through the means of the tidal action, points of solid rock might have been obliquely cut off in the same manner as on existing beaches; and that flat channels, resembling in every respect those which at present frequently separate small islands from larger ones, might have been worn between hummocks (such as those on one side of Meal-derry) and the lines of shelf.

If, again, we consider what must take place during the gradual rise of a group of islands, we shall have the currents endeavouring to cut down and deepen some shallow parts in the channels, as they are successively brought near the surface, but tending from the opposition of tides to choke up others with littoral deposits. During a long interval of rest in the upward movements, from the length of time allowed to the above processes, which essentially require time (though they are favoured by the *rise* of the land rather than by its remaining stationary), the tendency would often prove effective both in forming by accumulation of matter, isthmuses, and in keeping open channels. Hence such isthmuses and channels just kept open, would oftener be formed at the level, which the waters held during the interval of rest, than at any one other. These isthmuses and channels when left by the receding waves, might be called land-straits, for they would present smooth, flat, narrow surfaces, connecting more open spaces. During the rise of the land they would at first separate the heads of two adjoining creeks, and afterwards, the upward movements proceeding, they would form the watersheds between adjoining and opposite glens. By this means, I explain both the ordinary structure of the land in these mountains, where the waters divide, as already described; and more especially the remarkable fact of the exact coincidence of several such points with the lines of shelves,—the shelves only indicating the long interval of rest in the upward subterranean movements. It may be remembered that I described at the head of the Roy and of the glen near Kilfinnin, patches of alluvium or remnants of terraces on the sides of the land-straits, a little above the flat where the waters divide. This structure is in perfect accordance with the theory that drift matter began to accumulate in such parts at that period, when the tides in them were first checked, or otherwise affected by the rising of the land; and that the channels were finally closed at their present levels, solely from the long interval during which the sea acted at such levels. Hence, also, we might have expected, that patches of alluvium would occur (as is the case) on the sides both of the land-straits which are, and those which are not connected with shelves at corresponding levels.

From the levels taken by Mr. MacLean with Sir Lauder Dick, it appears that the upper limit of the Glen Gluoy shelf, which coincides with the division of the waters, is twelve feet higher than that of Glen Roy. The intervening space is nearly a mile in length, moderately broad, and very flat, having only a fall of the twelve feet; and Sir Lauder states[28] that he saw in this part the surface of the solid rock in the bed of the little stream. These facts seem at first to indicate that two periods of rest had supervened, one when the water stood at the level of the Glen Gluoy shelf, a second when at the upper level of Glen Roy after a rise of twelve feet, and that, nevertheless, the effects of these two periods of rest were confined respectively to separate, though closely adjoining glens. This circumstance if so interpreted, although improbable in the highest degree, could not be considered as subversive of the theory, after it has been ascertained that the upper shelves of Glen Roy are not prolonged into the valley of the Spean, and that the short intermediate one in Glen Roy does not extend for more than three quarters of a mile in that valley. There is, however, I suspect a more satisfactory explanation. In the First Narrow of the Strait of Magellan, the tide rises about forty feet, as Captain FitzRoy informs me, whilst eighteen miles to the west at Gregory Bay, the rise is only about twenty feet. Here then, and other instances might be adduced, in a distance of eighteen miles, the surface of the water must slope no less than twenty feet. Let us suppose a *rocky* barrier (and that of Glen Gluoy is rocky) to be elevated, by such movements as those now in progress in South America, across the strait, separating it into two portions. Might we not expect that the high water mark would rise several feet higher, in that portion of the former channel which was still open to the sea subject to the great tidal movement, than it would in the other connected only by tortuous passages with a different sea, where the rise of the tide was small? In such a labyrinth of channels as this part of Scotland must have presented when the sea stood at the level of the upper shelves, it is even probable that there would be inequalities in the rise of the tide in different parts; I conclude therefore that when the rocky barrier was upraised between Glen Gluoy and Glen Roy, a greater tide-wave, proceeding direct from the line of the Caledonian Canal, then a great strait, swept up this deep creek; whereas a smaller one reached by a circuitous course the *Bay* of Glen Roy, which, moreover, was connected by some other straits with the eastern sea.

Whoever walks over these mountains, and believes that each part has been successively occupied by the subsiding waters of the sea, will understand many trifling appearances, which otherwise, I believe, are unintelligible. Thus in Upper Glen Roy he will see in the level expanse, an old bay, filled up and leveled with tidal mud. Again at the Gap of Glen Collarig, with its flat bottom and cut off sides like a gateway, he will recognise a channel, at last choked up with matter drifted by the tides,

and now left in the state in which it was when the waters retired from it. The traces of supernumerary shelves will offer no perplexity to him, and will equally receive with the others a simple explanation. By the theory of the sea having acted at successive levels over the whole surface of the land, the great beds of shingle[29] and sand, such as those near the mouth of the Spean, have a cause assigned to them adequate to the effect. Lastly, the manner in which the deposits near the mouths of the larger valleys have been modeled into successive terraces, which in some parts at least appear not to have been formed by the river, receives elucidation. I may add, that in South America I have observed numerous instances of terraces in every respect similar to these, with sea shells abundantly scattered on their surface; and therefore where there could exist no obscurity regarding their origin.

In concluding this part of my paper I will recapitulate the course of the argument pursued. 1st. It is admitted by every one that the horizontal shelves are ancient beaches. 2nd. I showed that no lake theory could be admitted on account of the overwhelming difficulties in imagining the construction and removal at *successive* periods of *several* barriers of immense size, whether placed at the mouths of the separate glens, or at more distant points. 3rd. The alternative that the beaches, if not formed by lakes, must of necessity have been so by channels of the sea, was not advanced, only because it was thought more satisfactory to prove from independent phenomena, that a sheet of water *gradually subsiding* from the height of the upper shelves to the present level of the sea, occupied for long periods not only the glens of Lochaber, but the greater number, if not all the valleys of this part of Scotland; and that this water must have been the water of the sea. 4th. It was stated (the strongest argument being the ascertained fact of the land rising at the same time in one part and sinking in another,) that in all cases the land is the chief fluctuating element; and, therefore, that the above change of level in Scotland, independently attested by marine remains at considerable heights on both the eastern and western coasts, implies the elevation of the land, and not the subsidence of the surrounding waters. 5th. It was shown that in all such prolonged upward movements it might be predicted, that there would be intervals of rest in the action of the subterranean impulses. 6th. By an hypothetical case, the land was subjected to the above conditions, and its surface was found to be modeled in a manner wholly similar, even in detail, to the structure of the valleys of Lochaber as they now exist. 7th. The true theory being considered thus established, objections to it from the non-extension of the shelves, and from the absence of organic remains at great altitudes, were answered and shown not to be valid. 8th. Many points of detail in the structure of the glens of Lochaber, were shown to be easily explicable on the supposition, that the valleys had been occupied by arms of a sea subject to tides, and which

had gradually subsided during the rising of the land. Having attentively considered these several and independent steps of the argument, the theory of the marine origin of the "parallel roads of Lochaber" appears to me demonstrated.

I may here remark, that MacCulloch seems to have been aware of the great difficulties attending his theory: but having proved that the roads could not be works of art, or the effects of any great debacle, he argued, to use his expression from the dilemma of the case, that they must have been formed on the shores of a lake. The idea of a continent slowly emerging from beneath the sea, appears, and it is a very curious point in the history of geology, never to have occurred to him as a possibility, although he was so bold and ingenious a speculator. His paper was read in the beginning of 1817, and when we reflect that during the few latter years, proofs of such movements have accumulated from all quarters of the world, we must recognise how much of this all important change (the foundation-stone, I may add, of this paper) is due to the Principles of Geology by Mr. Lyell.

Section VII.—On the Erratic Boulders of Lochaber

I will now pass on to some other considerations which partly derive their interest as dependent on the truth of the foregoing theory. I have said, that the parent rock of many of the fragments lying on the shelves is not found in the immediate neighbourhood. These erratic boulders are generally of granite, and are from one to five and six feet in diameter; they are not confined to the shelves, but are scattered on the sides of the mountains. On the summit of the insulated hill of Meal-derry, above the level of the 972-feet shelf, there was one of large size, together with some well-rounded pebbles of rocks, which, I believe, do not occur there. In the gap of Glen Collarig the boulders on and near the upper shelves are frequent, as they likewise are in the pass between Upper and Lower Glen Roy; they occur also abundantly at the bottom of the latter valley, and on the side of Tombhran. From having found them in almost every part which I examined, I have little doubt that they are distributed in numbers over all the valleys and mountains, at least, to an elevation as great as that of the upper shelves: I make this latter restriction, because having descended the mountains only in a few places above that level, I cannot speak positively with respect to the greater heights. On the mountains, however, between Glen Roy and Glen Gluoy on a hillock north-north-west (*magnetic*) of the summit of Ben Erin, I found several masses of granite, one of which was four feet by three in width and two in thickness (together with a couple of pebbles from rocks not *in situ*) resting on the surface of the gneiss. This hillock seemed to be entirely composed of the latter rock, and it was separated from all other hills by a valley. On the

flanks of Ben Erin at about the same level, there were several boulders of granite, one of which was six feet across. Of those on the hillock (probably there were many others which I did not see in merely crossing the mountain,) the highest one was found by comparison with the Glen Gluoy shelf (by means of the barometer), to be 2200 feet above the level of the sea. I will describe in detail the spot where I found one other boulder, in as much as the whole of the district being composed of gneiss, it might be suspected that patches of granite occurred high up on the slopes of the mountains, and that the fragments had simply rolled down into their present positions. This, however, could not have happened in the case last described, nor in the following one: about twenty feet below the summit of a very sharp peak (1600 to 1700 above the sea) the whole of which consisted of tortuous layers of gneiss, there was a block of

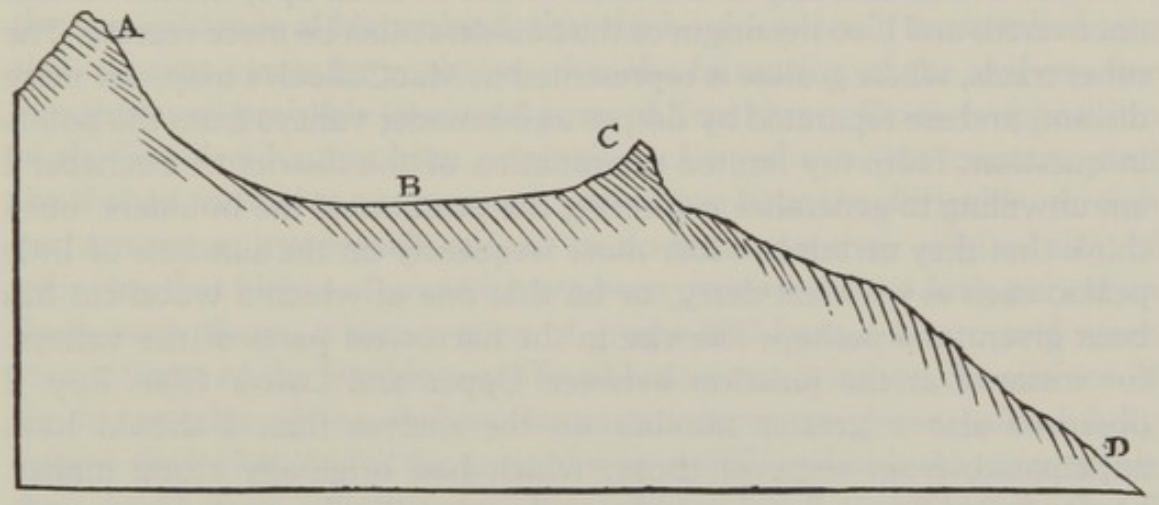


Fig. 4. Profile of gneiss peak to Habercalder in Great Glen. A, lofty mountain of gneiss; B, a peat moss 215 feet below the boulder, dividing the waters flowing on each side around the hill C; C, boulder of syenite, resting on gneiss 1,600 or 1,700 feet above the sea; D, Habercalder in the Great Glen of Scotland.

syenite with pink felspar, two feet eight inches across. The peak is wholly separated (as shown in the wood-cut, fig. 4.) from a lofty mountain also of gneiss, by a *broad* and quite flat valley, the highest part of which is 215 below the spot where the boulder lay. I may observe that I did not anywhere see another boulder of the syenite, nor a single one of granite on this side of the mountains, which is separated by a lofty ridge from the valleys of Glen Roy and Glen Gluoy, where the blocks of granite are so numerous. Between two branches, however, of the Tarf Water (which enters Loch Ness near Fort Augustus) on the summit of a hillock of gneiss, about 1200 feet above the sea, I noticed one of granite.

The granite of all the boulders which I observed in Glen Roy, and likewise of those on Ben Erin, has a uniform character; it is subject to much disintegration, and therefore I do not doubt that the boulders were originally much larger. In MacCulloch's Geological Map of Scotland, the

nearest granite *in situ* to the boulders on Ben Erin is seen to be at the source of the Roy, near Loch Spey, a distance in a north-east line, passing over mountain and valley, of between five and six miles. The granite there has the same lithological character with that of the boulders, and I do not doubt that it is the parent rock, at least, of those strewed along the course of the Roy. With respect to the boulders on Ben Erin, they are completely cut off from every granitic district by valleys, the *highest* point of which is 920 feet below that boulder, the altitude of which I measured; that is, it would be impossible to walk from granite *in situ* to these boulders without ascending at least that number of feet. I will only further add, that if a sheet of water were raised to the level of the Ben Erin boulders, there would be a line of open communication[30] between them and the granite of Loch Spey; although I must confess I much doubt whether in that case any of the rock *in situ* at Loch Spey would remain uncovered; and if so the origin of the boulders must be more remote. The other tracts, where granite is represented in MacCulloch's map, are more distant, and are separated by deeper and broader valleys from the points in question. From my limited examination of the district of Lochaber I am unwilling to generalize respecting the position of the boulders, but I think that they certainly occur most frequently on the summits of little peaks, such as on Meal-derry, or on that one of which a wood-cut has been given; and perhaps likewise in the narrowest parts of the valleys; for instance at the junction between Upper and Lower Glen Roy. I observed also a greater number on the shelves than I should have anticipated, from some of those, which had originally stood higher, having rolled down. But, I repeat, I will not positively say that such is the case; although with respect to the boulders on the peaks, as I observed five well-marked cases, even during my short examination of the country, I have little or no doubt that the observation is correct.

On any conceivable theory of the transportation of erratic blocks, whether by some overwhelming debacle, or by floating ice, or any other means, it will at once be evident that they must have been scattered over the country, either before the shelves were formed, or at the time of their formation, but not on account of the delicacy of the lines at any after period. According to the generally received opinion of geologists, the so-called "erratic block period" is recent, and therefore we obtain a rude method of estimating the age of the shelves, and consequently of the elevation of the whole central part of Scotland, at least to a height of 1278 feet (or that of the upper shelf) above the sea.

It may perhaps be worth while briefly to compare together, under the conditions here afforded, the two theories of the transportation of erratic boulders, which are alone worthy of consideration, namely, that of great debacles and of floating ice. I will not lay any stress on the difficulty of

imagining, in accordance with the first theory, a rush of water so impetuous as to transport vast masses of rock across profound valleys and up the steep sides of high mountains, for this difficulty has no special reference to the case of Lochaber; but those who believe in the past occurrence of so terrific an agitation of the waters of a deep sea, must in some manner account for the frequency of boulders in the most exposed places on the summits of hillocks, and likewise for so many having been left in narrow straits, where one would have anticipated the most impetuous rush of water. On the face of Tombhran I observed many boulders scattered on the shelves, which have been formed there not only by the accumulation of loose matter, but also by the deep excision of the solid underlying rock. Again, there were other boulders on the shelves on the rocky peninsula near the junction of Upper and Lower Glen Roy, where much of the gneiss has been worn away. Here it was not possible, from the non-existence of higher land, that the boulders could have rolled into their present places from above, after the formation of the shelves; nor was this at all probable in several parts of Tombhran. On the supposition of the boulders having been originally scattered over the country, and the shelves formed at a subsequent period, we have the difficulty, though perhaps not an insuperable one, as we do not know their original size, of believing that blocks of granite have been preserved for a long period on those very places, where a zone of gneiss had been cut into and worn away. Some of the boulders on Tombhran were lying on the surface of the lower edge of the shelves, in parts where, as above said, I fully believe the inclination of the ground was so trifling that it was impossible they could have rolled down from above; but I regret much that I omitted, from not having perceived its importance, to ascertain this point with certainty. If the fact be so, and I scarcely doubt it, it would prove that some action, so quiet as not to have disturbed the small quantity of earth and little stones, of which the shelves are formed, transported these boulders across deep arms of the sea, and left them on the surface of the ancient beaches. The theory, that all erratic blocks, circumstanced like these of Lochaber, have been transported by floating ice, wholly removes these difficulties; for the icebergs, in the first place, would generally land the fragments, with which they were charged, on the lower part of the beaches or shelves; and secondly, those which had arrived not long before a fresh elevation would have been exposed only to a small amount of tidal degradation. Thirdly, the icebergs would frequently be stranded on shoals and islets, over and round which the tides swept; and likewise they would be frequently driven on shore in the narrow parts of the channels, where the waters were pent up. So that in after times, when the land was drained, it is easy to perceive that the boulders would lie scattered in such places, as they now actually occupy

in the district of Lochaber. Lastly, this theory requires that every district where boulders are found should have been covered by the sea; here we have independent proofs that such was the case, at least to an elevation of 1278 feet.

In my Journal during the voyage of the Beagle, I have endeavoured to show that the erratic blocks of central Europe were probably transported at that period,[31] when its climate was more equable (chiefly consequent on the larger area of water), which favours a low limit of the snow line, and therefore the probability of glaciers, the parents of icebergs, descending in favourable places into the sea. It is therefore to this period, if this view be correct that we must refer the "parallel roads of Lochaber," and consequently the elevation of the land, not only of the 1278-feet portion (which it is certain has been elevated at an epoch not distant), but likewise of the whole altitude, whatever it may be, at which boulders occur. If there be others, as is most probable, at a greater height than that one on Ben Erin, which I observed in merely crossing the mountains at a point 2200 feet above the sea, then by so much the greater has the elevation of the land been within this same period. Mr. Blackadder (in a letter to Mr. Lyell) states he has seen on the west coast of Scotland, in the island of Mull, large fragments of quartz rock at the height of 2000 feet, of the same description as that found on some of the adjoining islands and mainland. In Sweden M. Sefström says that boulders occur at an elevation of 1500 feet; in Massachussets, in North America, they are found, according to Professor Hitchcock, at 3000; and on the Jura it is well known they occur, from low down, to an altitude of 4000 feet. It is interesting to discover, that in our own country the upward movements, within *the same period*, have been more than half as great as those which have affected the latter colossal chain. But regarding the exact period, allowance must be made, since on the one hand the glaciers of the Alps, situated ten degrees nearer the equator than those on the mountains of Lochaber, must have much earlier retreated upwards, and failed in descending to the level of the sea, during the change from the former to the present climate; whilst, on the other hand, to counteract the equatorial influence, they were appendages on a greater mass of snow accumulated on far loftier chains.

Section VIII.—On the Small Amount of Alluvial Action Since the Formation of Shelves

I now pass on to another consideration. MacCulloch was much struck with the fact, that in many cases where a shelf crossed a rivulet, I mean one of those silver-like threads of water which descend the flanks of steep mountains in nearly straight lines, it frequently entered a little way on

each side of the gully. From this fact it is evident that the gully must have been partly formed before or at the time when the shelves were sea-beaches. I particularly observed several instances of this structure. One which struck me most was in Glen Roy, opposite a gap in the mountain which leads to Glen Fintec; here two small threads of water were united at the point where the line of shelf crossed them, and at their junction the rock was much exposed, so that any one would have supposed that the furrow in which they flowed had been entirely hollowed out by their action. But the shelf curved in a little way on each side; and, what was more curious, the apex of turf above the point of junction of the two streamlets had evidently originally formed part of the shelf. By this it was shown that the entire hollow, with the exception of the actual beds of the streams, must have existed as an indentation or little cove on the line of ancient sea-beach. It appeared to me that the extent to which the shelves entered these furrows did not bear any close relation to the power of the streamlets now flowing in them: thus on Tombhran (in front of the houses of Roy) a great gorge which is impassable, and where the rock is bare and shattered, has been deeply cut into by the winter torrents, and yet the shelves enter only a very little way on each side; whereas in other cases we find a hollow or creek of some size, but with an insignificant stream flowing in it, for instance, that opposite the gap of Glen Fintec, which has not even removed the remnants of the shelf from the head of the gully, in which it has flowed ever since the retreat of the sea.

Without entering here into a full consideration how these gullies were originally formed, and whether the indentations made in the beach at one level might not be produced downwards to another, I will only remark, that the sea in most situations certainly does alter the form of its coast, and yet that an accurate map of any shore gives a line indented in such manner, that a series of them, if placed one above and a little behind another, would produce the same kind of furrowed surface which characterizes the mountains of Lochaber, as well as most others. I will further observe, that when travelling along the shores of northern Chile and Peru, where the alluvial action is reduced to an exceedingly small measure, and where it is not probable that within a recent period there has been any *great* change of climate, I was repeatedly much surprised at observing how absolutely similar all the minor inequalities of the surface (yet covered with beds of sea shells of existing species) were to those of countries, where almost every detail in outline is usually attributed to meteoric agency; I could perceive only one difference, namely, that the larger valleys had unusually flat bottoms. Although fully convinced of the truth of this fact, I confess I was astonished at discovering in the mountains of Scotland, which have been exposed during a vast period to

the destroying action of a wet and boisterous climate, clear proofs that almost every furrow and inequality has been left nearly in the state in which we now see it,[32] by the retiring waves of the sea. From the preservation of some of these beaches, one can point to the very spot, and declare so much was removed when the sea stood there, and so much since by the running streams of fresh water.

It may be asked, has the present alluvial action done nothing here? Something it assuredly has done, but I repeat, comparatively nothing to that which was effected before the sea retreated. In Chile I concluded that the action of the more rapid rivers and torrents was chiefly confined to removing the littoral and sublittoral deposits left by the arms of the sea; and secondarily in cutting, as soon as the upper beds were removed, a wall-sided gorge through the solid rock. It appeared, that as long as the river had its passage through the water-worn materials, from the great facility with which it changed its course, its bed was broad, but as soon as it reached the solid strata it became exceedingly narrow. These conclusions are in strict conformity with what I observed in the glens of Lochaber. Of the small amount of corrosion effected since the sea stood at a level of the upper shelves, there are some curious instances. Sir Lauder Dick, in describing in detail the head of Glen Gluoy,[33] concludes that the river has worn there, during the immense period which must have elapsed since the water (of the sea) retired from the 1278-feet shelf, a remarkable chasm, between fifty and sixty feet in depth, but only a *few feet* wide. The stream in the northern arm of Glen Turet has cut for itself a passage in the solid rock in only a part of the valley, between the middle and the 972-feet shelf. In Upper Glen Roy the southern stream falls into the plain by a cascade, to the upper edge of which on each side the 1226 shelf approaches close. I did not ascend the spot, but as far as I could judge, the water has not cut back more than at most a few yards, into the rock over which it falls. Other similar instances might be adduced. Although none of these streams form great bodies of water, yet when flooded by the winter rains they cannot be inconsiderable; and their action has been prolonged for so vast a period, that the geographical features, together probably with the climate of the country, have been greatly changed. The rocky crests of the mountains no doubt have suffered from the weather; but the perfection of the shelves over spaces many hundred yards in length, and in the case of Glen Roy (where the three shelves occur) of some hundred feet in vertical height, clearly proves that as the sea left the greater part of the surface, so does it now remain. Amongst mountains the bursting of temporary lakes may sweep away or accumulate vast quantities of rubbish in the valleys; earthquakes may hurl down piles of fragments; and torrents during the lapse of ages, or under favourable conditions (such as the descent of many

pebbles), may excavate a gorge of almost any depth, but which, as far as it is possible to judge, will always be narrow and steep-sided. All this must often have happened, and will so again; but the glens of Lochaber plainly show that the effects of ordinary alluvial action is exceedingly small, far smaller than any one would have anticipated. And as their outline does not differ in any marked degree from that of all other valleys, this conclusion may be extended to other cases.

In Glen Roy, where the three shelves can be seen near each other, little or no difference can be perceived in their state of preservation; indeed the upper one, I think, is more perfect than the one below it. From this fact an argument has been advanced by Dr. MacCulloch, that no long interval of time could have elapsed between their formation. But this view is quite inadmissible; either the worn and deeply notched rock of the shelves on Tombhran, or the buttresses on the middle shelf (as at the head of Lower Glen Roy), which are composed of large masses of well-rounded shingle, is sufficient, without considering the *intermediate shelf* and other appearances, to *prove* that the water must have remained at levels intermediate between the highest and the 972-feet shelf for very long periods. Hence the alternative is obvious, and is in direct accordance with what has already been advanced, namely, that the ordinary alluvial action is so exceedingly small, that whether the surface has been exposed during one, two or more whole epochs, no sensible difference can be perceived in the state of its conservation.

Of the many remarkable features in the geology of this district, few, perhaps, are more remarkable than this perfect preservation of its surface. We have a mound composed of soft materials so small, that it oftentimes cannot be distinguished, by a person standing on it, from the adjoining slope, but which it is not probable, from the structure of the mountains, was ever much larger; and yet this very mound, when viewed from a distance, will be seen to extend for many hundred yards, even miles, continuous and perfect, with the exception, perhaps, of a few small breaks, where some streamlet descends. On these same mounds we can sometimes distinguish those fragments which have been washed by the little waves of the ancient waters, from others which have since fallen; and at Loch Treig, at the height of 972 feet above the sea, the tide-scooped rocks appear as if scarcely a century had elapsed since they were washed by the ripple of the eddying currents. The preservation of the druidical mounds in Britain has often been adduced as a circumstance worthy of attention; but here during a period which cannot be reckoned by thousands of years, but only by those great revolutions of nature which are the effects of slow and scarcely sensible changes, works smaller than those ancient ones dedicated to superstition, retain each outline nearly as perfect as when first formed by the hand of nature.

These facts are interesting under another point of view, for they prove to us that we may trust the plain inference of our experiences. Although we see[34] the stone of many ancient buildings decaying and crumbling away, yet we know that others, as the obelisks of Egypt, have lasted more than three thousand years, with the hieroglyphics nearly perfect on them: now we cannot see any reason why their general outline, even in points of detail, should not last a hundred times three thousand years. Again, although we might expect the crest of a mountain range to be shattered, and the bed of a torrent to be worn down more or less deeply, yet if we look at a convex slope of soil clothed with turf, and drained on each side by rivulets, we can see no reason, as long as the vegetation is persistent, why such a slope (with the exception of any spot where a waterspout might burst, or a stroke of lightning fall) should not last for as many thousand centuries as the obelisks of Egypt shall remain entire. Of the justice of these inferences, conclusive evidence is afforded by the state in which we now see the mountains of Lochaber,—a state of which we approximately know the high antiquity.

Section IX.—On the Horizontality of the Shelves, and on the Equable Action of the Elevatory Forces

Sir Lauder Dick, with Mr. MacLean's assistance, seems to have determined within very small limits the absolute horizontality of the several shelves. A delicate eighteen-inch levelling instrument, made by Jones, was employed. Sir Lauder says,[35] "Directing the object-glass of the instrument to the nearer, and immediately opposite corresponding line of shelf, it applied all along most accurately to the horizontal hair; but when pointed to those further off (some of which were perhaps five or six miles distant), they appeared to sink sensibly below the hair, and this in proportion to their distance from the point where we stood; but they were nowhere observed to do so in a greater ratio than the allowance for the curvature of the earth at such rectilineal distances demanded. And, what was in our opinion most conclusive, when the telescope was pointed to, and made to traverse along any particular portion, which, from being directly opposite to the eye, might have been presumed to be nearly equidistant in all its parts, it was found to preserve an uniform relation to the horizontal hair." The same results were obtained in other instances; but yet the angle of depression of the distant shelves does not appear to have been actually measured, and its correspondence with the curve of the earth calculated. But it is quite certain that if any difference from that curve exists, it must be very small.[36] Here then is a case which supports apparently with more weight than perhaps any one hitherto advanced, the doctrine that the land is the stationary element in these

changes of level, and the ocean the fluctuating one; for it may well be asked, can we suppose that a whole country shall have been lifted up without the smallest ascertained flexure of the ancient coast lines? Without reverting to the argument of the movements now in progress, some upwards and some downwards, or to the difficulty of imagining a receptacle for a stratum of water, nearly 1300 feet thick, concentric with the globe, I will consider the phenomenon in another point of view. It appears from the facts given by Mr. Lyell in his Principles of Geology,[37] and in the Philosophical Transactions,[38] that a large territory in Sweden is now rising at the rate of three feet in a century; and that the area affected reaches from Gottenburgh to Torneo, and thence to North Cape (a distance of 1000 geographical miles), although the rate of elevation increases as we proceed northward. We may therefore safely conclude, that large spaces in Scandinavia have been elevated so equably, that at points several miles, if not leagues apart, the difference of elevation at the close of the past century, did not amount to one foot. In South America the whole coast of Chile has been elevated within the recent period; and during the great convulsions which affect that country, large spaces have been uplifted nearly to the same amount, although some parts a few feet more than others. On the eastern side of the same continent, the land has also risen within the same period, and as earthquakes are unknown there, the change probably has been, as in Sweden, so slow as to be insensible at any one time. On that side the traveller may ride for many hundred miles over plains, scarcely broken by a single undulation, and where the strata and surface are almost absolutely level: no one would there for one moment imagine that the elevatory forces had acted unequally, but rather he is astonished that the bottom of any sea or estuary should have been so uniform, as must have been that of which the plains of La Plata not long since formed the bed.

If then great plains and mountainous countries can be raised within such small limits of absolute horizontality, as undoubtedly has happened in the above cases, shall we, who are wholly ignorant of the mechanism of these movements, be justified in rejecting the plainest analogies, in supposing difficulties little short of physical impossibilities, and in believing that the reverse of what is ascertained in other cases has taken place in Lochaber, and all simply because the change of level has been more equable, than we in our ignorance could have anticipated? Every one, I think, who will attentively consider the above facts, will answer with me in the negative, marvellous though the fact be, that the beaches of Lochaber, raised on high so many hundred feet, should still follow the curvature of their ancient waters. On the contrary, a most important geological fact is established; namely that an area (twenty miles in length

and eighteen broad, and perhaps more, if the shelves on the banks of the Spey be included in it) has been raised 1278 feet above the level of the sea, so equably, that no deviation from the true curvature of the earth can be discovered by the ordinary means of leveling.[39]

Section X.—Speculations on the Action of the Elevatory Forces, and Conclusion

If we choose to enter on speculative grounds and to reflect on the secondary means which have caused these equable movements, two solutions occur. But first I must remark that the crust of the earth seems to yield easily to the forces which have acted on it from below; when we observe a brick-wall dashed to pieces by a cannon ball, or a pane of glass by a small stone, we say that both are fragile and yield easily; so when we examine the earth and find it fissured and refissured, one fragment let down and another raised high up (as we know to be the case where extensive sections have been obtained, as in our coal-pits or metalliferous districts), we must certainly admit, that the force which has broken up the crust in vertical planes relatively nearer to each other, compared with its thickness, than in the fissured pane of glass, easily overcame the resistance offered to it, however absolutely great that may have been. This same conclusion is forced on us, when we reflect that the very cause of the trembling of the ground in earthquakes seems due to the rending of the strata; and that earthquakes in many countries are of such frequent occurrence, that probably this hour will scarcely elapse without the crust somewhere yielding. If indeed the crust did not yield readily, partial elevations could not be so gradual as they are known to be, but they would assume the character of explosions. That there has been some real connexion in certain cases[40] between that state of the weather which is accompanied by a low barometer and the occurrence of earthquakes, can, I think, hardly be doubted; if we admit Mr. P. Scrope's explanation of this, that the diminution of atmospheric pressure (equal in some cases to an inch and half of mercury, spread over a very large area) determines the particular time at which the earthquake occurs, the force and tension being before almost balanced, we may be said to possess a rude measure of the force requisite in that area to overcome the coherence of the parts, as existing in the intervals of the recurrent earthquakes. If then the motive force acts so gradually that the earth's crust can acquire that degree of tension, which causes large portions of it to yield readily to a very slight additional impulse; and if, as we know undoubtedly to be the case, the crust has yielded in innumerable vertical planes, intersecting each other like a net-work, and running parallel to each other at very short distances, we are compelled to admit that the equable elevation of

so large an extent of country as Lochaber, must have resulted from the equable action of the elevatory forces, and not from the cohesion of its parts.

Bearing this in mind, the most obvious solution, but I very much doubt whether the correct one, is, that no force excepting the uniform expansion of solid matter from heat, could raise so equably the surface of a great *fragile* mass, as the district of Lochaber must be considered. I doubt this solution, first, because a very great expansion is necessary, especially if we include in these movements the elevation of the erratic blocks, now lying more than 2200 feet above the sea. Secondly, because the movements appear to have been of the same kind as those in the not distant country of Sweden; and there it has been shown by Mr. Lyell, that near Stockholm an alternate movement of more than sixty feet has taken place within the human period; and one is strongly tempted to believe that there is some relative connexion between the areas in Northern Europe which are rising and those which are quietly subsiding. These facts to be explicable on the theory of expansion, require, as it appears to me, far too capricious an action, in so *slowly* and far-pervading an influence as heat, to be admitted; whilst on the supposition of mechanical displacement such difficulties are not presented. Thirdly, because (and it is my chief reason for rejecting the agency of expansion by itself) the movements appear to have been of the same order with those now in progress in South America; and in that country the elevation of certain wide areas, as I endeavoured to show in a paper lately (March 7, 1838) read before the Geological Society,[41] cannot be attributed to any other cause than an actual *movement* in the subterranean expanse of molten rock: to speak only for example sake, such as would result from a change in position of those inequalities in the ellipticity of the earth's surface, which seem indicated by the measurements of arcs of meridians. It may also be inferred, from the facts given in that paper, that the fluidity of the nucleus must be tolerably perfect. In the volcano, even the lava which is propelled to the summit of a mountain, far beyond the subterranean isothermal line of melted rock, and poured out on the surface, is oftentimes so fluid, that it runs into thin sheets like molten metal. Also at the junction of the plutonic with the metamorphic formations, we see tortuous thread-like veins branching from the former into the latter, which could only have been injected when quite liquid. Here the rock has been melted at a great depth under an enormous pressure, and yet the fluidity must have been very perfect: such plutonic rocks moreover form the beds on which all others rest. Considering these latter facts, together with the inferences deduced from the phenomena observed in South America, it may be granted as not improbable in any high degree, that this part of Scotland when it was

upraised rested on matter possessed of considerable fluidity, which underwent a slow change of form. If this be granted, there is no great difficulty in conceiving that the surface of the interior molten matter might retain that degree of curvature proper to it, as the resultant of the unknown force with that of gravity and the centrifugal impulse. Moreover, as we must conclude from what we now see going on in South America and in Scandinavia, that the area affected was large, the difference between the amount of curvature of the fluid nucleus after the rise in that part of one or two thousand feet, would be exceedingly small, and its outline scarcely distinguishable from that of the ocean, and certainly not from that of a sea affected by various tides in confined channels, which in the case of Glen Roy affords the only standard of comparison. We may almost venture to say, that as the packed ice on the Polar Sea, with its hummocks and wide floes, rises over the tidal wave, so did the earth's crust with its mountains and plains rise on the convex surfaces of molten rock, under the influence of the great secular changes then in progress.

After these considerations I am far from thinking it an overwhelming difficulty, that the curvature of the shelves of Glen Roy over a space of four, or five, or perhaps even twenty miles should appear to be the same with that of the surface of the ocean, within that limit of accuracy which the nature of the case renders possible. On the contrary, I deduce from their curvature, first, that the district of Lochaber formed only a small part of the area affected; secondly, a confirmation of the view, which I deduced from the phenomena observed in South America, that the motive power in such cases is a slight additional convexity slowly added to the fluid nucleus; and thirdly, this additional fact, that we thus obtain some measure of the degree of homogeneous fluidity of the subterranean matter beneath a large area, namely, that its particles, when acted on by a disturbing force, arrange themselves in obedience to the law of gravity. And although we arrive at this conclusion with some surprise, when relating to the abysses of the nether regions, we see it habitually verified in volcanic countries, where a torrent of lava, checked by some obstacle, has expanded into a level sheet.

Mr. Lyell, in his Principles of Geology,[42] quotes a passage from Sir John Herschel's Astronomy,[43] to show that whatever may have been the original figure of the earth, the wearing down of the solid matter and its redeposition at the bottom of the sea, must tend continually to change the *actual* figure of the earth, as Playfair[44] expresses it, into the *statical* one: he then adds, "that the same remark applies to every stream of lava flowing on the surface, and if the volcanic action should extend to great depths, so as to melt one after another different parts of the earth, the whole interior might at length be remodeled under the influence of

similar changes, due to causes which may all be operating at this moment." Now if it be granted that the curvature of the shelves of Lochaber is due to the elevation of the district by means of a subterraneous expanse of fluid matter, the atoms of which obeyed the law of gravity, it cannot be doubted they would likewise obey that of the centrifugal force. Therefore, if the figure of the earth did not already very nearly approach to that of a spheroid of equilibrium, regions near the equator and others near the poles, during the changes of level now actually in progress, would be acted on by forces greatly different; and consequently as the crust does now yield (and has yielded in an infinite number of planes,) the statical form would be immediately acquired. This view is here given, because a directly opposite, and as I cannot but think incorrect one, has been advanced by Playfair.[45]

In concluding this paper, I will briefly indicate the chief points which receive illustration from the examination of the district of Lochaber by Sir Thomas Lauder Dick, Dr. MacCulloch, and myself. 1st. Nearly the whole of the waterworn materials in the valleys of this part of Scotland were left, as they now occur, by the slowly retiring waters of the sea; and the chief action of the rivers since that period has been to remove such deposits; and when this was effected, to excavate a wall-sided gorge in the solid rock. 2nd. During the vast period which must have elapsed since the sea stood at the level of the upper shelves, the alluvial action has been exceedingly small: steep slopes of turf over large spaces and the bare surface of rocks have been preserved even perfectly; and we see every main, as well as most of the lesser inequalities of the land, in the state in which they were then left. 3rd. The elevation of this part of Scotland from the level of the present beach to the height of *at least* 1278 feet has been extremely gradual, and was interrupted by long intervals of rest: it has taken place since the so-called "erratic block period." 4th. It is probable that the erratic blocks were transported during the quiet formation of the shelves. One was observed to occur at an altitude of 2200 feet above the level of the sea. 5th. The extraordinary fact that a large country has been elevated to a great height so equably, that the ancient beach-lines retain the same, or nearly the same curvature, which they had when bounding the convex surface of the ancient waters. Lastly. The inferences from this head, supported by other cases, namely, that a large area must have been upraised, and that this was effected by a slight change in the convex form of the fluid matter on which the crust rests; and, therefore, that the fluidity is sufficiently perfect to allow of the atoms moving in obedience to the law of gravity, and consequently of the effects of that law modified by the centrifugal impulse. Hence, that even the disturbing forces do not tend to give to the earth a figure widely different from that of a spheroid in equilibrium.

Postscript

I am much indebted to my friend Mr. Albert Way for his kindness in lending me the drawing, from which the accompanying lithographic sketch has been taken. It very faithfully represents the general appearance of Glen Roy.

1. [Read 7 February 1839.] *Philosophical Transactions of the Royal Society of London*, pt. 1, 1839, pp. 39–81.†

2. The valley of the Roy River, a tributary of the Spean, near Fort William, in Scotland. See Paul H. Barrett, "Darwin's Gigantic Blunder," *Journal of Geological Education* 21(1973):19–28 and Martin Rudwick, "Darwin and Glen Roy: A 'Great Failure' in Scientific Method?" *Studies in the History of Philosophical Science* 5(1974):97–185.†

3. Some rude measurements which I made with a mountain barometer lead me to suspect that these altitudes are at least a hundred feet too great. It is not a point of any importance with respect to the theory of the origin of the shelves, but I regret that I did not verify their height with more care.

4. Transactions of the Edinburgh Royal Society, vol. ix. p. 11.

5. These statements are founded on what I saw in Glen Collarig, where the lower shelf (the 972 feet one) blends into a slope, now rendered irregular by the action of the torrents, which rises (at the gap) to a height of more than a hundred feet above the level of the shelf. Again, near the head of Lower Glen Roy, the seam shelf blends into a similar kind of plain, which rises (at the base of a terrace, projecting from the next shelf to it,) ninety feet (barometrically measured) above the level of that shelf to which it may be said to belong. In the east arm of Glen Turet, the upper shelves in a like manner terminate in slopes, which rise above their proper levels.

6. Geological Transactions, vol. iv. (First Series), p. 320–338, and 387.

7. Edinburgh Royal Transactions, vol. ix. p. 12.

8. A metamorphic rock containing feldspar, with a streaked or banded appearance, and having visible crystals.†

9. After the elaborate arguments given by MacCulloch, to show that no sudden rush of water, or debacle, could have formed the shelves, I should not have offered any remarks on this point, had not so distinguished a person as Sir George MacKenzie (London and Edinburgh Philosophical Magazine, December 1835,) suggested such an hypothesis, without, however, it is fair to add, having visited the district. Each of the ten thousand pebbles, which together form any one buttress or little delta, and which, it is evident, were accumulated by the action of one streamlet, at the spot where it entered the expanse of ancient water,—each of these pebbles required time for its attrition,—each now plainly speaks against such an hypothesis.

10. I was informed, but whether correctly I do not know, that the hamlet (in the middle of which there is a mound with a round tower on it,) on the opposite side of the valley, and a mile or two south of Invergarry, was named Kilfinnin. I therefore shall denominate the small stream which flows towards the Caledonian Canal at that point by this name. In the same manner I shall call the larger stream which debouches by Habercalder, and its valley, by that name, not having been able to learn any more proper one.

11. The pass of Muckul, described by Sir Lauder, which separates the waters of the Spean from a branch of the Spey, I did not visit.

12. I have not thought it worth while to enter into all the possible cases of this hypothesis, but have merely taken the most obvious one, which was assumed by Sir Lauder. If any one has the boldness to come forward from the obscurity of past times, and state his belief that the broad barrier of the Spean was *erected* as well as removed altogether subsequently to the removal of the two barriers of Glen Roy, then the objection from the uniform breadth of the 972-feet shelf, where crossing the spot which must have been occupied by the barrier of Loch Roy, has less weight, but the other part of the argument remains valid. Again, on the hypothesis in the text, I have not entered into all the possible alternatives of the manner in which the bases of the Loch Roy barriers might have been removed, either when Loch Roy itself, or when Loch Spean was drained, or at some subsequent period by unknown causes connected with the drainage of the imaginary lakes.

13. It should be remembered that it is far easier to assert than to disprove. If to explain some phenomenon it was stated that the Thames near London was formerly crossed by a barrier some hundred feet in height, of which it was not pretended a vestige now remained, it is difficult to imagine what kind of evidence would be sufficient to prove the hypothesis false, as long as any one was found willing to admit such an assumption.

14. I may add, the same number of barriers are requisite, whether we suppose the existence of one, two, three, or as many lakes as glens; and the argument against MacCulloch's hypothesis of one lake, and against that of the separate lakes by Sir Lauder, are applicable to any hypothesis requiring an intermediate number.

15. Geological Transactions, vol. iv. p. 378.

16. Sir Lauder has represented three in his map (Edinburgh Royal Transactions) by the figures 5, 6, and 7. I cannot, however, by any means agree with him in the limits thus assigned to them. Is it meant to be asserted, that there is any barrier perfect, with the exception of such a gorge as the river is now cutting, at the lower end of number (7), on a level with the line at its upper extremity; or so nearly so as to allow of the upper part being considered as a supralittoral delta? Such did not by any means appear to me to be the case. Was not the barrier only supposed to have existed, as in the theory of the shelves? I must also observe that the fringe or deposit does not terminate a little way within the mouth of the Roy, as represented by the line marked (7). It appears to me unfortunate that Sir Lauder marked the limits of these deposits, which are accumulated in a *gentle slope*, in a similar manner as he has done the shelves, which are *horizontal*. Any one would suppose the lines 5, 6, and 7 were horizontal, like those marked 1, 2, 3, and 4. This difference alone indicates a corresponding one in their origin, as will hereafter be attempted to be shown.

17. Edinburgh New Philosophical Journal, vol. xxv. p. 376.

18. Edinburgh New Philosophical Journal, vol. xxv. p. 386 and 387. The elevated shells at Banff were observed by Mr. Prestwich, Proceedings of Geological Society, May, 1837.

19. Proceedings of the Geological Society, 1838, p. 669. I was informed by an intelligent quarryman that he had observed many broken sea shells in a gravel-pit, about two miles north of Grant Town, on the roadside to Forres, and therefore eighteen miles from the nearest sea-coast.

20. Edinburgh Philosophical Journal, p. 388.

21. An excellent summary of the argument is given by Mr. Lyell in his Elements of Geology, chap. v.

22. See p. 53, "On the Connexion of Certain Volcanic Phenomena in South America. . . ."†

23. Until I saw this shelf I doubted its existence, because I had not been able to discover others mentioned by MacCulloch: thus one is figured by him in a ravine branching from Glen Roy (improperly called by him Glen Fintec), which, though having ascended it, I was unable to see. Again MacCulloch states, that two shelves occur in Glen Gluoy, whilst Sir Lauder Dick, who seems to have examined most carefully this glen, could find only one. I may here remark, that should two shelves be hereafter discovered there at the same relative height from each other with those of Glen Roy, and this is stated to be the case by MacCulloch, the fact would be highly satisfactory on the theory of the shelves having been sea-beaches. From an excellent point of view, however, on the side of Ben Erin I could see no trace of a second shelf. MacCulloch also figures a supernumerary shelf at a point north-west of the houses of Glen Turet, at a level above that of the upper shelf of Glen Roy; a mound of alluvium, above, and nearly parallel to the shelf, certainly occurs there; but from the want of sharpness of outline, I should be unwilling to pronounce that it had formed a line of beach, although I should be far from feeling any surprise if this could be shown to have been the case.

24. Edinburgh New Philosophical Journal, vol. xxv. p. 380.

25. Transactions of the Royal Society, 1836, p. 11. and 15.

26. Mr. Lyell has had the kindness to give me the following observations on this point.

> In the country surrounding the fiord of Christiania, especially between Christiania and Dramman, and between Dramman and Holmstrand in Norway, deposits of clay and sand rest in horizontal beds on the gneiss, granite, porphyry, and other rocks. Large masses of this sand and clay reach in some places to elevations of more than 600 feet above the level of the sea, and nearly fill many upland valleys; but it is only in those patches which occur at the height of about 200 feet, and usually less than fifty feet above the sea, that shells (all of recent species) have been found. This sand and clay appear to have accumulated on the older rocks during their gradual upheaval from beneath the sea, so that greater elevation becomes a test of higher antiquity, and those patches which are found at small heights near the borders of the present fiord are very modern. Even in these last the shells are often in so advanced a state of decomposition as greatly to favour the theory that a more considerable lapse of time might be sufficient to obliterate all traces of their existence. Thus for example, on the banks of a small river about two miles above Töusberg at the place where the bridge crosses it, a section of loamy clay is laid open, the lowest part of which cannot be raised more than a few feet above the salt water of the fiord of Christiania. In the upper part of the mass for a thickness of fifteen feet no fossils can be detected, but somewhat lower faint casts of the *Mytilus edulis*, chiefly indicated by purple stains, are observable. Still lower down more perfect specimens of the same shell, together with *Cardium edule*, occur, but both in so soft a state as to crumble into dust when dried. With these the more solid *Cyprina islandica* and *Saxicava rugosa* are occasionally found, and although soft when first taken from the matrix are capable when dried of being preserved entire. If in the short period which has probably passed away since these shells near Töusberg were imbedded, the progress of decay can have proceeded so far, we may well suppose the percolation of water during antecedent ages of indefinite extent to have destroyed all signs of fossils in the more ancient and elevated patches of loam found more than 500 feet high in the adjacent hilly country.

27. I may observe that it very frequently happens, that shells are found only at some depths in these superficial deposits: this is the case in several of the gravel pits in Shropshire; in cutting the canal at the head of Loch Ness, the shells were met with at the bottom, whereas, the layers nearer the surface, as I can vouch, contain none. Mr. Smith speaking (p. 380 and 391. vol. xxv. Philosophical Journal,) of the clay beds on the west coast of Scotland, says, that the marine remains with which it abounds "are almost invariably found in the lower part of the bed." I infer that in all these cases shells originally existed in the upper part, but have since decayed: Mr. Smith, however, offers a different explanation. In the extensive and superficial beds of elevated shells on the coast of Peru, where rain does not fall, and where consequently loose matter is not washed from the surface, I have traced as I have ascended from the beach a most perfect gradation in the decay of the shells, until a mere layer of calcareous powder, without a vestige of structure, alone remained.

28. Edinburgh Transactions, vol. ix. p. 35.

29. I have before alluded to the fewness of the well-rounded pebbles near the upper shelves, excepting at the heads of the valleys, or on flat places. This is a difficulty; though it is one common to many regions, where we know that much denudation has taken place at some period or at another. Pebbles of most rocks may in the course of time decay, but those of quartz I should think (although Scoresby says this rock yields to the frosts of Spitzbergen) would be imperishable: if so, how comes it that quartz pebbles are not scattered over the surface of every mountain in which that rock is present, and in which the form of the land, its denuded state, or the presence of truncated dikes show that it must once, although perhaps countless ages since, have been beaten by the waves of the sea? Such pebbles, however, are not found on every mountain thus circumstanced: the explanation, I presume, rests in this; that every cause of disturbance, wind, rain, earthquakes and the fall of fragments all tend to move the pebbles in one direction alone, namely downwards. I am inclined to believe this view is correct, and that in the course of time, such pebbles are all rolled down, from having found on an isolated mountain of quartz in South America (the Sierra Ventana) a superficial patch of conglomerate, like part of an old beach, which seemed solely to owe its preservation to the pebbles having been cemented to the parent rock by oxide of iron, in the same manner as not unfrequently may be observed on some existing sea beaches. In the case of the shelves of Lochaber, it is probable, that only a few pebbles were originally formed, owing to the small power of the waves on the steep and protected shores of these ancient sounds.

30. This is a similar fact to what has been observed on the Jura. Sir James Hall (Edinburgh Royal Transactions, vol. vii. p. 143.) says "it is principally where the snowy summits are visible from the face of the Jura by means of some depression in the intervening hills, that we find these travelled masses."

31. I refer, of course, only to the more temperate and central parts of Europe, but it appears that boulders are sometimes transported in these regions, even at the present time. Sir James Hall, in his Memoir on the "Revolutions which have affected the surface of the earth" (Edinburgh Transactions of the Royal Society, vol. vii. p. 157.), states that in the Solway Firth (and therefore in salt water) "a large block of stone, four or five feet in diameter, lying within high-water mark, and well known as having served as the boundary of two estates, was during a stormy night in winter transported ninety yards, and the persons on the spot were convinced that this migration was performed by means of a large cake of ice, formed round the stone, and attached to it, and that the whole had been lifted and carried forward by the rising tide. The course of this stone was marked upon

the sand below by a deep and broad furrow, which remained visible for a long time afterwards, as I have been informed by several members of the Society, who saw it after an interval of more than a year." I presume from the position of the stone as a land-mark, and from the distance it was transported by the *rising* tide, that the furrow left by its passage must have been either oblique or parallel to the shore. What would have been the effect if this large and heavy block had been pushed over a surface of solid rock instead of sand? This question will recall to the mind of those who have read the late papers of Messrs. Charpentier, Venetz, and Agassiz, the case of the longitudinally and obliquely scratched rocks of the Alps. In the Addenda to my Journal during the voyage of the Beagle, I have endeavoured to show that the passage of ice, with imbedded fragments of rock, acting at successive levels on the surface of shoals during the gradual rising of the land, offers the most probable explanation of the scratches and grooves, which have justly excited so much attention in Scotland and other places.

32. It is scarcely possible to convey by language any accurate idea of the kind of inequalities which, from the shelves passing over them and into the intervening hollows, we know were so left by the sea. I hope any one who feels interested on this subject, will carefully examine the plates accompanying Sir Lauder Dick's paper (Edinburgh Transactions, vol. ix.), and especially Plate IV. The shelves on the left side (looking up the glen) bend into all the principal gullies; and on the right side, directly in front of the foreground, by looking close at the plate they will be seen to curve a little way into each of the perpendicular furrows (some thinning out and others commencing), the bottoms of which have evidently been much deepened by the descending streamlets. The idea given by these plates of the state of surface in these mountains, and of the manner in which the shelves bend round the headlands and enter the gullies, appears to me exceedingly faithful; although the glen itself, as represented, is too narrow and profound, and the sides much too steep. To view this Plate is a lesson full of instruction to the geologist, for he will scarcely fail to be astonished when he sees that the drawing is characteristic of any ordinary valley in a mountainous country, and at the same time to find himself compelled to admit, that even the little furrows, which it might be thought had been formed but yesterday, must have owed their origin, at least in great part, to the successive coves or indentations, continued one below another on ancient sea-beaches.

33. Edinburgh Transactions, vol. ix. p. 26.

34. Consult Professor Phillip's interesting paper on this subject. Geological Proceedings, vol. i. p. 323. April, 1831.

35. Edinburgh Transactions, vol. ix. p. 8.

36. I may here remark, that the equal elevation of the west coast of Scotland, and indeed of the whole British Islands and other parts of Europe, may be inferred from the facts collected by Mr. Smith in his paper in the Edinburgh New Philosophical Journal. This author says (vol. xxv. p. 388.), "The great terrace (known to be of marine origin from the presence of organic remains), the base of which seems *very generally* to be between *thirty and forty feet* above the sea, forms a marked feature in the scenery of the west of Scotland."

37. Book II. chap. xvii. On the gradual rise of the land in Sweden.

38. Transactions of the Royal Society, Part I. 1835, p. 33.

39. Considering the great importance of this conclusion, and the many points of interest connected with the subject of the 'parallel roads,' it is greatly to be desired that the admirable opportunity for a close examination, afforded by the intended Ordnance Survey, will be taken advantage of by the gentlemen, so well qualified for the task, who conduct it.

40. In my Journal during the voyage of the Beagle, I have mentioned (p. 431 and 432.) some instances of this.
41. See p. 53, "On the Connexion of Certain Volcanic Phenomena in South America . . . ."†
42. Principles of Geology, Book II. chap. xviii. p. 311. 5th edit.
43. Cabinet Cyclopedia. Astronomy, p. 120.
44. Illustrations of Huttonian Theory.
45. Illustrations of the Huttonian Theory, p. 488.

Note on a Rock Seen on an Iceberg in 61° South Latitude[1]

Having been informed by Mr. Enderby, that a block of rock, embedded in ice, had been seen during the voyage of the schooner *Eliza Scott* in the Antarctic Seas, I procured through his means an interview with Mr. Macnab, one of the mates of the vessel, and I learnt from him the following facts:—On the 13th March, when in lat. 61° S., and long. 103° 40′ E., a black spot was seen on a distant iceberg, which, when the vessel had run within a quarter of a mile of it, was clearly perceived to be an irregularly-shaped but angular fragment of dark-coloured rock. It was embedded in a perpendicular face of ice, at least 20 feet above the level of the sea. That part which was visible, Mr. Macnab estimated at about 12 feet in height, and from 5 to 6 in width; the remainder (and from the dark colour of the surrounding ice, probably the greater part) of the stone was concealed. He made a rough sketch of it at the time, as represented at p. 138. The iceberg which carried this fragment was between 250 and 300 feet high.

Mr. Macnab informs me, that on one other occasion (about a week afterwards) he saw on the summit of a low, flat iceberg, a black mass, which he thinks, but will not positively assert, was a fragment of rock. He has repeatedly seen, at considerable heights on the bergs, both reddish-brown and blackish-brown ice. Mr. Macnab attributes this discolouration to the continued washing of the sea; and it seems probable that decayed ice, owing to its porous texture, would filter every impurity from the waves which broke over it.

Every fact on the transportation of fragments of rock by ice is of importance, as throwing light on the problem of 'erratic boulders,' which has so long perplexed geologists; and the case first described possesses in some respects peculiar interest. The part of the ocean, where the iceberg was seen, is 450 miles distant from *Sabrina* land[2] (if such land exists), and 1400 miles from any certainly known land. The tract of sea, however, due S., has not been explored; but assuming that land, if it existed there, would have been seen at some leagues distance from a vessel, and considering the southerly course which the schooner *Eliza Scott* pursued immediately prior to meeting with the iceberg, and that of Cook in the

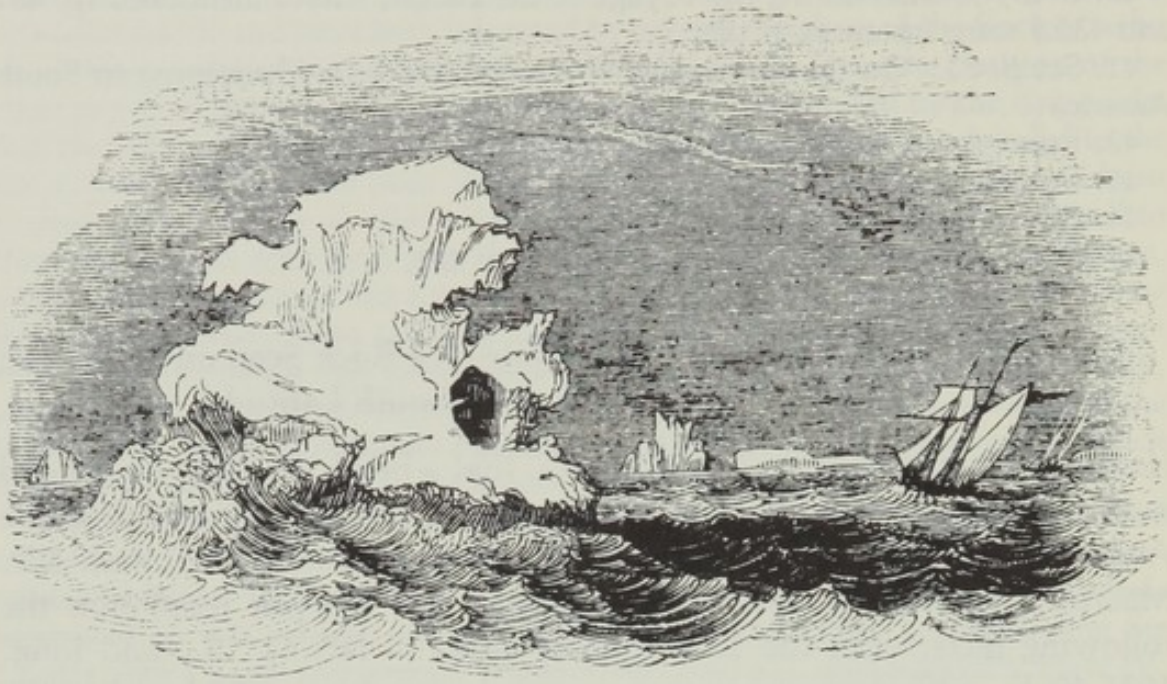


Fig. 1. Rock embedded in iceberg. Rock about 12 feet in height, one-third of the distance up a 300-foot high iceberg.

year 1773, it is exceedingly improbable that any land will hereafter be discovered within 100 miles of this spot. The fragment of rock must, therefore, have travelled at least thus far from its parent source; and, from being deeply embedded, it probably sailed many miles farther on before it was dropped from the iceberg in the depths of the sea, or was stranded on some distant shore. In my Journal, during the voyage of H.M.S. *Beagle*, I have stated (p. 282), on the authority of Captain Biscoe, that, during his several cruises in the Antarctic Seas, he never once saw a piece of rock in the ice. An iceberg, however, with a considerable block lying on it, was met with to the E. of South Shetland, by Mr. Sorrell (the former boatswain of the *Beagle*), when in a sealing vessel. The case, therefore, here recorded is the second; but it is in many respects much the most remarkable one. Almost every voyager in the Southern Ocean has described the extraordinary number of icebergs, their vast dimensions, and the low latitudes to which they are drifted: Horsburgh[3] has reported the case of several, which were seen by a ship in her passage from India, in lat. 35° 55′ S. If then but one iceberg in a thousand, or in ten thousand, transports its fragment, the bottom of the Antarctic Sea, and the shores of its islands,[4] must already be scattered with masses of foreign rock,—the counterpart of the "erratic boulders" of the northern hemisphere.

1. *Journal of the Royal Geographical Society of London* 9(1839):528–29.†
2. Antarctica.†
3. Philosophical Transactions, 1830, p. 117.

4. M. Cordier, in his instructions (L'Institut, 1837, p. 283) for the voyage of the *Astrolabe* and *Zélée*, says, that the shores of South Shetland were found, by the naturalist of an American expedition in 1830, covered with great erratic boulders of granite, which were supposed to have been brought there by ice. It is highly desirable that this fact should be inquired into, if any opportunity should hereafter occur.

On a Remarkable Bar of Sandstone off Pernambuco, on the Coast of Brazil[1]

In entering the harbour of Pernambuco, a vessel passes close round the point of a long reef, which, viewed at high water when the waves break heavily over it, would naturally be thought to be of coral-formation, but when beheld at low water it might be mistaken for an artificial breakwater, erected by cyclopean workmen. At low tide it shows itself as a smooth level-topped ridge, from thirty to sixty yards in width, with even sides, and extending in a *perfectly* straight line, for several miles, parallel to the shore. Off the town it includes a shallow lagoon or channel about half a mile in width, which further south decreases to scarcely more than a hundred yards. Close within the northern point ships lie moored alongside the reef to old guns let into it.

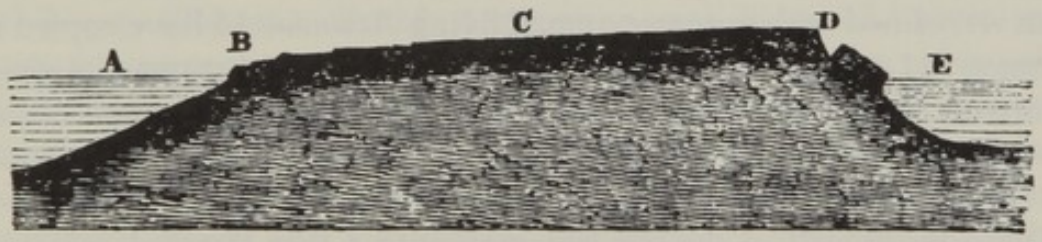


Fig. 1. Transverse section of Pernambuco Bar; vertical heights considerably exaggerated. A, level of the sea at low water; B, subsided masses, thickly coated with Serpulae, &c.; C, summit of the bar, which generally slopes a little seaward; but the slope in the woodcut has been unintentionally somewhat increased; D, subsided masses of bare sandstone; E, surface of the harbour or lagoon.

The accompanying woodcut represents, at low water spring tides, a transverse section of the northern part of the bar, where a section of about seven feet in height is exhibited on the inner side. It consists of a hard pale-coloured sandstone, breaking with a very smooth fracture, and formed of siliceous grains, cemented by calcareous matter. Well-rounded quartz pebbles, from the size of a bean, rarely to that of an apple, are imbedded in it, together with a very few fragments of shells. Traces of stratification are obscure, but there was an included layer in one spot of stalactitic limestone, an eighth of an inch in thickness. In another place some false strata, dipping landwards at an angle of 45°, were capped by a

horizontal mass. On each side of the ridge quadrangular fragments have subsided, as shown in the woodcut; and the whole mass is in some places fissured, apparently from the washing out of some soft underlying bed. One day, at low water, I walked a full mile along this singular, smooth, and narrow causeway, with water on both sides of me, and could see that for nearly a mile further its form remained unaltered. In Baron Roussin's[2] beautiful chart of Pernambuco (*Le Pilote du Brésil*) it is represented as stretching on, in an absolutely straight line, for several leagues; how far its composition remains the same, I know not; but from the accounts I received from intelligent native pilots, it seems to be replaced on some parts of the coast by true coral reefs.

The upper surface, though on a large scale it must be called smooth, yet presents, from unequal disintegration, numerous small irregularities. The larger imbedded pebbles stand out supported on short pedestals of sandstone. There are, also, many sinuous cavities, two or three inches in width and depth, and from six inches to two feet in length. The upper edges of these furrows sometimes slightly overhang their sides; they end abruptly, but in a rounded form. One of the furrows occasionally branches into two arms, but generally they are nearly parallel to each other, and placed in lines transverse to the sandstone ridge. I know not how to account for their origin, without they be formed by the surf, as it daily breaks over the bar, washing to and fro pebbles in depressions, originally only slight. Opposed to this notion is the fact, that some of them were lined with numerous small living *Actineae*.[3] I have copied this passage, as I at the time wrote it, because furrows of a somewhat similar nature on the surface of rocks have lately received much attention, and are supposed invariably to indicate the former action of a waterfall, over the edge of a moving glacier.

The exterior part of the bar is coated with a thin layer of calcareous matter; this, on the outer subsided masses, which can only be reached between the successively breaking waves at low water, is so thick, that I could seldom expose the sandstone with a heavy hammer. I procured, however, some fragments where the layer was between three and four inches in thickness; it consists chiefly of small *Serpulae*,[4] including some *Balani*, and a few very thin paper-like layers of a *Nullipora*.[5] The surface alone is alive, and all within consists of the above organic bodies filled up with dirty white calcareous matter. The layer, though not hard, is tough, and from its rounded surface resists the breakers. Along the whole external margin of the bar, I only saw one very small point of sandstone which was exposed to the surf. In the Pacific and Indian Oceans the outer and upper margin of the coral reefs are protected, as will be described in a forthcoming work,[6] by a very similar coating; but there it is almost exclusively formed of several species of *Nulliporae*. Lieut.

Nelson, in his excellent memoir on the Bermudas (Geol. Trans., vol. v. part 1. p. 117), has described reefs, formed, as he states, but I cannot avoid suspecting only coated, by similar masses of *Serpulae*. I inquired from some old pilots, whether there was any tradition of change in the form and dimensions of this sandstone bar; but they were unanimous in answering me in the negative. It is astonishing to reflect, that although waves of turbid water, charged with sediment, are driven night and day, by the ceaseless tradewind, against the abrupt edges of this natural breakwater, yet that it has lasted in its present perfect state for centuries, or more probably thousands of years. Seeing that the surface on the inner side does gradually wear away, as shown by the pebbles on the sandstone pedestals, this durability must be entirely owing to the protection afforded by the thin coating of *Serpulae* and other organic beings: it is a fine example, how apparently inefficient, yet how effectual, are the means of preservation, like those of destruction, which nature employs.

I believe similar bars of rock occur in front of some of the other bays and rivers on the coast of Brazil: Baron Roussin states that at Porto Seguro there is a "*quay*" similar to that of Pernambuco. Spaces of several hundred miles in length on the shores of the Gulf of Mexico, the United States, and southern Brazil are formed by long narrow islands and spits of sand, including very extensive shallow lagoons, some of which are several leagues in width. The origin of these linear islets is rather obscure: Prof. Rogers (Report to British Association, vol. iii. p. 13.) gives some reasons for suspecting that they have been formed by the upheaval of shoals, deposited where currents met. These phaenomena, it is very probable, are connected in their origin with the same causes which have produced the remarkable bar of sandstone off Pernambuco. The town of Pernambuco stands on a low narrow islet and on a long spit of sand, in front of a very low shore, which is bounded in the distance by a semicircle of hills. By digging at low water near the town the sand is found consolidated into a sandstone, similar to that of the breakwater, but containing many more shells. If, then, the interior of a long sandy beach in one part, and in another the nucleus of a bar or spit extending in front of a bay became consolidated, a small change, probably of level, but perhaps simply in the direction of the currents, might give rise, by washing away the loose sand, to a structure like that in front of the town of Pernambuco, and along the coast southward of it; but without the protection afforded by the successive growth of organic beings, its duration would be short, if indeed it were not destroyed before being completely exhibited.

1. *London, Edinburgh, and Dublin Philosophical Magazine and Journal of Science* 19(1841):257–60.†

2. Albin René Roussin, *Le Pilote du Brésil, ou Description des côtes de l'Amérique Meridionale* . . . (Paris: impr. royale, 1826).†
3. Sea anemones.†
4. Marine annelid worms that build twisted calcareous tubes.†
5. Lime-secreting algae; previously thought to be animals.†
6. Charles Darwin, *The Structure and Distribution of Coral Reefs. Being the First Part of the Geology of the Voyage of the Beagle* (London: Smith, Elder, 1842).†

Humble-Bees[1]

Perhaps some of your readers may like to hear a few more particulars about the humble-bees which bore holes in flowers, and thus extract the nectar. This operation has been performed on a large scale in the Zoological Gardens:—Near the refection-house there is a fine bed of Stachys coccinea, every flower in which has one, and sometimes two, small irregular slits, or orifices, on the upper side of the corolla near its base. I observed some plants of Marvel of Peru, and of Salvia coccinea, with holes in similar positions; but in Salvia Grahami they were without exception cut through the calyx, which is in this species elongated. The tubular corolla of Pentstemon argutus is rather broader than in the above flowers, and two holes are always bored in it by the side of each other, and just above the calyx. All these orifices are so small that they might easily be overlooked; I first noticed them a week since, when, from the brown colour of their edges, they appeared to have been made some time before. The beds of Stachys and Pentstemon are frequented by numerous humble-bees of many very different kinds; at one moment I saw between twenty and thirty round a bed of the latter flower; they fly very quickly from flower to flower, and always alight with their heads just over the little orifices, into which they most dexterously insert their proboscis and in the case of the Pentstemon, first into the orifice on one side and then into the other, so that they thus extract the nectar on both sides of the germen.[2] Besides the humble-bees, I saw some hive-bees on the Pentstemon; they were, however, much less dexterous, and generally alighted across the flower, or on the calyx, and thus lost time. The orifices in all the above-mentioned flowers are made on the upper side of the corolla: I was, therefore, surprised to find, close by, a large bed of the common Antirrhinum in which all the flowers had one or two irregular slits, or holes, on the under side of the corolla at its base, close to the small protuberance which represents the spur in Linaria, and therefore directly in front of the nectary at the foot of the germen. From the position of these orifices they cannot be seen without turning up the flower; but the humble-bees seemed to understand this method of picking pockets full as well as the other, and never hesitated where to go, but quickly flew from

the under side of one flower to that of another. Now I can speak positively, as far as the experience of part of two summers goes, that country humble-bees are not so cunning, and invariably crawl into the flower by forcing open the elastic lower lip; and a very pretty spectacle it is to watch them. All the flowers of Salvia Grahami and the Antirrhinum which I looked at in different parts of the garden, were bored; and out of the many hundreds in bloom in the two large beds of Stachys and Pentstemon, I could not find one without its little orifice, nor did I see one bee crawl in at the mouth. Nevertheless I found, and the fact appears to me very curious, two separate plants of the Stachys coccinea, and one large one of the Pentstemon argutus, with all their flowers unbored; from the scratches on the lower lip of the flowers of two former plants I have no doubt that many bees had entered in the usual way, and I actually saw one bee crawling into the flowers of the Pentstemon. One is tempted to conjecture that in these plants each humble-bee as it came, not finding a hole ready cut, thought it less trouble to extract the nectar by the mouth than to make one; but that on the beds of the same flowers, where very many bees were rivalling each other in getting honey, some few set to work boring holes, and others copied the example. From the comparative fewness of the hive-bees on the Pentstemon, their evident awkwardness in finding the orifices, and the smallness of their mandibles, I can hardly doubt they were profiting by the workmanship and the example of the humble-bees: should this be verified, it will, I think, be a very instructive case of acquired knowledge in insects. We should be astonished did one genus of monkeys adopt from another a particular manner of opening hard-shelled fruit; how much more so ought we to be in a tribe of insects so pre-eminent for their instinctive faculties, which are generally supposed to be in inverse ratio to the intellectual! Moreover, from what I have above stated regarding the Antirrhinum, I much suspect that the practice of boring holes in its flowers is likewise a piece of acquired knowledge, whether the Humble-bees do it instinctively or not in other cases. Although I have said that country humble-bees appear to be less cunning than London ones, yet I confess I saw this June, in Staffordshire, some in the act of cutting holes at the base of the corolla of the Rhododendron azaleoides; the greater number entered the mouth of the corolla, as indeed was evident from the quantity of pollen on the stigma, brought by the bees from neighbouring Azaleas—this hybrid not having a single grain of pollen of its own. One bee was seen which entered the mouth of some of the flowers and cut holes in others; this shows that the orifices are made simply to save trouble, and not because the bee cannot extract the nectar from the long tube. In the Stachys and Pentstemon it is also evident that the bees cut the holes because they can fly much quicker from the upper surface of one flower to that of another, than clamber in

on the fringed edge of the lower lip. I have no doubt by this means they are able to visit twice the number of flowers in the same time. Your correspondent (p. 485)[3] says that the Honeysuckle is sometimes bored; I never happened to notice this, but I have seen pollen-gathering humble-bees show much skill in forcing open the yet-closed mouth of the young flowers and extracting the pollen; flowers which had been open, apparently even for a day, they at once passed over, whereas the nectar-seeking humble-bees stopped at them. If the mouth of the flower was absolutely close, without any one segment having started, the bees from the difficulty of the attempt immediately gave it up. Your correspondent attributes the failure of his Bean-crop to the apertures made by the bees; but when we remember how the petals of many flowers may be manipulated in hybridising them, without preventing their fructification, we may well doubt this view. But I conceive they may be indirectly the cause of the crop failing, not by their making the orifice, but by their not extracting the nectar in the manner nature intended them; for I have observed that when papilionaceous flowers are mature, (and actually in the case of the Bean,) bees alighting on the wing-petal, as they always do to reach the nectar at the base of the standard-petal, depress the wing-petals together with the keel, by which movement the grains of pollen together with the stigma are forced out, and both rubbed against one side of the bee's body, already generally well dusted with the pollen of other flowers of the same species. If all those flowers, even hermaphrodite ones, which are attractive to insects, almost necessarily require their intervention, as is supposed with much probability by Christian Sprengel (Entd. Geheim.),[4] to remove the pollen from the anthers to the stigma, what unworthy members of society are these humble-bees, thus to cheat, by boring a hole into the flower instead of brushing over the stamens and pistils, the, so imagined, final cause of their existence! Although I can believe that such wicked bees may be injurious to the seedsman, one would lament to see these industrious, happy-looking creatures punished with the severity proposed by your correspondent.[5] Moreover, the florist, I believe, ought rather to praise them for this ingenious method of obtaining the nectar, instead of by the old-fashioned natural one; for let him look how torn and scratched the lower petals of some flowers are—for instance, those of the Mimulus roseus, and the wing-petals of some Everlasting-peas. The little orifice which the bees make, in order to avoid clambering in at the mouth, is hardly visible; whereas all the flowers in some beds of the Mimulus, at the Zoological Gardens, are sadly defaced. Let any one who doubts the use of bees in the fructification of hermaphrodite flowers, watch and admire the manner in which the flat surface of the divided stigma of this Mimulus licks the back of the entering bees, which is generally well dusted already with pollen;

and then how admirably the two divisions of the stigma endowed with a sensitive faculty, close like a forceps on the included granules of pollen! I will only farther remark, that after the facts here noticed, one may well doubt C. K. Sprengel's view, that the streaks and spots of colour (saft-maal) on the corolla of most nectariferous flowers, serve as guides to insects, that they may readily find out where the nectar-vessel lies. I think the bees which flew so quickly from flower to flower on the under sides of the Antirrhinum, or those which bored the pair of holes on the Pentstemon, or those which bored through calyx and corolla in the Salvia, would tell Mr. Sprengel, that although he might want such aids, they did not. I know hardly any flower which bees open and insert their proboscis into, more rapidly, than the common tall Linaria, which has a little purplish well-closed flower; I have watched one humble-bee suck twenty-four flowers in one minute; yet on this flower there are no streaks of colour to guide these quick and clever workmen.

1. *Gardeners' Chronicle and Agricultural Gazette*, no. 34, 21 August 1841, p. 550.†
2. Ovary.†
3. Ruricola, "Humble-Bees," *Gardeners' Chronicle*, no. 30, 24 July 1841, p. 485.†
4. Christian Konrad Sprengel, *Das entdeckte Geheimniss der Natur im Bau und in der Befruchtung der Blumen* (Berlin: Friedrich Vieweg, 1793).†
5. Ruricola recommended destroying "the humble-bees' nests at the end of the summer, and [employing] children to catch and kill the females . . . as soon as the first blossoms have expanded."†

On the Distribution of the Erratic Boulders and on the Contemporaneous Unstratified Deposits of South America[1]

1. Description of the Boulder Formation in the Valley of the Santa Cruz p. 145
2._____in Tierra del Fuego and the Strait of Magellan p. 149
3._____in the Island of Chiloe p. 154
4. Remarks on the Glaciers of Tierra del Fuego and on the transportal of Boulders p. 158

1. Boulder Formation in the Valley of the Santa Cruz

During the survey of the shores of South America, southward of the Rio Plato, by Capt. FitzRoy in H.M.S. Beagle, I did not meet with any boulders on the eastern plains of the continent until we arrived on the banks of the river Santa Cruz, in lat. 50° 10′ S. Nor did they occur there near the coast, but were first noticed in ascending the river at the distance of about 100 geographical miles from the Atlantic, and 67 from the

nearest slope of the Cordillera. Twelve miles further west, in lon. 70° 50′ W., that is, fifty-five miles from the mountains, they were extraordinarily numerous; consisting of compact clay-slate, feldspathic rock, a quartzose chloritic schist, and basaltic lava; and they were generally of an angular form, and many of them resembled fragments of rock at the foot of a precipice. The size of some was immense: I measured a square one of chloritic schist, which was five yards on each side and projected five feet above the ground; a second, which was more rounded, was sixty feet in circumference, and stood six feet above the ground; how much of each was buried beneath the surface I could not ascertain. There were innumerable other fragments from two to four feet square. The vast open plain on which they lay scattered, is 1400 feet above the level of the sea; its surface is somewhat but not greatly irregular, and the inequalities appear to have been caused chiefly by the denudation of loose matter from an irregular field and hummocks of lava. The plain slopes very gently and with much regularity to the Atlantic, where the sea-cliffs are about 800 feet high; it rises somewhat more abruptly towards the Cordillera, near which its height is above 3000 feet. The Cordillera in this latitude are not very lofty, the highest pinnacle being only 6400 feet above the level of the sea.

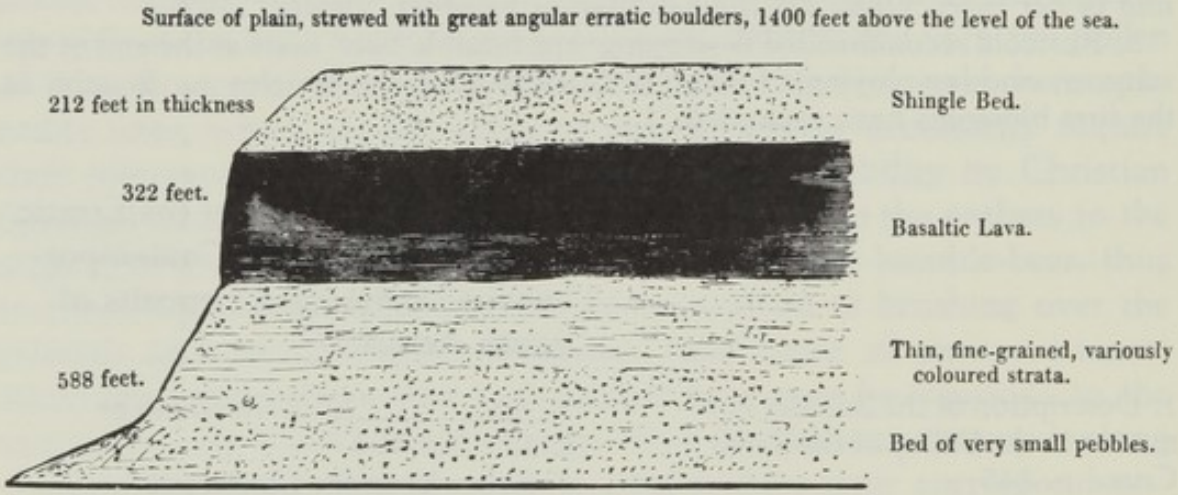
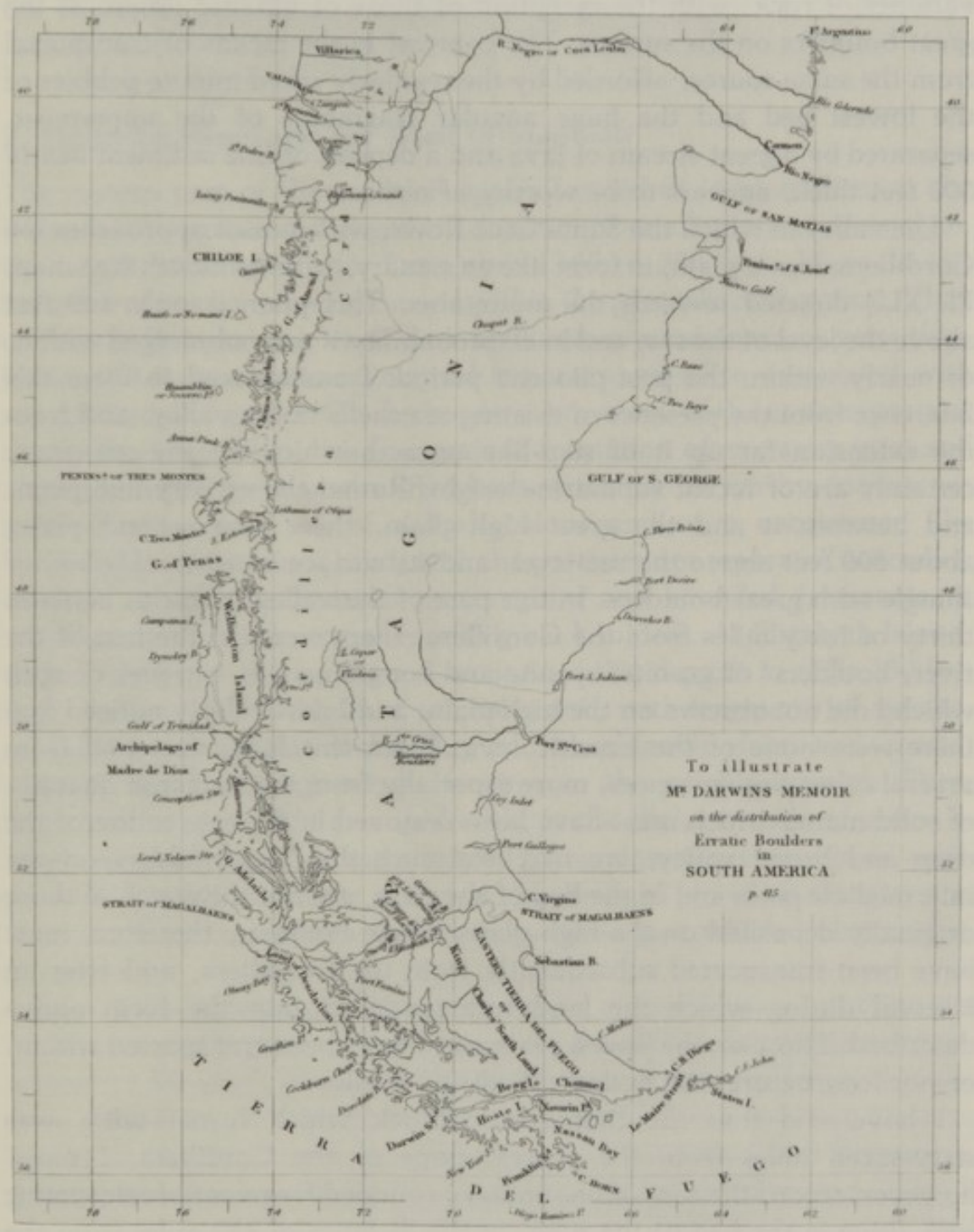


Fig. 1. River Santa Cruz, section 280 feet above level of sea.

The accompanying section (Fig. 1.), exhibited on the banks of the river in the longitude above referred to, will give a sufficient idea of the composition of the plain on which the boulders lie. The upper bed is 212 feet in thickness, and exhibits indications of being coarsely stratified. It is composed of well-rounded shingle[2] with great angular blocks strewed on the surface, and probably imbedded (for owing to the state of the section I was unable to ascertain this point) in the whole upper part of the stratum. The shingle bed is continued without interruption to the coast, and is there certainly of submarine origin. From its general



Pl. XL. Map to illustrate distribution of erratic boulders in South America.

similarity throughout this space, I have no reason to doubt that the whole was accumulated under similar circumstances. The lowest bed represented in the section is composed of minute pebbles of the same varieties of rock, with the exception of those of basaltic origin, as the great boulders on the surface. The contrast in the means of transportal from the same source, afforded by the regularly sifted minute pebbles of the lowest bed and the huge angular fragments of the uppermost, separated by a great stream of lava and a deposit of fine sediment nearly 500 feet thick, appears to be worthy of notice.

The valley in which the Santa Cruz flows, widens as it approaches the Cordillera, into a plain, in form like an estuary, with its mouth (see map, Pl. XL.) directed towards the mountains. This plain is only 440 feet above the level of the sea, and in all probability it was submerged within, or nearly within, the post-pliocene period. I am induced to form this inference from the presence of existing sea shells in the valley, and from the extension far up it of step-like terraces which on the sea-coast, certainly are of recent submarine origin. Round the estuary-like plain, and between it and the great high plain, there is a second plain, about 800 feet above the sea-level, and its surface consists of a bed of shingle with great boulders. In this part of the valley, namely, between thirty or forty miles from the Cordillera, there were, in the bed of the river, boulders[3] of granite, syenite and conglomerate, varieties of rock which I did not observe on the high plain; and I particularly noticed that there were none of the basaltic lava. From this latter fact and from several other circumstances, more especially from the immense quantity of solid matter which must have been removed in the excavation of the deep and broad valley, we may feel sure that the boulders on the intermediate plain and in the bed of the river, are not the wreck of those originally deposited on the high plain. These boulders, therefore, must have been transported subsequently from the Cordillera, and after an interval during which the land was modelled into the form above described. Those on the lowest plain must have been transported within, or not long before, the period of existing shells.

I have said that the first erratic block which I met with, was sixty-seven miles from the nearest slope of the Cordillera; I must, however, record the case of one solitary rounded fragment of feldspathic rock lying in the bed of the river, at the distance of 110 miles from the mountains. This fragment was seven feet in circumference, and projected eighteen inches above the surface, with apparently a large part buried beneath it. As its dimensions are not very great, we may speculate on some method of transportal different from that, by which the plain near the mountains was strewed with such innumerable boulders; for instance, of its having been imbedded in a cake of river ice. Its solitary position is, however, a singular fact.

I met with erratic boulders nowhere else in Patagonia: Captain King, however, states, in his "Sailing Directions,"[4] that the surface of Cape Gregory, a headland of about 800 feet in height, on the northern shore of the Strait of Magellan, is strewed with great fragments of primitive rocks.

2. Tierra del Fuego and the Strait of Magellan

The eastern part of Tierra del Fuego is formed of large outliers of the Patagonian formation, fringed by deposits of much more recent origin, the height of which varies from about 100 to 250 feet. These lower, irregular plains have been elevated within the post-pliocene period. They consist of fine-grained, earthy or argillaceous sandstone, in very thin, horizontal, but sometimes inclined laminae, and often associated with curved layers of gravel. On the borders, however, of the eastern parts of the Strait of Magellan, this fine-grained formation often passes into, and alternates with, great unstratified beds, either of an earthy consistence and whitish colour, or of a dark colour and of a consistence like hardened coarse-grained mud, with the particles not separated according to their size. These beds contain angular and rounded fragments of various kinds of rock, together with great boulders. At Elizabeth Island, within the Strait, there are good sections of this deposit in cliffs 150 feet high, and composed chiefly of whitish earth, with fragments of syenite, greenstone, feldspathic rocks, clay and hornblendic slates and quartz, most of which do not occur, *in situ*, in the neighbourhood. These fragments are generally arranged without the slightest trace of order,—large and small, angular and rounded being close together; but in some parts of the cliff, the mass is divided by beds of stratified shingle, and these are most frequent in the upper part,—a fact which I observed in other places.

Few of the fragments much exceed in size a man's head, but there are numerous large boulders on the beach. In the cliff at Cape Negro, which is close to Elizabeth Island, and is of the same height and of nearly the same nature, I saw a great boulder imbedded. This deposit at Nuestra Señora de Gracia is rather finer grained, and contains fewer fragments; some of which are perfectly rounded, some quite angular; and a single one, of considerable size, is often imbedded by itself in fine-grained and fine-laminated matter. I here, also, observed a boulder at least four feet in diameter, projecting from the face of the cliff. In a neighbouring cliff, a whitish mass fills up hollows in an underlying finer-grained bed. North of Cape Virgins, close outside the mouth of the Strait, the cliffs are between 200 and 300 feet in height; and they consist of an argillaceous sandstone in horizontal laminae, as fine as roofing-slate, which in several places is interstratified with two or three beds of the coarse nature just

described, each stratum being from five feet to twenty thick. These beds often thin out and become curvilinear at each end. The imbedded fragments are of the same nature and shape as before mentioned; and their parent rock cannot be less, and probably is considerably more, than 120 geographical miles distant. In the other cases above described, the distance must be at least sixty miles. The mountains, from which they all probably came, lie west and south-west.

The numerous boulders before noticed on the beach at the foot of the cliffs on Elizabeth Island, consist of the same varieties as the smaller imbedded fragments, and are from one to four feet in diameter; the outline being irregularly angular, with only the edges blunted. In the other places above mentioned, and likewise at the base of the mountains on the line of coast extending south to Port Famine, boulders are numerous on the sea-beaches. Although I saw only two in the cliffs, yet as the boulders do not, as far as I was able to observe, occur scattered on the surface of the ground, and as a large area has evidently been denuded, I concluded that most of the blocks were originally enclosed in the deposit, and that after they were washed out, they had been driven onwards by the surf during gales, and collected at the foot of the retreating cliffs. At St. Sebastian's Bay, however, on the east coast of Tierra del Fuego, this explanation is scarcely applicable, for many gigantic boulders there lie in a protected position at the base of a naked cliff about 200 feet in height, and entirely composed of thin strata of fine-grained sandstone, with a few layers of small, well-rounded pebbles. As it is very improbable that the boulders were ever included in a deposit of this nature, we must suppose that they were originally thrown down either on the surface or in a thin superficial bed, which has subsequently been removed. I may specify, that one of these boulders, composed of syenite and shaped somewhat like a barn, was forty-seven feet in circumference, and projected about five feet above the sand-beach. There were many others half this size, and they all must have travelled at least ninety miles from their parent rock.

The position of the boulders in St. Sebastian's Bay is, in another respect, interesting; for the form of the land clearly shows, that long anterior to the total amount of elevation attested by upraised recent sea shells, a wide channel (indeed, introduced in all the charts before the voyage of the Beagle) connected the middle part of the Strait of Magellan with the open sea. During the same period, a very low neck of land near Cape Negro, now strewed with boulders and bordered by cliffs of the unstratified deposit, must have formed a straight channel between the great land-locked bay called Otway Water, and the eastern arm of the Strait of Magellan. Shoal Harbour, which lies in this line, is scattered over with enormous angular fragments of rock, projecting from five to

eight feet above the level of the sea, and giving to it a singular appearance. The unstratified beds of hardened mud and whitish earth, containing a few boulders and numerous smaller angular as well as rounded fragments, occur only in the neighbourhood of the Strait of Magellan, and are probably connected in their origin with the existence of an ancient channel which had nearly the same direction with the present one: so also it is evident, that the distribution of the numerous great boulders now lying on the surface (whether or not all were originally imbedded in the unstratified deposit) is likewise connected with the course of formerly existing sea-channels. The currents off Cape Horn set almost constantly from the west, as is known to the cost of all those who have to double[5] it; hence they probably set in a similar direction through the above ancient channels, when more open and less crooked than the Strait of Magellan now is. It is in accordance with this circumstance, that, in the districts just described, and in that which we are immediately to treat of, the boulders and smaller fragments have all travelled from mountains situated to the west.

In a space about forty miles broad at the extreme south-eastern part of Tierra del Fuego, including Navarin and several smaller islands, the shores are fringed at about an equal height, by a deposit very closely resembling the unstratified beds in the Strait of Magellan. On the south side of Navarin Island it forms a small plain (the only level land in that part of the country), fronted by a line of cliff several miles in length, and about sixty feet in height. In this cliff there is not a trace of stratification; and the earthy, rather argillaceous mass contains fragments, some angular, but mostly rounded, of all sizes, from mere particles to great boulders, of nearly the same composition as the fragments in the Strait of Magellan. Similar rocks do not occur *in situ* within sixty miles; and probably some exist only at a considerably greater distance. Within the eastern mouth of the Beagle channel, forming part of the above-mentioned area, the cliffs are higher, and the beds are sometimes regularly interstratified with layers of shingle. I cannot more accurately describe the appearance of the cliffs around Navarin Island, than by the remark which, at the time, I entered in my note-book, "that a vast debacle appeared to have been suddenly arrested in its course." But this explanation always appeared to me, from the width and openness of the channels both to the east and west, and from the proofs of the very gradual elevation of the land in the neighbouring countries, to be encumbered with the greatest difficulty. Hence the origin of these beds, as well as of those in the Strait of Magellan, which, although unstratified, are of submarine formation, remained quite inexplicable to me. This deposit resembles the till[6] of Scotland, the boulder formation of Northern Europe and of the eastern coast of England, in the following

respects, which clearly indicate, as Mr. Lyell[7] has remarked, some peculiar origin; first, in the entire absence of stratification in one part of a bed, which in another shows, either throughout its whole thickness or in alternate beds, signs of regular deposition; secondly, in the close juxtaposition of fragments of far-transported rocks, varying in size from minute pebbles to boulders, some being rounded and others angular; and lastly, as I believe, in the frequent occurrence of a capping of gravel. Mr. Lyell, after having examined this kind of deposit (which for briefness I will call by the Scotch term 'till') in the several countries above specified, ascribes its origin to the deposition, in a tranquil sea, of mud, sand and blocks, from melting drift-ice; but in the area, near the eastern mouth of the Strait of Magellan, where the finest sediment has been arranged in horizontal laminae, and the coarse shingle in beds, it appears strange that stratification should be so entirely, and often suddenly, absent in the till alone. The mere dropping of the fragments seems hardly sufficient to explain this; for we have seen that both angular and rounded fragments sometimes occur imbedded in the finest laminated matter. Perhaps the disturbing action of the icebergs when stranded, as suggested by Mr. Lyell, may account for this remarkable deficiency of stratification in the till.

I will only further add, that I looked in vain for any marine remains in these till-deposits, and a similar deficiency has been remarked in those of Europe. We must not suppose that their absence can be accounted for by a bottom of this nature being unfavourable to the existence of marine animals, for both in the retired and only partially protected bays of Tierra del Fuego, kelp (*Fucus giganteus*) grows in a depth of from two to twenty fathoms on the loose round stones, and between the roots of the kelp innumerable creatures live: in the open sea also, where there was no kelp, I found numerous *Terebratulae* and other shells, on stones lying in mud. But when we reflect how great a number of icebergs, some charged with foreign matter, but very many more without any, must, on the above theory, have been drifted to the spot while the till was accumulating; and that these icebergs being lifted up and down by the tides, as well as being broken into pieces and many times stranded, would plough up large tracts of the bottom of the sea, part of the difficulty in explaining the absence of marine remains in the till is removed, for we can hardly conceive the existence of any animal on a soft bed of mud and stones, disturbed at intervals with great violence.

An interesting description is given by Wrangell[8] of the fragments of ice off the coast of Siberia, often raised into a vertical position, and which, to use his words, "are driven against each other with dreadful crashes, are pressed downwards, and reappearing again on the surface covered with the torn up green mud, which we had often seen on the highest

hummocks." The particular case thus described happened 100 miles from the main-land, where the water however was only about fifteen fathoms deep: many of the hummocks were about 100 feet high. Wrangell states, that within the line of large hummocks the sea was generally tranquil, and strewed with only small fragments of ice; so that in this case undisturbed strata of gravel or other matter might easily (during the gradual elevation of the land, believed to be there in progress) accumulate over the disturbed beds; and in these latter deposits it is not probable that any organic remains would be enclosed.

There are two sections at Gregory Bay, in the eastern part of the Strait of Magellan, which are worthy of description. The cliffs are composed of the usual blackish indurated till, in some places interstratified with yellowish argillaceous sandstone, including a few large pebbles. In one cliff about twenty-five feet high (Fig. 2, which is traced from an outline made upon the spot), the main part consists of finely laminated yellow mud (B), which, a little further to the right, includes many fragments of

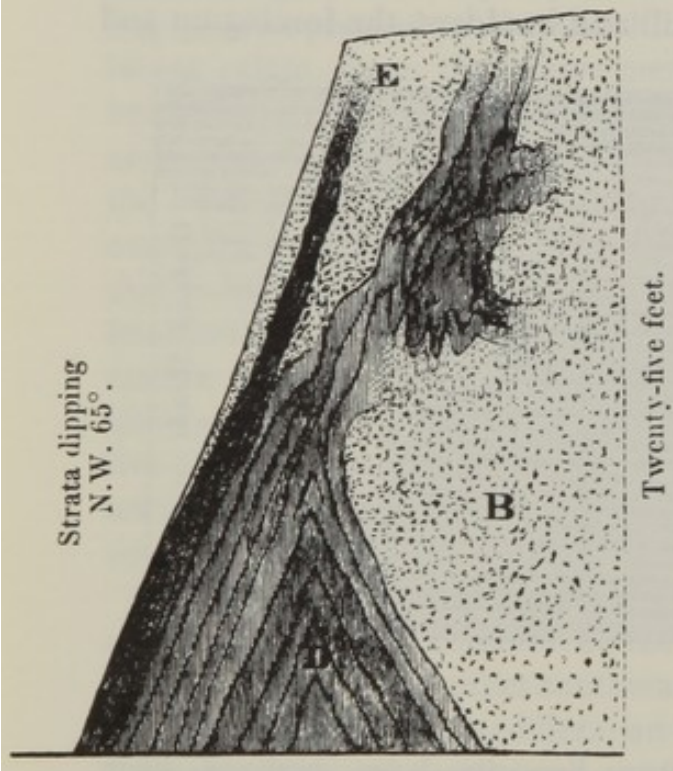


Fig. 2. Gregory Bay in the eastern part of the Strait of Magellan: dipping strata. B, yellow sandy mud; D, blackish mud or sandy clay; E, a stratum which gradually disappeared in the surrounding mass; its included layers were much convoluted.

rock and loses its laminated character. On the left it alternates with layers of blackish mud (D), which are inclined at an angle of 65°, and at the foot of the cliff form a regular saddle. Many of these layers lose themselves in the yellow sandy mud in the most singular convolutions. In another cliff, Fig. 3. a bed, about eighteen inches thick and thirty feet in

length, of fine sandy clay, lying in a coarser sort, dips gently at one end, and at the other is bent back under itself. The subordinate layers in this stratum are curved in basin-, or rather urn-shaped folds about a foot wide, and are placed at nearly equal intervals, so as to resemble some architectural ornament. They cannot always be traced from one basin to the other. The extreme degree of their curvature shows that they were not deposited in so many furrows at the bottom of the sea: it may, perhaps, be conjectured, that during the great and unequal pressure to which the whole mass has been subjected, the finer-grained laminated matter of which these urn-shaped basins consist, yielded more readily, and slided in between the parts exposed to a less force. With respect to the agency by which both sections have been contorted, from the general undisturbed state of the whole country and of the immediately joining beds, and likewise from the peculiarity of the flexures in Fig. 2, I could not at the time persuade myself it was by ordinary violence from below, and no other solution occurred to me. Mr. Lyell[9] has shown, that in the districts in Europe where the till and boulders occur, most curiously contorted layers are directly superimposed on undisturbed beds, and he suggests as one explanation, the lateral force exerted by stranded icebergs. As we have here also the till and boulders, the forcing up and

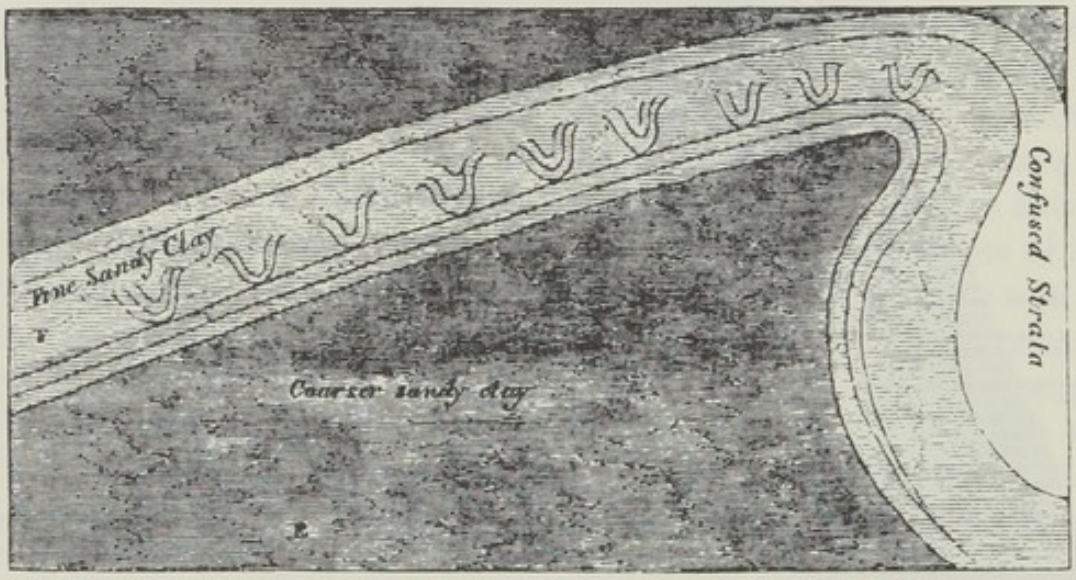


Fig. 3. Gregory Bay: confused strata.

mingling together of these sedimentary deposits, have, perhaps, been effected by this same agency[10]; an agency, however, which in most cases appears merely to have prevented the separation of the drifted materials into distinct layers.

3. Island of Chiloe

Passing from the extreme southern part of the continent along the west

coast, I did not land south of lat. 47°. It was, however, between lat. 49° and 50° that the fragment of granite, described in my Journal, was seen floating on an iceberg twenty miles from its parent glacier, and therefore we may feel sure that erratic boulders occur in this space. Between lat. 47° and the southern extremity of Chiloe I landed on several parts of the seaward coast, but did not notice a single boulder: as, however, it is separated from the Cordillera by intervening high land, the absence of erratic blocks might have been expected.

At Chiloe I again found transported boulders in vast numbers. This island is a hundred miles long, extending from lat. 41° 46′ to 43° 26′; and it lies parallel to the Cordillera, at the distance of about thirty miles from their base. It consists of mica-slate with two volcanic formations, largely bordered, chiefly on the eastern and northern sides, with horizontally stratified beds of tertiary sandstone and volcanic grit; the sandstone in some places passing into a loosely aggregated conglomerate. On the eastern coast the land is indistinctly modelled into successively rising plains, of which the superficial parts, and the whole thickness of some of the lower plains are composed of stratified shingle. This accumulation, where it caps the tertiary deposits, is evidently of subsequent formation; but I do not pretend to distinguish, in all cases, the shingle beds of more recent origin, from the above-mentioned tertiary conglomerate. The boulders occur in extraordinary numbers on the whole line of the eastern and northern beaches, and likewise to the height of at least 200 feet on the land. I saw a few imbedded in the cliffs of gravel; and as many extensive reefs show that there has been much denudation, I presume that many of the masses were originally included in the gravel. The boulders are likewise very numerous on the islets, which lie close to the eastern coast of Chiloe, and are separated from it by channels, which, although very narrow, vary in depth from 50 to more than 300 feet. In two places where I visited the outer or western coast of Chiloe, I did not see any transported blocks, nor did I during a ride of about thirty miles across the central high land.

The boulders consist of several varieties of granite and syenite; those of the latter rock are common on the northern beaches; but all the masses which I noticed thirty miles southward were of granite. Their parent rock probably exists in the Cordillera: an intelligent resident pointed out to me several varieties of syenite and granite at the northern end of the island, which, he assured me, he had seen forming whole mountains in the vicinity of Reloncavi Sound, which is situated in the same latitude: if so, these boulders must have travelled more than forty miles. I saw no granite or syenite in Chiloe, there is certainly none on the northern or on the eastern coast, the whole of which I examined, and I feel pretty sure none occurs in the northern part of the island; but it is not improbable that some of the western heights, which were estimated at 3000 feet, may

be formed of granite. If any of the boulders have come from the heights of Chiloe, they have crossed a broad and level border of the tertiary deposits; but it is far more probable that all came from the Cordillera. The larger boulders were quite angular, and resembled fragments at the foot of a steep mountain. One mass of granite at Chacao was a rectangular oblong, measuring fifteen feet by eleven, and nine feet high: another on the north shore of Lemuy islet was pentagonal, quite angular, and eleven feet on each side; it projected about twelve feet above the sand, with one point sixteen feet high: this fragment of rock almost equals the larger blocks on the Jura. There were very many others from two-thirds to a quarter of these dimensions. The boulders amongst the islets were in fewer numbers and more rounded than those on the open parts of the eastern coast; but I believe this remark applies only to the smaller masses, which may have been rounded subsequently to their first transportal.

The position of the boulders at the extreme northern point of Chiloe, where a headland about 250 feet high is joined to Lacuy peninsula by a quite low neck, deserves further notice. This headland, from the similarity of its composition, height and stratification, must once have been continuous with the coast of Chiloe. The ancient currents of the sea, which almost insulated the headland, deposited on its inland side, and on the opposite coast, beds of regularly stratified shingle. Some boulders were enclosed in these strata, and many very large angular ones of syenite were lying both on the low sandy isthmus, and on its sides at a height of 150 feet; and transported blocks were certainly far more numerous here than in any other part of the surrounding country. Anterior to the elevation, which has taken place within the post-pliocene period, the headland must have been an island, and the present low neck of land the bottom of a channel, open to the rush of tidal waters which flowed between Chiloe and the mainland of America. We thus see, even more clearly than in Tierra del Fuego, that there is an evident relationship between the distribution of the boulders, and the lines of either anciently or now existing straits. From this consideration, I was at first surprised at the occurrence of numerous blocks in the tortuous channels, between the islets and eastern coast of Chiloe; but I overlooked the fact, that, anterior to the modern period of elevation just alluded to, the middle part of Chiloe, in the line of the Lake Cucao, must, from its lowness, have been breached by a transverse channel. Had the space between Chiloe and the Cordillera been converted into land, the boulders, in their position with respect to their probable parent rocks, in their size and angular shape, would have resembled those on the Jura; the blocks of granite now lying between the islets, being the representatives of those which, M. Agassiz[11] has lately shown, occur in the interior valleys of that range.

Of the few imbedded boulders which I saw, most were in the stratified gravel; but I find in my note-book two sections obtained in the southern half of the island, and described as consisting of hardened mud, including angular as well as rounded fragments of far-transported rocks, and in one instance a boulder. These deposits evidently are similar to those called 'till,' and I feel nearly sure that they were unstratified; but not being then aware of their interest, I merely compared them to the deposits in eastern Tierra del Fuego, and I here record only what I at the time wrote down. On the inner side of one of the Chonos Islands, a little south of Chiloe, in lat. 43° 50', there are cliffs about 300 feet high, which I likewise only partially described, as formed of blackish hardened mud with scattered pebbles of various sizes, some well rounded, some but slightly: as I especially noted that the gravel in the upper part of the cliff was stratified, I presume there was little arrangement in the lower. In one layer of loose sand at the base of this cliff, where the hardened mud passed into laminated sandy clay, I found a quantity of comminuted marine shells with a fresh aspect, but too much broken to be characterized. At Chiloe also, in one place where a mass of till passed into fine-grained, laminated beds, I found two or three fragments of a *Cytheraea*.[12] I must, however, observe, that the absence of marine remains in these beds of till, is much less remarkable than in those of Great Britain, because the surrounding stratified formations here contain but very few shells. Indeed in Chiloe I found none, excepting where the sandstone contained calcareous concretions.

With respect to the age of the boulder formation of Chiloe, and I may add, of Tierra del Fuego, I have no precise information. The whole east coast of the island has been elevated certainly from twenty to forty feet, and probably much more, within the post-pliocene period, for on the peninsula of Lacuy (near where I have described the boulders on the low neck) I found, at a height of 350 feet, a great bed of existing shells, out of which forest trees were growing. I have stated, that in eastern Tierra del Fuego the boulder formation has also been elevated within this same period. Without we suppose that the boulders were deposited in a deep sea, which does not appear very probable from the nature and stratification of the accompanying deposits, or without we suppose great oscillations of level of the land, the boulder formation must have been accumulated since the commencement of the post-pliocene aera, or but little before it. From indirect evidence, also, and therefore to be received with limited confidence, I came to the same conclusion with regard to the boulders on the lower plain at Santa Cruz. It is worthy of notice, that geologists have referred the dispersal of the boulders which lie scattered over the temperate parts of the northern hemisphere to this period.

I have stated in my Journal, that northward of Chiloe (lat. 41° 47') I met with no fragments of far-transported rock which can be classed with

the erratic boulders just described;—their great size, frequent angularity, and complete separation by wide valleys or arms of the sea from their parent source, being taken as their distinctive characters: I had opportunities of observing them in the country around Valdivia, Concepcion, and many parts of central and northern Chile. In these same districts I did not meet with any beds of till. North of the Strait of Magellan, on the sea-shores of Patagonia[13] and La Plata, neither boulders nor beds of till occur in the same latitudes, under which both are present on the Pacific. With respect to the erratic masses, we may infer from what has been shown in the ascent of the Santa Cruz, that their absence is owing to the wide space separating the shore of the Atlantic from the Cordillera. In my Journal I have endeavoured to show in detail, that, in the northern parts both of the Old and New World, and in the southern parts of South America, the dispersal of boulders has been limited, in approaching the tropics, to nearly the same latitudes, and that no true blocks of this description have been observed in the inter-tropical regions[14]; and we may now be permitted to suspect that beds of 'till' will be found to be confined to the same parallels of latitude as the boulders.

4. Remarks on the Glaciers of Tierra del Fuego, and on the Transportal of Boulders

In the remainder of this paper I will make a few remarks on the glaciers of Tierra del Fuego, and on the means of transportal of the boulders. I had no opportunity of landing on any glacier, but we passed in the Beagle and Magdalen channels within two miles of several. The mountains were covered with snow, and the glaciers formed many short arms, which descended to the beach, and terminated in low perpendicular cliffs of ice. The surface of these glaciers, even to a considerable height on the mountains, was of a bright azure colour and perfectly clean, as were the floating fragments. This is what might have been expected, from the shortness of the glaciers, from their not being bordered by precipices, and from their not being formed by the junction of two or more smaller streams of ice. Although the chief characteristic of the climate of the southern parts of South America seems to be its equability, yet the glaciers cannot descend very slowly, for large masses are continually breaking from the cliffs of ice. We were witnesses of one fall in the Beagle channel; and the water was strewed with smaller pieces: Capt. King[15] mentions several bays and channels in Tierra del Fuego almost choked up with them; Mr. Bynoe[16] informed me that as many as fifty icebergs were seen together in Sir G. Eyre's Sound: behind the peninsula of Tres Montes, in a latitude corresponding to that of the lake of Geneva, some Spanish missionaries,[17] in an account of their voyage, describe an arm of

the sea as crowded with icebergs of all sizes. Some of the fragments of ice which are thus detached are of immense size: Mr. Kirke[18] met with one in an inland creek, situated in the corresponding latitude as Paris, which was estimated at forty-two feet in height, and was aground, where bottom could not be obtained with a line of 126 feet. This mass was therefore at least 168 feet in height, equal to a lofty church: how violently must the tranquil waters of the retired creek have been agitated when it fell! Capt. King, describing another case,[19] compares the crash to a broadside reverberating through these lonely regions. In the Beagle channel the insignificant fall which we witnessed, caused a wave that nearly destroyed our boats, though hauled up on the shore, and at the distance of half a mile from the cliff of ice. These waves seem to displace and drive before them the fragments of rock lying on the beach. Although the glaciers I saw were quite clean, many of the icebergs described by Mr. Kirke in Sir G. Eyre's Sound were dark coloured, and on the surface of one, several blocks of granite and serpentine were found.

The glaciers in the Beagle channel were generally bordered by a tongue of land, formed of huge fragments of rock, and many boulders were strewed on the neighbouring shores. The only glacier which I approached closely, descended to the head of a creek, formed on one side by a wall of mica-slate, and on the other by a broad promontory, about fifty or sixty feet high, and apparently composed entirely of enormous fragments, chiefly of granite. One of these was ninety feet in circumference, and projected six feet above the sand. This promontory, which originally no doubt was a lateral moraine, projects nearly half a mile beyond the extremity of the glacier, and is in parts covered by old trees: hence we must infer, that the glacier formerly extended considerably further than it now does.

It would be useless even to allude to the difficulties which affect every theory of the transportal of erratic boulders, excepting that by the agency of ice; but after the remarkable discoveries of Venetz, Charpentier, Agassiz, and others, of the great extension in Europe of moraines formed by ancient glaciers, it is necessary to observe, that neither the 'till' beds of eastern Tierra del Fuego, which pass into and are regularly interstratified with a great formation of horizontally laminated sandstone, containing marine remains; nor the stratified gravel and till, which form low plains on the shores of Chiloe, and cap in regular beds the tertiary strata, can have been produced like ordinary moraines; and, therefore, that the imbedded boulders cannot have been propelled by the glaciers themselves. I am led to the same conclusion with respect to the till of southern Tierra del Fuego, which forms a level plain and a fringe around several islands, and which in one part passes into a regularly

stratified deposit. The boulders on the lower levels at the head of the Santa Cruz river are strewed on land, which certainly has been modelled by the action of the sea. Those on the 1400 feet plain are sixty-seven miles from the Cordillera, of which the highest pinnacle is only 6400 feet, and the general range considerably lower; this little inclination of the surface, with the absence of mounds or ridges on it, and the angularity of the fragments, are opposed to the notion that the blocks have been pushed to this great distance by glaciers. Hence I conclude, that in the two first-mentioned districts it is quite certain, and in the three latter highly probable, that the boulders were transported by floating ice.

The fact of many of the blocks on the northern end of Chiloe being different from those thirty miles southward, where there must anciently have been a channel across the island, is not opposed to the foregoing conclusion: for the tidal currents must have drained, according to the number and position of the seaward channels, determinate spaces of the area between Chiloe and the Cordillera; and according to the situation of the spot whence the iceberg with its cargo of rock was first launched, so would it be swept towards one or the other channel. The varying winds, no doubt, would partly influence the course of icebergs, but, from their floating very deeply, the currents would act far more powerfully on them. Nor is the circumstance of the boulders on the high and low plain of Santa Cruz being of different kinds of rock any difficulty; for after the change of level in the land, necessary to account for the existence of the lower plains, we might have anticipated that some of the glaciers which formerly debouched on the coast would cease doing so; and that rocks hitherto submerged beneath the sea would become exposed, and their fragments falling on the glaciers would be transported with the icebergs.

It appears that masses of floating ice, by which fragments of rock are conveyed, are produced in two ways, and under circumstances considerably different although often acting together, namely, by the breaking off of icebergs from glaciers descending into the sea, and by the actual freezing of the surface of the sea or its tributary streams. Great boulders can be included in ice by this latter means only (with rare exceptions) where the winter is extremely cold, as in the Gulf of Bothnia[20] and on the shores of North America. A large proportion of the fragments thus enclosed will generally have been exposed to the wearing influences of the sea-beach; and from the ice being in a sheet, they will be liable to be repeatedly stranded in shallow places, and thus to become still more worn. The other method of transportal, namely, by the descent of glaciers to the sea-level, and the production of icebergs, is far from necessarily requiring an extremely cold winter; for the low descent of glaciers seems to depend (other circumstances being alike) in a much greater degree on the summer not being hot enough to melt the ice and snow, than on the winter being very cold. Hence, as I have endeavoured

to show in my Journal (chap. xiii.), glaciers in South America descend to the sea from mountains not very lofty, and in latitudes extraordinarily low compared with those in Europe under which the same phaenomenon takes place; and yet, the vegetable and animal productions of this kind of climate have, in some degree, an inter-tropical character.

M. Agassiz has shown that blocks of rock are not imbedded in the ice of the Swiss glaciers, except high up near their sources, and that those numerous masses which lie on the surface, from not being exposed to much abrasion, remain angular: hence only loose angular blocks of rock (as was the case with those on the floating ice in Sir G. Eyre's Sound) can be transported by icebergs, detached from the glaciers of temperate countries. And to effect this, the icebergs must be floated off perpendicularly and in large masses, for otherwise the loose fragments would be at once hurled into the sea. These remarks do not necessarily apply to icebergs formed under a polar climate, for if a glacier in its descent, reached the sea before the fragments of rock which had fallen on the soft snow had come to the surface, icebergs would be produced with imbedded fragments of rock: I have described in the 'Geographical Journal'[21] the case of one huge fragment thus circumstanced, seen drifting far from land in the Antarctic Ocean.[22]

As one of the above two methods of conveying erratic boulders, namely, that by icebergs from glaciers, is now in action on the South American shores, we are naturally led to conclude, that this was the chief agent in the enormous amount of transportal formerly effected over a more extended area. It would indeed require the strongest evidence to make one believe that the surface of the sea, or even of rivers, between lat. 41° and 42°, had ever been frozen thickly enough to enclose the huge masses of rock which we now find stranded on the island of Chiloe. The angularity of their forms at this latter place and at Santa Cruz, accords with their transportal by icebergs; but it is not improbable that the other agency, namely, the freezing of the sea, may formerly have been instrumental in Tierra del Fuego, and especially in the southern parts of that country, where the boulders frequently show signs of attrition, as if they had been worn on a sea-beach. In endeavouring, therefore, to determine, in any country where boulders occur, the nature of the climate during their dispersal, we should attend not only to the character of the contemporaneous organic productions, but likewise to the shape of the fragments and to their position; for these circumstances would aid us in discovering whether they had been imbedded in sheet-ice, or carried on the surface of deeply-floating icebergs.

1. [Read 14 April 1841.] *Transactions of the Geological Society of London*, 2d ser., 6(1842):415–31.†

2. Coarse, rounded stones, larger than gravel, up to 8–10 inches in diameter.†

3. I may observe, that it can be clearly shown (Journal of Researches, p. 216)

that the river itself, although large and rapid, has scarcely any power in transporting fragments even of inconsiderable size.

4. Philip Parker King, *Sailing Directions for the Coasts of Eastern and Western Patagonia* . . . (London: Great Britain & Ireland Admiralty, Hydrographic Department, 1832).†

5. To sail in an opposite direction.†

6. Unstratified glacial deposits of sand, clay, gravel, and boulders.†

7. Mr. Lyell on the Boulder Formation of East Norfolk. Philosophical Magazine, 1840, p. 348.

8. Wrangell's 'Voyage to Siberia and the Polar Sea,' translated by Major Sabine, p. 257.

9. Philosophical Magazine, 1840, p. 379: Mr. Lyell on the Boulder Formation, &c., Proceedings, vol. iii. p. 178.

10. Capt. W. Graah, in his Expedition to the East Coast of Greenland, states that there is a part of the coast which derives its name of *Puisortok* from ice "shooting up from the bottom of the sea in such a manner and in such masses, as in many years to make it utterly impassable." The cause of this singular phaenomenon is unknown. Capt. Graah suggests amongst other causes, that these masses may be the remains of icebergs frozen to the bottom; but is it not much more probable that the icebergs were first driven deeply into the soft bed of the sea, and that they did not become disengaged until their whole upper parts had been washed away and their buried sides loosened by the melting of the ice?

11. Louis Agassiz, "On the erratic blocks of the Jura," *Edinburgh New Philosophical Journal* 24(1828):176–79.†

12. A clam.†

13. I may here mention, that on East Falkland Island, although situated in the same latitude with Tierra del Fuego, and lying only 250 miles eastward of it, and with mountains above 2000 feet in height, I did not observe any erratic boulders. As it may occur to some geologists that the island may have received its chief elevation, subsequently to the period of the dispersal of the boulders on the main-land, I will observe, that the facts are directly opposed to such a view, for I could not find any elevated marine shells on this island; whereas I did not land on a single point of the coast of Patagonia, or of eastern Tierra del Fuego, without meeting with them.

14. In my Journal (p. 289, and Appendix, p. 615), where I have considered the *apparent* exceptions to this statement, I accidentally omitted one case. Near Rio de Janeiro I met some large-sized boulders of greenstone, containing iron pyrites; they were perfectly rounded, and therefore wanted that character of angularity, which, though far from being always a concomitant, may, where it is present, be considered as eminently distinctive. I could not see the greenstone *in situ* in the immediate neighbourhood, but the extreme rankness of the vegetation quite precluded accurate investigation. Mr. Caldcleugh (Travels, vol. ii. p. 195) observed greenstone boulders on the road to Villa Rica, and Spix (Travels, Eng. Transl. vol. i. p. 272) observed others on the road to Santa Cruz. Mr. Fox, Minister Plenipotentiary at Rio de Janeiro, informed me that he found similar blocks on the islands of St. Sebastian and St. Catherine, and at Port Alegre on the southern coast of Brazil. Nevertheless, it is not improbable that in all these cases the parent rock was not far distant. I found two greenstone dykes near Rio de Janeiro: Von Eschwege mentions others, and Mr. Fox observed one on St. Sebastian. Besides many obvious means of transportal to moderate distances of large fragments of rock, now that geologists generally admit that most countries have undergone slow oscillations of level, we must not overlook the power which

the surf during gales would have, on an exposed and gently inclined surface, of driving onwards blocks of rock from the top of one line of beach to that of another, as the sea gradually encroached on the land.

15. Voyages of the Adventure and Beagle, vol. i. pp. 56, 58, 140, 258.
16. The Author's Journal of Researches, p. 283.
17. Ibid., Appendix, p. 613.
18. Voyages of the Adventure and Beagle, vol. i. p. 337.
19. Ibid., vol. i. p. 140.
20. North part of Baltic Sea, between Sweden and Finland.†
21. Geographical Journal, 1839, p. 528.
22. Dr. Martens observed many fragments of rock imbedded only just above the level of the sea in the lateral wall of the glaciers at Spitzbergen, but he never saw any in the cliffs of ice facing the sea—Edinburgh New Phil. Journal, 1841, pp. 173 and 176.

Notes on the Effects Produced by the Ancient Glaciers of Caernarvonshire, and on the Boulders Transported by Floating Ice[1]

Guided and taught by the abstract of Dr. Buckland's memoir "On Diluvio-Glacial Phaenomena in Snowdonia and the adjacent parts of North Wales,"[2] I visited several of the localities there noticed, and having familiarized myself with some of the appearances described, I have been enabled to make a few additional observations.

Dr. Buckland has stated that a mile east of Lake Ogwyn[3] there occurs a series of mounds, covered with hundreds of large blocks of stone, which approach nearer to the condition of an undisturbed moraine, than any other mounds of detritus noticed by him in North Wales. By ascending these mounds it is indeed easy to imagine that they formed the north-western lateral moraine of a glacier, descending in a north-east line from the Great Glyder mountain. But at the southern end of Lake Idwell the phaenomena of moraines are presented, though on a much smaller scale, with perfect distinctness. On entering the wild amphitheatre in which Lake Idwell lies, some small conical, irregular little mounds, which might easily escape attention, may be seen at the further end. The best preserved mounds lie on the west side of the great black perpendicular face of rock, forming the southern boundary of the lake. They have been intersected in many places by streams, and they are seen to consist of earth and detritus, with great blocks of rock on their summits. They at first appear quite irregularly grouped, but to a person ascending any one of those furthest from the precipice, they are at once seen to fall into three (with traces of a fourth) narrow straight linear ridges. The ridge nearest the precipice runs someway up the mountain, but the outer one is longer and more perfect, and forms a trough with the mountain-side, from 10 to 15 feet deep. On the eastern and opposite side of the head of

the lake, corresponding but less developed mounds of detritus may be seen running a little way up the mountain. It is, I think, impossible for any one who has read the descriptions of the moraines bordering the existing glaciers in the Alps, to stand on these mounds and for an instant to doubt that they are ancient moraines; nor is it possible to conceive any other cause which could have abruptly thrown up these long narrow steep mounds of unstratified detritus against the mountain-sides. The three or four linear ridges evidently mark the principal stages in the retreat of the glacier; the outer one is the longest, and diverges most from the great wall of rock at the south end of the lake. The inner lines distinctly define the boundary of the glacier during the last stage of its existence. At this period a small and distinct glacier descended from a narrow but lofty gorge on the north-western end of the lake; and here remnants of a terminal moraine may be traced in the little mounds, forming a broken semicircle round a rushy plain, scarcely more than a hundred yards in diameter. The rocks are smoothed, mammilated and scored, all round the lake, and at some little depth beneath the surface of the water, as I could both see and feel. Similar marks occur at great heights on all sides, far above the limits of the moraines just described, and were produced at the time when the ice poured in a vast stream over the rocky barrier bounding the northern end of the amphitheatre of Lake Idwell.

I may here mention, that about eighty yards west of the spot where the river escapes from the lake, through a low mound of detritus, probably once a terminal moraine, there is an example of a boulder broken, as described by Charpentier and Agassiz, into pieces, from falling through a crevice in the ice. The boulder now consists of four great tabular masses, two of which rest on their edges, and two have partly fallen over against a neighbouring boulder. From the distance, though small in itself, at which the four pieces are separated from each other, they must have been pitched into their present position with great force; and as the two upright thin tabular pieces are placed transversely to the gentle slope on which they stand, it is scarcely possible to conceive that they could have been rolled down from the mountain behind them; one is led, therefore, to conclude that they were dropped nearly vertically from a height into their present places.

The rocky and steep barrier over which the ice from the amphitheatre of Lake Idwell flowed into the valley of Nant-Francon, presents from its summit to its very foot (between 400 and 500 feet) the most striking examples of boss or dome-formed rocks; so much so, that they might have served as models for some of the plates in Agassiz's work on Glaciers. When two of the bosses stand near and are separated only by a little gorge, their steep rounded sides are generally distinctly scored with

lines, slightly dipping towards the great valley in front. The summit of the bosses is comparatively seldom scored; but on one close to the bridge over the river Ogwyn, I remarked some singular zigzag scores. At this spot the cleavage of the slate is highly inclined, and owing apparently to the different degrees of hardness of the laminae, smooth and gentle furrows have been produced by the grinding of the ice, transversely to the scores, and to the probable course of the glacier. Here, as well as in some few other places, I noticed an appearance which made it vividly clear that these bosses had been formed by some process quite different from ordinary aqueous or atmospheric erosion; it is the abrupt projection from the smooth surface of a boss of a piece of rock a few yards square, and one or two feet in height, with its surface smoothed and scored like the boss on which it stands, but with its sides jagged: if a statuary were to cut a small figure out of a larger one, the abrupt projecting portions, before he quite completed his work, might be compared to these masses of rock: how it comes that the glacier, in grinding down a boss to a smaller size, should ever leave a small portion apparently untouched, I do not understand.

On the summit of some of the bosses on this barrier there are perched boulders: but this phaenomenon is seen far more strikingly close to Capel-Curig, where almost every dome of rock south of the Inn is surmounted by one or more large angular masses of foreign rock. The contrast between the rude form of these blocks, and the smooth mammillated domes on which they rest, struck me as one of the most remarkable effects produced by the passage of the glaciers. On the sides of the mountains above Capel-Curig, I observed some boulders left sticking on very narrow shelves of rocks, and other boulders of vast size scattered in groups. The largest boulder I noticed there was about 26 feet in length by 12 in breadth, and buried to an unknown thickness.

Proceeding down the great straight valley of Nant-Francon, which must formerly have conveyed the united glaciers from Lakes Idwell and Ogwyn, we continue to meet with boss-formed rocks till below the village of Bethesda. From this point towards Bangor these boss-formed rocks become rare; at least it is certain that a large number of hummocks of rock with rugged surfaces project, whereas higher up in this valley, and in all the great central valleys of Snowdonia, such unground hummocks are not to be met with. At Bethesda, unstratified masses of whitish earth, from ten to forty feet in thickness, full of boulders mostly rounded, but some angular, from one to four feet square, are first met with. This deposit is interesting from the boulders being deeply scored, like the rocks *in situ* over which a glacier has passed. The scores are sometimes irregular and crooked, but generally quite parallel, as I distinctly saw over the entire side of one large block. Some of the blocks

were scored only on one side, others on two sides, but from the difficulty of turning over the larger ones, I do not know which case is most common. I saw one large block on which the scores on the opposite sides were all parallel; and another irregularly conical one, four feet in length, of which three-fourths of the circumference was marked with parallel striae, converging towards the apex. In the smaller *elongated* blocks, from six to twelve inches in diameter, I observed that the striae were generally, if not always, parallel to their longer axes, which shows that when subjected to the abrading force, they arranged themselves in lines of least resistance. Out of three large blocks which remained imbedded in a perpendicular cliff, the vertical sides of two were scored in horizontal lines, and of the third in an oblique direction. These several facts, especially the parallel striae on the upper and lower surfaces, show that the boulders were not scored on the spot where they are now imbedded, as seems to have been the case with the boulders described by Mr. Maclaren[4] in the till near Edinburgh. The contrast is very striking in the state of the surface of these boulders, and those which lie scattered high up on the sides of the adjoining hills and of the great central valleys, or are perched on the worn bosses of naked rock; such boulders, as I particularly noticed, present no signs of scores or striae, as might have been anticipated, if, as is supposed, they were transported on the surface of the glaciers. In the quarries which I examined, namely, below Bethesda, and at some little height on the eastern side of the village, the till rested on slate-rocks, not worn into bosses. I found, however, a rather smooth pap of greenstone marked with a few deep scores. The till forms, at the height probably of 600 feet above the sea, a little plain, sloping seaward; and between Bethesda and Bangor, there are other gently inclined surfaces composed of till and stratified gravel. Considering these facts, together with the proofs of recent elevation of this coast, hereafter to be mentioned, I cannot doubt that this till was accumulated in a sloping sheet beneath the waters of the sea. In composition it resembles some of the beds of till in Tierra del Fuego, which have undoubtedly had this origin. I presume the scored, rounded, and striated boulders were pushed, in the form of a terminal moraine, into the sea, by the great glacier which descended Nant-Francon.

Mr. Trimmer[5] reports, on the *authority of some workmen*, that sea-shells have been found on Moel Faban, two miles N.E. of Bethesda. I ascended this and some neighbouring hills, but could find no trace of any deposit likely to include shells. This hill stands isolated, out of the course of the glaciers from the central valleys; it exceeds 1000 feet in height; its surface is jagged, and presents not the smallest appearance of the passage of glaciers: but high up on its flanks (and perhaps on its very summit) there are large, angular and rounded boulders of foreign rocks.

Along the sea-coast between Bangor and Caernarvon, and on the Caernarvonshire plain, I did not notice any boss-formed hillocks of rock. The whole country is in most places concealed by beds of till and stratified gravel, with scattered boulders on the surface: some of these boulders were scored. From the account given by Mr. Trimmer[6] of his remarkable discovery of broken fragments of Buccinum, Venus, Natica, and Turbo, beneath twenty feet of sand and gravel, on Moel Tryfan (S.E. of Caernarvon), I ascended this hill. Its height is 1192 feet[7] above the sea; it is strewed with boulders of foreign rock, most of them apparently from the neighbouring mountains; but near the summit I found the rounded chalk-flints[8] and small pieces of white granite alluded to by Dr. Buckland. Its form is conical, and it stands isolated: wherever the bare rock protrudes its surface is jagged, and shows no signs of being in any part worn into bosses. The contrast between the superficial part of the bare rock on this hill and on Moel Faban, with that of the rocks within the great central valleys of Caernarvonshire, is very remarkable; it is a contrast of precisely the same kind as may be observed in these same valleys by ascending on either side above the reach of the ancient glaciers. A little way down the hill, a bed two or three feet in thickness, of broken fragments of slate mixed with a few imperfectly rounded pebbles and boulders of many kinds of rock, is seen in several places to rest on the slate, the upper surface of which, to the depth of several feet, has been disintegrated, shattered and contorted in a very curious manner. The laminated fragments, however, sometimes partially retain their original position.

I did not succeed in finding any fragments of shells, but near the summit of the hill on the eastern or inland side, I found beds, at least twenty feet in thickness, of irregularly stratified gravel and boulders, with distinct and quite defined layers of coarse yellow sand, and others of a fine argillaceous nature and reddish colour. These beds closely resemble those of Shropshire and Staffordshire, in which are found (as I have myself observed in very many places) fragments of sea-shells, and which every one, I believe, since the publication of Mr. Murchison's chapters on the drift of these counties, admits are of submarine origin. It may therefore be concluded that the layers of coarse and argillaceous sand, and of gravel, with far-transported pebbles and boulders, do not owe their origin to an inundation, but were deposited when the summit of Moel Tryfan stood submerged beneath the surface of the sea. As there are no marks of the passage of glaciers over this mountain (which indeed from its position could hardly have happened), we must suppose that the boulders were transported on floating ice; and this accords with the remote origin of some of the pebbles, and with the presence of the sea-shells. Within the central valleys of Snowdonia, the boulders appear

to belong entirely to the rocks of the country. May we not conjecture that the icebergs, grating over the surface, and being lifted up and down by the tides, shattered and pounded the soft slate-rocks, in the same manner as they appear to have contorted the sedimentary beds of the east coast of England (as shown by Mr. Lyell),[9] and of Tierra del Fuego? Although I was unable to find any beds on Moel Faban likely to preserve sea-shells, yet, considering the absence of the marks of the passage of glaciers over it, I cannot doubt that the boulders on its surface were transported on floating ice.

The drifting to and fro, and grounding of numerous icebergs during long periods near successive uprising coast-lines, the bottom being thus often stirred up and fragments of rock dropped on it, will account for the sloping plain of unstratified till, occasionally associated with beds of sand and gravel, which fringes to the west and north the great Caernarvonshire mountains.

In a paper read before the Geological Society,[10] I have remarked that blocks of rock are transported by floating ice under different conditions; 1st, by the freezing of the sea, in countries where the climate does not favour the low descent of glaciers; 2nd, by the formation of icebergs by the descent of glaciers into the sea, from mountains not very lofty, in latitudes (for instance in that of Geneva, or of the mouth of the Loire, in the southern hemisphere) where the surface of the sea never freezes; and 3rd, by these two agencies united. I have further remarked that the condition and kind of the stones transported, would generally be influenced by the manner of production of the floating ice. In accordance with these views, I may remark that it does not seem probable from the low level of the Chalk-formation in Great Britain, that rounded chalk-flints could often have fallen on the surface of glaciers, even in the coldest times. I infer therefore that such pebbles were probably inclosed by the freezing of the water on the ancient sea-coasts. We have, however, the clearest proofs of the existence of glaciers in this country; and it appears, that when the land stood at a lower level, some of the glaciers, as in Nant-Francon, reached the sea, where icebergs charged with fragments would occasionally be formed. By this means we may suppose that the great *angular* blocks of Welch rocks, scattered over the central counties of England, were transported.[11] I looked carefully in the valleys near Capel-Curig and in Nant-Francon for beds of pebbles, or other marks of marine erosion, but could not discover any: when, however, Moel Tryfan and Faban stood beneath the level of the sea, inland creeks of salt-water must have stretched far up or quite through these valleys, and where they were deep, the glaciers (as at present in Spitzbergen[12]) would have extended, floating on the surface of the water, ready to become detached in large portions. From the presence of boss-formed rocks low

down in the valley of Nant-Francon, and on the shores of the Lakes of Llanberis (310 feet above the sea), it is evident that glaciers filled the valleys after the land had risen to nearly its present height; and these glaciers must have swept the valleys clean of all the rubbish left by the sea. As far as my very limited observations serve, I suspect that boss or dome-formed rocks will serve as one of the best criterions between the effects produced by the passage of glaciers and of icebergs.[13]

Dr. Buckland has described in detail the marks of the passage of glaciers along nearly the whole course of the great central Welch valleys; I observed that these marks were evident at the height of some hundred feet on the mountain-sides, above the water-sheds, where the streams flowing into the sea at Conway, Bangor, Caernarvon, and Tremadoc, divide: hence it appears that a person starting from any one of these four places (or from some way up the valley where the glacier ended), might formerly, without getting off the ice, have come out at either of the other three places, or low down in the valleys in which they stand. The mountains at this period must have formed islands, separated from each other by rivers of ice, and surrounded by the sea. The thickness of the ice in several of the valleys has been great. In the vale of Llanberis I ascended a very steep mountain, E.N.E. of the upper end of the upper lake, which slightly projects where the valley bends a little. For the lower 1000 feet (estimated, I think, correctly) the marks left by the glacier are very distinct, especially near the upper limit, where there are boulders perched on bosses of rock, and where the scores on the nearly vertical faces of rock are, I think, more distinct than any others which I saw. These scores are generally slightly inclined, but at various angles, seaward, as the surface of the glacier must formerly have been. But on one particular face of rock, inclined at an angle of somewhere about fifty degrees, continuous, well-marked and nearly parallel lines sloped upwards (in a contrary sense to the surface of the glacier) at an angle of 18° with the horizon. This face of rock did not lie parallel to the sides of the main valley, but formed one side of the sloping end of the mountain, over and round which the ice appears to have swept with prodigious force, expanding laterally after being closely confined by the shoulder above mentioned. At this point, where the glacier has swept to the westward, and has expanded, its surface seems in a short space to have declined much: for on a hill lying about a quarter of a mile N.W. of the shoulder, and forming a lower part of the same range (it stands S.S.E. of the Victoria Inn, and has a reddish summit), the marks of the passage of the glacier are at a considerably lower level. At the very summit, however, of this hill, several large blocks of rock have been moved from their places, as if the ice had occasionally passed over the summit, but not for periods long enough to have worn it smooth.

I cannot imagine a more instructive and interesting lesson for any one who wishes (as I did) to learn the effects produced by the passage of glaciers, than to ascend a mountain like one of those south of the upper lake of Llanberis, constituted of the same kind of rock and similarly stratified, from top to bottom. The lower portions consist entirely of convex domes or bosses of naked rock, generally smoothed, but with their steep faces often deeply scored in nearly horizontal lines, and with their summits occasionally crowned by perched boulders of foreign rock. The upper portions, on the other hand, are less naked, and the jagged ends of the slaty rocks project through the turf in irregular hummocks; no smooth bosses, no scored surfaces, no boulders are to be seen, and this change is effected by an ascent of only a few yards! So great is the contrast, that any one viewing these mountains from a distance, would in many cases naturally conclude that their bases and their summits were composed of quite different formations.

1. *London, Edinburgh, and Dublin Philosophical Magazine and Journal of Science* 21(1842):180–88.†

2. Read before the Geological Society, December 15th, 1841, and the Abstract is published in the Athenaeum, 1842, p. 42. (An Abstract of Dr. Buckland's paper, from the Proceedings of the Society, will appear in an early number of the Philosophical Magazine.—Edit.)

3. About 3 miles west of Capel Curig, North Wales.†

4. Geology of Fife and the Lothians, p. 212.

5. Proceedings of the Geological Society, vol. i. p. 332, or Phil. Mag. S. 2. vol. x. p. 143. Mr. Trimmer was one of the earliest observers of the scores and other marks on the rocks of North Wales. He has also remarked that "some of the larger blocks amid the gravel have deep scratches upon their surface." Mr. Trimmer himself found broken sea-shells in the diluvium at Beaumaris.

6. Proceedings of the Geological Society, vol. i. p. 332. (Phil. Mag. *loc. cit.*)

7. Murchison's Silurian System, p. 528.

8. I may mention, that at Little Madely, in Staffordshire, I have found chalk-flints in the gravel-beds, associated with existing species of sea-shells.

9. "On the Boulder Formation of Eastern Norfolk;" Phil. Mag., S. 3, vol. xvi. May 1840, p. 351.

10. May 5th. 1841, "On the distribution of the Erratic Boulders, and on the contemporaneous unstratified deposits of South America." (Phil. Mag. S. 3, vol. xix. p. 536.)

11. On the summit of Ashley Heath in Staffordshire, there is an angular block of syenitic greenstone, four feet and a half by four feet square, and two feet in thickness. This point is 803 feet above the level of the sea. From this fact, together with those relating to Moel Tryfan and Faban, we must, I think, conclude that the whole of this part of England was, at the period of the floating ice, deeply submerged. From the reasons given in my paper (Phil. Trans., 1839; Phil. Mag. S. 3, vol. xiv. p. 363), I do not doubt that at this same period the central parts of Scotland stood at least 1300 feet beneath the present level, and that its emergence has since been very slow. The boulder on Ashley Heath probably has been exposed to atmospheric disintegration for a longer period than any other in this part of England. I was therefore interested in comparing the state of its lower

surface, which was buried two feet deep in compact ferruginous sand (containing only quartz pebbles from the subjacent new red sandstone), with the upper part. I could not, however, perceive the smallest difference in the preservation of the sharp outlines of its sides. I had a hole dug under another large boulder of dark green felspathic slaty rock, lying at a lower level; it was separated by 18 inches of sand, (containing two pebbles of granite, and some angular and rounded masses of new red sandstone) from the surface of the new red sandstone. One of the rounded balls of this latter stone had been split into two, and deeply scored, evidently by the stranding of the boulder.

12. Dr. Martens on the Glaciers of Spitzbergen, New Edinb. Phil. Journ. 1841, (vol. xxx.) p. 288.

13. In the Appendix to my Journal of Researches (1839), I endeavoured to show that many of the appearances attributed to debacles, and to the movements of glaciers on solid land, would in all probability be produced by the action of stranded icebergs. I have stated (p. 619), on the authority of Dr. Richardson, that the rocky beds of the rivers in North America which convey ice, are smoothed and polished; and that (p. 620) the icebergs on the Arctic shore drive before them every pebble, and leave the submarine ledges of rock absolutely bare.

Remarks on the Preceding Paper, in a Letter from Charles Darwin, Esq., to Mr. Maclaren[1]

I have been so much pleased with the very clear, and, at the same time, in many points quite original manner in which you have stated and explained my views, that I cannot refrain from troubling you with my thanks.[2] Your third objection appears to me much the most, indeed the only, formidable one, which has hitherto occurred to me. I fear I shall be tempted to reply to it at great length, but perhaps sometime you will find leisure to read my attempted vindication. With respect to the first objection, I can hardly admit that we know enough of the laws of elevation and subsidence to argue against the theory, because the areas of different movements are not more distinct. Some have been startled at my view on directly the reverse grounds to your objection, viz. that, according to their notions of probability, the areas of the same movements were too large and uniform. With respect to your second objection, all those who believe that exceedingly slow and gradual elevations are the order of nature, must admit a great amount of contemporaneous denudation, which would tend to annihilate the characteristic form of the fringing-reefs during their upheaval, and leave merely a coating on the up-raised land of coral-rock either thicker or thinner, according to the original thickness, rate of growth of the reef at each successive level, and the rate of elevation; indeed I am surprised that there exists even one case, viz. at Mauritius, where the peculiar moat-like

structure of a mere fringing-reef has been partially preserved on dry land.

Your third criticism strikes me as a very weighty and perplexing one. It had passed through my head, but I had not considered it with nearly the attention it deserved, otherwise I assuredly would have noticed it in my volume.[3] I had always intended to examine the limestone formations of England for comparison, but was prevented by bad health; I was, however, led away from the subject, and baffled when I consulted published accounts, for the limestones all appeared to be uniformly spread out, and most, if not all of them, to be associated with layers of earthy matter, whereas a formation of the nature of a group of atolls, would consist of separate large patches of calcareous rock, which would be quite pure.—I was thus led from the subject, and did not reflect on their want of thickness. The want of thickness, however, in any limestone formation, until it be first shewn to be analogous in structure, form, and composition, to a barrier-reef, an atoll or group of atolls, evidently cannot be brought forward as any argument against the theory of the long-continued subsidence of reefs of these classes. During the elevation of all reefs in open seas, I think there can be no doubt (as is dwelt on at p. 117, 3d. vol.) that a considerable thickness of the exterior would be denuded, and the only parts preserved would be those which had accumulated in lagoons or lagoon-channels; these would be chiefly sedimentary, and in some cases might contain (p. 117) scarcely any coral; within barrier-reefs such beds would often be associated with much earthy sediment. Mr. Lyell, in a note just received, in which he alludes to your criticisms, speaks of the limestones of the Alps and Pyrenees, as being of enormous thickness, namely, about 4000 feet. I do not know what their composition is, but I have no doubt that the strata now accumulating *within* the barrier-reef of Australia and New Caledonia, are chiefly formed of horizontal layers of calcareous sediment and not of coral.

I suspect that denudation has acted on a far grander scale than in merely peeling the outsides of upraised reefs. My theory leads me to infer that the areas, where groups of atolls and barrier-reefs stand, have subsided to a great amount and over a wide space. Now it appears to me probable, that a subterranean change, producing a directly opposite movement, namely, a great and widely extended elevation, would be extremely slow, and would be interrupted by long periods of rest, and perhaps of oscillation of level. When I think of the denudation along the fault, which goes across the northern carboniferous counties of England, where 1000 feet of strata have been smoothed away; when I think how commonly volcanic islands, formed of very hard rock, are eaten back in cliffs from 100 or 200 to 800 or 1000 feet in height, I hardly see where we

can stop, with respect to the probable limits of erosion on the comparatively soft, generally cavernous, tabular, though wide, masses of coral rock, standing exposed in great oceans during very slow changes of level. Most of the atolls which have been raised a few hundred feet are mere wrecks, and at the Friendly Archipelago where there are upraised atolls, there are large irregular reefs, also, which I have always thought were probably the basal vestiges of worn down atolls. Many submerged reefs, which may have had this same origin, occur outside the line of elevation of the Salomon and New Hebrides archipelagoes. The great steepness of the shores of upraised reefs (p. 65. Ehrenberg quoted, and p. 51.) would probably be unfavourable to the growth of new reefs, and therefore to the protection afforded by them. I can conceive it very possible, that should, at some period, as far in futurity as the secondary rocks are in the past, the bed of the Pacific, with its atolls and barrier reefs, be raised in reefs, by an elevation of some thousand feet, and be converted into a continent, that scarcely any, or none of the existing reefs would be preserved; but only widely spread beds of calcareous matter derived from their wear and tear. As a corollary from this, I suspect that the reefs of the secondary periods (if any, as is probable, existed), have been ground into sand, and no longer exist. This notion will certainly at first appear preposterous; its only justification lies in the probability of upward movements after long periods of subsidence, being exceedingly slow and often interrupted by pauses of rest, and perhaps of oscillations of land, during all which the soft coral rock would be exposed to the action of waves never at rest.

This notion, preposterous as it will probably appear, would not have occurred to me, had I not several times, from independent reasons, been driven to the conclusion, that a formation to be preserved to a very distant area (or which probably is the same thing, to be elevated to a great height from its original level *over a wide area*) must be of great extent, and must be covered by a great thickness of superincumbent matter in order to escape the chances of denudation. I have come to this conclusion chiefly from considering the character of the deposits of the long series of formations piled one upon another, in Europe, with evidence of land near many of them. I can explain my meaning more clearly by looking to the future; it scarcely seems probable, judging from what I see of the ancient parts of the crust of the earth, that any of the numerous sub-littoral formations (i.e. deposits formed along and near shores, and not of great width or breadth), now accumulating on most parts of the shores of Europe (and indeed of the whole world), although, no doubt, many of them must be of considerable thickness, will be preserved to a period as far in the future, as the lias or chalk are in the past, but that only those deposits of the present day will be preserved

which are accumulating *over a wide area, and which shall hereafter chance to be protected by successive thick deposits.* I should think that most of the sublittoral deposits of the present day will suffer, what I conclude the sublittoral formations of the secondary aeras have generally suffered, namely, denudation. Now, barrier and atoll coral reefs, though, according to my theory, of great thickness, are, in the above sense, not widely extended; and hence I conclude they will suffer, as I suspect ancient coral reefs have suffered—the same fate with sublittoral deposits.

With respect to the vertical amount of subsidence, requisite by my theory to have produced the spaces coloured blue on the map, more facts regarding the average heights of islands and tracts of land are wanted than all those, even if perfectly known, which this one world of ours would afford; for the question of the probable amount, or, which is the same thing, the probable thickness of the coral-reef, resolves itself into this,—What is the ordinary height of tracts of land, or groups of islands of the size of the existing groups of atolls (excepting as many of the highest islands or mountains in such groups, as there usually occur of "encircled islands" in groups of atolls)? and likewise what is the ordinary height of the single scattered islands between such groups of islands?—subsidence sufficient to bury all these islands (with the above exception) my theory absolutely requires, but no more. In my volume, I rather vaguely concluded that the atolls, which are studded in so marvellous a manner over wide spaces of ocean, marked the spots where the mountains of a great continent lay buried, instead of merely separate tracts of land or mountainous islands; and I was thus led to speak somewhat more strongly than warranted, of the probable vertical amount of subsidence in the areas in question.

Mr. Lyell in the note alluded to, thinks we are much too ignorant of intra-tropical geology (and ignorant enough we certainly are) to affirm that calcareous rocks of the supposed thickness of coral reefs, do not occur. I am inclined to lay considerable stress on this. I do not expect the foregoing view will appear at all satisfactory to any one besides myself,—I believe, however, there is more in it than mere special pleading. The case, undoubtedly, is very perplexing; but I have the confidence to think, that the theory explains so well many facts, that I shall hold fast by it, in the face of two or three puzzles, even as good ones as your third objection.

1. *Edinburgh New Philosophical Journal* 34(1843):47–50.†

2. The article to which Darwin has reference is Charles Maclaren, "On Coral Islands and Reefs, as Described by Mr. Darwin," *Edinburgh New Philosophical Journal* 34(1843):33–47. The following excerpt from Maclaren's article (pp. 46–47) summarizes his criticisms of Darwin's book.

This theory explains the phenomena under consideration better than any other which has been proposed, and it is not at variance with the principles of geology, which teach us, that some parts of the crust of the globe are rising, and others subsiding at the present day. It seems to us, however, that it is attended with difficulties, of which some are perhaps apparent but others are real.

First, The anomalous facts are rather numerous. An inspection of the map shews that atolls and barrier reefs occur in "areas of elevation," and fringing reefs and volcanoes in "areas of subsidence," unless we confine these areas within very narrow limits. We grant, however, that this objection may admit of an answer. For instance, in an area that is rising, corals may take root upon a subaqueous rock or bank when it comes within less than 200 feet of the surface, and raise upon it an atoll. Again, a volcano like that of Monte Nuovo, near Naples, may break out in an area that is stationary or subsiding; and thus the indications of elevation and subsidence may be found intermingled.

Secondly, If the theory is correct, we would expect to find in areas of elevation, fringing reefs in a great variety of stages—some 2 or 3 feet above low water, some 2 or 3 yards, some with the lagoon channel almost, and others with it altogether, obliterated. That there are examples of this transition from the fringing reef to the coral rock on dry land, and that corals are found at considerable heights, we do not deny; but they occur, in our opinion, much more rapidly than they ought to do, considering that the areas supposed to be uprising are of great extent, and many of them often visited and well known.

Thirdly, What seems to us the most serious objection to the theory, remains to be stated. On the outside of coral reefs very highly inclined, no bottom is sometimes found with a line of 2000 or 3000 feet, and this is by no means a rare case. It follows that the reef ought to have this thickness; and Mr. Darwin's diagrams, pages 48 and 98, shew that he understood it so. Now, if such masses of coral exist under the sea, they ought somewhere to be found on *terra firma;* for there is evidence that all the lands yet visited by geologists have been at one time submerged. But neither in the great volcanic chain, extending from Sumatra to Japan, nor in the West Indies, nor in any other region yet explored, has a bed or formation of coral, even 500 feet thick, been discovered, so far as we know. We state this objection, not as conclusive against the theory, but as one deserving the able and ingenious author's consideration.†

3. *The Structure and Distribution of Coral Reefs* (London: Smith, Elder, 1842).†

Double Flowers—Their Origin[1]

The inclosed specimens appear to me curious, as in some degree connected with the origin of double flowers. They consist of plants of the Gentiana amarella, found in a wild state, covered with abortive buds, or rather minute double flowers. Each head consists of innumerable small petal-like purplish scales, having in their centre a tuft of still smaller green scales. A plant covered with these little heads not infrequently bears, especially near the top of the stem, one or two more perfect flowers. By examining these, a series can be shown, by which the stamens are seen to become deformed, and gradually to pass into small petals and scales. The pistil also can be traced, becoming more and more

foliaceous. The change in the pistil has been effected in several flowers, whilst the stamens have remained nearly perfect. In the same manner I have observed in double Violets and some other garden flowers, that the pistil, contrary to the general rule, is metamorphosed before the stamens. In other semi-perfect flowers of the Gentiana, the divisions of the corolla and the number of the stamens, with their filaments flattened, are increased; in others, besides the five ordinary stamens, in an imperfect state, the divisions of the corolla are partially converted into stamen-like bodies: if this conversion had been effected, the flower would have become apetalous. In a Bladder-nut (Staphylea) growing in a shady wood, I last summer noticed a similar fact, namely, that the petals showed a tendency to form additional stamens. The plants of the Gentiana bearing the little tufts are generally, but not always, dwarfer than the perfect plants; their leaves are less pointed, and the entire plant is much less symmetrical. The much greater number of the imperfect flowers on one plant than are ever produced of the perfect, shows, I presume, that the metamorphic change must be determined early in the plant's life. Except in their small size, less beauty, and in the occasional presence on the same stem of flowers in different stages of monstrosity, these purple tufts seem to be essentially similar in their nature to the double flowers of Horticulturists.

The plants of the Gentiana in both states grow mingled together on a very hard, dry, bare chalk bank; but those with the abortive flowers grow on rather the barest spots, where it was surprising that anything could grow. You state in your "Theory of Horticulture,"[2] that the origin of double flowers is not well understood. Some have attributed it to excess of food; but the dry chalk bank surely was not too rich a soil; and I may mention that late last autumn, I found on an adjoining field of wretchedly sterile clay, great numbers of the Ranunculus repens, producing semi-double flowers, some having three, some additional rows of petals. The partial or entire sterility of double flowers is generally attributed to their doubleness; but is not this putting the effect before the cause? It is well known that plants (and indeed animals, as I could show by a series of facts) when placed out of their natural conditions, become, often from apparently slight and unintelligible causes, sterile. How many American plants fail in producing pollen in this country! the anthers of the Persian and Chinese Lilacs, as I observed this summer, are as destitute of good pollen as if they had been hybrids. Other plants produce good pollen, but are defective, as it appears, in their ovules, as their germen never swells. Linnaeus has remarked that most Alpine plants, when cultivated in the lowlands, are rendered quite sterile. In most of these cases, we see that sterility is compatable with long life and health. Is it, then, too bold a theory to suppose that all double flowers

are first rendered by some change in their natural condition, to a certain degree, sterile; and that their vessels being charged with organizable matter in excess, (which would be greatly formed by high cultivation,) it is converted into petals—the organs which are nearest in their morphological nature and position to those whose functions are checked? Is there any shadow of truth in this theory, or is it an abortive one, as are the buds of the Gentiana? (We can only say that this is at least as reasonable an hypothesis as any that we have seen; but the greater frequency of double flowers in gardens where soil is rich, than in fields where it is poor, offers some difficulty in the way of Mr. Darwin's speculation.)[3] P.S.—I also send a curious Cabbage-leaf, grown into the form of a perfect funnel, like the fold of paper into which grocers put sugar. It was borne on a long footstalk from the centre of an old stalk, from which a Cabbage had been cut this summer. I remember that De Candolle describes pitchers at the end of the leaves of some Cabbages, which he compares to those of the Nepenthes. Is this leaf something of the same kind?

1. *Gardeners' Chronicle and Agricultural Gazette*, no. 36, 9 September 1843, p. 628.†
2. John Lindley, *The Theory of Horticulture; or an Attempt to Explain the Principal Operations of Gardening upon Physiological Principles* (London: Longman, Orme, Brown, Green, and Longmans, 1840).†
3. Statement of the *Gardeners' Chronicle* editor.†

Observations on the Structure and Propagation of the Genus *Sagitta*[1]

The species of this genus[2] are remarkable from the simplicity of their structure, the obscurity of their affinities, and from abounding in infinite numbers over the intra-tropical and temperate seas. The genus was founded by MM. Quoy and Gaimard[3]; three species have been figured and described by M. A. d'Orbigny, and lately Prof. E. Forbes has added a species to the British fauna, and has given many particulars regarding the structure of the genus. Scarcely any pelagic animal is more abundant: I found it in lat. 21° N. in the Atlantic, and again off the coast of Brazil in 18° S.; between latitudes 37° and 40° S., the sea, especially during the night, swarmed with them. They generally appear to swim near the surface; but in the Pacific, off the coast of Chile, I obtained specimens from a depth of four feet. They are not confined exclusively to the open ocean, as supposed by M. d'Orbigny; for near the shore of Patagonia, where the water was only ten fathoms in depth, they were very numerous.

All the individuals which I caught had two pair of lateral fins, but I do

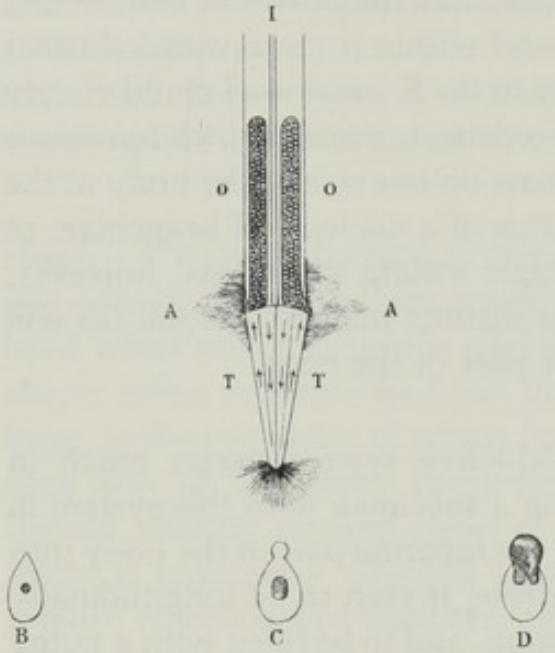
not suppose that they all belong to the same species: those obtained in lat. 37° to 40° S. appear certainly to be the *S. exaptera* of D'Orbigny; and the few following observations, which relate chiefly to their propagation, apply, when not otherwise stated, to this species. M. d'Orbigny and Prof. Forbes have provisionally placed this genus amongst the nucleo-branch mollusca; but the evidence is hardly conclusive.

Head.—The linear-lanceolate head, which is of a transparent, gelatinous and adhesive texture, is separated from the body by a distinct neck. The head when not in action is slightly flattened and of a truncate-conical shape; when in action its basal part assumes a semilunar or horse-shoe form, in the concavity of which lies the longitudinally-folded mouth. On each arm of the fleshy horse-shoe, a comb, formed of eight strong, curved, slightly hooked claws or teeth, is attached. The animal when lively is constantly clasping these bristle-like teeth together, over its mouth; when clasped together, and the head in a state of inaction, they appear to be situated much nearer to the mouth than when their fleshy bases are expanded in action. The middle teeth are the longest; besides their clasping action and the power of movement in their fleshy bases, each separate tooth can move itself laterally further from or nearer to the adjoining ones. The mouth opens on the oblique surface of a part projecting up, between the two fleshy arms. Close to the mouth there are two other rows of exceedingly minute teeth, which have not been noticed by other observers, and which I discovered only with a lens of high power. These two rows of little teeth project inwards and transversely to the two great upright combs of teeth; so that when these latter are clasped over the mouth the minute teeth cross them, thus effectually preventing any object from escaping which might be caught by the longer curved teeth. I could not see any vestige of eyes or of tentacula.

Locomotive Organs.—The animal moves quickly by starts, bending its body. The two pair of lateral fins and that on the tail lie in the same horizontal plane: viewed with a lens of small power they appear formed of a delicate membrane, but under a lens of 1/20th of an inch focal distance they appear to consist of excessively fine transparent rays, touching each other, like the barbs of a feather, but not, as it appeared to me, actually united by a membrane. The tail, besides being used as a locomotive organ, serves as a means of attachment; for the animal when placed in a basin of water sometimes adhered by its tail so firmly to the smooth sides, that it could not be detached by a considerable agitation of the water. Out of the innumerable specimens which I procured, I never saw one fastened by its teeth to the ova of pelagic animals, or to other bodies, as M. d'Orbigny has observed in some of his species.

Internal Viscera.—Within the body, in the same plane with the longitudinally folded mouth, there is a flattened tube or cavity, which in the specimens obtained in lat. 18° S. I observed had the power of contracting and enlarging itself in different parts, and within it there was a distinct peristaltic movement. Within this cavity in the *S. exaptera* I could clearly discern in the posterior half of the body a delicate vessel, which I presume is the intestine, for it appeared to terminate on one side of the body at the base of the tail. I could discover no vestige of a nucleus, of branchiae, of a liver, or of a heart. In some exceedingly young specimens, however, just liberated from the egg, there was a distinct pulsating organ (as will hereafter be mentioned) in the anterior part of the body.

Propagation.—The state of the reproductive system varies much in animals caught at the same time. Taking a specimen with this system in a high state of development, the tail, or the tapering part of the body into which the intestinal tube does not penetrate, is seen to be longitudinally divided by an exceedingly delicate partition, and to be filled with a pulpy finely-granular matter. The column of matter on each side of the central division also *appears* (but whether really so I do not know) to be divided, making altogether four columns, as is shown in the diagram. The whole of this matter is in a state of steady and regular circulation, something like that of the fluid in the stems of the *Chara*. The matter flowed upwards in the two outer columns, and downwards towards the point of the tail in the two middle columns. The circulation in the up-flowing columns was most vigorous on their outer sides; and in the down-flowing columns on their insides, that is, on each side of the central partition: this would be accounted for, if we might suppose that the two surfaces of the central partition were covered with cilia, vibrating in a direction opposite to that in which other cilia situated on the inside of the membrane forming the tail were also vibrating. The stationary condition of the granular matter between the two streams, travelling in opposite directions, perhaps gives the appearance of the partition on each side of the central one. The circulation at the base of the tail was twice as rapid as it was near the apex: where most rapid I found that a granule travelled over the 1/250th of an inch on the micrometer in five seconds; allowing for the slower rate in other parts, I calculated that in an individual, the tail of which was 3/20ths of an inch in length, a granule performed its entire circuit in about six minutes. I could distinctly follow the granules descending one column, turning the angle, and again ascending. In specimens with the reproductive system in a lesser stage of development, the tail contained very little granular matter; and in proportion as this was less in quantity, so was the circulation less and less vigorous: in some specimens no granular matter, and perhaps, consequently, no circulation, was visible.



Pl. I. Structure of Sagitta. I. intestinal tube; *oo*, ovaries; AA, apertures of the ovaries, and lateral fins; TT, tail divided into four columns of circulating granular matter, the course of which is shown by the arrows; B, egg just liberated from the ovary; C, egg in first state of change; D, egg in a succeeding state.

When the tail is filled with vigorously circulating matter two large *cul-de-sacs* or gut-shaped ovaries are invariably present, extending, as represented (*oo*) in the diagram, from the base of the tail along each side of the intestinal tube. These are filled with ova, which in the same animal are in different stages of development, and vary in length from 1/100th to 1/50th of an inch; their shape is pointed oval (Plate I. fig. B), and they are attached by the pointed end in rows to the sides of the ovaries: those of full size are detached by a very slight touch. When the ovaries contain many eggs nearly perfect (but not at other times), a small conical and apparently perforated protuberance can be seen on each side (AA) of the body, through which without doubt the eggs are expelled. In different individuals the ovaries are of different sizes and the eggs in different stages of development: before any of the eggs are perfected the ovaries are merely filled with granular matter; but this is invariably of a coarser texture than that within the tail. The ovaries when not containing granular matter are contracted into a very small size[4] (B). In great numbers of specimens taken in latitude 18° S. and between 37° and 40° S., I invariably observed that there existed a close relationship between the quantity of circulating matter within the tail and the size of the ovaries; from this circumstance, and from the similarity of the granular matter in the ovaries, before any of the eggs are perfected, with that in the tail, except that the granules are in this latter part of less size, I think it almost certain that the granular matter is first formed within the tail, and that it then passes into the ovaries, where it is gradually developed into ova. I could not, however, trace any opening from the one part into

the other, but at the bottom of each ovary there was a space, where a closed orifice might have been situated.

A well-developed egg presents, when liberated by a touch from a torn open ovary, the appearance represented at (B) in the diagram. The egg is transparent, and contains within it an exceedingly minute globule. Twice on one day and once again a week afterwards, I clearly observed the following curious phaenomenon take place: the apex of the egg, a few minutes after having been liberated from its attachment, began and continued to swell, and soon assumed the form shown by (C). Whilst this was going on, the small internal globule also appeared to be swelling, and at the same time the transparent fluid with which the ovum and its enlarged apex were charged, became more and more opake and granular. The apex continued enlarging until it became nearly the same size with the ovum from which it proceeded; and as this took place, all the granular matter was slowly expelled from the original capsule into the newly-formed one, in a manner which seemed to show that it was effected by the contraction of a lining membrane as represented at (D). Directly that this was completed the two balls slowly separated; one being left a mere empty husk, and the other consisting of a spherical mass of granular matter, within which a minute globule could be discovered. I presume that this was the same globule as seen within the egg in its first state (as at B), and that the appearance of its swelling was caused by the transparent fluid round it being first converted into granular matter. I have reason to suppose from what follows that this little globule contains only air. The whole phaenomenon was effected in about ten minutes; and in one case I watched the entire process without taking my eye from the microscope.

On the 27th and 29th of September 1832, we passed[5] through the same tract of sea (off Bahia Blanca on the coast of northern Patagonia) where twenty-five days previously I had observed such great numbers of the *S. exaptera* with their ovaries distended with eggs, and I now found infinitely numerous ova floating on the surface. They were in different states of maturity; those least developed presented a sphere of granular matter contained within a larger spherical case. In the next stage the granular matter collects in a linear manner on one side of the inner sphere, and projects slightly beyond its outline; it then soon forms a distinct prominent rim, extending round two-thirds of the circumference of the inner sphere. This prominent rim is the young animal; a fine vessel is seen extending within its entire length, and one extremity enlarges into a head: the tail is first liberated from its attachment on the surface of the inner sphere, and lastly the head: the young animal, when thus released, lies in a curved position within the outer case, with the inner sphere, on the circumference of which it was developed, pushed on one side, and its

function apparently ended. The central intestinal vessel is now much more distinct: an excessively fine membrane-like fin is discernible round the end of the tail; and the young animal being liberated from the outer spherical capsule, progresses by a starting movement like that of a full-grown *Sagitta*. At the anterior extremity, near the head, a pulsating organ can be distinctly seen. The ovum in all these stages contains a minute globule, which causes it to float on the surface of the water, and apparently is formed of air: I presume that it is the same globule with that seen in the egg, when first released from the ovary. The change in the floating ova from the state in which the inner sphere consists of granular matter without any trace of a young animal to the succeeding states must be rapid; for on the 27th of September all the ova were in this first state, whilst on the 29th the majority contained partially developed young ones. These floating ova were 1/14th of an inch in diameter, whereas the spherical balls of granular matter which I saw expelled from their pointed oval cases were barely the 1/50th of an inch in diameter; but as the eggs within the ovaries were of different sizes, according to their states of maturity, we might expect that their growth would continue after having been expelled from them. I will conclude by expressing a hope that these few observations on the propagation of this curious genus may aid more competent judges than myself in ascertaining its true affinities.

1. *Annals and Magazine of Natural History, including Zoology, Botany, and Geology* 13(1844):1–6.†

2. *Sagitta:* arrowworms, marine invertebrates, Chaetognatha.†

3. Annales des Sciences Naturelles, tom. x. p. 232. M. d'Orbigny's observations are given in his grand work (Mollusques, p. 140). Prof. E. Forbes four years since made his first communication on this genus before the Wernerian Society, and a second one at the Meeting of the British Association for the present year.

4. I also remark in my MS. notes, that the granular matter within the tail is sometimes contracted into small kidney-shaped bodies; I cannot help suspecting that I ought in every case to have written that the ovaries were contracted into this form.

5. I may add, that in the beginning of April, off the Abrolhos, on the coast of Brazil, in lat. 18° S., numerous specimens of a four-finned *Sagitta* had their ovaries filled with eggs apparently ready to be expelled.

Brief Descriptions of Several Terrestrial *Planariae*, and of Some Remarkable Marine Species, with an Account of Their Habits[1]

In my Journal I have given a brief account of the discovery of several species of terrestrial *Planariae:* it is my intention here to describe them. They all belong to the genus *Planaria*, as restricted by A. Dugès in his memoir[2] on these animals, and to that of *Polycelis* of Ehrenberg. They

may, however, form a section of the genus, being characterized by their more convex and narrow bodies; their more distinctly defined foot; their terrestrial habits; and frequently by their longitudinal bands of bright colours. From their colours, from their convex bodies, from their manner of crawling and the track of slime which they leave behind, and from their places of habitation, they present a striking analogy with some terrestrial gasteropods, especially with *Vaginulus*, with which snail I have several times found them associated under stones. I suspect that, differently from their aquatic congeners, they live on vegetable matter, namely on decayed wood; I suspect this, from having found them repeatedly under this substance, and from having kept some specimens in a box for twenty-one days with nothing else for food, where they increased considerably in size. The species which live under stones, both on the grassy undulating land of northern La Plata, and on the arid, rocky hills of central Chile, generally inhabit small sinuous chambers, like those frequented by earth-worms, in which they lie coiled and knotted up. They are often found in pairs; and I once discovered a pair attached together by their lower surfaces, apparently in copulation. None of these species have the quick and vivacious movements of the marine species: they progress by a regular wave-like movement of the foot, like that of a gasteropod, using the anterior extremity, which is raised from the ground, as a feeler. One species which I tried could crawl well through moss; another being placed on dry paper was almost killed by it. I put several specimens into fresh water, but they appeared wholly unused to it, and would soon have perished; they seem, however, to prefer damp situations, and the specimens of *P. Tasmaniana*, which I kept in a box with rotten wood, having been neglected to be moistened, all perished, except one large individual which survived quite uninjured, although the wood had become perfectly dry. These animals (especially the *P. Tasmaniana*) had an immediate apprehension and dislike of light, which they showed by crawling, when the lid of the box was taken off, to the under side of the pieces of rotten wood. My observations, as far as they go, on the structure of these terrestrial species, agree with those given by Dugès on the structure of the aquatic species. The figure given by this author of the ramified digestive vessels of *P. lactea* is quite similar to a drawing that I made of this part in the *P. pallida* from Valparaiso (which, from being nearly colourless, allowed the best opportunity of observation), except in the entire absence of ramifications on the internal sides of the two posterior prolongations of the main digestive cavity. There is generally a colourless space round the alimentary and genital orifices. The mouth-sucker is bell-shaped, with a very short oesophagus: when contracted it forms either a globular or star-shaped hard ball: I never saw it voluntarily protruded, but have no doubt that it can be, for

immersion into very weak spirits of wine or salt water caused its exsertion, and on being touched it was immediately retracted. This mouth-sucker is highly contractile, and retains its irritability long after the death and even dissolution of the rest of the body: the external orifice, through which it is protruded, consists of a transverse slit. The genital orifice, also, consists of a transverse slit; in the aquatic species it is generally, if not always circular. In my notes on several of the species, I find it stated that the under surface or foot is thickly studded with very minute, angular, opake, white specks: may not these serve for the necessarily copious secretion of slime? These animals, when placed on a slip of glass, frequently propelled a globule of air, between their foot and the glass, from their anterior extremity towards their tail; and as the air came in contact with successive parts of the foot, a violent corpuscular movement (curiously resembling microscopical eels disturbed by a stick, and struggling in mud) was produced in the slimy surface. I could never perceive it in any part of the foot, except when in contact with air; but it was evident, though less energetic, on parts of the back, and at the extreme anterior extremity of the body. I presume that the appearance is due to vibratile cilia; and it is worthy of remark, that M. Dugès[3] suspects that the foot, in the freshwater species, is the chief seat of this respiratory action, from having observed that they frequently arch their bodies, so as to allow fresh water to circulate under it. The position of the black eye-spots varies in the different species: it is remarkable that, in the *P. elongata* from Tres Montes, I could perceive no trace of these ocelli, although this is the largest species. According to Prof. Ehrenberg's arrangement, depending on the presence and number of the ocelli, this species would rank in his genus *Typhoplana;* but from the variability in number and position of these imperfect organs of vision, I should doubt whether they ought to afford generic characters. In the *P. pallida* I examined the ocelli with a strong lens, and found that they were not truly circular; the black part lies within a transparent envelope; in this species they are seated on the upper margin of the body, in groups of two and three, exactly over the extreme lateral subdivisions of the intestinal vessel. I was not able to see ova within any of the terrestrial species. The texture of the body, its prompt dissolution into fluid after death, its power of healing wounds, its irritability and contractile powers, appear to be exactly similar in the terrestrial and in the aquatic species, as described by Dugès. I will not here repeat the description which I have given in my Journal (p. 31)[4] of the bisection of the *P. Tasmaniana*, and the production of two perfect individuals (with the exception of the external orifice for the mouth-sucker) in the course of twenty-five days. I will only add, that an individual being divided into many fragments, each crawled in the proper direction, as if furnished with its proper anterior extremity.

I found altogether twelve terrestrial species; two in the forests of Brazil; three on the grassy, open country northward of the Rio Plata; one on the arid hills near Valparaiso in Chile, and three in the damp wooded country southward of central Chile: the most southern locality was in lat. 46° 30′ S. I found also one species in New Zealand (which I lost), another in Van Diemen's Land, and a third at the Mauritius; the latter I had not time to examine. Hence it appears that the terrestrial section of this genus is widely diffused; but as far as is at present known, only in the southern hemisphere. The existence of terrestrial *Planariae* is analogous to that of terrestrial leeches in the forests of southern Chile and of Ceylon.

1. Planaria vaginuloides

Alimentary orifice situated at two-thirds of the entire length of the body from the anterior extremity; width of orifice 1/60th of an inch: at the distance of 3/10ths of an inch posteriorly, lies the genital orifice, very plainly marked. Ocelli numerous, placed at regular intervals on the anterior extremity; irregularly, round the edges of the foot. Anterior part of the body elongated, with the extremity much pointed and grooved on the under side: tail bluntly pointed; body convex, flattened on the top. Sides and foot coloured dirty "orpiment orange"[5]; above, with two stripes on each side of pale "primrose-yellow," edged externally with black; on centre of the back a stripe of glossy black; these stripes become narrow towards both extremities. Length when fully extended 2 3/10ths of an inch; breadth in broadest part 13/100ths of an inch.

Hab.—Under the bark of a decayed tree in the forest: Rio de Janeiro (June).

2. Planaria elegans

Position of the orifices as in *P. vaginuloides*. Anterior part of the body little elongated. Ocelli absent on the anterior extremity, and only a few round the margin of the foot. Colours beautiful; back snow-white, with two approximate lines of reddish brown; near the sides with several very fine parallel lines of the same tint; foot white, exteriorly clouded, together with the margin of the body, with pale blackish purple: body crossed by three colourless rings, in the two posterior of which the orifices are situated. Length 1 inch; breadth more uniform, and greater in proportion to length of body, than in the last species.

Hab.—Same as in *P. vaginuloides*.

3. Planaria pulla

Mouth-sucker, when protruded and contracted in spirits of wine, globular. Ocelli numerous, placed at regular intervals on the anterior part of the body. Body slightly flattened, gradually increasing in width from the anterior extremity, which is much pointed and grooved beneath. Back rich "umber-brown," with a central narrow streak of "broccoli-brown" reaching entire length: foot broccoli-brown, with two clear spaces for the orifices. Length when fully extended 1 9/10ths of an inch; breadth 1/10th of an inch.

Hab.—Very frequent under stones: Monte Video and Maldonado (June and August).

4. Planaria bilinearis

Ocelli numerous, placed at regular intervals. Body subcylindrical, narrow, of nearly uniform breadth. Colour above pale dirty yellow with two stripes of "umber-brown," which become narrower and unite at the two extremities. Length when fully extended 1 3/10ths; breadth 7/100ths of an inch.

Hab.—Same as *P. pulla* (June and August).

5. Planaria nigro-fusca

Alimentary orifice situated at rather less than two-thirds of the entire length from the anterior extremity: genital orifice, with the body contracted, is situated at the 25/100ths of an inch posteriorly. Ocelli very numerous; those on the extreme tip very minute and placed at regular intervals; those on the margin of the body grouped by two and three together. Body much depressed, tapering suddenly towards the anterior extremity; tail abruptly terminated in a point. Above uniform blackish brown, beneath pale. Length when fully extended 2 inches; breadth 3/10ths of an inch.

Hab.—Under rotten wood: Maldonado (May).

6. Planaria pallida

The alimentary and genital orifices 2/10ths of an inch apart, when the body is partially contracted: mouth-sucker when dissected out of the

body 15/100ths of an inch in length; its margin very sinuous. Ocelli numerous; eleven close together, being placed on the anterior extremity; and the others in groups of two and three on the sides, and chiefly on the anterior half of the body. Body much depressed and flat, with both extremities finely pointed. Upper and lower surfaces white, with the pinkish intestinal vessel seen through. Length when crawling 3 inches; breadth 2/10ths of an inch.

Hab.—Under stones on the dry hills near Valparaiso (July).

7. Planaria elongata

Alimentary and genital orifices obscure. Ocelli absent: posterior extremity very obtusely rounded. Above "umber-brown," with a narrow medial line of darker brown; sides narrowly edged with pale brown, bordered with the umber-brown; beneath pale brown. Length when crawling 5 inches, when closely contracted 1 4/10ths of an inch, breadth when crawling 13/100ths, when contracted 4/10ths of an inch.

Hab.—On rotten wood in mountain-forests: C. Tres Montes, lat. 46° 30′ S., Western America (December).

8. Planaria semilineata

Body convex. Above greenish black, with minute white punctures; on anterior half of body four parallel bands of "gall-stone yellow," of which only the central and approximate pair are prolonged into the posterior half of body: foot leaden colour, with colourless spaces for the orifices.

Hab.—Under stones, on one of the Chonos Islands (north of C. Tres Montes) (December).

9. Planaria maculata

Edges of the body very thin; breadth nearly uniform. Upper surface quite black, with numerous, oblong, variously sized spots of yellow: foot mottled white and black. Length when crawling 1 7/10ths; breadth 2/10ths of an inch.

Hab.—Forest of Valdivia (February).

10. Planaria Tasmaniana

Mouth-sucker widely extensile: alimentary orifice placed nearly in centre of the body; genital orifice 1/10th of an inch posteriorly, but when the animal crawls it is 2/10ths of an inch distant. Genital orifice very distinct, submargined. Ocelli scattered round the entire margin of the foot, but most frequent at the anterior extremity. Both extremities pointed. Colour dirty "honey-yellow," with a central dark brown line bordered on each side with a broader line of pale "umber-brown;" foot quite white. Length when crawling 1 5/10ths; when contracted 8/10ths of an inch.

Hab.—Beneath decayed trees in the woods of Van Diemen's Land: frequent (February).

I will now briefly describe five marine species of *Planaria*, which are remarkable, either as presenting novel points of structure, hereafter probably forming the types of new subgenera, or from the situations which they inhabit.

1. Planaria (?) oceanica. Plate V. fig. 1. Under-surface Magnified

Anterior extremity neck-shaped, with two ear-like processes. Ocelli, I believe, absent. Posterior extremity broadly rounded. Membranous margin of body jagged. Length 2/10ths of an inch. Colour pale, uniform. Near the neck there is a quadrangular, internal, clear space, apparently lined by a membrane, within which there is a dark-coloured spot, and externally close by it an orifice, which the animal can dilate and contract at pleasure. Close behind this there is an internal oval space, within which there is a second dark spot united to a delicate vessel; I was unable to distinguish any orifice near this point: these organs form, I presume, the reproductive system. Close behind these organs there is a dark space formed by the union of eleven, branching, intestinal cavities, in the centre of which there is a longitudinal orifice situated rather behind the centre of the body. Through this orifice the animal can protrude a folding mouth-sucker: when it begins to unfold it is seen to be drawn into eight folds, as represented at (B).

Hab.—Open ocean, lat. 5° S., long. 33° W. (February).

This I believe is the first instance of a species of this genus being found in the open sea, at the distance of 150 miles from the nearest part of S. America, and 80 miles from the small island of Fernando Noronha.

2. Planaria (?) formosa

Body much depressed, oval. In the posterior half, on the under side, there is a very large alimentary orifice with folding lips (but apparently with no exsertile mouth-sucker), from which the two main intestinal cavities branch. Near the anterior extremity there is a minute orifice, and between it and the mouth a second orifice: these the animal can dilate and contract; they lie over an opake, wedge-formed, internal mass, and form, I presume, two genital orifices. Back dotted with purplish red, with a central band of "vermilion-red," edged with white: this band sends off three branches on each side; at the extremity of each of the two anterior branches there is a longitudinal group of black ocelli, and before these two other circular groups, forming together four groups of ocelli. Length when extended half an inch. Inactive in its movements.

Hab.—On corallines, at a depth of 30 fathoms, in southern Tierra del Fuego (December).

3. Planaria (?) macrostoma. Plate V. fig. 2. Under-side Magnified

External alimentary orifice situated in the posterior half of body: mouth-sucker nearly subcylindrical, bell-shaped, very long; when contracted within the body it lies in a serpentine position; when partly protruded it has the figure as represented; when fully extended it tapers only slightly from its mouth to its base, and is so long, that the animal can pass it from the under surface over the entire width of its back. Its base is united, in the middle of the body, to the three principal branches of the intestinal cavity; the two posterior branches unite and form a ring, enclosing the space in which the mouth-sucker and its external orifice are situated. The three main branches receive the moss-like subdivision of the intestinal cavity, which reach all round nearly to the margin of the body. The main, medial, intestinal cavity ends at the anterior extremity in a small, opake, wedge-formed mass; on each side of which, nearly on the dorsal surface, a black ocellus is situated. Between the lateral branches on each side of the medial cavity, seven or eight internal spherical cavities lie, including opake balls, which I presume are immature ova; the anterior ones were most developed: they were not present in the smaller specimens, or in all the full-grown ones. I was unable to discover any genital orifice, though no doubt one or two exist: near the posterior extremity (at B) there was a colourless space, but I could not see any orifice. Anterior extremity square, truncate, with the edges thin and prehensile; the animal attaches itself by this part, almost like a leech with its sucker, and thus drags its body: posterior extremity broadly rounded.

Above, faintly coloured brownish purple in striae, with a colourless space over the alimentary orifice. Length 2/10ths; breadth 6/100ths of an inch.

Hab.—Congregated in numbers under stones, in *brackish water;* Chonos Archipelago (west coast of S. America) (December).

The arrangement of the main branches of the intestinal cavity is the same as in the terrestrial *Planariae*, with the exception of the two posterior branches being united near the extremity of the body into a ring, which structure I have not met with described in any other species. Hence this species probably ought to form the type of a new subgenus. I may here mention that I found amongst these islands an elongated marine species (with a very distinctly formed head placed on a narrow neck) which had the power of crawling either backwards or forwards,—a power I have never seen in any other species.

4. Planaria (?) incisa. Plate V. fig. 3. Under-surface Magnified

Body oval, very much depressed, highly contractile; margin sinuous, anteriorly deeply indented, posteriorly less so. Ocelli very numerous and crowded together in several rows on the indented anterior (as is known by its progression) margin. Along the centre of the body an intestinal vessel extends, and in the middle of this (B) there is a well-closed orifice, through which the animal can protrude a thin, much-folded, sinuated mouth-sucker; this when fully expanded is quite as wide as the body. Posteriorly, on each side of the central vessel, there is a mass, apparently of immature ova. Near the posterior extremity there is a second subterminal orifice (D), through which, when the animal was placed in spirits, a little globular mass was protruded, like a small, much-contracted mouth-sucker. Near to the anterior extremity there are two slightly retractile paps with orifices, of which the anterior one is the largest. From this point diverging rays (intestinal cavities?) are sent off, which reach nearly to the margin of the entire body: when the animal contracts itself, the back is raised in slight ridges, corresponding with these rays. This species, therefore, has four orifices on its under surface. Back finely reticulated with brownish purple. Length 1 inch; breadth three-quarters of an inch.

Hab.—Under stones on the sea-beach, St. Jago; Cape Verd Archipelago (February).

This species is exceedingly active and irritable in its habits: it lives, like a *Nereis*, under stones firmly imbedded in the beach at low-water mark.

It has the power of adhering with great tenacity to smooth stones: another allied species had the same power, could also swim well by a vertical movement of its body, and frequently rolled itself into a ball.

With respect to the four orifices: I presume, as in the *P. formosa*, the two anterior ones belong to the reproductive system. The central orifice undoubtedly is the mouth: the posterior one would naturally be thought to be the anus; but I am doubtful of this, considering the little globular body which was protruded through it, and from the existence in the following allied genus of a double mouth.

Diplanaria (nov. genus)

Alimentary orifice double, with two exsertile mouth-suckers. Two genital orifices in the posterior part of the body. A large forked ovarium (?). Ocelli in four groups, two superficial and two more deeply seated. The characters here given appear to me absolutely to require the institution of a new genus.

Diplanaria notabilis. Plate V. fig. 4. Under-side Magnified

Body very much depressed, with the edges very thin; anterior extremity thrice as broad as the posterior. On the under surface, towards the anterior extremity, there is a clear space, over which, on the back, the ocelli are situated; into this space, on all sides, the branching, clear, intestinal cavities enter. Each intestinal cavity generally bifurcates three times before its fine extremities reach the margin of the body. Towards the posterior extremity there is a second clear space (with the two orifices D and E), into which also the surrounding intestinal branching cavities enter; these two spaces are united by two longitudinal clear spaces (obscured by ovules in the drawing) passing on each side of the elongated, opake, white, central organ. This organ, when the animal is contracted, has the appearance represented in the drawing, namely of an internal, elliptic mass, narrowing at each end, with deeply sinuated borders, and with two external, perfectly closed orifices over it, as shown at (B) and (C). But when these two orifices are opened, from both of them broad, shallow, saucer-like mouth-suckers are protruded, as represented at (F); these, when contracted within the body, appear united, and form a single, elliptic, sinuated body. These two mouth-suckers are quite similar; they are much shallower than those of any other species of the family which I have seen; their membranous edges are very thin, narrow, transparent and sinuous: in the act of contraction they become folded in a complicated manner, like the bud of a flower. I was able easily to dissect them out of the body, and they retained, in the characteristic

manner described by Dugès, and as in the terrestrial *Planariae*, an extreme degree of irritability and contractile power, long after the rest of the body had ceased to live.

In the elliptic space surrounding the two mouth-suckers when contracted, and between the mouths of the lateral, branching, intestinal cavities, innumerable ova are arranged in groups, from two to four in each; these are represented in the drawing only by double dots. These ova were easily separated; they are spherical, 3/500ths of an inch in diameter, and contain a central opake mass. In the posterior clear space there are two minute, but quite distinct, orifices (D and E), which I do not doubt are the reproductive pores: into this clear space a large fork, filled with opake white matter, enters, as is shown in the drawing; this matter consists of minute, white globules in chains, imperfectly united together: I believe these are immature ova, and hence I suppose that the fork is the ovarium, from which the ova pass into the clear spaces surrounding the mouth-suckers and are there matured.

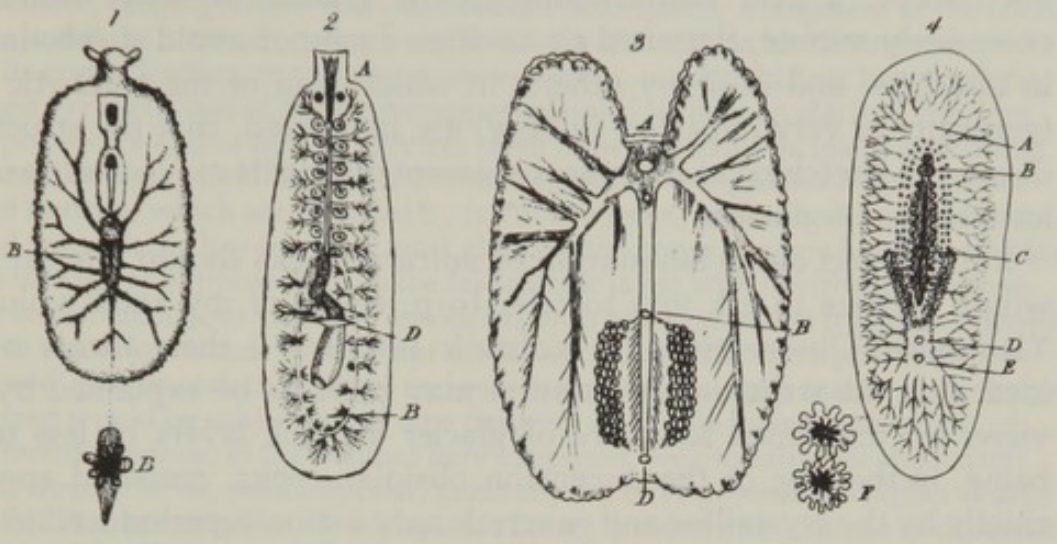
The ocelli are black and circular, and are arranged in four groups, two of which are round, and two in elongated bands inclined to each other: the ocelli in the bands are not seated on the dorsal surface, but deep within the body, near the ventral surface. Colour pale "tile-red," darkest on the dorsal ridge, with colourless spaces over the genital orifices and over the ocelli. Length 55/100ths of an inch; breadth of anterior part of body 3/10ths; of posterior part 1/10th of an inch.

Hab.—Under stones in tidal pools, Chonos Archipelago (Western S. America) (December).

This animal is very active, can crawl quickly, and can swim well by the movements of its thin marginal edges: it can adhere firmly to stones.

This is the most complicated and singular form of the large family of *Planariae* which I have seen or met with described. The presence of two alimentary orifices and two mouth-suckers is another and interesting point of affinity between the *Planariae* and the true parasitic worms, in which the number of mouths so often exceeds one. I believe that the presence of the large forked ovarium, and of groups of ocelli situated at different depths, are peculiarities of structure confined to this genus. If the small mass protruded from the posterior orifice (D) of the *Planaria* (?) *incisa* was really a small contracted mouth-sucker, this species is closely allied to our present new genus; with the chief difference of the two genital orifices being near the anterior, instead of the posterior extremity.

I will conclude by remarking, that the family of *Planariae* is most widely diffused, and is adapted to the most different stations; on the land, it is adapted to forests and plains, in hot, temperate, and dry climates; in water, under all latitudes, to fresh, brackish and salt, on sea-beaches, at the depth of 30 fathoms, and in the open ocean.



1. Planaria oceanica. *3 Planaria incisa.*
2. ..... macrostoma. *4 Diplanaria notabilis.*

C Darwin del.

Pl. V. Four species of *Planaria:* (1) *oceanica,* (2) *macrostoma,* (3) *incisa,* (4) *Diplanaria notabilis.*

1. *Annals and Magazine of Natural History, including Zoology, Botany, and Geology* 14(1844):241–51.†
2. Annales des Sciences Naturelles, October 1828.
3. Annales des Sciences Naturelles, October 1828, p. 28.
4. *Journal of Researches into the Geology and Natural History of the Various Countries Visited by H.M.S. Beagle, under the Command of Captain FitzRoy, R.N., from 1832–1836* (London: Colburn, 1839).†
5. The colours, when placed between inverted commas, signify that they are given by comparison with Patrick Syme's Nomenclature. [See Patrick Syme, *Werner's Nomenclature of Colours, with Additions, Arranged so as to Render it . . . Useful to the Arts and Sciences,* 1st ed. (Edinburgh, 1814), *Werner's Nomenclature of Colours . . . (with Plates),* 2d ed. (Edinburgh: Blackwood; London: Cadell, 1821).†]

Extracts from Letters to the General Secretary, on the Analogy of the Structure of Some Volcanic Rocks with That of Glaciers[1]

I take the liberty of addressing you, knowing how much you are interested on the subject of your discovery of the veined structure of glacier ice. I have a specimen (from Mr. Stokes's collection) of Mexican obsidian, which, judging from your description, must resemble, to a considerable degree, the zoned ice. It is zoned with quite straight parallel lines, like an agate; and these zones, as far as I can see under the

microscope, appear entirely due to the greater or lesser number of excessively minute, flattened air cavities. I cannot avoid suspecting that in this case, and in many others, in which lava of the trachytic series (generally of very imperfect fluidity) are laminated, that the structure is due to the stretching of the mass or stream during its movement, as in the ice-streams of glaciers. * * *

If the subject of the lamination of *volcanic* rocks should interest you, I would venture to ask you to refer to p. 65–72 of my small volume of 'Geological Observations on Volcanic Islands.'[2] I there throw out the idea, that the structure in question may perhaps be explained by your views on the zoned structure of glacier ice, the layers of less tension being, in the case of the Ascension obsidian-rocks, rendered apparent, chiefly by the crystalline and concretionary action superinduced in them, instead of, as in zoned ice, by the congelation of water. * * *

How singular it at first appears, that your discoveries in the structure of glacier ice should explain the structure, as I fully believe they will, of many volcanic masses. I, for one, have for years been quite confounded whenever I thought of the lamination of rocks which have flowed in a liquified state. Will your views throw any light on the primary laminated rocks? The laminae certainly seem very generally parallel to the lines of disturbance and movement.

1. *Proceedings of the Royal Society of Edinburgh* [*1844–1845*] 2(1851):17–18.†

2. The laminated, volcanic rocks of Ascension, consist, as described by Mr. Darwin, of excessively thin, quite parallel layers of minute crystals of quartz (determined by Professor Miller) and diopside; of atoms of an oxide of iron, and of an amorphous, black augitic mineral; and, lastly, of a more or less pure feldspathic stone, with perfect crystals of feldspar placed lengthways. The following is a portion of the passage referred to:—

> Several causes appear capable of producing zones of different tension in masses semiliquified by heat. In a fragment of devitrified glass I have observed layers of sphaerulites, which appeared, from the manner in which they were abruptly bent, to have been produced by the simple contraction of the mass in the vessel, in which it cooled. In certain dykes on Mount Aetna, described by M. Elie de Beaumont, as bordered by alternating bands of scoriaceous and compact rock, one is led to suppose that the stretching movement of the surrounding strata, which originally produced the fissures, continued, whilst the injected rock remained fluid. Guided, however, by Professor Forbes's clear description of the zoned structure of glacier ice, far the most probable explanation of the laminated structure of these feldspathic rocks appears to be, that they have been stretched, whilst slowly flowing onwards in a pasty condition, in precisely the same manner, as Professor Forbes believes, that the ice of moving glaciers is stretched and fissured. In both cases, the zones may be compared to those in the finest agates; in both, they extend in the direction in which the mass has flowed, and those exposed on the surface are generally vertical. In the ice, the porous laminae are rendered distinct by the subsequent congelation of infiltrated water; in the stony feldspathic lavas by subsequent crystalline and concretionary action. The fragment of glassy obsidian in Mr. Stokes's collection, which is zoned with minute air-cells, must strikingly

resemble, judging from Professor Forbes's description, a fragment of the zoned ice; and if the rates of cooling and the nature of the mass had been favourable to its crystalization, or to concretionary action, we should here have had the finest parallel zones of different composition and texture. In glaciers, the lines of porous ice and of minute crevices seem to be due to an incipient stretching, caused by the central parts of the frozen stream moving faster than the sides and bottom, which are retarded by friction. Hence, in glaciers of certain form, and towards the lower end of most glaciers, the zones become horizontal. May we venture to suppose that, in the feldspathic lavas with horizontal laminae, we see an analogous case? All geologists who have examined trachytic regions have come to the conclusion, that the lavas of this series have possessed an exceedingly imperfect fluidity; and as it is evident that only matter thus characterized would be subject to become fissured, and to be formed into zones of different tensions, in the manner here supposed, we probably see the reason why augitic lavas, which appear, generally, to have possessed a higher degree of fluidity, are not, like the feldspathic lavas, divided into laminae of different composition and texture. Moreover, in the augitic series, there never appears to be any tendency to that kind of concretionary action, which, we have seen, plays an important part in the lamination of rocks of the trachytic series, or, at least, in rendering that structure apparent.

On the Origin of Mould[1]

As you have noticed a communication made by me to the Geological Society in 1837, on the Formation of Mould,[2] I should be much obliged if you would correct an error into which I have fallen. In a postscript to that paper I state that marl was put on a pasture field, since ploughed, 80 years ago: I should have said 30 years, as I mistook the figures in the paper sent me. I found out this on visiting the place four years and a-half subsequently, and examining the old occupier of the farm. Wishing to ascertain the accuracy of the stated depth at which the marl now lies buried, I had three long holes dug in different parts of the field, and in each I found the marl, together with some cinders and broken pottery, in a layer 13 inches beneath the bottom of the potato-furrows, which were about four inches beneath the general surface; so that these substances are now buried at a depth of no less than 17 inches. They will never, probably, be undermined by the worms, to any much greater depth, as they almost rest on the general substratum of pure white sand. I particularly examined the occupier, whether the field had ever been ploughed to a greater depth than six or eight inches, and he positively assured me that it never had. My original informant, therefore, rather underrated the depth at which the marl now lies; although probably in the interval of four and a-half years, between our observations, some soil may have been removed by the worms from beneath the marl. In the other fields, formerly examined, I found that the layers of lime and cinders were, in almost every case, about an inch lower than they previously were. It was curious to observe in some of the holes how distinct three layers were preserved; the uppermost of cinders being two inches beneath the surface

(on the former occasion one inch below), the middle layer of lime at four inches, and the lowest of cinders and burnt marl, at from 10 to 12 inches. I found this lowest layer wherever I dug, and likewise the other layers, but less regular, owing to different parts of the field having been limed and cindered at different periods. When digging in this field, after a long drought, I noticed, that one single clod of earth, about as large as a man's two hands, was penetrated by eight upright, cylindrical worm-holes, nearly as large as swan-quills, so that I could see through them. Now this shows the quantity of earth in a small space, which is often probably removed by the worms and brought to the surface. The boggy field mentioned in the postscript to my Paper, on which two years and a half before a thick layer of bright red sand had been strewed, and which, I was informed, was then buried three-fourths of an inch beneath the surface, I found four years and a half subsequently (*i.e.* seven years from the sand being put on) was exactly two inches beneath the surface. In that field (also rather boggy) which I have described in my Paper, as first reclaimed 15 years before, the burnt marl was buried at a depth of four inches; so that in these two cases the rate of sinking, or more properly of being undermined, has been nearly the same, namely, about two inches in seven years. In the fields, however, more particularly alluded to in this notice, in which the marl that was put on thirty-four years and a half before, then lay seventeen inches beneath the surface, the rate of being undermined has been much quicker, namely, three inches and four-tenths of an inch every seven years. This field is dry, and consists of black, poor, very light sandy soil. It has also been ploughed, which may make some difference; though it is clear, from the uniformity of the layer, that the marl must have sunk beneath the depth at which the plough could disturb it before the pasture had been broken up. I am surprised at the red sand on the most boggy field having been buried as much as two inches in the seven years, for I never saw a field on which there were so few worm-castings. One cannot, however, judge of the number of worms in a field from inspection at any one season.

1. *Gardeners' Chronicle and Agricultural Gazette*, no. 14, 6 April 1844, p. 218.†

2. See p. 49, "On the Formation of Mould."†

Manures, and Steeping Seeds[1]

Some of your readers may be amused at the style, as well as at the matter of the following quotations from "The Curiosities of Nature and Art in Husbandry and Gardening," published in 1707.[2] They show that the value of the inorganic parts of manure, and the advantage of steeping seeds, were well known at that time. "The whole secret of multiplication

consists in the right use of salts. Salt, says Palissy,[3] *is the principal substance and virtue of dung.* A field may be sown every year, if we restore to it by stercoration what we take from it in the harvest." . . . "Seeing all multiplication depends on salts, the main business is to get together a great quantity at little expense, that the profit may be the greater." The author then describes a method of making liquid manure, in three old casks, into which objects are separately thrown, according to the ease with which they decompose. He further urges the importance of burning all wild plants, and of carefully dissolving the soluble parts of their ashes, and then proceeds—"Take as many pounds of saltpetre or nitre as you have acres of land to sow. For each acre dissolve a pound of saltpetre in twelve pints of the water that sanks from the dunghill. When the saltpetre is quite melted, throw in a little of those salts of plants (i.e. ashes) according to the quantity you have of them. This liquor is then called the 'Universal Matter,' because nitre is truly the universal spirit of the elementary world. This is the main point of the whole secret of multiplication. We will for the future call the water that is got ready in the casks, *Prepared Water,* and the water from which the salts are extracted from plants, and the nitre, *Universal Matter.* For one acre, take twelve pints of the prepared water, and mix with it immediately the universal matter, in which there ought to be a pound of dissolved nitre. The vessel into which you put these liquors must be large enough to contain the corn which you design for one acre. Then strew in your corn into these liquors; there must be two inches of water above the seed. Leave the corn to soak for twelve hours, and stir it up and down every two. If by that time it do not swell, let it lie longer till it begin to plump up considerably. One third less of seed than usual will serve for an acre; nay, you may safely use but half as much, and mingle among it some straw cut very small, that the sower may take it up by handfuls and sow it in the ordinary way, as I have said already." The explanation the author offers of the use of soaking seeds is whimsical. He says that the first action is to "cut the covers that infold the sprouts," and that the second action is "to serve each grain of corn, as it were, instead of a loadstone, to attract the nitre of the earth, which the subterranean fires have reduced and driven into steams and vapours in the low and middle region of the air, for the nourishment of vegetables and of animals. This is not a vain imagination, a chimera, or empty notion."

1. *Gardeners' Chronicle and Agricultural Gazette,* no. 23, 8 June 1844, p. 380.†

2. Pierre le Lorrain de Vallemont, *Curiosities of Nature and Art in Husbandry and Gardening . . .,* trans. William Fleetwood (London: Brown, Roper, Coggan, 1707).†

3. Bernard Palissy, *Le Moyen de devenir Riche* (Paris: Fouet, 1636).†

Variegated Leaves[1]

Mr. Groom has stated in last Number that the leaves of some of his Pelargoniums have become regularly edged with white in consequence of his having watered the plants with sulphate of ammonia which had been exposed to the air for some time. Last autumn I planted many young Box-trees; and I have for some weeks observed that nearly all the young leaves in most of them are symmetrically tipped with white, giving the young branches a mottled appearance. I counted twelve trees thus affected. The older leaves are rarely tipped, with the exception of two bushes, in which they are regularly tipped, and the younger ones much less so. Mr. Groom states that in his Pelargoniums the older leaves are chiefly affected. The Box-trees are quite healthy, and growing well. I gave to some of them nitrate of soda, but it has made no difference in this variegation. Those growing in deep shade are not tipped, nor are some older trees. These facts may appear trivial; but I think the first appearance, even if not permanent, of any peculiarity which tends to become hereditary (as I fear is the case with the variegated Sycamore) deserves being recorded.

1. *Gardeners' Chronicle and Agricultural Gazette*, no. 37, 14 September 1844, p. 621.†

What Is the Action of Common Salt on Carbonate of Lime?[1]

I should be extremely obliged if any of your chemical readers would inform me whether salt and carbonate of lime (under the form of sea-shells) would, if slightly moistened and left in great masses long together, act in any degree on each other? It is, I believe, known that masses of the same substances will act on each other, of which smaller quantities will not. I do not ask this question for agricultural purposes (though possibly the answer might be of some interest in that point of view), but from having found in Peru a great bed of upraised recent shells, mixed with salt, which are decayed and corroded in a singular manner, so that the surfaces of the shells are scaling off and falling into powder. I may mention, as explaining one element in the value of sea-shells as manure, that they are dissolved by water with greater facility than apparently any other form of carbonate of lime: one proof of this I observed in a curious rock, from Chili, chiefly composed of small fragments of recent shells, which are all enveloped and cemented together by a pellucid calcareous deposit; but in some parts of this rock the little included fragments are in every stage of decay and disappearance; in other parts they are entirely dissolved, the little calcareous envelopes being left quite empty. Here we see that water, capable of

dissolving shelly matter, has penetrated through their thin films or envelopes of carbonate of lime, without having acted on them; these films, moreover, being a deposition from water within quite recent times.

1. *Gardeners' Chronicle and Agricultural Gazette*, no. 37, 14 September 1844, pp. 628–29.†

An Account of the Fine Dust which Often Falls on Vessels in the Atlantic Ocean[1]

Many scattered accounts have appeared concerning the dust which has fallen in considerable quantities on vessels on the African side of the Atlantic Ocean. It has appeared to me desirable to collect these accounts, more especially since Professor Ehrenberg's remarkable discovery that the dust consists in considerable part of Infusoria[2] and Phytolitharia.[3] I have found fifteen distinct statements of dust having fallen; and several of these refer to a period of more than one day, and some to a considerably longer time. Other less distinct accounts have also appeared. At the end of this paper I will give the particular cases, and will here only refer to the more striking ones and make a few general remarks.

The phaenomenon has been most frequently observed in the neighbourhood of the Cape Verd Archipelago. The most southern point at which dust is recorded to have fallen is noticed by Capt. Hayward,[4] on whose vessel it fell whilst sailing from lat. 10° N. to 2° 56′ N.; the distance from the nearest of the Cape Verd Islands being between 450 and 850 miles. Respecting the northern limit, the water for a great distance on both sides of C. Noon (in lat. 28° 45′) is discoloured, owing in part, according to Lieut. Arlett,[5] to the quantities of falling dust. Hence the phaenomenon has been observed over a space of at least 1600 miles of latitude. This dust has several times fallen on vessels when between 300 and 600 miles from the coast of Africa: it fell, in May 1840, on the Princess Louise[6] (in lat. 14° 21′ N. and long. 35° 24′ W.) when 1030 miles from Cape Verd, the nearest point of the continent, and therefore half-way between Cayenne in S. America and the dry country north of the Senegal in Africa.

On the 16th of January (1833), when the Beagle was ten miles off the N.W. end of St. Jago, some very fine dust was found adhering to the under side of the horizontal wind-vane at the mast-head; it appeared to have been filtered by the gauze from the air, as the ship lay inclined to the wind. The wind had been for twenty-four hours previously E.N.E., and hence, from the position of the ship, the dust probably came from the coast of Africa. The atmosphere was so hazy that the visible horizon was only one mile distant. During our stay of three weeks at St. Jago (to

February 8th) the wind was N.E., as is always the case during this time of the year; the atmosphere was often hazy, and a very fine dust was almost constantly falling, so that the astronomical instruments were roughened and a little injured. The dust collected on the Beagle was excessively fine-grained, and of a reddish brown colour; it does not effervesce with acids; it easily fuses under the blowpipe into a black or gray bead.

In 1838, from the 7th to the 10th of March, whilst Lieut. James in H.M.S. Spey was sailing, at the distance of from 330 to 380 miles from the continent, between lat.21° 10′ N., long. 22° 14′ W., and lat. 17° 43′ N., long. 25° 54′ W., considerable quantities of dust fell on his vessel, four packets of which, together with a written communication, I owe to the kindness of Mr. Lyell. The dust which fell on the first day (or the 7th) was preceded by a thick haze, and it is coarser than that which fell on the succeeding days: it contains numerous irregular, transparent, variously coloured particles of stone about the 1/1000th of an inch square, with some few a little larger, and much fine matter. The fact of particles of this size having been brought at least 330 miles from the land is interesting, as bearing on the distribution of the sporules of cryptogamic[7] plants and the ovules of Infusoria. The dust which fell on the three succeeding days resembles in appearance and in its action under the blowpipe that collected by myself off St. Jago, and is so excessively fine, that Lieut. James was obliged to collect it with a sponge moistened with fresh water. As the wind continued nearly in the same direction during the four above-mentioned days, and the distance from the land was only a little increased after the first day, it would appear probable that the coarser dust was raised by a squall with which the breezes on this coast so often begin blowing.

With respect to the direction of the wind during the falls of dust, in every instance where recorded it has been between N.E. and S.E.; generally between N.E. and E. In the case however given by the Rev. W. Clarke,[8] a hazy wind which had blown for some time from E. and S.E. first fell calm, and was succeeded for a few hours by a S.W. wind, and then returned strongly to the east; during this whole time dust fell. With respect to the time of year, the falls have always occurred in the months of January, February, March and April; but in the case of the Princess Louise in 1840, as late as on the 9th of May. In the one year of 1839, it has chanced that dust has been recorded as having fallen in the Atlantic (as may be seen in the references) on the 14th and 15th of January, and on the 2nd, 4th, 9th, 10th, 11th, 12th and 13th of February. I may add, that Baron Roussin,[9] during his survey of the north-western African coast, found, that whilst the wind keeps parallel to the shore, the haze and dust extend seaward only a short distance; but when during the above four specified months the harmattan blows from the N.E. and E.N.E.,

accompanied by tornados, the dust is blown far out, and is raised on high, so that stars and all other objects within 30° of the horizon are hidden.

From the several recorded accounts[10] it appears that the quantity of dust which falls on vessels in the open Atlantic is considerable, and that the atmosphere is often rendered quite hazy; but nearer to the African coast the quantity is still more considerable. Vessels have several times run on shore owing to the haziness of the air: and Horsburgh[11] recommends all vessels, for this reason, to avoid the passage between the Cape Verd Archipelago and the main-land. Roussin also, during his survey, was thus much impeded. Lieut. Arlett found the water so discoloured,[12] that the track left by his ship was visible for a long time; and he attributes this in part to the fine sand blown from the deserts, "with which everything on board soon becomes perfectly caked."

Professor Ehrenberg[13] has examined the dust collected by Lieut. James and myself; and he finds that it is in considerable part composed of Infusoria, including no less than sixty-seven different forms. These consist of 32 species of siliceous-shielded *Polygastrica*[14]; of 34 forms of *Phytolitharia*, or the siliceous tissues of plants; and of one *Polythalamia*.[15] The little packet of dust collected by myself would not have filled a quarter of a tea-spoon, yet it contains seventeen forms. Professor Ehrenberg remarks, that as 37 species are common to several of the packets, the dust collected by myself, and on four successive days by Lieut. James, must certainly have come from the same quarter; yet mine was brought by a E.N.E. wind, and Lieut. James's by a S.E. and E.S.E. wind. The Infusoria are all old known species, excepting one allied to a Hungarian fossil; and they are of freshwater origin with the exception of two (*Grammatophora oceanica* and *Textilaria globulosa*), which are certainly marine. Prof. Ehrenberg could not detect any of the soft parts of the Infusoria, as if they had been quickly dried up, and hence it would appear that they must have been caught up by the wind some time after having been dead. The greater number of the species are of wide or mundane distribution; four species are common to Senegambia and S. America, and two are peculiar to the latter country: moreover it is a very singular fact, that out of the many forms known to Professor Ehrenberg as characteristic of Africa, and more especially of the Sahara and Senegambian regions, none were found in the dust. From these facts one might at first doubt whether the dust came from Africa; but considering that it has invariably fallen with the wind between N.E. and S.E., that is, directly from the coast of Africa; that the first commencement of the haze has been seen to come on with these winds; that coarser particles have first fallen; that the dust and hazy atmosphere is more common near the African coast than farther in the Atlantic; and lastly, that the months

during which it falls coincide with those when the harmattan blows from the continent, and when it is known that clouds of dust and sand are raised by it, I think there can be no doubt that the dust which falls in the Atlantic does come from Africa. How to explain the enigma of the absence of characteristic African forms and of the presence of two species from S. America, I will not pretend to conjecture. Finally I may remark, that the circumstance of such quantities of dust being periodically blown, year after year, over so immense an area in the Atlantic Ocean, is interesting, as showing by how apparently inefficient a cause a widely extended deposit may be in process of formation; and this deposit, it appears from the researches of Prof. Ehrenberg, will in chief part consist of freshwater Polygastrica and of Phytolitharia.

1. [Read 4 June 1845.] *Quarterly Journal of the Geological Society of London. Proceedings of the Geological Society*, pt. 1, 2(1846):26–30.†

2. Microscopic organisms; formerly including also siliceous skeletons of diatoms.†

3. Siliceous cells of plants, generally grasses and other monocotyledons.†

4. Nautical Magazine, 1839, p. 364. The dust fell from the 9th to the 13th of February 1839, whilst sailing from (lat. 10° N., long. 29° 59′) to (lat. 2° 56′ N., long. 26° 30′ W.). The wind on the 9th was E.N.E., on the 10th N.E. by E., and on the three succeeding days N.E.

5. Geographical Journal, vol. vi. p. 296. "Survey of some of the Canary Islands and part of the coast of Africa, by Lieut. W. Arlett, R.N."

6. Edinburgh New Phil. Journal, vol. xxxii. p. 134. The account is taken from Berghaus' Almanack of the dust which fell on the Princess Louise on Jan. 14th and 15th, 1839, between (lat. 24° 20′ N., long. 26° 42′ W.) and (lat. 23° 05′ N., and long. 28° 18′ W.): and again in 1840 from the 6th to the 9th of May, whilst between (lat. 10° 29′ N., long. 32° 19′ W.) and (lat. 16° 44′ N., long. 36° 37′ W.). During the voyage of a vessel of the same name, in which Dr. Meyen was a passanger (Reise um Erde, Th. i. s. 54) on the 27th of October 1830, the sails were observed to be stained by a powder, which Dr. Meyen considered to be a minute Cryptogamic plant: the date would lead me to believe that in this case the phaenomenon was different from that of the dust described in this paper.

7. Nonflowering plants.†

8. Proceedings of the Geolog. Soc. vol. iv. p. 145. The dust described by the Rev. W. Clarke fell February 2nd to the 4th, 1839, when between (lat. 21° 14′ N., long. 25° 6′ W.), and nearly (lat. 12° 36′ N., long. 24° 13′ W.). The direction of the wind has been already given in the paper; as it also has been, when the dust was collected by Lieut. James and myself. Mr. Clarke has since written a communication on the subject for the 'Tasmanian Journal' (vol. i. p. 321), to which I am indebted for two references.

9. Nautical Magazine, 1838, p. 824.

10. Nautical Magazine, 1837, p. 291. Mr. Burnett, on February 12th to 15th, in sailing from (lat. 4° 20′ N., long. 23° 20′ W.) to (lat. 8° N., long. 27° 20′ W.), a distance of 300 miles, with the wind N.E., preceded by a S.E. squall which veered to E.S.E. and then to N.E., had the sails, rigging and mast covered with red dust. The dust began to fall as soon as the wind became N.E.: the atmosphere was very hazy. The nearest land was 600 miles distant. The same phaenomenon was observed by Mr. Burnett in April 1836.

Mr. Forbes gives an account (Sharon Turner's S. Hist. of the World, p. 149) of dust which fell on a ship when 600 miles from the coast, between C. Verd and the R. Gambia: the wind all the previous night had been N.E.

In the Edinb. New Phil. Journal (vol. vii. p. 402) there is another account of dust which fell in considerable quantities on March 29th, 1821, in lat. 11° 3′ N., when 300 miles from the nearest part of Africa. [Correct reference: James Alexander, "Blowing Sand," *Edinburgh Philosophical Journal* . . . 7(1822):404.†]

In Howard Malcolm's Travels (vol. ii. p. 200) there is a similar account of dust which fell during several days in February on a ship north of the equator, when more than 1000 miles from the coast of Africa: the wind was N.E.

11. Horsburgh's East Indian Directory, p. 11.

12. In Tuckey's Narrative of the Congo Expedition (p. 10), a discoloured sea and a hazy atmosphere are described on the 9th of April in lat. 22° N. and long. 19° 9′ W., when 32 leagues from the main-land.

It may be worth here recording that Sir A. Burnes (Travels in Cabool, p. 223), in describing Khoten, a region of Upper Asia, adds, "it is said that its productiveness depends upon clouds of red dust, which always fall or are blown in this part of Asia." But he thinks that the statement requires confirmation.

13. These microscopic organized bodies have been described in the 'Monatsberichten der Berlin Akad. der Wissens. Mai 1844; u. 27 Februar 1845.' In the latter paper a full list of the names is given: the column marked St. Jago includes those collected by myself.

14. Ciliated Protoza: the Ciliata, or Infusoria.†

15. Foraminifera: Protoza that generally have calcareous shells.†

On the Geology of the Falkland Islands[1]

The Falkland Islands being a British colony, and the most southern point at which palaeozoic fossils have hitherto been discovered, I am induced to lay a short account of the geological structure of these islands before the Society. They stretch from 51° to 52° 30′ south, and extend about 130 miles in longitude. My examination was confined to the eastern island; but I have received, through the kindness of Captain Sulivan and Mr. Kent, numerous specimens from the western island, together with copious notes, sufficient to show the almost perfect uniformity of the whole group.

The low land consists of pale brown and bluish clay-slate, including subordinate layers of hard, yellowish, sometimes micaceous, sandstone: in the clay-slate organic remains are exceedingly rare, whilst in some of the layers of sandstone they are extremely numerous, the same species being generally grouped together. Messrs. Morris and Sharpe have kindly undertaken to describe these fossils in a separate notice: they consist (as I am informed by them) of three new species of *Orthis*, which have a Silurian character; three of *Spirifer*, which rather resemble Devonian forms, and approach closely to some of the Australian species described by Messrs. G. B. Sowerby and J. Morris[2]; one species both of

Atrypa and *Chonetes*, the latter approaching very closely some of the varieties of *C. sarcinulata* of Europe; an *Orbicula* and an *Avicula*, the species not determinable; and lastly, a fragment of a *Trilobite* and numerous traces of Crinoidea, apparently related to the genus *Actinocrinus*. The concurrence of these several organic forms in this remote part of the southern ocean, giving to the aggregate so close a general resemblance with the palaeozoic groups of the north, is an interesting circumstance. None however of the species appear absolutely identical with northern forms, or with the Silurian and Devonian mollusca described by M. A. d'Orbigny from the Bolivian Cordillera; these later, eleven in number, are likewise all distinct from, though several are most closely related to, northern forms: two crustaceans however and a Graptolite appear to be identical with European species. With respect to the thirty-four or thirty-five palaeozoic mollusca from Australia,[3] Mr. Morris has come to the conclusion that all are new, with the exception of one Terebratula: some of the species, moreover, have required the institution of new genera. Mr. Lonsdale has likewise found that the palaeozoic Australian corals are almost all new species. Although the frequent and close general resemblance of the palaeozoic fossils in very distant parts of the world is extremely remarkable, especially when we compare intra- and extra-tropical districts (as in the case of those described by M. d'Orbigny), yet I conceive that the opinion, that the further we look back in time, the more widely distributed the same species of shells were, must be greatly modified.

We should bear in mind, that at the present day shells inhabiting seas, which instead of being divided by impassable barriers of land stretching north and south, are bordered by coasts running east and west or are interspersed with islands, often have enormous ranges: Mr. Cuming informs me, that he has upwards of a hundred species of shells from the eastern Coast of Africa identical with those collected by himself at the Philippines and at the eastern coral-islands of the Pacific Ocean: now the distance from these islands to Eastern Africa is equal to that from pole to pole. Under similar circumstances Dr. Richardson has found that fishes have immense ranges. Moreover we should bear in mind, how few genera of shells are confined to particular regions of the world, that is, if we compare the extra-tropical zones together and the inter-tropical zones together. Hence, from the distribution of existing mollusca, we ought not to feel surprised at the fossil species of the same period, in the most distant quarters of the same great zones, being sometimes identical, or differing only by specific characters. It is however right to add, that not only all the existing shells of the Falkland Islands and of Tierra del Fuego are specifically different from those of the northern hemisphere, but I think that they differ more palpably in form than do the palaeozoic

species from the same quarters: in this comparison however of the living shells, the littoral species are included; and these no doubt always show the effects of climate and other external influences more plainly than deep-water genera, such as probably were Spirifer and Orthis.

The low clay-slate and sandstone districts of the Falkland Islands are broken by numerous ranges varying in height from a few hundred feet to between 2000 and 2500 feet, and all composed of stratified quartz. This rock varies from an arenaceous mixture to a pure white granulo-crystalline mass; it sometimes contains minute imperfect scales of mica arranged in parallel planes, and often small specks of a white substance, like earthy feldspar, exhaling an aluminous smell, but quite infusible under the blowpipe. Occasionally the rock assumes a curious brecciated appearance (apparently of concretionary origin), in which angular fragments of nearly pure quartz are imbedded in an opake siliceous paste, partly formed of the white earthy matter. I have observed these white and yellowish earthy specks in the quartz rocks of several other countries, and likewise in a calcareous rock in one of the Cape Verd islands, produced by the flowing of submarine lava over a recent shelly mass. The rock in this latter case is compact; and in a series of specimens, the gradual separation of the little specks of earthy matter, either through their mutual attraction, or more probably by the segregating influence of the stronger attraction of the atoms of carbonate of lime, could be most distinctly traced. There is good evidence that the quartz of the Falkland Islands has been softened by heat; and the analogy is so perfect between the little earthy specks in the two cases, that I believe they have been similarly produced.

I nowhere actually saw the superposition of the clay-slate[4] on the quartz, but in several places on the sea-shore I traced the most gradual transitions between these two widely different formations. It was particularly curious to observe how insensibly the gently inclined planes of stratification in the quartz disappeared, and the highly inclined cleavage-laminae of the clay-slate, extending in their usual course, appeared: it was impossible to point out where the stratification ended and the cleavage commenced. From the manner in which the clay-slate and sandstone often come up on each side to the base of the quartz ranges, I have no doubt that this rock is a lower and more arenaceous formation metamorphosed.

The many parallel ranges of quartz in the eastern part of the group extend east and west, but in the more westerly parts they run W.N.W. and E.S.E.: on the west side, however, of the great Sound between the two main islands, there is, according to Captain Sulivan, a fine range, 2000 feet in height, at right angles to the usual direction, and extending

N.N.E. and S.S.W. The outline of the indented coast, and the position of the outlying islets, are in accordance with these axes of elevation. The cleavage-planes of the clay-slate strike almost invariably in the same direction with the quartz ranges: the laminae are either vertical or highly inclined, generally at an angle above 50°, and dip either north or south, but most frequently to the south. The coincidence in direction (but not in dip) between the stratification of the quartz and the cleavage of the slate was strikingly seen at the western end of the Wickham Heights, which bend from their usual east and west line into a W. 35° N. course; and here at the foot of the hills I found the slate with an almost vertical cleavage striking in the unusual line of W. 30° to 40° N. I may add, that I found on the mainland of South America the cleavage-planes, with a high but variable dip, extending uniformly over extremely large areas, in the same direction as at the Falkland Islands, and in the same line with the prevailing axes of elevation, but intersected at right angles by other subordinate axes: I will not however here enlarge on this subject.

The beds of sandstone included in the clay-slate in the lower and less troubled parts of the island are either horizontal, or dip in various directions, most commonly to the south, at angles between 10° and 20°. I repeatedly observed that the clay-slate had exactly the same highly inclined cleavage above and below these beds. Where this occurred, the sandstone generally broke, when struck, in the line of the cleavage, and transversely to its own planes of division, and the seams were full of fossil shells: Professor Sedgwick[5] has remarked the same fact in beds of limestone similarly situated; and it shows that the molecular arrangement even of these compact rocks has undergone some change. The strike of the cleavage, although coincident with the main lines of elevation, seems to have no reference to the minor flexures; and it preserves a remarkable uniformity whether the stratification of the clay-slate (distinguishable only by the intercalated beds of sandstone) has remained horizontal, or has been tilted at small angles in various directions. Captain Sulivan, who was so kind as to observe carefully the cleavage of the rocks, has however given me a drawing and minute description of some clay-slate beds, exposed in a cliff on the southern coast, in which the cleavage in some of the beds strikes perpendicularly without having been in the least influenced by the minor flexures; whilst in others it is exactly at right angles to each flexure. The beds have been crushed into numerous successive folds, one of which is represented in the following woodcut.

Captain Sulivan states, that in some of the strata the cleavage "in every part, however much twisted, was perpendicular to the horizon;" in others "it was perpendicular to every curve." I have never myself seen an instance of this structure, and I believe it is a new and interesting case.

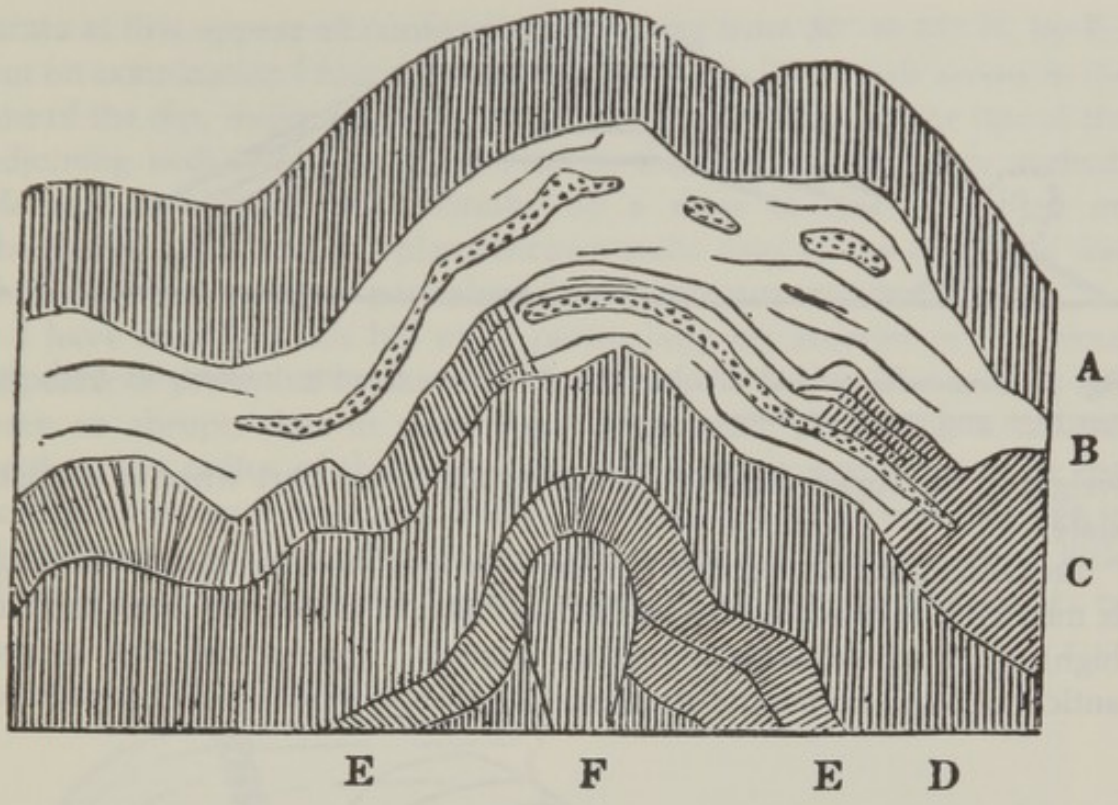


Fig. 1. A,D,F: Beds of clay-slate, with cleavage-laminae perpendicular to the horizon. E and part of C: Similar beds, with the cleavage at right angles to every flexure. B and parts of C: Beds of imperfect, non-laminated clay-slate, with intercalated seams of sandstone represented by the dotted parts. F: Nucleus or core of clay-slate formed by the lateral crushing of the strata, about two feet high and one foot broad. These nuclei occur in almost all the folds.

The remaining facts which I have to give refer entirely to the structure of the ranges, composed of quartz rock. In crossing the eastern island in a N.N.W. and S.S.E. direction, in a line intersecting the head of Berkeley Sound, we find north of it several low, parallel, interrupted, east and west ranges, with their strata all dipping a little west of south, at angles varying between 20° and 40°. South of Berkeley Sound the first range we come to is a short one, rising like all the others through the clay-slate formation: the strata near the summit of the principal hill are most regularly arched, with a curvature of 28° in the line of our imaginary section, and of from 14° to 16° in the line of the ridge: on the summit itself they are horizontal. A regular, flat-topped, oval dome (of which a section is here given) has thus been produced. A valley having been hollowed out near the summit, a very curious scene of natural architecture is presented, which excited the utmost astonishment in the old voyager Dom Pernetti. At the northern and southern base of this hill, the strata, instead of being, as near the summit, dome-shaped, dip directly inwards at angles of 40° and 50°: I have little doubt, from what I saw in other places, that these strata form parts (as shown by the dotted lines in the section) of outwardly bulging flexures, produced apparently by the

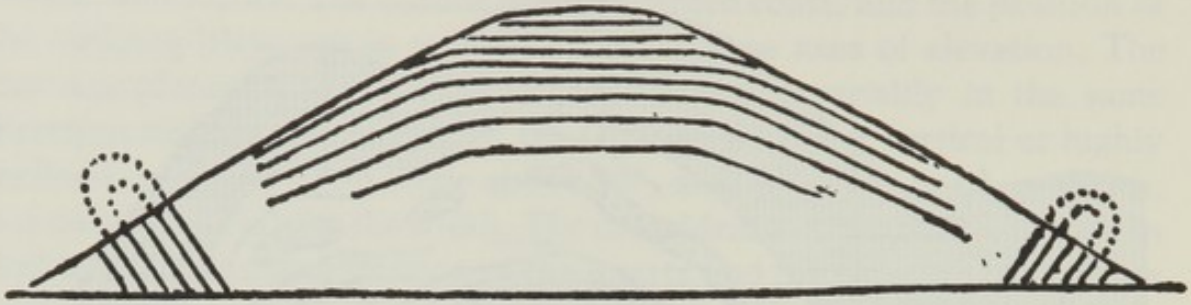


Fig. 2. Dome-shaped hill of quartz, with strata dipping inwards at both the northern and southern base.

weight of the superincumbent mass on the lower part when in a pasty state.

Proceeding in our southern course, a second short east and west range is met with, formed by three principal hills, of which the first (960 feet high) is anticlinal with a broken summit. The second hill is also anticlinal, with horizontal strata on its broad summit, showing traces of

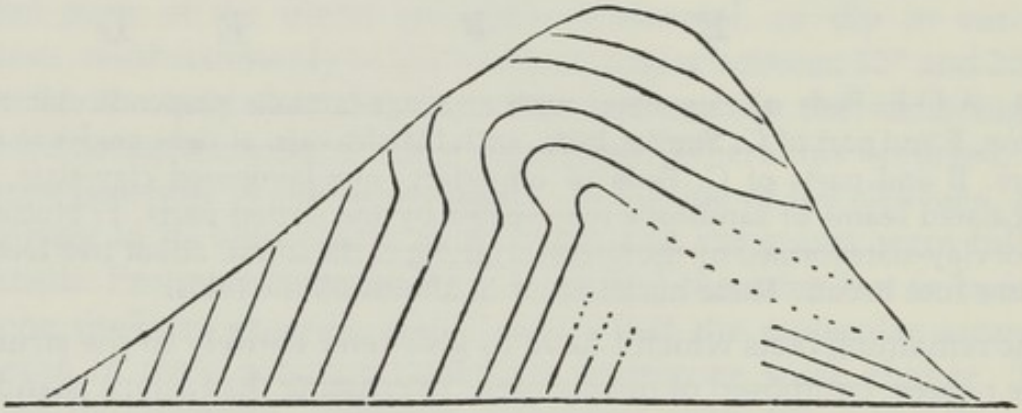


Fig. 3. Hillock of quartz with summit of axis-plane thrown over to the south.

curvature towards the edges: the inclination is rather greater on the south than on the north side. Between this second and third hill there is an anticlinal hillock, the strata on its south side dipping at an angle of 59°, and its summit folded as represented in the diagram. We here see that the upper part of the axis-plane, to use a convenient term of the Professors Rogers, has been pushed over to the south. Throughout the third hill the

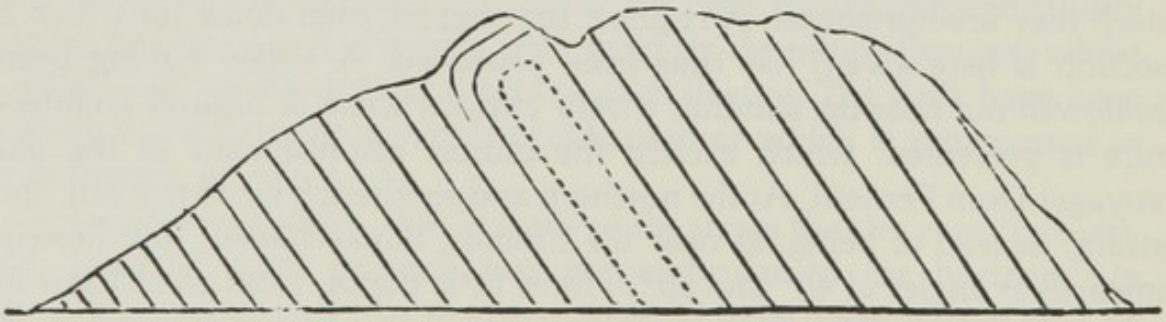


Fig. 4. Strata of quartz dipping 50° to 55° north, with a fold in the middle, only fifty yards across.

strata at first appear all conformable, dipping from 50° to 55° N. by E.; but on examination I found a small portion, only fifty yards across in the line of the dip, inclined at an angle of 26° southward; and the tips of the adjoining beds were, as represented in the diagram, abruptly arched. Hence this hill has been formed by a mass of strata doubled on themselves, with the axis-plane thrown quite over to the south, as was the case with the upper part alone in the above-mentioned hillock.

I have described this hill more particularly on account of a curious appearance presented by the arched parts of the strata. The arching has been so abrupt, that in some loose fragments, presenting a natural section, the radius of the curve is seen to be only seven feet. The end section of one such fragment, twelve feet in length, is accurately given in the following woodcut (No. 5), but allowance must be made for a little displacement from an open fissure crossing it. In this case the convex or

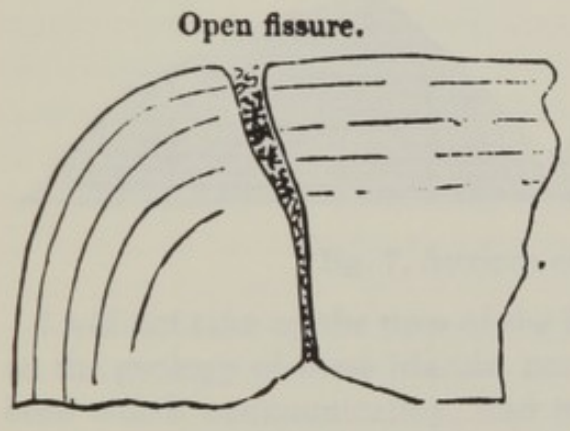


Fig. 5. Base of an arched fragment of quartz.

outer and exposed surface is remarkably even and smooth; it is traversed in the line of the axis of curvature by numerous parallel veins, from the tenth to the twentieth of an inch in thickness, and from half an inch to two inches apart from each other: these often thin out at both ends, but where one thins out, another commences either a little above or below. The veins are partially filled by transverse threads of quartz very imperfectly crystallized. The quartz-rock must obviously have been in a pasty condition, when it suffered without fracture such abrupt curvatures; and it was impossible to examine these veins without recognising in them the effects of the stretching, and in the fibres or imperfect crystals of quartz, the adhesive nature of the ductile mass.[6] This hill, as well as the two others in the range, show traces of a *quâquâversal* or dome-shaped stratification; and we can thus understand the occurrence of some few veins at right angles to those numerous ones in the line of the principal curvature; for there must have been some stretching in two directions. I may add that the arched strata in the more regularly dome-shaped hill before described (No. 2), were intersected by a

rectangular network of similar veins, almost equally numerous in both directions. All these greatly arched masses of quartz are very brittle.

Referring once again to the fragment last figured (No. 5), it is seen to be divided by interrupted lines of stratification, concentric with the outer and convex and now accidentally exposed surface, but firmly united together. Captain Sulivan however found in another place innumerable similar fragments, in which the concentric layers were separate, so that the ground was strewed with gigantic semi-cylinders of quartz, like draining or ridge tiles; he measured one, represented in the diagram annexed (No. 6) and found it twenty feet in length, with a nearly regular

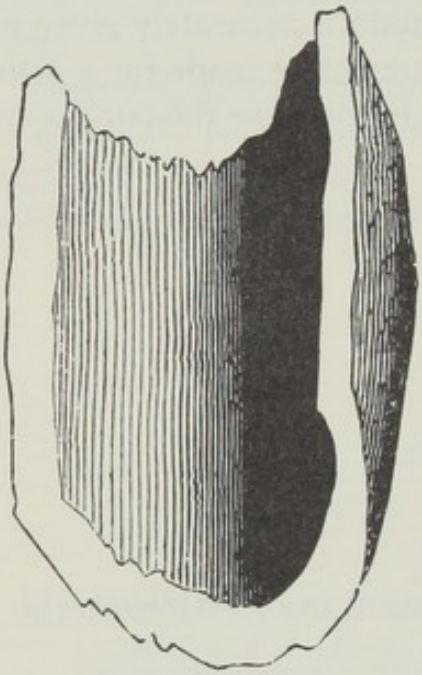


Fig. 6. Semi-cylinder of quartz.

diameter of twelve feet. In this instance the edges or rim on both sides are of equal thickness; but in some other cases, whilst the rim on one side was two feet thick, on the other it thinned off to a knife-edge, evidently in consequence of the unequal pressure it had undergone.

Crossing a wide valley of slate and sandstone we come to the chief mountain-axis of the island, varying from 1500 to 2500 feet in height, and running nearly east and west. The strata on its northern flank dip northward; on the summit, which is from one to two miles broad, they are horizontal; on its southern side they are almost vertical with a southerly dip, and with their summits close to the horizontal beds abruptly arched; so that in this main range we have the same peculiar form of elevation, so common in all the smaller hills. At the southern base the strata were in some places folded in the shape of upright arched gateways. I may mention that fifteen miles to the westward, at the foot of this same range, I found two hillocks of quartz only twenty yards apart, with the strata dipping at exactly the same angle of 40° to S.S.W., and therefore apparently quite conformable; but on close inspection the ends

of the beds on the inner side of one hillock were seen to be arched in such a manner, as to show that they had been doubled on themselves, with the axis-plane inclined at an angle of 40°.

A wide undulatory district of slate and sandstone extends southward of the main range; but on the coast, Captain Sulivan again found two east and west quartz ranges: one of these is transversely intersected by a creek (near Port FitzRoy), and two good sections, a hundred feet in height, are exposed. These are given in the following diagram on account of the complexity of the curvatures, almost resembling those produced by the mingling together of two viscid fluids; and because in crossing the country any one would be apt to think that the dome-formed hills had been produced by single impulses from below, whereas we now see that perpendicularly beneath one dome, another may lie hidden in the solid rock.[7]

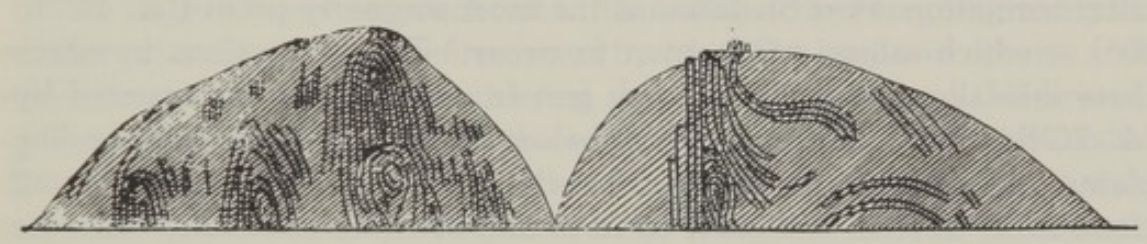


Fig. 7. Sections of quartz domes.

I will not take up the time of the Society by giving any further details on the geology of these islands; nor would the foregoing account have been worth communicating, had it not been for the interest which is justly taken in ancient fossils coming from a very distant quarter of the world.

1. *Quarterly Journal of the Geological Society of London. Proceedings of the Geological Society*, pt. 1, 2(1846):267–74.†

2. Strzelecky's Physical Description of New South Wales, &c., p. 279 *et seq.*

3. Strzelecky, *ante cit.*, and the Appendix to C. Darwin's Volcanic Islands.

4. Captain Sulivan seems to have found on the western island subordinate beds of a conglomerate or coarse grauwacke. On this island there appear also to be traces of tertiary and boulder formations, corresponding with those of Tierra del Fuego. Captain Sulivan observed on the western island numerous basaltic dikes.

5. Geological Transactions, 2nd Ser., vol. iii. p. 477.

6. In a paper by M. Elie de Beaumont read before the Soc. Philomathique, May 1839 (L'Institut, 1839, p. 161), it is stated that M. Gaudin was able to draw out threads of melted quartz: M. Gaudin also found that quartz (differently from alumina) retained its viscidity for some time when cooling,—a fact to be borne in mind when we attempt to account for the remarkable flexures which nearly all the quartzose ranges in this island, and likewise in many other parts of the world, have undergone.

7. It is singular in how many points the old quartz-rock of Anglesea, as described by Professor Henslow in his admirable paper in the Cambridge Phil. Trans. (vol. i. p. 359), agrees with that of the Falkland Islands. The quartz of

Anglesea is granulo-crystalline, and contains white earthy spots and a little mica; it passes insensibly into an overlying chloritic schist, and this again into clay-slate. The strata have been in a pasty condition, and have been singularly curved: they strike in the same direction with the laminae of the overlying slates, but their average inclination is less.

Origin of Saliferous Deposits: Salt-Lakes of Patagonia and La Plata[1]

Salinas or natural salt-lakes occur in various formations on the eastern side of the continent of South America, in the argillaceo-calcareous deposits of the Pampas, in the sandstone of the Rio Negro, where they are very numerous, in the pumiceous and other beds of the Patagonian tertiary formation, and in small primary districts in the midst of this latter formation. Port St. Julian is the most southerly point (lat. 49° to 50°) at which salinas are known to occur.[2] The depressions in which these salt-lakes lie are from a few feet to sixty metres (as asserted by M. d'Orbigny, Voy. Géol., p. 63)[3] below the surface of the surrounding plains; and according to this same author, near the Rio Negro they all trend either in N.E. and S.W. or in E. and W. lines coincident with the general slope of the plain. These depressions in the plain generally have one side lower than the others, but there are no outlets for drainage. Under a less dry climate an outlet would soon have been formed, and the salt washed away. The salinas occur at different elevations above the sea; they are often several leagues in diameter; they are generally very shallow, but there is a deep one in a quartz rock formation near Cape Blanco. In the wet season the whole or a part of the salt is dissolved, being re-deposited during the succeeding dry season. In a large salina northward of the Rio Negro, the salt at the bottom during the whole year is between two and three feet in thickness.

This salt rests almost always on a thick bed of black muddy sand, which is fetid, probably from the decay of the burrowing worms inhabiting it. (This mud in some places rests on gravel, and in one case the salina is in an alluvial plain within the valley of the Rio Negro.) When I visited the salina about fifteen miles above the town of El Carmen on the Rio Negro and three or four miles from the banks of that river, the salt was beginning to crystallize; and on the muddy bottom there were lying many crystals of sulphate of soda, generally placed crossways, and, imbedded in the mud, were numerous crystals of sulphate of lime from one to three inches in length. M. d'Orbigny states that some of these crystals are acicular, and more than even nine inches in length, others are macled and of great purity; those I found all contained some sand in the centre. As the black and fetid sand overlies the gravel, and that overlies the regular tertiary strata, I think there can be no doubt that these

remarkable crystals of sulphate of lime have been deposited from the waters of the lake. The inhabitants call the crystals of selenite *padre del sal*, and those of the sulphate of soda *madre del sal;* they assured me that both are found under the same circumstances in several of the neighbouring salinas, and that the sulphate of soda is annually dissolved and is always crystallized before the common salt on the muddy bottom. The association of gypsum and salt in this case appears to me interesting, considering how generally these substances are associated in the older stratified formations.

Mr. Reeks has analysed for me some of the salt from the salina near the Rio Negro: he finds it composed entirely of chloride of sodium, with the exception of 0.26 of sulphate of lime and 0.22 of earthy matter; there are no traces of iodic salts. Some salt from the salina Chiquitos in the Pampean formation is equally pure.

With respect to the origin of salt in the salinas, the foregoing analysis seems opposed to the view entertained by M. d'Orbigny and others, and which seems so probable, considering the recent elevation of this line of coast, namely, that it is due to the evaporation of sea-water and to the drainage from the surrounding strata impregnated with sea-salt. I was informed (I know not whether accurately) that on the northern side of the salina, on the Rio Negro, there is a small brine-spring which flows at all times of the year; if this be so, the salt in this case at least is probably of subterranean origin. It appears at first very singular that fresh water can often be procured in wells, and is sometimes found in small lakes, quite close to these salinas. I am not aware that this fact bears particularly on the origin of the salt, but perhaps it is rather opposed to the view of the salt having been washed out of the surrounding superficial strata, but not to its having been the residue of sea-water left in depressions as the land was slowly elevated.

1. *Quarterly Journal of the Geological Society of London. Proceedings of the Geological Society*, pt. 2, 2(1846):127–28. Slightly edited, and extracted from Charles Darwin, *Geological Observations on South America* . . . (London: Smith, Elder, 1846, pp. 73–75).†

2. According to Azara (Travels, vol. 1. p. 56) there are salt-lakes as far north as Chaco (lat. 25°) on the banks of the Vermejo. The salt-lakes of Siberia appear to occur in depressions very similar to those of Patagonia.—*Pallas's Travels*, Engl. Tr., vol. 1. p. 284.

3. Alcide Dessalines d'Orbigny, *Voyage dans l'Amérique Méridionale, le Brésil, la Republique Orientale de l'Uruguay* . . . , 9 vols., vol. 3, *Géologie* (Paris: Pitois-Levrault, 1835–47).†

Salt[1]

Your correspondent "W. B. N." (see p. 117)[2] must, I think, have seen salt from other salinas than those described by me; probably (as I infer from his statement that the salt is brought into Buenos Ayres in ox-waggons), from the salinas north of S. Ventana. The salt from the Rio Negro, from the S. Chiquitas and from San Julian, instead of being an "amorphous mass," yielding "a soft powder," is coarsely crystallized, some of the cubes being even 3 or 4 inches square. Instead of being mixed with much earth, the salt presents an expanse as white as newly fallen snow, which, viewed from a distance, as I well remember to my cost, might readily be mistaken for a lake. Your correspondent seems to think that by the term purity, I imply freedom from dirt, but in my work I explain that I mean, "the absence of those other saline bodies found in all sea-water,"—a remarkable fact, which I state after the careful analysis of Mr. T. Reeks of the Museum of Econom. Geology. The salt consists entirely of chloride of sodium, with the exception of only 0.26 of sulphate of lime, and 0.22 of earthy matter. This fact having been ascertained, and the mass being well crystallised, it still appears to me that its lesser value for curing meat is probably owing to its purity, in the sense in which I have perhaps inappropriately used the term, that is, the absence of those other saline substances found in sea-salt. I should not, however, have ventured on this opinion, had not Prof. Johnston come to the conclusion "that those salts answer best for preserving cheese which contain most of the deliquescent chlorides." I must yet think that the experiment of adding some of the muriates of lime and magnesia to the salt *from the Rio Negro*, would be very well worth trial by the owners of the Saladeros near Buenos Ayres.

1. *Gardeners' Chronicle and Agricultural Gazette*, no. 10, 6 March 1847, pp. 157–58.†

2. W.B.N., "Native Patagonian Salt," *Gardeners' Chronicle and Agricultural Gazette*, no. 8, 20 February 1847, p. 117.†

A Natural History of the Mammalia. By G. R. Waterhouse, Esq., of the British Museum. Illustrated with Engravings on Wood and Coloured Plates. London, H. Baillière. [A Review][1]

The first volume of this excellent work, in which every species in the class Mammalia will be described in detail, is now completed. The author is already favourably known to the public by various monographs, and by

papers in this Journal, on the Rodentia, Marsupiata and other animals. His former connexion with the Zoological Society and his present position in the British Museum (where he is at present chiefly employed on fossil Mammalia),—his extensive acquaintance with the works of foreign naturalists, as shown by the numerous references in this publication,—together with several visits undertaken solely from his love of science to the museums on the continent, eminently fit him for the great work here commenced. We use this expression advisedly, for it must not be supposed that we have here merely a compilation; original descriptions, and measurements generally taken from more than one specimen, are in the majority of cases given. The dental and osteological details are described with particular care, and are illustrated by distinct and careful plates: in the precision of these details, we imagine we see the effects of Mr. Waterhouse's long and ardent attachment to entomology. Although the work is not a compilation, the author has not neglected any source of information; and in this first volume, which is confined to the Marsupiata, he is much indebted to Mr. Gould's admirable labours in Australia. Mr. Waterhouse however often differs from Mr. Gould with respect to specific characters, and we rejoice to see no signs of that rage to create new species, so prevalent amongst zoologists.

A distinguishing feature in this work is the notice of all fossil species, interpolated in their proper places; hence, when the whole is completed, we shall have a comprehensive view of the entire class of Mammalia, as far as known; and the accident of extinction will not remove from the series, as is too often the case in systematic works, allied or intermediate forms. Many curious and original remarks are interspersed on the affinities of the various genera and families; but we find no trace of those fanciful speculations on analogies—such as between a mouse's nose and a snipe's beak, or between oxen and poultry—which we fear must have lowered us in the estimation of continental naturalists. In reference to affinities, we must express our regret that the Marsupiata were not ranked, in conformity with Prof. Owen's views, as a sub-class distinct from the placental mammifers. Whether we view classification as a mere contrivance to convey much information by a single word, or as something more than a *memoria technica*, and as connected with the laws of creation, we cannot doubt that where such important differences in the generative and cerebral systems, as distinguish the Marsupiata from the Placentata, run through two series of animals, they ought to be arranged under heads of equal value. We are not convinced by the ingenious remarks on this subject given at p. 17; we cannot admit that numerical differences in the number of the species in two groups, or their geographical distribution, or a somewhat hypothetical statement that the amount of difference is greatest amongst the lower forms in each class,

ought to be taken into account in a system of classification; we believe that our best botanists, who may well serve as guides on this subject, eschew such considerations, and confine themselves to the strict rule of difference in structure. Should this rule be disregarded, some naturalists would admit habits (useful as they undoubtedly are)—some would admit analogies, or, as well expressed by Lamarck, adaptations in widely different beings to similar external conditions,—some would admit the supposed order of the appearance of organic beings (as has been suggested) on the surface of the earth, as aids or bases of classification;—the result would be, that no two naturalists would agree in the same conclusion, and our system, instead of becoming a solid and simple edifice, would be a labyrinth of blind passages.

An admirable feature in Mr. Waterhouse's work is the great attention paid to Geographical distribution, that noble subject of which we as yet but dimly see the full bearing. The following remarks (p. 537) give us an excellent summary on the distribution of the Mammalia on the Australian continent:—

> Australia may be conveniently divided into five principal divisions or districts, of which the east, west, north and south portions of the main land will each form one province, and Van Diemen's Land[2] the fifth. Of these provinces, the northern one has the greatest number of species peculiar to it, since out of ten species discovered in that part of Australia, eight are not found elsewhere. The Marsupiata of the eastern district are for the most part distinct from those of the opposite side of the continent, there being but eight species, out of upwards of sixty inhabiting the two provinces, which are found in both. But if the three districts mentioned are characterized by the few species which they have in common, South Australia must be characterized by an opposite quality, that of having a comparatively large proportion of species identical with those of other districts; indeed I know of but four species which are peculiar to this district: it possesses sixteen species in common with Western Australia, and fifteen in common with Eastern Australia. Western Australia possesses one genus (*Tarsipes*)[3] which is peculiar to it, and one sub-genus (*Macrotis*)[4]; none of the other districts of continental Australia possess any genera which are not found elsewhere. About half of the species found in Van Diemen's Land are peculiar to that island—in fact, nine out of twenty: of the remainder, the greater portion are found on the eastern part of the main land. This island, moreover, possesses one genus (*Thylacinus*)[5] and one sub-genus (*Sarcophilus*)[6] which are now peculiar to it. Examples of both these sections have, however, been found in a fossil state on the main land.

Speaking strictly we have here four divisions, for South Australia does not appear from these remarks, zoologically considered, to deserve to be

ranked as a subdivision. New Guinea, however, and the adjacent islands form a well-marked fifth subdivision, and an interesting table is given (at p. 3) of the ranges of the quadrupeds inhabiting them. The fact of South Australia possessing only few peculiar species, it having apparently been colonized from the eastern and western coasts, is very interesting; for we believe that Mr. Robert Brown has shown that nearly the same remark is applicable to the plants; and Mr. Gould finds that most of the birds from these opposite shores, though closely allied, are distinct. Considering these facts, together with the presence in South Australia of upraised modern tertiary deposits and of extinct volcanos, it seems probable that the eastern and western shores once formed two islands, separated from each other by a shallow sea, with their inhabitants generically though not specifically related, exactly as are those of New Guinea and Northern Australia, and that within a geologically recent period of series of upheavals converted the intermediate sea into those desert plains which are now known to stretch from the southern coast far northward, and which then become colonized from the regions to the east and west. We will only further point out an interesting table (p. 536) showing that in South America, Brazil is the metropolis of the Didelphidae,[7] a family which, as Mr. Waterhouse remarks, curiously replaces in that continent the Insectivora[8] of the Old World.

Most of the genera are illustrated by elegant and spirited copperplates; there are also many woodcuts; some few however of these latter are rather unfortunate works of art. The plates are printed on excellent paper, and the whole work is got up in a style creditable to the publisher. The Marsupiata, though highly interesting in their structure and affinities, yet are less so in their habits than the higher mammalia; but from some scattered notices we clearly see that this amusing part of the subject will not be neglected. To the professed naturalist we believe that this work will be almost indispensable; but we also strongly recommend it to those who do not come under this class, but yet are interested in the wide field of nature. We do not doubt that Mr. Waterhouse is conferring by this publication a real service on natural science; we therefore trust to his continued perseverance, and we heartily wish him all success.

1. *Annals and Magazine of Natural History, including Zoology, Botany, and Geology* 19(1847):53–56.†
2. Tasmania.†
3. Mouselike Australian marsupial.†
4. Peramelas: bandicoots.†
5. Tasmanian wolf.†
6. Tasmanian devil.†
7. Opossums.†
8. Moles, shrews, and so on.†

On the Transportal of Erratic Boulders from a Lower to a Higher Level[1]

It will, I think, be generally admitted that the most valid objection which has been advanced against the theory of the transportal of erratic boulders by floating ice, lies in the fact of their having not unfrequently been carried from a lower to a higher level. Mr. Hopkins,[2] indeed, referring to certain boulders of a peculiar conglomerate described by Prof. Phillips, considers this fact as affording an absolute proof of the diluvial theory, since, he adds, "it is evident that no floating ice could possibly transport a boulder from the depths of the vale of Eden over the heights of Stainmoor."[3] Prof. Hitchcock has several times alluded to similar cases in North America as offering a very great difficulty.

The first instance recorded, as far as I know, of the transportal of boulders from a lower to a higher level, is by Prof. Phillips,[4] who in 1829 described numerous large blocks of grauwacke not far from Kirby Lonsdale,[5] scattered over the mountain limestone from a height of 50 to 100 feet above the parent rock, which lies immediately beneath. He adds, "Further on, to an elevation of 150 feet, the blocks are still numerous, and they may be seen, by ascending one ledge after another, almost to the top of the Fell,[6] 500 feet above their original position. They appear," he continues, "to have been driven up at a particular place by a current towards the north, and afterwards carried along the surface of the limestone in a narrow track toward the summit of the Fell." The conglomerate alluded to by Mr. Hopkins has been transported from the bottom of the valley of the Eden, where the rock lies *in situ* at the height of 500 feet above the level of the sea, to and over the pass of Stainmoor at the height of 1400 feet[7]: therefore the boulders now lie 900 feet above their original position. In 1838 I observed many boulders of granite strewed on Ben Erin on the western side of Glen Roy,[8] up to the height of 2200 feet above the sea; the granite resembled in character that seen *in situ* at the head of the Spey, and which, in Macculloch's Geological Map, is likewise the nearest district of granite: if, as I believe, the boulders came from this place, they must have been carried up at least 900 feet. Mr. Maclaren[9] has described (1839) numerous blocks of sandstone on the higher parts of Arthur's Seat,[10] "400 feet above any spot where sandstone now exists *in situ*." Quite recently Mr. D. Milne[11] has noticed other boulders on the same hill, belonging to the coal series, and remarks "that there is no place in the neighbourhood from which these blocks could have come which is not at least 200 feet below their level." In the Isle of Man, the Rev. J. G. Cumming has observed with great care a striking case, and has most kindly communicated to me the details, which will immediately appear in his work[12]: near South Barrule there is a hillock

formed of granite, quite different in nature from any other rock in the island; this mass of granite is about three-fourths of a mile square, and is 757 feet above the level of the sea; from this point the boulders are thickly dispersed to the south-west, and they can be continuously followed up to a height of 788 feet above the summit of the present boss. Mr. R. Mallet informs me that facts of a similar nature have been observed in Ireland. More striking cases occur in the United States, in New England, in New York, and in northern Pennsylvania. Prof. Hitchcock observes,[13] that the Silurian rocks of New York and the quartz in the valleys of western Massachusetts have undoubtedly been carried over and left upon the Hoosac and Taconic mountains, at a height of "upwards of one thousand or two thousand feet." Lastly, I may mention the analogous case of the chalk-flints, associated with boulders of various kinds, observed by the Dean of Westminster and myself on Moel Tryfan, at the height of 1392 feet above the level of the sea, and which (as well as the chalk-flints at the intermediate point of the Isle of Man[14]) there is good reason to believe must have come from Ireland, and therefore, at least in the case of North Wales, from a considerably lower level.

The first point to consider is whether, in these several instances, the boulders have really come from a lower level, or whether they may not (and I am indebted to Sir H. De la Beche for this caution) have been derived from strata now entirely denuded, but which formerly extended up to the same level with the boulders. Or secondly, whether the boulders, after having been deposited, may not have been raised by an unequal elevatory movement above their parent district, or the district itself have been depressed by subsidence below them. With respect to the former supposed greater extension and subsequent denudation of the parent rock,—in such cases as those near Edinburgh it is possible that this may be sufficient to account for the phaenomenon. Where the boulders are of granite, as at Glen Roy and the Isle of Man, this view implies that a mass of that rock has been worn down, equalling in thickness the difference in level between the existing mass *in situ* and the boulders: in North America, where the boulders lie from 1500 to 2000 feet above their source, the denudation on this view must have been immense, and it must all have been effected within the glacial period, as the low country is covered with boulders; this likewise is the case with the boss of granite in the Isle of Man. Can it be supposed with any probability that the chalk-formation formerly extended in Ireland up to a height of nearly 1400 feet? In the case of the boulders described by Prof. Phillips, I am assured by him that the above view is quite inadmissible; and he has pointed out to me conclusive reasons, but which, considering his high authority, I do not consider it necessary to give in detail; I will only mention that the grauwacke was planed down level, before the thick

mass of mountain-limestone on the surface of which the boulders lie was deposited on it, and that at a short distance the grauwacke is quite cut off by the Craven fault[15]: the conglomerate beds, whence the boulders at a height of 900 feet on Stainmoor were derived, are horizontal.

With respect to subsequent unequal elevation having caused the boulders now to lie above their parent rocks, the simple fact of the number of points, irregularly placed both in Great Britain (namely, in northern and central Scotland, in the Lake district, North Wales, Isle of Man and Ireland,) and likewise in the United States, appears to me to render this view extremely improbable; for on such a view it must be admitted that in Great Britain and America several great mountains and mountain-chains have been formed so lately as during the glacial period, and this is a proposition to which few geologists will be inclined to assent. Moreover, in the case of Stainmoor, it is known that its crest now holds, one part with another, the same relative level as it did during the glacial period, for the boulders have crossed it only in one notch or gap, which is now the lowest part; and certain chains of hills which would at present intercept boulders coming from one quarter likewise did so at the glacial epoch.

In the Isle of Man the parent granite and the boulders which lie 788 feet above it are scarcely more than two miles apart, and in the intermediate tract, thickly covered with the boulders, Mr. Cumming has in vain searched for evidence of a fault. In the Lake district there is, I think, conclusive evidence that unequal elevation is not the true explanation, for the boulders there lie so close to the rocks *in situ* that there would necessarily be, if the boulders had been subsequently upraised, a fault or abrupt flexure, in one case of 900 and in the other of 500 feet. Hence we must conclude, in accordance with the views of the several authors who have described the above cases, that the boulders have really been drifted nearly as many feet upwards (that is, making in almost every instance some allowance for the subsequent denudation of the parent rock) as they now lie above their original source.

Those who believe in the powerful agency of ice in moving boulders will probably at first conclude that icebergs have in some manner transported them from a lower to a higher level. But the most obvious method by which fragments of rock can get on icebergs is by their having first fallen from the surrounding precipices on glaciers entering the sea, and therefore they must have come from a higher to a lower level. It seems impossible, owing to the temperature of the water, that at any considerable depth, boulders could be frozen into the bottom of icebergs; and even if at lesser depths they did become so frozen[16] or mechanically wedged in, and if by the icebergs being overturned they were saved from being soon thawed out, yet they could be deposited above their former

level only by so much as the ice under water had decreased in thickness in the interval of the boulders having been caught up and dropped. In any case the notion of icebergs having, in the States of New York, New England, and northern Pennsylvania, lifted up numerous boulders from a depth of between 1000 and 2000 feet, is quite inadmissable.

In my paper on the Boulders of the Southern hemisphere, read before this Society,[17] I pointed out that there were two methods, essentially distinct both in the requisite climate and in the results produced, by which fragments of rock are transported; namely, by icebergs and by coast-ice. Icebergs now transport fragments of rock on the west coast of South America, in the latitude of the central parts of Europe, under a temperate climate where the sea, even in protected bays, is never frozen. On the other hand, in the northern parts of the United States and in the Gulf of Bothnia, where the climate is excessive, but yet under a latitude where glaciers never descend to the level of the sea, fragments of rock are annually enclosed by the freezing of the coast-water, and are thus transported. In the polar regions both actions concur. Icebergs will transport such fragments of rock as fall on the parent glaciers, and these are generally quite angular. From the vast size of the bergs, the fragments will often be transported to great distances, and when deposited it must be in deep water, and therefore (as well as from the original descending movement of the glacier) at a much lower level than the parent rock: when once dropped, they will probably never again be moved by ice. On the other hand, coast-ice will transport whatever fragments of rock or pebbles lie on or near the shore. These fragments, from being repeatedly caught in the ice and stranded with violence, and from being every summer exposed to common littoral action, will generally be much worn; and from being driven over rocky shoals, probably often scored. From the ice not being thick, they will, if not drifted out to sea, be landed in shallow places, and from the packing of the ice be sometimes driven high up the beach, or even left perched on ledges of rock. By this agency boulders will probably not be carried to such great distances as by icebergs, and the limit of their transportal will perhaps be more defined. In South America there is a considerable difference in the state of the boulders in Tierra del Fuego, where a large proportion are much rounded, and on the plains of Patagonia and in Chiloe further from the pole, where the boulders are larger and quite angular. I attributed the presence of these latter to the exclusive action of icebergs; whilst in Tierra del Fuego coast-ice appears formerly to have come into play. On Moel Tryfan[18] the well-rounded fragments of chalk-flints were in all probability transported by coast-ice: though I cannot doubt, from the extraordinary manner in which the laminae of the slate rocks have there been shattered, that icebergs have likewise been driven against them when

under water; so that both actions seem there to have concurred. Some other distinguishing characters between the action of coast-ice and of icebergs will presently be pointed out; and it is by coast-ice, as I believe, that boulders have been transported from a lower to a higher level.

To take the case of North America: Mr. Lyell[19] has shown, from an independent train of reasoning, that this country during the glacial period slowly subsided to a considerable amount: several American geologists have come to a similar conclusion, and they believe that the subsidence amounted to two or three thousand feet, or even more. Let us suppose a sinking movement to be now going on in the estuary of the St. Lawrence or on the coast of Labrador, where we know, from the observations of Lieut. Brown and Capt. Bayfield, as given by Mr. Lyell[20] (and illustrated by striking sketches), that annually an enormous number of boulders, both on and near the coast, are frozen into the coast-ice and transported to shorter or greater distances; can we doubt, that if during the year the land sunk a few inches or feet, the boulders, whilst actually frozen in or when refrozen during the ensuing winter, would be lifted up and landed so many inches or feet higher up on the coast? Capt. Bayfield, as stated by Mr. Lyell,[21] saw masses of rock, "carried by ice through the straits of Belle Isle, between Newfoundland and the continent, which he conceives may have travelled in the course of years from Baffin's Bay." Now if during this probably long course of years,—for the boulders seem generally to be transported only a short distance each winter,—the land had subsided one or two hundred feet, is it not almost certain that they would have been landed so many feet higher up with respect to their former level, in the same manner as would have happened with so much drift timber? It is indeed paradoxical thus to speak of the boulders having been carried up, whilst the land has gone down; for, in fact, the boulders are merely kept by the floating ice at the same level, whilst the land sinks.

No doubt during this process some boulders would be dropped in water too deep to allow of their being refrozen, and they would be thus left behind. Scarcely any form of land would prevent the boulders from being annually landed on a temporary resting-place: even a line of perpendicular cliff, if not of very great length, would probably only cause the tidal currents to drift the coast-ice further onwards; a few more boulders, perhaps, being dropped there than elsewhere. I can see only one difficulty of any weight to this view, namely, that the boulders would be ground down into mud and destroyed from having been stranded such innumerable times, as must have happened with those which were kept up to the same absolute level during a sinking of the land of many hundred feet. On an exposed coast, where the breakers had power to dash pebbles against the boulders, I have no doubt that this would take place, more especially with boulders small enough to be

themselves rolled over. But on a broken coast, amongst islands and in bays, I do not believe that this would happen. We may infer from the fact of scored rocks having been observed both in Scotland and in North Wales, dipping under the surface of lakes, in a quite unaltered condition, that the action of simple water, and of such little waves as lakes can produce, even when prolonged from the glacial period to the present day, is absolutely as nothing; and in sheltered bays, the force of the waves is not very much greater than in lakes. Moreover, in South America I have seen many boulders lying on sea-beaches, exposed to the wash of rather open channels, and which, so far from having been destroyed, yet retained their angles perfect.

Nevertheless it might certainly be expected that boulders which had thus been buoyed up by coast-ice during long-continued ages would be well-rounded. According to Prof. H. D. Rogers, this is the case with the majority of the boulders in North America: those at Glen Roy were rounded, but they were composed of granite subject to disintegration; this likewise is the case with those in the Isle of Man: Mr. Cumming however informs me that the boulders, with some marked exceptions, "diminish in number and size the further we proceed" from the granitic boss. The boulders on Arthur's Seat, judging from the remarks of Messrs. Maclaren and Milne, are rounded. Those near Kirby Lonsdale, which now lie, according to Prof. Phillips, 500 feet above their parent rock, are not rounded; but they are composed of slate, a rock very little liable to be rounded, and they appear to lie in a sort of train up a valley surrounded by mountains, which must formerly have been a well-protected bay. It would be interesting to ascertain whether those boulders which now stand highest above the parent rock are more worn than those at a lower level, which latter I believe to have been dropped during the long-continued buoying-up process.

We have seen that, according to Mr. Lyell, the northern parts of the United States did actually subside during the glacial period. I am not aware that anyone has attempted to show that Great Britain was similarly affected during this same period. The following considerations, however, appear to me to render it in some degree probable: in Staffordshire there are many great and perfectly angular boulders of northern rocks, which almost every geologist believes were transported on icebergs, now lying at the height of above 800 feet above the sea; and on Moel Tryfan, at a height of nearly 1400 feet, there are stratified beds of the glacial epoch (as known by the included shells discovered by Mr. Trimmer), which beds, after careful examination, I cannot doubt were deposited in the ordinary manner under the sea. On the other hand, the character of the miocene formations, on the east coast of England, belonging to an epoch just antecedent to the glacial, lead to the

conclusion that the land then did not hold a level widely different from the present one: if so, unless we suppose a great inequality in the changes of level between the east and west coasts of England, the land must have sunk after the miocene age to allow of the deposition of the glacial deposits at the heights above specified. This conclusion accords perfectly with Professor E. Forbes's statement[22] that all the organic remains seen by him, from the glacial formation, indicate a depth of less than 25 fathoms. As far then as these considerations can be at all trusted, we are, according to the view given in this paper, in a position to explain the transportal of the boulders from a lower to a higher level, in Great Britain as well as in the United States. I will make only one other remark on this head: though I believe that Great Britain subsided during the glacial period, yet I conceive it must also have subsequently attained during this same prolonged period a considerable portion of its present height. I infer this from the plain marks of true glacier action, low down the valleys in North Wales, within 300 feet of the present level of the sea.[23]

A second objection of apparently considerable weight has been advanced against the theory of floating ice; namely, that in some instances the blocks decrease very regularly in size in proceeding from their source. Prof. H. D. Rogers[24] says that this is markedly the case in going southward in the United States. According to Mr. Hopkins[25] it is also the case in the Lake district; "the blocks becoming smaller as we approach the coast of Yorkshire, till they degenerate into pebbles in the more remote localities, in which the Cumbrian rocks can be identified." He adds, "These facts are strongly in favour of those views which would refer the transport of these masses to diluvial currents." This sorting of the boulders does not always hold good: on the plains of Patagonia the two largest boulders which I saw were near the outskirts of the deposit. Sir R. Murchison also remarks on the vast size of the many boulders in the south-east parts of Shropshire, near the southern limit of his northern drift, though he elsewhere states that the boulders generally decrease in size in going from north to south. In these cases, if we look at the boulders as having all been transported on icebergs, there certainly appears no reason why they should have been dropped from such immense masses of ice, with any approach to order according to their size and to their distance from their source. But this does not hold good with boulders transported in sheets and fragments of coast-ice: here the buoying agent is not of disproportionate power to its burthen; as the ice decays, the heaviest fragments would naturally be apt to drop out first; and it would appear from the accounts given to us, that the largest boulders during some winters escape being moved at all, whilst the smaller ones are drifting onwards. Moreover, the boulders (and great

stress may probably be laid on this point) which had travelled furthest, would, from having been repeatedly stranded, and necessarily so every summer, be most worn, and therefore would be smaller than those which had travelled to a shorter distance.

I have shown, in my volume on South America, that the sea has the power by some means of sorting the pebbles which lie at the bottom, their size decreasing with surprising regularity, even till they pass into sand, with the increasing depth. There is some difficulty in understanding how this is effected: Playfair has suggested that the undulations of the sea propagated downwards from the surface, *tend* to lift up and down the pebbles at the bottom, and that such are liable, when thus quite or partially raised, to be moved onwards even by a very weak current. Should, therefore, a boulder formation be exposed during subsequent changes of level to the action of the sea, pebbles derived from it, and decreasing in size with perfect regularity according to their distance from their source, might be thus spread out. Hence I conceive that from a group of mountains, which had once existed as an island, boulders, decreasing in bulk with some degree of regularity, and beyond them pebbles degenerating with perfect regularity into sand, might be spread out, thus simulating the effects of a great debacle, which in rushing along had insensibly lost its power, and yet that both boulders and pebbles had been transported by the ordinary currents of the sea; aided, in the one case, by floating coast-ice, in the other, apparently by the undulating movement of the water.

The two objections, therefore, which have been here discussed, cannot, I think, any longer be considered as absolutely fatal to the theory of floating ice; and thus far the hypothesis of a debacle is no longer necessary.

If the explanation here given of the transportal of boulders from a lower to a higher level be hereafter proved correct, we gain, in all cases where the horizontal distance between the boulders and the parent rock is not so great as to allow of the probability of subsequent unequal movements of elevation, a valuable measure of subsidence during a defined period. We are accustomed to precise measurements of elevation, from the ascertained heights of upraised marine remains; but it seemed quite hopeless to expect this, even in a lesser degree, with respect to subsidence,—that movement which hides under the sea the surface affected. It is marvellous that Nature should have thus marked by buoys made of stone, the former sinking of the earth's crust, and likewise, I may add, its subsequent elevation; and that on these blocks of stone the temperature, during the long period of their transportal, may be said to be plainly engraved. Moreover, it is thus shown that the subsidence during no one entire summer was so great as to carry the coast-boulders

beneath that small limit of depth at which the salt water during each ensuing winter became frozen.

Note.—After this paper was read, Mr. Nicol objected that when the parent rock was once submerged, no further supply of boulders could be derived from it, and consequently if afterwards, each time they were afloat, only one boulder out of a hundred was dropped in water too deep for it to be refrozen in the coast-ice, after a certain time there would be none left to be carried up, during the continued subsidence, to the higher levels. This appears to me an objection of much force. I would, however, remark in the first place, that I do not suppose that the boulders over the whole area of subsidence are carried far up, but only those in certain favourable situations. Secondly, several Arctic voyagers have stated that the pack-ice frequently piles up and leaves masses of boulders at a height of even 20 and 30 feet above high-water mark; now after a subsidence, the ice during the first gale would drive these boulders still higher up, and so onwards and upwards, with scarcely any tendency to carry them out to sea. In a bay open to the prevailing winds, and without any river entering it, I should imagine that the coast-ice would rarely be drifted outwards. Thirdly, I believe that any floating object thrown into the water not far from an extensive coast-line, is generally driven *soon* on shore: this certainly seems to be the case with the wrecks of boats; and if so, any ice-borne boulders, carried by the wind off the land, would generally be again thrown on the coast.

1. *Quarterly Journal of the Geological Society of London. Proceedings of the Geological Society* 4(1848):315–23.†
2. Journal of the Geol. Soc. vol. iv. p. 98.
3. Valley of River Eden, about 40 miles southeast of Carlisle, England. Stainmoor, at the head of the pass over the Pennines, east of the Upper Eden Valley.†
4. Transactions of the Geol. Soc. vol. iii. (second series) p. 13.
5. Westmoreland, 15 miles northeast of Lancaster, England.†
6. Wild, upland fields.†
7. Treatise on Geology (Lardner's Encyclop.), by John Phillips, vol. i. p. 270.
8. Philosophical Transactions, 1839, p. 69.
9. Geology of Fife, &c. p. 47.
10. Volcanic hill, 822 feet high, in Edinburgh.†
11. Edinburgh New Philosophical Journal, vol. xlii. p. 167.
12. The Isle of Man, its History, &c., by the Rev. J. G. Cumming.
13. Geology of Massachusetts, vol. i. (Postscript, p. 5a), and Address to Association of American Geologists, 1811.
14. The Rev. J. G. Cumming in Transactions of British Association, 1845, p. 61.
15. Runs through Settle, York.†
16. See some excellent remarks on this subject in Sir H. De la Beche's Anniversary Address for 1818, p. 68 *et seq.*
17. Transactions of the Geol. Soc. vol. vi. (second series) p. 430.
18. London Philosophical Journal, vol. xxi. p. 186.

19. Travels in North America, vol. i. p. 99, and vol. ii. p. 48.
20. Principles of Geology, 7th edit. p. 222.
21. Ibid. p. 231.
22. Memoirs of the Geological Survey, p. 376.
23. Since the above was written, I have found that Mr. Trimmer, in his interesting paper on the Geology of Norfolk (Journal of the Royal Agricultural Society, vol. vii. part 2), has shown that that district subsided at least 600 feet, and was likewise upraised during the boulder or glacial period.
24. Address to the Association of American Geologists, 1844, p. 45.
25. Journal of the Geological Society, vol. iv. p. 98.

Geology[1]

A person embarked on a naval expedition, who wishes to attend to Geology, is placed in a position in some respects highly advantageous, and in others as much to the contrary. He can hardly expect during his comparatively short visits at one place, to map out the area and sequence of widely extended formations; and the most important deductions in geology must ever depend on this having been carefully executed; he must generally confine himself to isolated sections and small areas, in which, however, there can be no doubt many interesting facts may be collected. On the other hand, he is admirably situated for studying the still active causes of those changes, which, accumulated during long-continued ages, it is the object of geology to record and explain. He is borne on the ocean, from which most sedimentary formations have been deposited. During the soundings which are so frequently carried on, he is excellently placed for studying the nature of the bottom, and the distribution of the living organisms and dead remains strewed over it. Again, on sea-shores, he can watch the breakers slowly eating into the coast-cliffs, and he can examine their action under various circumstances: he here sees that going on in an infinitesimally small scale which has planed down whole continents, levelled mountain-ranges, hollowed out great valleys, and exposed over wide areas rocks, which must have been formed or modified whilst heated under an enormous pressure. Again, as almost every active volcano is situated close to, or within a few leagues of the sea, he is admirably situated for investigating volcanic phenomena, which in their striking aspect and simplicity, are well adapted to encourage him in his studies.

In the present state of the science, it may be doubted whether the mere collection of fragments of rock without some detailed observations on the district whence they are brought, is worthy of the time consumed and the carriage of the specimens. The simple statement that one part of a coast consists of granite, and another of sandstone or clay-slate, can hardly be considered of any service to geology; and the labour thus thrown away might have been more profitably spent, and thus saved the

collector much ultimate disappointment. It is now generally recognised that both the sedimentary rocks, and those which have come from below in a softened state, are nearly the same over the whole world. A mere fragment, with no other information than the name of the place where collected, tells little more than this fact. These remarks do not at all apply to the collection of fossil remains, on which subject some remarks will presently be made; nor do they apply to an observer collecting suites of rock-specimens, with the intention of himself subsequently drawing up an account of the structure and succession of the strata in the countries visited. For this end, he can hardly collect too copiously; for errors in the naming of the rocks may thus be corrected, and the careful comparison of such specimens will often reveal to him curious relations which at the time he did not suspect.

In order to make observations of value, some reading and much careful thought are necessary; but perhaps no science requires so little preparatory study as geology, and none so readily yields, especially in foreign countries, new and striking points of interest. Some of the highest problems in geology wait on the observer in distant regions for explanation; such as, whether the successive formations, as judged of by the character of their fossil remains, correspond in distant parts of the world to those of Europe and North America, or whether some of them may not correspond to blank epochs of the north, when sedimentary beds either were not there accumulated, or have been subsequently destroyed. Again, whether the lowest formation everywhere is the same with that in which living beings are first present in the countries best known to geologists. These and many other such wide views in the history of the world are open to any one, who, applying thought and labour to his subject, has the good fortune to geologise in little frequented countries.

A person wishing to commence geology, is often deterred by not knowing the names of the rocks; but this is a knowledge, he may rely on it, easily acquired. With half a dozen named crystalline rocks, or even by patiently familiarizing his eye (aided by a lens) to the aspect of the feldspar and quartz in granite, he will know the two most essential ingredients in most igneous rocks; and in granite he will often find the glittering scales of mica replaced by a dark green mineral, less hard than the feldspar and quartz; and then he will know the third most important mineral, hornblende. The sedimentary rocks can hardly be described, except by the terms in common use: impure limestone, which cannot be readily recognized by the eye, can be distinguished by its effervescence with acids. By the repeated comparison of freshly fractured sedimentary and igneous rocks, such as sandstone and clay-slate on the one hand, and granite and lava on the other, he will learn the difference between crystalline and mechanical structure; and this is a very necessary point.

Let no one be deterred from geology by the want of mineralogical knowledge; many excellent geologists have known but little; and from this reason its value has perhaps sometimes been underrated, for many of the obscurer points in geology, such as the nature of the metamorphic changes in rocks, and all the phenomena of metallic and other veins, almost [i.e., always] require such knowledge. The appearances presented by the different forms of stratification (that is, the original planes of deposition) may be soon learnt in the field; though no doubt the beginner would be aided by the diagrams given in many elementary works.

The two most useful works which the geologist can carry with him are without doubt the 'Principles' and the 'Elements of Geology,' by Sir Charles Lyell.[2] He should procure a treatise on mineralogy, for instance, 'Phillips's Mineralogy,' by Allan.[3] If he has the opportunity to procure others, Sir H. Delabeche's 'Researches in Theoretical Geology'[4] would be particularly desirable from discussing many of the questions which ought especially to engage the attention of a sea voyager. As he will probably visit many volcanic regions, Dr. Daubeny's 'Treatise on Volcanos'[5] would be extremely useful; and a list is there given of special treatises on the volcanic countries likely to be visited by him. The 'Description Physique des Isles Canaries,' by Von Buch,[6] may be cited as a model of descriptive powers. The voyager in the Temperate and Polar regions ought to have Agassiz' work on Glaciers.[7]

The geologist fortunately requires but little apparatus: a heavy hammer, with its two ends wedge-formed and truncated; a light hammer for trimming specimens; some chisels and a pickaxe for fossils; a pocket-lens with three glasses (to be incessantly used); a compass and a clinometer, compose his essential tools. One of the simplest clinometers is that constructed by the Rev. Prof. Henslow: it consists of a compass and spirit-level, fitted in a small square box; in the lid there is a brass plate, graduated in a quadrant of 90 degrees, with a little plumb-line to be suspended from a milled head at the apex of the quadrant. The line of intersection of the edge of the clinometer, when held horizontally, with the plane of the stratum, gives its strike, range, or direction; and its dip or inclination, taken at right angles to the strike, can be measured by the plumb-line. In an uneven country, it is not easy without the clinometer to judge which is the line of greatest inclination of a stratum; and it is always more satisfactory to be certain of the angle than to estimate it. A flat piece of rock representing the general slope can usually be found, and by placing a note-book on it, the measurement can be made very accurately. In studying the cleavage or slaty structure of rocks, accurate observations are indispensable. A mouth blow-pipe with its apparatus, and a book with instructions for its use (Phillips's Mineralogy contains brief directions), teaches a little mineralogy in a pleasant manner. Besides

the above instruments, a mountain barometer is often very necessary: a portable level would, in the case of raised sea-beaches and terraces, be useful. Messrs. Adie and Son, of Edinburgh, sell a hand-level, a foot in length, which is fitted with a little mirror on a hinge, so that the observer, whilst looking along the level, can see when the bubble of air is central, and thus instantly find his level in the surrounding district. This is a very valuable instrument. Mr. R. Chambers, moreover, and others have found, that an observer having previously ascertained the exact height of his eye when standing upright, can measure the altitude of any point with surprising accuracy; he has only to mark by the level a recognizable stone or plant, and then to walk to it, repeat the process, and keep an account how many times the levelling has been repeated in ascending to the point, the height of which he wishes to ascertain.

A few cautions may be here inserted on the method of collecting. Each single specimen ought to be numbered with a printed number (*those which can be read upside down having a stop after them*) and a book kept exclusively for their entry. As the value of many specimens entirely depends on the stratum or locality whence they were procured being known, it is highly necessary that every specimen should be ticketed on the same day when collected. If this be not done, in after years the collector will never feel an absolute certainty that his tickets and references are correct. It is very troublesome ticketing every separate fossil from the same stratum, yet it is particularly desirable that this should be done; for when the species are subsequently compared by naturalists, mistakes are extremely liable to occur; and it should always be borne in mind, that misplaced fossils are far worse than none at all. Pill-boxes are very useful for packing fossils. Masses of clay or any soft rock may be brought home if small fossil shells are abundant in them. Rock-specimens should be about two or three inches square, and half an inch thick; they should be folded up in paper. To save subsequent trouble, it will be found convenient to pack up and mark outside, sets of specimens from different localities. These details may appear trifling; but few are aware of the labour of opening and arranging a large collection, and such have seldom been brought home without some errors and confusion having crept in.

To a person not familiar with geological inquiry, on first landing on a new coast, probably the simplest way of setting to work is for him to imagine a great trench cut across the country in a straight line, and that he has to describe the position (that is, the angle of the dip and direction) and nature of the different strata or masses of rock on either side. As, however, he has not this trench or section, he must observe the dip and nature of the rocks on the surface, and take advantage of every river-bank or cliff where the land is broken, and of every quarry or

well, always carrying the beds and masses in his mind's eye to his imaginary section. In every case this section ought to be laid down on paper, on as nearly as possible the real proportional scale, copious notes should be made, and a large suite of specimens collected for *his own* future examination. The value of sections, with their horizontal and vertical scales true to nature, cannot be exaggerated, and their importance has only lately been appreciated to the full extent. The habit of making even in the rudest manner sectional diagrams is of great importance, and ought never to be omitted: it often shows the observer palpably, and before it is too late (a grief to which every sea-voyager is particularly liable), where his knowledge is defective. Partly for the same reason, and partly from never knowing, when first examining a district, what points will turn out the most important, he ought to acquire the habit of writing very copious notes, not all for publication, but as a guide for himself. He ought to remember Bacon's aphorism, that "Reading maketh a full man, conference a ready man, and *writing an exact man;*" and no follower of science has greater need of taking precautions to attain accuracy; for the imagination is apt to run riot when dealing with masses of vast dimensions, and with time during infinity. After the observer has made a few traverses of the country, and drawn his sections (and the coast cliffs often afford him an invaluable one), he will be himself astonished how, in the most troubled country, over which the surface has been broken up and re-cemented, almost like the fragments of ice on a great river, how all the parts fall into intelligible order. He will in his mind see the beds first horizontally stretched out one over the other in a fixed order, and he will then perceive that all the disturbance has arisen from a few nearly straight cracks, on the edges of which the beds have been upturned, and between which he will sometimes find great wedges of once heat-softened, but now crystalline rocks. He will find that large masses of strata have been removed and denuded, that is ground down into pebbles and mud, and long ago drifted away to form in some other area newer strata. He will now have a good idea of the physical structure of his district; and this much can be acquired with much greater facility than he will at first readily anticipate.

In examining a district to make a section, many minor points of detail will occur for observation, which can hardly be specified; such as the nature and cause of the transitions and alterations of the different strata, the source of the sediment and pebbles, the alterations in chemical nature, either of the whole mass, or of parts, as in concretions; the presence, and grouping and state of the fossil remains; the depth and condition of the old sea-bottom, when the beds were deposited, and an infinity of similar points. Probably the best method of obtaining this power of observation, is to acquire the habit of always seeking an

explanation of every geological point met with; for one mental query leads on to another, and this will at the same time give interest to the observer's researches, and will lead him to compare what is before his eyes, with all that he has read of or seen. With his increasing knowledge he will daily find his powers of observation, his very vision, become deeper and clearer. No one, however, must expect to solve the many difficulties which will be encountered, and which for a long time will remain to perplex geologists; but a ray of light will occasionally be his reward, and the reward is ample.

Organic Remains.—In the sectional diagram which we have supposed to be made, the simple superposition of the beds gives their relative antiquity; but the best section which a sea-voyager can hope to make will seldom include but a small portion of the long sequence of known geological formations. And as the voyager seldom passes over large districts, he will rarely succeed in placing in proper order, by the aid of superposition alone, the formations which he successively meets with even in the same country. Hence he must, more than any other geologist, rely on the characters of the embedded organic remains, and must sedulously collect every specimen and fragment of a specimen. By the means of fossil remains, not only will he be enabled to arrange (with the help of naturalists on his return home) the formations in the same country according to their age, but their contemporaneity with the deposits of the most distant parts of the world can thus and by no other method be ascertained; for it is now known that at each geological epoch the marine animals partook in the most distant quarters of a general similarity, even when none of the species were identically the same: thus beds have been recognized in North and South America, and in India, which must have been deposited when the chalk in Europe was accumulating beneath the sea.

It is highly necessary most carefully to keep the fossils found in different strata separate; it will often occur in passing upwards from one bed to another, and occasionally even without any great change in the character of the rock, that the fossils will be wholly different; and if such distinct sets of fossils are mingled together, as if found together, undoubtedly it would have been better for the progress of science that they had never been collected. As there is some inconvenience in keeping the fossils collected on the same day separate, this caution is the more requisite. The collector, if he be not an experienced naturalist, should be very cautious in rejecting specimens, from thinking them the same with what he has already got; for it requires years of practice to perceive at once the small, but constant, distinctions which often separate species:

the same species, moreover, if collected in different localities, or in beds one placed far above the other, are generally more valuable to the geologist than new species.

In formations from a few hundred to a thousand feet and upwards in thickness, the whole of which does actually belong to the same geological age, and is therefore characterized by the same fossils, most curious and important results may be sometimes deduced, if the positions or relative heights at which the groups of fossils are embedded be noted; and this is a point usually neglected. For, thanks to the researches of Professor E. Forbes, the depth of water under which a collection of shells lived can now be approximately told; and thus the movement of the crust of the earth, whilst the strata including the shells were accumulating, can be inferred. For instance, if at the bottom of a cliff, say 800 feet in height, a set of shells are buried, which must have lived under water only 50 or 100 feet in depth; it is clear that the bottom of the sea must have sunk to have allowed of the deposition of the 700 feet of superincumbent submarine strata; subsequently the whole 800 feet must have been upraised. For this same purpose, and for other ends, it is desirable that it should be noted which species are the most numerous, and whether layers are composed exclusively of single kinds. It should be also remarked, whether the more delicate bivalve shells retain their two valves united, and whether the burrowing kinds are embedded in their natural positions, as these facts show that the shells have not been drifted from afar. Where there are fossil corals, it should be observed whether the greater number of specimens are upright, in the positions in which they grew. The remark formerly made that the collection of mere fragments of rock is of little or no use to geology, is far from applicable to fossil remains. Every single fossil species, bones, shells, crustacea, corals, impressions of leaves, petrified wood, &c., should be collected, and it is scarcely possible to collect too many specimens. Even a single species without any information of any kind, if it prove a quite new form, will be valuable to the zoologist; if it prove identical with, or closely allied to a known species, it may interest the geologist. A set of fossils, however, and still more several sets, with their superposition known, cannot fail to be of the highest value; they will tell the age of the deposit, and perhaps give the key to the whole geology of the country: some of the highest problems in this science wait for solution on large collections carefully made in distant regions.

A collection of recent shells (both those living on the coast and those to be procured by the dredge off it) from the same country or island at which a collection of tertiary fossil shells is made, is generally of very great service to the palaeontologist who undertakes the description of the fossils. The collecting [of] recent shells will, moreover, with the aid of a

little study, teach the geologist some conchology, and this is an acquirement yearly becoming more necessary: the geologist should exert himself to learn some general zoology.

The bones of vertebrated animals are much more rarely found than the remains of the lower marine animals, and they are almost in proportion more valuable. A person not acquainted with the science will hardly be able to imagine the deep interest which the discovery of a skeleton, if of higher organization than a fish, in any of the oldest formations would most justly create. The age of such a formation would have to be judged of by the co-embedded shells, and therefore, if possible, part of the slab containing the bones should include one or two shells to demonstrate their contemporaneity. Bones, however, from any formation are sure to be valuable; even a single tooth, in the hands of a Cuvier or Owen, will unfold a whole history; the heads, jaws, and articular surfaces are the most valuable; but every fragment should be brought home. Where bones are found close together, and especially if some of the parts lie in their natural positions, they should be packed together. Every bone, if found even six inches beneath the black vegetable mould, should be collected; there can be no doubt that many most valuable relics have been neglected, from the supposition that they belonged to still living animals. Low cliffs of mud, gravel, and clay on the banks of streams and on sea-shores (as well as in bared reefs extending from them), are the most likely places for the discovery of the remains of quadrupeds. Gravel-beds under streams of lava; fissures in volcanic rocks; peat-beds, and the clay or marl underlying peat, are all favourable places. Fishes' bones are found occasionally in all sedimentary strata, and are highly interesting.

Caverns.—These most frequently occur in limestone rocks, and they have yielded a truly wonderful harvest of remains in Europe, South America, and Australia. The bones generally occur in mud, under a stalagmitic crust produced by the dripping of the lime-charged water, which requires being broken up by a pickaxe. As caverns have often been used by wild races of man as places of habitation and burial, a most careful examination should be made to detect any signs of the surface having been anciently broken up near where the bones are found. Even small islands, not now inhabited by any land quadruped, if not very distant from a continent, are almost as likely to contain osseous remains as larger tracts of land. The interest of the discovery of the remains of land quadrupeds in an oceanic island would be extreme: for instance, it has been stated that the tooth of a mastodon has been found in one of the Azores; and if this were confirmed, few geologists would doubt that these islands had once been united to Europe, thus enlarging wonderfully

our ideas of the ancient geography of the Atlantic: so also the remains of a mastodon are said to have been brought from Timor, thus perhaps indicating the road by which this great quadruped formerly reached Australia.

Fossil Footsteps.—As allied to organic remains, fossil footsteps may be here referred to. They have been observed in Europe and North America, but hitherto in no other part of the world. These curious vestiges not only proclaim the former existence of reptiles and birds at very remote periods, and in rocks often not containing a fragment of bone, but they generally prove that the level of the land subsided after the animal had left its impress on the ancient sea-beach, thus allowing thousands of feet of strata to be thrown down over them. The best place for searching for footsteps is in quarries of sandstone, in which the strata are separated by seams of shale. The best indication of their probable occurrence is the rock being "rippled," that is marked with narrow little wavy ridges, such as occur on most sandy shores when the tide is down, and which indicate that the now rocky surface was once either a tidal beach or a shallow surface, over which the ancient animals walked. In the case of fossil footsteps being found, the largest slab which could possibly be removed ought to be brought away, and accurate drawings, or, still better, casts, made of several of the footsteps. A plan from accurate measurement ought to be taken of any row of steps. The value of such fossil footsteps would be in a manifold degree increased, if the age of the deposit could be determined by shells found in the same stratum, or above it.

Coal Deposits.—The origin of coal presents a most curious and difficult problem in geology; and though a vast amount of information has been accumulated on the subject, yet good observations in distant countries would be of the highest value. A very brief statement of the most prominent difficulties in the theory of its origin will, perhaps, be the best guide for further inquiries. If we look first to the coal itself, the frequency with which, both in Europe and North America, upright vegetables have been found in and on the coal, and the curious relation between the presence of coal and the nature of the clayey bed (abounding with roots) on which it rests, can leave no doubt that, in these so frequent instances the vegetation, whence the coal has been derived, grew on the spot where now embedded. The regularity and wide extent of the beds of coal, and especially of certain subordinate seams in them, the stratification and fineness of the deposits alternating with the coal, and the rarity of channels (such as would have been formed by a stream or river) cutting through the associated strata, all seem pretty clearly to indicate

that the coal was not formed on the surface, like a mass of peat, but under water. What, then, was the nature of those vast expanses of shallow water under which the coal was accumulated? The character of the upright fossil plants, according to our present knowledge, absolutely contradicts the idea of their having lived in the sea; yet occasionally strata, containing undoubted marine remains, are associated with the carboniferous series. On the other hand, how can we believe that lakes, allowing of course their beds slowly to sink, could contain the enormous thickness, amounting in some instances to several thousand yards, of the coal-bearing strata? From these few remarks it will be seen how many points deserve careful examination in any new coal district; the chief points being, the presence of upright vegetables and trunks of trees (of the position of which careful drawings should be made), and whether furnished with roots; the nature of the beds on which the coal rests, and generally of all the strata; the continuousness and form of the strata, and whether ripple-marked; the existence of marine animal remains, and whether such lived on the spot, or were drifted into their present positions; and many other similar points. It is superflous to observe that all fossil plants should be collected; those found upright should be carefully distinguished from those embedded horizontally. The contents of any upright stems and of the roots should be examined; as it appears they have generally first become hollow from decay, and then been filled up with mud, which in some instances is charged with seeds and leaves.

Salt Deposits.—Information is much required on this subject; and this is a case in which good suites of specimens, illustrating the nature of the rocks beneath and above the salt, would possess much interest. Do they contain any organic remains? Did such live on the spot where now buried? Do the rocks show signs of having undergone in any degree the action of heat? Are the strata regular, or are they crossed by oblique layers, showing the probable action of currents? Are there ripple-marks, or beds of coarse pebbles, or other indications of the strata having been deposited in shallow water? What is the thickness, form, and dimensions of the beds of salt? Specimens of the salt, and of any associated saline substances, ought to be brought home in bottles for analysis. The origin of beds of salt, found in formations of very different ages in different parts of the world, is at present quite obscure; some authors attribute it to the sinking of superficial sea-water, rendered more saline by evaporation; others to the evaporation of sea-water periodically overflowing extensive low sandy tracts, like parts of the Run of Cutch[8]; others suspect that its deposition is in some unknown way connected with the sea's bottom having been heated by volcanic action. In some countries there are large lakes of brine, often covering thick beds of salt; these deserve examina-

tion: on what does such salt or brine rest, whether on the bared underlying strata, or on sand or gravel, such as cover the surrounding country? Does the salt contain the remains of animals or plants? Specimens of the salt ought to be brought home in bottles, and attention paid, whether beneath it there is any thin layer of other saline substances.

Cleavage.—The slaty structure of rocks will at first perplex the young geologist; for in proportion as it becomes well developed, the planes of stratification or of original deposition become obscure, and are often quite obliterated. As the sea-voyager, and especially the surveyor, often visits numerous points on the same line of coast, he possesses some great advantages for studying this subject, and numerous observations made with care would probably give striking results. The range or strike of the cleavage is uniform over surprisingly large areas; whereas both the angle and point of dip vary much; but there is reason to believe that the planes of inclination, examined across a wide tract transversely to the range, will fall into order and show that they are the truncated edges of a few great curves or domes. The relation of the cleavage-planes to those of the stratification, or axes of elevation, should be carefully noted, and likewise to the general outline of the whole country. Long sections at right angles to the strike of the cleavage, with the dip carefully protracted on paper, would be highly interesting. When two chains of hills, each having its independent cleavage, cross each other, careful observations should be made. In all cases, any mineralogical difference, however slight, in the parallel cleavage-layers, deserves attention; but observations on this head would be hardly trustworthy, without the planes of stratification were so distinct that there could be no possibility of confounding (as has often happened) cleavage and stratification. Where a stratum of sandstone, or of any other rock without cleavage, is interstratified with a slaty rock, the surface of junction ought to be minutely examined, to see if the slate has slipped along the planes of cleavage, or whether again the mass has not been either stretched or compressed at right angles to these same planes. Fossil shells have been found by Mr. Sharpe in slaty rocks, which have had their shapes greatly altered, and all in the same direction; here then we have a guide to judge of the amount and direction of the mechanical displacement which the surrounding slate-rocks have undergone.[9] Observations on cleavage, to be useful, must be numerous and very accurately made.

The foliation of the metamorphic schists, that is, the origin of the layers of quartz, mica, feldspar, and other minerals, of which gneiss, micaceous, chloritic, and hornblendic schists are composed, is intimately connected with the cleavage of homogeneous slaty rocks. Nearly all the proposed observations on cleavage are applicable to foliation. Wherever

large districts of foliated and ordinary slaty rocks unite, observations would be most desirable. These foliated rocks have all undergone metamorphic action, that is, they have been mineralogically altered and rendered crystalline by chemical attraction, aided by heat; but this is a most obscure subject, one on which it would appear that much further light will not be thrown without the aid of a profound knowledge of mineralogy or chemistry. It is now known that granitic rocks, which have been fluidified (as may be told by their sending great veins into, and including fragments of, the overlying rocks), are foliated in a more or less perfect degree: in these cases the relation of the planes of foliation with those of the adjoining rocks, which have been metamorphosed but not fluidified, would be eminently curious.

Nature of the Sea-bottom.—As every sedimentary stratum has once existed as the bed of the sea or of a lake, the importance of observations on this head is obvious; and no one is so favourably circumstanced for making them as a naval officer on a surveying expedition. The limits of depth under different latitudes at which the various marine animals live or are found strewed dead, is perhaps the most important point for further investigation which can be suggested in the science of geology: scarcely any observations with the dredge have been made within the tropics. Not only the shells, corals, sea-urchins, crabs, &c., brought up from different stated depths, should be preserved, but the proportionate numbers of each kind be carefully noted, as well as the nature of the sea-bottom. An observer could not labour too much in this line, and especially if he would subsequently himself undertake to tabulate and work out the results.[10]

There is another point of view under which the bed of the sea would amply repay long-continued observations. It is well known that the nature of the bottom often changes very regularly in approaching a coast; the pebbles, for instance, increasing in size in a surprisingly steady ratio with the decreasing depth. But the means by which the pebbles are thus sorted is not known: is it by the oscillation of the waves at ordinary periods, or only during gales; or is it by the action of currents? A chart, with the nature of the bottom carefully noted on it and the currents laid down, would by itself throw some light on this question. The nature of the pebbles being observed, perhaps a point would be found whence they radiated. Excellent observations have been made by engineers on the travelling of shingle-beaches, but scarcely anything is known of their movement under water. In what condition are the pebbles?—are they encrusted (as often happens) with delicate corallines?—after a heavy gale are the spines of such corallines found broken? In narrow channels where there are rapid currents, and in the open sea in front of straits, where the

water often suddenly deepens, what is the nature of the bottom? To what depth does the sea in a storm render the water muddy? How far from the beach, and to what depth, does the recoil of the waves, or the "undertow," act, for instance, on light anchors? At what depth can the sea wear solid rock? This may sometimes be judged of by the nature of the bottom; thus, where soft mud overlies the rocky surface, we may infer that the sea can hardly now be a destroying agent, even if the inclination of the strata on the adjoining coast shows that rocky strata must once (probably, when the land stood at a different level) have extended much further. Is it at the line of high or low water, or between them, that the breakers most vigorously eat into coast-cliffs? Gigantic fragments of rock, much too large to be themselves rolled about, may be seen at the foot of almost every line of high cliffs; by what means in the course of time will these be removed, as must have happened with their innumerable predecessors? Are they slowly worn away or broken up? It may be well to recollect that in the tropics the powerful action of frost in splitting stones is entirely eliminated. Our observations, moreover, on the alluvial and sub-littoral deposits of these latitudes are not perplexed by the ancient effects of floating ice. The spray of salt water, above the line of breakers, corrodes by chemical decomposition calcareous rocks; does this play any important part on other rocks? Most bold coasts are fronted by sharp promontories and even isolated pinnacles; are these *exclusively* due to the greater hardness of the rocks composing them, or do not the breakers act more efficiently when eddying round any slight projection?

Rocks rising steeply out of the open ocean, and exposed to the incessant wash of the heaviest surf, are often thickly coated over with various marine animals, and this would seem to indicate that pure water has not the power of gradually wearing away hard rocks, though the waves may occasionally tear off large fragments. Is the washing to and fro of pebbles, or of sand, a necessary element in the corroding power of waves on hard rocks? but how comes it that small land-locked harbours, where the waves can hardly have force to move the shingle, should ever be surrounded by cliffs, which, in most cases, clearly prove that considerable masses of rock have been worn down into mud and removed? Again, at a moderate depth, where the bottom is covered with shingle, does the rolling to and fro of the pebbles wear away solid rock? if, so, the pebbles would be clean, and the submarine rocky surface probably worn into furrows or channels at right angles to the beach. Where there are violent currents and eddies, are deep round holes worn in the bottom, like those produced by eddies at the foot of cascades? This, perhaps, might be ascertained by a long pole at the turn of the tide: deep round holes have been observed on rocks formerly covered by the sea, and their origin has perplexed geologists. Any person steadily attending to these subjects will occasionally be enabled to form an opinion on

points at first appearing hopelessly obscure to him. The common deep-sea lead, especially if made a little bell-shaped and well armed, gives a surprisingly good picture of the bottom. There can be no doubt that whoever will for a long period collect and compare observations, made over wide areas and under different circumstances, will arrive at many curious, novel, and important results.

An observer occasionally may arrive at a district where lately some great aqueous catastrophe has occurred, such as the bursting of a lake temporarily formed by a slip, or the rush of a great earthquake-wave over low land. In such cases all the effects produced, such as the thickness and nature of any deposit left—whether stratified irregularly or continuously—whether any rocky surface, over which the debacle has passed, be scored or smooth; all such points should be minutely described, and measurements taken of any great blocks which may have been transported: the great desideratum is accuracy and minuteness.

Ice Action.—The voyager in the Polar Seas would render an excellent service to geology by observing all the effects which icebergs produce in rounding, polishing, scoring, and shattering solid rocks, and likewise in transporting gravel and boulders. Floating ice under two forms is known to transport fragments; namely, coast-ice, in which the stranded boulders are frozen, and icebergs formed by glaciers entering the sea, on the surface of which masses of rock had previously fallen from the surrounding precipices. It is obvious that in the latter case the fragments would generally be quite angular, and they could not be landed in water shallower than the thickness of the submerged ice, requisite to float the berg. On the other hand, the boulders frozen in coast-ice would generally be previously water-worn, and they could be landed on an ordinary beach, and might be driven by the force of the pack high and dry, and perhaps left piled in strange positions. All facts illustrating the difference in the results produced by coast-ice and true icebergs would be very valuable. Do the boulders fixed on coast-ice, when driven over rocky shoals, become themselves scored? Wherever there was reason to believe that a surface has been scored by recent ice-action, a minute description and drawings ought to be made of the depth, length, width, and direction of the grooves; and even large slabs brought home. On true icebergs are the fragments of rock generally fixed or loose; when icebergs turn over, are fragments frequently seen embedded in that part which was under water; and how were they fixed there? The nature, number, size, form, and frequency of occurrence of all fragments of rock seen on floating ice ought to be recorded, and the distance from their probable source. A polar shore, known from upraised organic remains to have been lately

elevated, would be eminently instructive. Do great icebergs force up the mud and gravel at the bottom of the sea in ridges like the moraines of glaciers? Can shells, or marine animals, live in a shallow sea, often ploughed up and rendered turbid by the stranding of icebergs? The dredge alone could answer this. The means to distinguish the effects of ancient floating ice from those produced by ancient glaciers is, at present, a great desideratum in geology. M. Agassiz' work on Glaciers, with its admirable plates, ought to be procured by any one going to the colder regions of the north or south.

Erratic boulders occur in Europe, N. America, and in the southern parts of S. America, which, it is believed by most geologists, were transported by ice; those near mountains, by ancient glaciers; and those on the lowlands, by floating ice. Erratic boulders, when not of gigantic size, may be confounded with rounded stones, transported by occasional great floods or by the coast-action of the surf during slow changes of level of the land. Masses of granite, from often disintegrating into large, apparently water-worn boulders, and then rolling downwards, have several times been erroneously described as belonging to the erratic class. Where the nature of all the rocks in the vicinity is not perfectly known, great size and the angularity of the fragments (though by no means a constant concomitant) are the most obvious distinctive characters; but even when the surrounding country is not at all known, the composition of a single isolated hill or small island may easily be ascertained, and if large fragments of foreign rock lie strewed on its surface, these may be assumed almost certainly to be erratic boulders. Here, however, a caution has been found necessary; for in the case of fragments of *sedimentary* rocks, they may be the last remnant of a denuded overlying formation. Wherever erratic boulders are found, their composition, form—especially attending to whether they are angular, water-worn, or scored, and their size, from actual though rude measurements, should be given.

Both in the north and south a peculiar formation called "till" has been found connected with erratic boulders; it consists generally of mud, containing angular and rounded stones of all sizes up to the largest boulders, mingled in utter confusion, and generally without any stratification. Such deposits should be examined. Sometimes when they are stratified, the upper beds have been found violently contorted, whilst the lower ones are undisturbed, showing that the violence has not proceeded from below, as in ordinary geological cases. Sir C. Lyell has suggested that this effect has been produced by the stranding of great icebergs.

As far as our present knowledge goes, the above enumerated phenomena—such as scored, mamillated, and polished rocks, moraines, erratic boulders, and beds of till, though occurring in latitudes where

glaciers do not now occur, where the sea is never frozen, and where icebergs are never drifted, yet have not been observed in either hemisphere higher than about latitude 40°. Hence, on whatever coast ancient ice-action might be discovered, the limit of latitude towards the tropics at which it ceases ought to be carefully investigated. Observations are much wanted on the west coast of N. America and the east coast of Asia; and again in New Zealand and other islands of the Southern Ocean. The period of the ice-action is pretty well ascertained in Europe and North America, and a very great service would be rendered to geology if the same point could be clearly made out in the southern hemisphere; for it might greatly influence our ideas on the climate of the world during the late tertiary periods. Any shells embedded in "till" (though unfortunately, of very rare occurrence) would decide this point, and it might probably be closely judged of, if till or boulders were found resting on, or covered by, shell deposits.

Distribution of Organic Beings.—As geology includes the history of the organic inhabitants, as well as of the inorganic materials, of the world, facts on distribution come under its scope. Earth has been observed on icebergs in the open ocean; portions of such earth ought to be collected, washed with fresh water, filtered, gently dried, wrapped up in brown paper, and sent home by the first opportunity, to be tried, with due precautions, whether any seeds still alive are included in it. Again, the roots of any tree cast up on an island in the open ocean should be split open, to see if any earth or stones are included (as often happens), and this earth ought to be treated like that from icebergs: it is truly surprising how many seeds are often contained in extremely small portions of earth. Any graminivorous bird, caught far out at sea, ought to have the contents of its intestines dried for the same object. The zoologist who, with a towing-net, fishes for floating minute animals, ought to observe whether seeds are thus taken. These experiments, though troublesome, undoubtedly, would be well worth trying. All facts or traditional statements by the inhabitants of any island or coral-reef, on the first arrival of any bird, reptile, insect, or remarkable plant, ought to be collected. In those rare cases in which showers of fish, reptiles, shells, earth, seeds, confervae, &c., have fallen from the sky, every fact should be recorded, and specimens collected.

Volcanic Phenomena.—The voyager will probably have ample opportunities of examining volcanic islands, and perhaps volcanoes in eruption. With respect to the latter, he ought to record all that he sees: should the exact position of the orifice be known, he might, perhaps, by

observing some point in a cloud, measure with a sextant to what height the fragments were shot forth, and the height of the often flat-topped column of ashes. Having surveying instruments, he ought to map, as carefully as time will permit, any crater remarkable for its size, depth, or peculiar form. M. Élie de Beaumont has found that, owing to the fluidity of lava, streams never consolidate into a thick, moderately-compact mass, except on a surprisingly gentle inclination. On a slope of above 2° or 3° the stream consists of extremely irregular masses, often forming a hollow vault within. Fresh observations on this point are much wanted in regard to lavas of different composition. The measurements can easily be made by a sextant and artificial horizon.[11] In making such observations, comparatively recent streams must be chosen, so that there can be no doubt that the whole consists of a single stream: this cannot be judged of without examining the whole line between the two points of measurement, for some liquid lavas thin out to a very fine edge; and two streams, one over the other, may be thus very easily mistaken for a single one. The composition, thickness, and degree of cellularity of any lava-stream, of which the slope is measured, ought to be described as seen on the sides of fissures, and wherever its internal structure can be made out.

Round many active and extinct volcanoes, both on continents and on islands, there is a circle of mountains, steep on their inner, and gently inclined on their outer flanks. The volcanic strata, of which they are composed, everywhere dip away from the central space, but at a considerably higher angle than it is believed lava can consolidate into such thick and compact masses. These mountains form the so-called "craters of elevation," the origin of which has excited much controversy, and which demand further examination. There is a grand range of mountains of this class at the Mauritius and at St. Jago in the Cape de Verdes, parts only of which have been described. The chief points to attend to are, the *inclination* of the streams by actual measurement, their thickness, compactness, and composition; the form and height of the mountains, whether traversed by very many dikes, of which the common direction ought to be recorded; how far the mountains stand apart, and the diameter and outline of the rude circle which they together form. In fact, a most useful service would be rendered by mapping any of these "craters of elevation," or, what would be more feasible, drawing from actual measurements two sections at right angles to each other, across the circle.

Some streams of lava, especially those belonging to the trachytic series (harsh, generally rather pale-coloured lavas, with crystals of glassy feldspar), are laminated. The course of the layers with respect to the course of the stream ought to be minutely studied, both on the surface, at the termination, and flanks of the stream; and, if by a most

fortunate chance there should have been formed a transverse section, throughout its entire thickness: this would be a very interesting subject for investigation. A series of specimens ought to be brought away to illustrate the nature of the lamination.

Aërial Dust.—Fine brown-coloured dust has often fallen on vessels far out at sea, more especially in the middle of the Atlantic. This should be collected; the direction and force of the wind (and the course of any upper current, as shown by the movement of the clouds) on the same day, and for some previous days, ought to be recorded, as well as the date, and the position of the ship. Such dust has been shown by Ehrenberg to consist, in many cases, almost entirely of the siliceous envelopes of infusoria. The distance to which real volcanic dust is blown is, likewise, in some respects well worth determining.

Elevation of the Land.—The changes of level, often accompanying earthquakes, will be treated of by Mr. Mallet,[12] but a few remarks on the nature of the evidence to be sought on changes of level not actually witnessed by man, may be here inserted. Many appearances, such as lines of inland cliffs, of sand-hillocks, eroded rocks, and banks of shingle, often indicate the former effects of the sea on the land when the latter stood at a lower level. But the best evidence, and the only kind by which the period can be ascertained (for the appearances above enumerated, though well preserved, may sometimes be of considerable antiquity), is the presence of upraised recent marine remains. On land which has been elevated within a geologically recent time, sea shells are often found, either embedded in thin layers of sand and mould, or scattered on the bare surface. In these cases, and especially in the latter case, great caution is requisite in testing the evidence; for man, birds, and hermit-crabs often transport, in the course of ages, an extraordinary number of shells. In the case of man, the shells generally occur in heaps, and there is reason to believe that this character is long preserved. To distinguish the shells transported by animals from those uplifted by the movement of the earth, the following characters may be used:—Whether the shells had long lain dead under water, as indicated by barnacles, serpulae, corallines adhering to their *insides;* whether the shells, either from not being full grown or from their kind, are too small for food; remembering that certain shells, as mussels, may be unintentionally transported by man or other animals in their young state adhering to larger shells; and lastly, whether all the specimens have the same appearance of antiquity. Some shells, which have been exposed for many ages, yet retain their colours in a surprising manner. The very best evidence is afforded by barnacles and boring shells being found attached

to or buried in the rock, in the same positions in which they had lived; these may be sometimes found by removing the earth or birds' dung covering points of rock. Where shells are embedded in a superficial layer of soil, though it may appear exactly like vegetable mould, specimens of it should be preserved, for the microscope will sometimes reveal minute fragments of marine animals. In all these cases, specimens of the shells, though broken and weathered, and having a wretched appearance, must carefully be preserved; for a mere statement that such upraised shells resembled those still living on the beach is absolutely of no value. It should be noticed whether the proportional numbers between the different kinds appear to be nearly the same in the upraised shells and in those now cast on the beach. The height at which the marine remains occur above the level of the sea should be measured. In confined situations, where the change of level appears to have been small, much caution must be exercised in receiving any evidence; as a change in the direction of the currents (resulting from alterations in neighbouring submarine banks) may cause the tide to flow to a somewhat less height, and thus give the appearance of the land having been upraised.

Wherever a tract of country can be proved to have been recently elevated, its surface, as exhibiting the late action of the sea, is a fertile field for observation. On such coasts, terraces rising like steps, one above another, often occur. Their outline and composition should be studied, diagrams made of them, and their height measured at many and distant parts of the coast. There is reason to believe that in some instances such terraces range for surprisingly long distances at the same height. Where several occur on opposite sides of a valley a spirit level is almost indispensable, in order to recognize the corresponding stages. Where ranges of cliffs exist, the marks of the erosion of the waves may sometimes be expected to occur, and as these generally present a defined line, it is particularly desirable that their horizontality should be ascertained by good levelling instruments, and if not horizontal, that their inclination should be measured. Where more than one zone of erosion can be detected all should be levelled, for it does not necessarily follow that the several lines are parallel. Along extensive coasts, and round islands which have been uplifted to a considerable height, and where we now walk over what was, within a late geological period, the bed of the sea, it would be well to observe whether extensive sedimentary deposits have been upraised; for it has often been tacitly assumed that sedimentary deposits are in process of formation on all coasts.

Subsidence of the Land.—This movement is more difficult to detect than elevation, for it tends to hide under water the surface thus affected. Evidence, therefore, of subsidence is very valuable; and this movement,

moreover, has probably played a more important part in the history of the world than elevation, for there is reason to believe that most great formations have been accumulated whilst the bed of the sea was sinking. Subsidence may sometimes be inferred from the form of the coast-land; for instance, where a line of cliffs, too irregular to have been formed by elevation alone, plunges precipitously into a sea so profoundly deep that it cannot be supposed that the now deeply submerged portions of the cliff have been simply worn away by the currents. The direct evidence of subsidence, if not witnessed by man, is almost confined to the presence of stumps of trees, peat-beds, and ruins of ancient buildings, partly submerged on tidal beaches. Ancient buildings may sometimes afford such evidence in unlikely situations: it has been asserted, that in one of the volcanic islands in the Caroline archipelago there are ruins with the steps covered by the sea. Again, at Terceira, at the Azores, there is an old church or monastery said to be similarly circumstanced.

Coral Reefs.[13]—The most important point with respect to coral reefs, which can be investigated, is, the depth at which the bottom of the sea, *outside the reef*, ceases to be covered with a continuous bed of living corals. This can be ascertained by repeated soundings with a heavy and very broad bell-shaped lead, armed with tallow, which will break off minute portions of the corals or take an exact impression of them: it can thus also instantly be seen how soon the bottom becomes covered with sand. This limit of depth ought to be ascertained in different seas, under different latitudes, and under different exposures. For collecting specimens of the corals, it is to be feared that the dredge would become entangled, but chains and hooks may be lowered for this purpose. There is reason to suspect that different species of corals grow in different zones of depth; so that in collecting specimens, the depth at which each kind is found, and at which it is most abundant, should be carefully noted. It ought always to be recorded whether the specimen came from the tranquil waters of a lagoon or protected channel, or from the exposed outside of the reef. The small reefs within the lagoons of certain atolls (or lagoon-islands) in the Indian Ocean all rise to the surface; whereas in other atolls not a single reef rises within several fathoms of the same level. It would be a curious point to ascertain whether the corals in these cases consisted of the same species; and if so, on what possible circumstance this singular difference in the amount of their upward growth has depended.

Any facts which can elucidate the rate at which corals can grow under favourable circumstances, will ever be interesting: nor should negative facts, showing that within a given period reefs have not increased either

laterally or vertically upwards, be neglected. In a full-grown forest, to judge of its rate of growth, a part must be first cut down; so is it probably with reefs of corals. The aborigines of some of the many coral islands in the great oceans might perhaps adduce positive facts on this head; for instance, the date might be known when a channel had been cut to float out a large canoe, and which had since grown up.

For the classification of coral reefs, the most important point to be attended to, is the inclination of the bed of the adjoining sea; and secondly, the depth of the interior lagoon in the case of atolls, and of the channel between the land and the reef, in Encircling or Barrier, and in Fringing reefs. Whenever it is practicable, soundings ought to be taken at short ascertained distances, from *close* to the breakers in a straight line out to sea, so that a sectional outline might be protracted on paper. In those cases in which the bottom descends by a set of ledges or steps, their form ought to be particularly attended to; and whether they are covered with sand or by dead or living coral; and whether the corals differ on the different ledges: the same points should be attended to within the lagoon, wherever its bed or shore is step-formed: the origin of these steps or ledges is at present obscure. In the Indian and Pacific Oceans there are entire reefs, having the outline of atolls or lagoon-islands, lying several fathoms submerged; there are likewise defined portions of reefs both in atolls and in encircling reefs similarly submerged. It would be particularly desirable to ascertain what is the nature of these submerged surfaces, whether formed of sand or rock or living or dead corals. In some cases two or more atolls are united by a linear reef; the form of the bottom on each side of this connecting line ought to be examined. Where two atolls or reef-encircled islands stand very near each other, the depth between them might be attempted by deep soundings: the bottom has been struck between some of the Maldiva atolls.[14] Generally the form and nature of the reefs encircling islands ought to be compared in every respect with the annular reefs forming atolls.

On the shores of every kind of reef, especially of atolls and of land encircled by barrier reefs, evidence of the slow sinking of the land should be particularly sought for; for instance, by stumps of trees, the foundation-posts of sheds, by wells or graves or other works of art, now standing beneath the level of high-water mark, and which there was good reason to believe must have once stood above its level. The observer must bear in mind that cocoa-nut trees and mangroves will grow in salt-water. If such evidence be found, inquiry ought to be made whether earthquakes have been felt. On the other hand, all masses of coral standing so much above the level of the sea that they could not have been thrown up by the breakers during gales of wind, at a period

when the reef had not grown so far out seaward, should be investigated and their height measured. There is reason to believe that some coral-reefs have been thought to have been upraised, owing to the effect of the lateral or horizontal extension of the reefs having been overlooked; for the necessary result of this outward growth is gradually to break the force of the waves, so that the rocks, now further removed from the outer breakers, become worn to a less height than formerly, and the more inland corals, not being any longer constantly washed by the surf, cease to live at a level at which they once flourished. It is indispensable that specimens of all upraised corals, and especially of the shells generally associated with them, should be collected; for there can be no doubt that ancient strata containing corals, have in some instances been confounded with recent coral-rock. The importance of ascertaining whether coral-reefs have undergone, or are undergoing, any change of level, depends on the belief that all the characteristic differences between Atolls and Encircling reefs on the one hand, and Fringing reefs on the other, depend on the effect produced on the upwardly-growing corals by the slow sinking or rising of their foundations.

A thick and widely-extended mass of upraised recent coral-rock has never yet been accurately examined, and a careful description of such a mass—especially if the area included a central depression, showing that it originally existed as an atoll—is a great desideratum. Of what nature is the coral-rock; is it regularly stratified or crossed by oblique layers; does it consist of consolidated fine detritus or of coarse fragments, or is it formed of upright corals embedded as they grew? Are many shells or the bones of fish and turtle included in the mass, and are the boring kinds still in their proper positions? The thickness of the entire mass and of the principal strata should be measured, and a large suite of specimens collected.

In conclusion, it may be re-urged that the young geologist must bear in mind, that to collect specimens is the least part of his labour. If he collect fossils, he cannot go wrong; if he be so fortunate as to find the bones of any of the higher animals, he will, in all probability, make an important discovery. Let him, however, remember that he will add greatly to the value of his fossils by labelling every single specimen, by never mingling those from two formations, and by describing the succession of the strata whence they are disinterred. But let his aim be higher: by making sectional diagrams as accurately as possible of every district which he visits (nor let him suppose that accuracy is a quality to be acquired at will), by collecting for his own use, and carefully examining numerous rock-specimens, and by acquiring the habit of patiently seeking the cause of everything which meets his eye, and by comparing it with all that he has himself seen or read of, he will, even if without any previous

knowledge, in a short time infallibly become a good geologist, and as certainly will he enjoy the high satisfaction of contributing to the perfection of the history of this wonderful world.

1. Section VI, pp. 156–95, in John F. W. Herschel, ed., *A Manual of Scientific Enquiry; Prepared for the Use of Her Majesty's Navy: and Adapted for Travellers in General* (London: Murray, 1849).†

2. Charles Lyell, *Principles of Geology: Or, the Modern Changes of the Earth and its Inhabitants, Considered as Illustrative of Geology*, 3 vols, 6th ed. (London: Murray, 1840); *Elements of Geology*, 2d ed., 2 vols. (London: Murray, 1841).†

3. William Phillips, *An Elementary Treatise on Mineralogy: Comprising an Introduction to the Science;* 5th ed.... by Robert Allan ... *with Numerous Additions to the Introduction* by Francis Alger (Boston: Ticknor, 1844).†

4. Henry Thomas De la Beche, *Researches in Theoretical Geology* (London: Knight, 1834).†

5. Charles Daubeny, *A Description of Active and Extinct Volcanos* ..., 2d ed. (London: Taylor, 1848).†

6. Leopold Von Buch, *Description Physique des Îles Canaries, Suive d'une Indication des Principaux Volcans du Globe ... Traduite ... par C. Boulanger* (Paris: Levrault, 1836).†

7. Louis J. R. Agassiz, *Études sur les Glaciers* (Neuchâtel: Jent et Gassmann, 1840).†

8. Run of Cutch: Rann of Cutch or Kutch (large salt waste in South Sind, West Pakistan).†

9. With respect to further observations on this important point, Mr. Hopkins remarks, in his paper 'On the Internal Pressure of Rock Masses' (Cambridge Phil. Transact., vol. viii.), that "the observer should direct his attention especially to those cases in which the inclination of the cleavage planes to the bedding is either small, or nearly 45°."

10. The best kind of dredge, and the manner of using it, are described under the Zoological Section.

11. M. Élie de Beaumont gives the following directions (Mémoires pour servir, &c., tom. iv. p. 173):—

> The method I am in the habit of employing for these kinds of measurements is simple and easy, and a description of it may save useless trouble to others. I place on the edge of the sextant, and behind the fixed mirror, a small piece of white paper, in which there is a narrow opening (ouverture étroite) corresponding to the axis of the telescope. On the exterior surface of the paper a black line is drawn, perpendicular to the plane of the graduated circle, and passing through the centre of the opening above mentioned. A quantity of mercury is poured into a vessel sufficient to form a plane horizontal surface of a certain extent. The telescope of the sextant is then directed vertically over the mercury, and the image of the black line sought for. When this is found, I am certain that the visual ray from the image in the mercury can only deviate from the perpendicular, in so far as the line is not without breadth, and the opening has a sensible size. These two sources of error can be diminished so that the maximum of error shall not exceed a minute. Being once certain of the verticality of the visual ray from the image of the black line, I have only to make the image of any object reflected from the moveable mirror coincide with that of the black line, to have the angle between the vertical and the line drawn from the centre of the instrument to the object in question, which may be any distant point on the surface of a bed of lava, a glacier, a road, a river, &c.

12. R. Mallet, "Earthquakes," in John F. W. Hershel, ed., *A Manual of Scientific Inquiry* . . . (London: Murray, 1849, pp. 196–223).†

13. The only work specially written on this subject is 'The Structure and Distribution of Coral Reefs,' by Mr. Darwin.

14. About 700 miles west of Ceylon.†

[Remarks on Cirripedia][1]

'On the Occurrence on the British Coast of a Burrowing Barnacle, being a type of a new order of the class Cirripedia,' by Mr. A. Hancock.[2]—The animal which was the subject of this paper is called by the author *Alcippe lampas*. It inhabits the dead shells of various species of mollusca, which it appears to penetrate, and constructs for itself a residence by some process of boring. The author had an opportunity of watching its developement from the egg; and during the early parts of its existence it presented all the characters of many of the forms of entomostracous Crustacea. The author made some remarks on the relation of this animal to the other orders of Cirripedes, and proposed to constitute for it a new order which he called Cryptosomata. The paper was illustrated by drawings of the animal and dissections of some of its parts.

Mr. Darwin remarked that having been employed for a considerable time in drawing up a monograph on the Cirripeda for publication by the Ray Society, he felt great interest in Mr. Hancock's paper—more especially as he had collected in South America an allied form, inhabiting cavities in the *Concholepas Peruviana*. Its main affinity to the genus described by Mr. Hancock lies in the number and position of the cirri and the great developement of the labrum:—the metamorphosis and organs of generation appear to be considerably different. Mr. Darwin stated that he possessed the type of another and quite distinct order of Cirripedes, entirely destitute of any shell, covering, or peduncle, without cirri, and with a suctorial mouth of very peculiar structure. Having remarked on the vast external differences between the common cirripedes and such forms as the last mentioned and that described by Mr. Hancock, and that inhabiting the Concholepas, Mr. Darwin stated that the main and unfailing character of a cirripede consists in the manner in which it becomes attached to foreign bodies. This is effected at first by the voluntary act of the larva, or more strictly pupa; afterwards a thick fluid or soft tissue debouches by the penultimate or ultimate segment of the prehensile antennae, and so permanently attaches them to the surface: the antennae are thus preserved, whilst all the other external organs of the pupa are moulted and lost. During the continued growth of the cirripede, the cementing substance in many genera is emitted from fresh orifices placed symmetrically round, but further and further from the centre of the basis. The most remarkable circumstance with respect to

this cementing substance is that it is certainly secreted from glands which are actually continuous portions of the branching ovarian tubes or caeca. Finally, Mr. Darwin observed, that had Mr. Hancock examined specimens, instead of drawings, of the Lithotria in the rock, he would almost certainly have acknowledged its power of excavating cavities.—Prof. Milne-Edwards suggested that the secretion by which the cirripedes were enabled to attach themselves to foreign bodies was produced by a gland at the base of the antennae, similar to that which occurs in some species of macrourous Crustacea.—Mr. Darwin in reply stated that the gland in the cirripedes was truly ovarial.—Prof. Allman referred to the instance of a burrowing barnacle which had been discovered in the shells of some turtles brought from the West Indies, and described by the Rev. W. Hincks. It was a large species, measuring an inch and three-quarters.—Mr. Jeffreys inquired if the cirripedes were in the habit of moulting.—Mr. Darwin stated that their life was very active and their changes frequent, and some species moulted twice in a week.—Dr. MacDonald thought that the structure of Mr. Hancock's animal and its earlier changes would throw some light on the structure of Trilobites.

1. *Athenaeum. Journal of Literature, Science, and the Fine Arts*, no. 1143, 22 September 1849, p. 966.†

2. Report, Section D, Natural History, Including Physiology, Nineteenth Meeting of the British Association for the Advancement of Science.†

On British Fossil Lepadidae[1] [This paper was withdrawn by the author with the permission of the Council.] [Abstract.]

Mr. Darwin noticed that great confusion exists in the nomenclature of the comparatively few species of Cirrhipeds, hitherto found in a fossil state; arising both from the easy separation of the several dissimilar valves soon after the death of the animal, and from the imperfect characters afforded by the valves themselves, which are, as it were, but parts of the crustacean carapace, neither accompanied with, nor distinctly impressed by, any of the soft parts of the animal. He then pointed out such particular valves as were sufficiently distinct, and had sufficiently constant characters to be considered as characteristic of genera,—as, for instance, the keel, or dorsal, valve in *Scalpellum*, and the scutal, or inferior lateral, valve in *Pollicipes*. The pedunculated cirrhipeds (*Lepadidae*) were stated to have made their first appearance in the lower oolite, and to have reached their culminant point in the cretaceous epoch. The absence of sessile cirrhipeds in the earlier and secondary formations, and their occurrence for the first time in the eocene deposits,

were then noticed, the author dwelling on the characters of the genus *Verruca*, and pointing out that, as the type of a group intermediate between, and of equal value with, the sessile and the pedunculate cirrhipeds, it offered no real exception to the rule that sessile cirrhipeds do not occur in the secondary formations; but that, on the contrary, it harmonizes with the law of relation between serial affinities of animals and their first appearance on this earth. Mr. Darwin concluded with a few observations on the comparative ranges of recent and fossil cirrhipeds, and on the close affinities between the extinct and the living forms.

1. *Quarterly Journal of the Geological Society of London. Proceedings of the Geological Society* 6(1850):439–40.†

Bucket Ropes for Wells[1]

I suffer from the serious misfortune of a well 325 feet deep. It is worked by two buckets, and a chain, which, from its great length, is necessarily very heavy. Would a wire rope (galvinised) answer? This, I presume, might be tight and thin; it would have to carry, at each end, a strong and heavy bucket, holding 12 gallons. The rope would have to work over, and, I presume, once quite round, a wheel only 14 inches in diameter. Would any of your correspondents have the charity to give the result of any actual experience of light wire rope; such would be of value, probably to others, as well as to myself.

1. *Gardeners' Chronicle and Agricultural Gazette*, no. 2, 10 January 1852, p. 22.†

On the Power of Icebergs to Make Rectilinear, Uniformly-Directed Grooves across a Submarine Undulatory Surface[1]

Having been induced to believe, with many geologists, that certain continuously scored and polished surfaces of rock were due to icebergs, and not to glaciers, I have nevertheless always felt much difficulty in understanding how long, rectilinear scratches, running in one given direction across an *undulatory* surface, could have been thus formed. Others have felt this same difficulty, and it has been advanced as an insuperable difficulty by the opponents of iceberg action. The following considerations, though possessing little or no novelty, have in my own case removed the difficulty. But first, to give one instance of such

scratches, I may quote a passage from Agassiz,[2] who, in describing the state of the surface near Lake Superior, says, "nothing is more striking in this respect than the valleys or depressions of the soil running E. and W., where we see the scratches crossing such undulations at right angles, descending along the southern gentle slope of a hill, traversing the flat bottom below, and rising up the next hill south in unbroken continuity." He proceeds to state that the scratches run up even steep northern slopes, though the southern faces of the hills are generally rugged. A glacier driven straight forwards over its unequal bed would perfectly account for these facts; but not so, at first appearance, floating ice, whether that of coast-ice or of icebergs. For such masses being borne along on the level ocean, would, when driven on shore or against a submarine hill, be deflected, as it might be thought, from their course, and mark the rocks horizontally or nearly so,—some allowance being made for the rise and fall of the tide. And although during either the submergence or emergence of the land, the whole surface of a mountain might become thus marked, yet the successive scores at each level would all be nearly horizontal. No doubt short inclined grooves might be formed by masses of ice being driven by gales up the beach; but as sea-shores run in every possible direction, it is obvious that such grooves could follow no uniform course, nor could they be of any considerable length; hence grooves thus made would not be comparable with those now under discussion.

The plasticity of glaciers, as shown by the manner in which they immediately expand after passing through gorges, and in which they mould themselves to every sinuosity and prominence in their beds, is now, thanks to the labours of a few eminent men, familiarly known to every geologist. It is asserted by some authors that glacier ice is most plastic when most charged with water, and the lower part of an iceberg must be water-logged. Again, a glacier, for instance of 1000 feet in thickness, must press on its bed with the whole immense weight of the superincumbent ice; but in an iceberg 1000 feet thick, as the whole floats, there will of course be no pressure on a surface exactly level with its bottom, and if driven over a prominence standing up at the bottom of the sea some 50 or 100 feet above the basal line of the berg, only the weight of as much ice as is forced up above the natural level of the floating mass, will press on the prominence. It may therefore, I think, be concluded that an iceberg could be driven over great inequalities of surface easier than could a glacier. That the weight of a comparatively thin sheet of ice is sufficient to groove rocks, we may infer from the case described by Sir C. Lyell of the scores made by the packed *shore-ice* on the coast of the United States. That icebergs do not break up when grounded, as *à priori* might have seemed probable, is obvious from the simple fact of their

having been often observed in this condition in open turbulent seas. Let anyone who has witnessed the crash of even so small an object as a ship, when run into by another having only a barely perceptible movement, reflect on the terrific momentum of an iceberg, some mile or two square, and from 1000 to 2000 feet in thickness, when, borne onwards by a current of only half a mile per hour, it runs on a submarine bank: may we not feel almost certain, that, moulding itself like a glacier (of which it originally was a portion), but owing to its water-logged state and little downward pressure moulding itself more perfectly than a glacier, it would slide straight onwards over considerable inequalities, scratching and grooving the undulatory surface in long, straight lines? In short, if in our mind's eye we look at an iceberg, not as a rigid body (as has hitherto been always my case) which would be deflected or broken up when driven against any submarine obstacle, but as a huge semi-viscid, or at least flexible mass floating on the water, I believe much of the difficulty will be removed which some have experienced in understanding how rectilinear grooves could be formed continuously running, as if regardless of the outline of the surface, up and down moderately steep inequalities, now existing as hills on the land. It should be borne in mind that the course of deeply-floating icebergs is determined by the currents of the sea, and not, as remarked by Scoresby, by the shifting winds; and as the currents of the sea are well known to be definite in their course, so will be the grooves formed by current-borne icebergs. It is indeed difficult to imagine any difference between the effect on the underlying surface, of a glacier propelled by its gravity, and that of a mountainous island of ice driven onwards by an oceanic current, except that the iceberg would perhaps have the power, from the causes above specified, of even more closely moulding itself, and, as it were, of *flowing* straight over submarine obstacles, than has a glacier on the dry land.

One other point is perhaps worth considering. I have elsewhere[3] endeavoured to show that the action of coast-ice and of icebergs must be considerably different in transporting boulders; the worn stones on the beach being imbedded in coast-ice, and fragments of rock which had originally fallen on the parent-glacier being carried by icebergs as on rafts. But when we reflect that icebergs are driven onwards year after year in certain definite directions by the currents of the sea,—that they float so deeply as to have been seen aground at the depth of 1500 feet,—that when stranded they must (as I conceive) mould themselves to the inequalities of the bottom and slide some distance over it,—it can hardly be doubted that they also must, like glaciers on the land, push in certain determinate directions moraines before them. Although a fragment of rock or an irregularly formed moraine may by any one iceberg be propelled for only a very short distance, yet in the course of years the

transportal can hardly fail to become far extended, the boulders being rolled over large inequalities of surface, and even up heights by the action of successively smaller bergs: an abyss, however, deeper than the deepest-floating iceberg would, of course, absolutely stop this rolling or pushing action. Finally, in the case of every mass of erratic boulders, we have now to determine, and I believe hereafter it will be so determined, whether they were transported by glaciers or by floating ice, and in this latter case whether imbedded in coast-ice, strewed on the surface of icebergs, or pushed onwards as a subaqueous moraine.

1. *London, Edinburgh, and Dublin Philosophical Magazine and Journal of Science* 10(1855):96–98.†
2. Lake Superior, its Physical Character, &c., by L. Agassiz, p. 406.
3. Transactions of the Geological Society, vol. vi. (2nd series) 1841. p. 430.

Does Sea-Water Kill Seeds?[1]

I have begun making some few experiments on the effects of immersion in sea-water on the germinating powers of seeds, in the hope of being able to throw a very little light on the distribution of plants, more especially in regard to the same species being found in many cases in far outlying islands and on the mainland. Will any of your readers be so kind as to inform me whether such experiments have already been tried? And, secondly, what class of seeds, or particular species, they have any reason to suppose would be eminently liable to be killed by sea-water? The results at which I have already arrived are too few and unimportant to be worth mentioning.

1. *Gardeners' Chronicle and Agricultural Gazette*, no. 15, 14 April 1855, p. 242.†

Does Sea-Water Kill Seeds?[1]

As you have done me the honour to notice favourably my wish to ascertain experimentally the power of resistance in seeds to the injurious action of sea-water, you may perhaps like to have a report. As such experiments might naturally appear childish to many, I may be permitted to premise that they have a direct bearing on a very interesting problem, which has lately, especially in America, attracted much attention, namely, whether the same organic being has been created at one point or on several on the face of our globe. As geologist I feel a special interest on the possibility of plants being transported by sea to distant islands, owing to the great influence which it is very obvious the views of the late ever-lamented Edward Forbes[2] have had on the subsequent writings of

botanists and zoologists. Forbes, as is well known, boldly supposed that the north coast of Spain had formerly been directly continuous with Ireland, and he extended the continent of Europe as far as and beyond the Azores. To imagine such enormous geological changes within the period of the existence of now living beings, on no other ground but to account for their distribution, seems to me, in our present state of ignorance on the means of transportal, an almost retrograde step in science—it cuts the knot instead of untying it. Weighty objections might, I think, be urged against Forbes' hypothesis as applied in the above and many other cases, but this is not the proper place to discuss such a question. As I had not the least notion when I began, whether or not the seeds would be all killed by a single week's immersion, I at first took only a few, selecting them almost by chance from the different great natural families; but I am now trying a set chosen on philosophical principles by the kindness of Dr. Hooker.

The sea-water has been made artificially with salt procured from Mr. Bolton, 146, Holborn Bars, which has been tested by better chemists than men, namely, by numerous sea animals and algae having lived in it for more than a year. The seeds were placed in separate bottles, holding from 2 to 4 oz. each, out of doors in the shade: the mean temperature has during the period been about 44°, rising during one week to a mean of nearly 48°. Most of the seeds swelled in the water, and some of them slightly coloured it, and each kind gave to it its own peculiar and strong odour. The water in which the Cabbage and Radish seeds were placed became putrid, and smelt offensively in a quite extraordinary degree; and it is surprising that any seeds, as was the case with the Radish, could have resisted so contaminating an influence; as the water became putrid before I had thought of this contingency, it was not, and has never been, renewed. I also placed seeds in a quart bottle in a tank filled with snow and water, to ascertain whether the seeds kept at the temperature of 32° would better resist the salt water; this water, like that in the small bottles, to my surprise became turbid and smelt rather offensively. In the following list I have no reason to suppose, except in the cases where so stated, that the seeds have endured their full time.

(1) Seeds of common Cress (Lepidium sativum) have germinated well after 42 days' immersion; they give out a surprising quantity of slime so as to cohere in a mass. (2) Radishes have germinated less well after the same period. (3) Cabbage seed: after 14 days' immersion only one seed out of many came up; I think this is rather strange considering that the Cabbage is a sea-side plant; in the ice-cold salt water, however, several have come up after 30 days' immersion. (4) Lettuce seed has grown well after 42 days; (5) of Onion seed only a few have germinated after the same period; (6) Carrot and (7) Celery seed well after the 42 days; (8) Borago officinalis, (9) Capsicum, (10) Cucurbita ovifera, have ger-

minated well after 28 days' immersion; the two latter, rather tender kinds, were also tried in the ice-cold water, and have germinated after 30 days' immersion. (11) Savory, or Satureja, has grown somewhat less well after 28 days. (12) Linum usitatissimum: only one seed out of a mass of seeds (which gave out much slime) came up after the 28 days, and the same thing happened after 14 days; and only three seeds came up after the first seven days' immersion, yet the seed was very good. (13) Rhubarb, (14) Beet, (15) Oracle, or Atriplex, (16) Oats, (17) Barley, (18) Phalaris canariensis, have all germinated excellently after 28 days; likewise these six latter after 30 days in the ice-cold water. (19) Beans and (20) Furze, or Ulex: of these a few survived with difficulty 14 days; the Beans were all killed by 30 days in the ice-cold water. (21) Peas germinated after seven days, but were all dead after 14 days' immersion out of doors, and likewise after 30 days in the ice-cold water. (22) Trifolium incarnatum is the only plant of which every seed has been killed by seven days' immersion; nor did it withstand 30 days in the ice-cold salt water. (23) Kidney Beans have been tried only in the latter water, and all were dead after the 30 days.

As out of these 23 kinds of seed, selected almost at hap-hazard, the five Leguminosae alone have as yet been killed (with the exception of the Cabbage seed, and these have survived in the ice-cold water), one is tempted to infer that the seeds of this family must generally withstand salt water much worse than the seeds of the other great natural families; yet from remarks in botanical works, I had expected that these would have survived longest. It has been really curious to observe how uniform, even to a day, the germination has been in almost every kind of seed, when taken week after week out of the salt water, and likewise when compared with the same seeds not salted—all of course having been grown under the same circumstances, namely, in glasses on my chimney-piece, so that the seeds from the day of being planted have been always under my eye. The germination of the Rhubarb and Celery alone has been in a marked degree altered, having been accelerated. With respect to Convolvulus tricolor, not included in the above list, I may mention that many of the seeds germinated and came out of their husks, whilst still in the salt water, after six or seven days' immersion.

To return to the subject of transportal, I may state that in "Johnston's Physical Atlas"[3] the rates of 10 distinct currents in the Atlantic (excluding drift currents) are given, and the average of them is 33 nautical miles per diem; hence in 42 days, which length of immersion seven out of the eight kinds of seed as yet tested have already stood, a seed might be readily carried between 1300 and 1400 miles.

I will conclude this too lengthy communication by observing that all the 40—50 seeds which I have as yet tried sink in sea-water; this seems at first a fatal obstacle to the dissemination of plants by sea currents; but it

may be doubted whether most seeds (with the exception of the winged kinds), when once shed, are so likely to get washed into the sea as are whole or nearly whole plants with their fruit by being carried down rivers during floods, by water-spouts, whirlwinds, slips of river-cliffs, &c., continued during the long lapse of geologically modern ages. It should be borne in mind how beautifully pods, capsules, &c., and even the fully expanded heads of the Compositae close when wetted, as if for the very purpose of carrying the seed safe to land. When landed high up by the tides and waves, and perhaps driven a little inland by the first inshore gale, the pods, &c., will dry, and opening will shed their seed; and these will then be ready for all the many means of dispersal by which Nature sows her broad fields, and which have excited the admiration of every observer. But when the seed is sown in its new home then, as I believe, comes the ordeal; will the old occupants in the great struggle for life allow the new and solitary immigrant room and sustenance?

1. *Gardeners' Chronicle and Agricultural Gazette*, no. 21, 26 May 1855, pp. 356–57.†

2. Edward Forbes. See, for example, "On Some Indications of the Molluscous Fauna of the Azores and St. Helena," *Report of the Twenty-first Meeting of the British Association for the Advancement of Science; Held at Ipswich in July 1851* 20(1852):76–77; "On the Extinct Landshells of St. Helena," *Quarterly Journal of the Geological Society of London* 8(1852):197–99; and "On the Manifestation of Polarity in the Distribution of Organized Beings in Time," *Edinburgh New Philosophical Journal* 57(1854):332–37.†

3. Alexander Keith Johnston, *The Physical Atlas: A Series of Maps & Illustrations of the Geographical Distribution of Natural Phenomena Embracing: I. Geology. II. Hydrography. III. Meteorology. IV. Natural History* (Edinburgh and London: Blackwood, 1850).†

Nectar-Secreting Organs of Plants[1]

In the account compiled by Gärtner ("Beiträge zur Kenntniss der Befruchtung," p. 75, 1844) of the various organs in plants from which nectar is secreted, no mention is made of the stipulae of the leaves of the common Vetch and Bean. On two occasions I have observed hive bees by the thousands industriously visiting the little dark (but sometimes colourless) glands on the under side of the stipulae of the Vetch. On a hot day, on each gland a minute drop of nectar may be seen almost with the naked eye, and which is sometimes so large as to be just perceptibly sweet. I have seen the hive and another species of bee, a moth, ants, and two kinds of flies, sucking these drops. The hive bee never once even looked at the flowers, but attended solely to the stipulae; whereas, at the very same time, two kinds of humble bee were sucking the flowers, and

never visited the stipulae. I noticed the hive bees on three successive hot days thus employed; but on the overcast morning of the 12th, after the previous very rainy day, not one was to be seen at mid-day, but numbers of humble bees were sucking the flowers: at 4 o'clock P.M., however, after some hot sunshine, a little glittering drop of nectar studded every gland, and the hive bees, by their mysterious means, had found it out, and were swarming all over the field. The fact of nectar being secreted by an organ quite distinct from the flower (though known in other cases) seems to me of some little interest, as showing that those botanists cannot be correct who believe that nectar is a special secretion for the purpose of tempting insects to visit flowers, and thus aid in their fertilisation. No one probably who has attended to this subject will dispute that insects in very many cases do thus aid the act of fertilisation; but we must, I think, look at the nectar as an excretion which is only incidentally (as is so often done by nature) made use of for a further but most important object.

1. *Gardeners' Chronicle and Agricultural Gazette*, no. 29, 21 July 1855, p. 487.†

Shell Rain in the Isle of Wight[1]

I earnestly hope that "C." of Winchester[2] will give some more particulars regarding the fall of shells at Osborne. Were any of the shells living? Over how wide an area did they fall? During how long a time are they believed to have fallen? At what hour and on what day? Did only one kind of shell fall? I hope "C." will forgive me for suggesting to him how very desirable it is that so extraordinary and very interesting a fact should be authenticated by the narrator's name. It is really almost a duty towards the science of natural history to do so. Were the Zua identified by any good conchologist?—this seems to me an important point. (The Zua was very obligingly identified by Dr. Baird of the British Museum. Our correspondent's name is C. Winchester; he is the intelligent foreman in the Royal Gardens at Osborne, and will, we hope, furnish the additional information asked for.)[3]

1. *Gardeners' Chronicle and Agricultural Gazette*, no. 44, 3 November 1855, pp. 726–27.†

2. C. Winchester, "Shell Rain in the Isle of Wight," *Gardeners' Chronicle*, no. 43, 17 October 1855, p. 710. (Note Darwin's mistake in Winchester's name.)†

3. Parenthetical statement by the editor of the *Gardeners' Chronicle*. This article was followed by four more on the subject of shells raining from the sky, in which a lively controversy ensued between Winchester and another correspondent, C. R. Bree. See *Gardeners' Chronicle*, 10 November 1855, p. 743; 17 November 1855, p. 758; 1 December 1855, p. 789; and 15 December 1855, pp. 821–22.†

Vitality of Seeds[1]

Several statements have been published on the number of years during which seeds preserved in a dry state have retained their power of germinating, but much less seems to be known in regard to seeds lying naturally near the surface of the ground; therefore you may, perhaps, think the following case, though very far from a striking one, worth publishing. An arable field 15 years ago was laid down in pasture; nine years ago last spring, a portion was deeply ploughed up and planted with trees, and in the succeeding summer, as far as I can trust my memory, plenty of Charlock, which abounds in this neighbourhood, came up; but if my memory plays me false the case will prove so much the stronger. From being badly ploughed the whole of the land in the course of the year became covered with Grass and coarse weeds, and has remained so ever since, and the trees have now grown up. It is very improbable, from the well-known habits of the Charlock, that it could have grown in the little wood after the first year or two; and though almost daily visiting it I have not noticed a plant. But this spring I had some Thorn bushes pulled up, and it was so done that not more than one or two (I speak after comparison) hand's breadth of earth was turned up. To my surprise in July I happened to observe on one of the little patches of earth no less than six dwarf Charlock plants in flower; on each of two other patches three plants; and on the fourth one plant. This made me on July 21st have three separate plots of ground, each 2 feet square, in different rather open parts of the wood, cleared of thick Grass and Weeds, and dug one spit[2] deep. By August 1st many seedlings had come up, and several of them seemed to be cruciferous plants; so I marked with little sticks 11 of them on one of the beds; six on the second bed; and five on the third bed; two or three died, all the rest grew up and proved to be Charlock. I can state positively that no Charlock was growing near these beds; and I do not believe there was any within a quarter of a mile, as the little wood is surrounded by Grass land. Now, to my mind, this seems good evidence that the Charlock seed had retained its vitality within a spit's depth of the surface during at least eight or nine years. In most cases, when plants spring up unexpectedly, as when a wood has been burnt down, it is not possible to feel sure (as has been remarked to me by Dr. Hooker) that the seeds had not been strewed about during the last year or two by birds or other means. Had the several Charlock plants come up on one spot alone, I should have thought that some accident had brought a pod there, and that I had overlooked during the previous years a few Charlock plants, but it seems to me improbable in the highest degree that on each of the 6 plants,[3] taken by simple hazard, several (in one of the cases 11) seeds should have been dropped by some unknown agency, having been brought from a quarter of a mile distance. But if when the land was

ploughed, 9 years ago (or when arable, 15 years ago) the whole was, as I believe, almost covered by Charlock, the seed would have been scattered everywhere, ready to spring up at whatever point the land might subsequently be stirred up. I will only further remark that the power in seeds of retaining their vitality when buried in damp soil may well be an element in preserving the species, and, therefore, that seeds may be specially endowed with this capacity; whereas, the power of retaining vitality in a dry and artificial condition must be an indirect, and in one sense accidental, quality in seeds of little or no use to the species.

1. *Gardeners' Chronicle and Agricultural Gazette*, no. 46, 17 November 1855, p. 758.†
2. Spade-thrust.†
3. Should be, "6 plots of ground."†

Effect of Salt-Water on the Germination of Seeds[1]

As you have published notices by Mr. Berkeley[2] and myself on the length of time seeds can withstand immersion in sea-water, you may perhaps like to hear, without minute details, the final results of my experiments. The seed of Capsicum,[3] after 137 days' immersion, came up well, for 30 out of 56 planted germinated, and I think more would have grown with time. Of Celery only 6 out of some hundreds came up after the same period of immersion. One single Canary seed grew after 120 days, and some Oats half germinated after 120; both Oats and Canary seed came up pretty well after only 100 days. Spinach germinated well after 120 days. Seed of Onions, Vegetable Marrow, Beet, Orache[4] and Potatoes, and one seed of Ageratum mexicanum[5] grew after 100 days. A few, and but very few, seed of Lettuce, Carrot, Cress, and Radish came up after 85 days' immersion. It is remarkable how differently varieties of the same species have withstood the ill effects of the salt water; thus, seed of the "Mammoth White Broccoli" came up excellently after 11 days, but was killed by 22 days' immersion; "early Cauliflower" survived this period, but was killed by 36 days: "Cattell's Cabbage" survived the 36 days, but was killed by 50 days; and now I have seed of the wild Cabbage from Tenby growing so vigorously after 50 days, that I am sure that it will survive a considerably longer period. But the seed of the wild Cabbage was fresh, and some facts show me that quite fresh seed withstands the salt water better than old, though very good seed. With respect to an important point in my former communication of May 26th, permit me to cry *peccavi;* having often heard of plants and bushes having been seen floating some little distance from land, I assumed—and in doing this I committed a scientific sin—that plants with ripe seed or fruit would float

at least for some weeks. I always meant to try this, and I have now done so with sorrowful result; for having put in salt-water between 30 and 40 herbaceous plants and branches with ripe seed of various orders, I have found that all (with the exception of the fruit of evergreens[6]) sink within a month, and most of them within 14 days. So that, as far as I can see, my experiments are of little or no use (excepting perhaps as negative evidence) in regard to the distribution of plants by the drifting of their seeds across the sea. Can any of your readers explain the following sentence by Linnaeus, pointed out to me by Dr. Hooker, "Fundus maris semina non destruit"? Why does Linnaeus say that the bottom of the sea does not destroy seeds? The seeds which are often washed by the Gulf Stream to the shores of Norway, with which Linnaeus was well acquainted, float, as I have lately tried. Did he imagine that seeds were drifted along the bottom of the ocean? This does not seem probable, from the currents of the sea, at least many of them, being superficial. P.S. In my communication on Charlock seed lately printed by you, there is a misprint of "6 plants" for "6 plots of ground," which makes nonsense of the sentence.[7]

1. *Gardeners' Chronicle and Agricultural Gazette*, no. 47, 24 November 1855, p. 773.†
2. M.J. B[erkeley], ["Results of Experiments on Immersing Seeds in Sea-Water,"] *Gardeners' Chronicle*, no. 35, 1 September 1855, p. 580.†
3. Genus of plants including peppers, chilies, and so on.†
4. Certain plants of saline or akaline regions; greasewood is an example; *Atiplex hortensis* is a garden species.†
5. *Ageratum* is an aster of the American tropics.†
6. Should be, "fruit of Euonymus."†
7. See p. 260, "Vitality of Seeds."†

Effect of Salt Water on the Germination of Seeds[1]

In my communication of last week it is printed by mistake that the fruit of "evergreens," instead of the fruit of the Euonymus, did not sink after immersion in salt water during a month. I may add that I think that the experiments on immersion of seeds in sea water have some little interest, as showing that we cannot infer from seeds of certain orders long retaining their power of germination in a dry condition, that these same seeds will retain it under different conditions. Thus the Solaneae and Leguminosae are believed to keep longest when preserved in the ordinary way in a dry state, and the Solaneae seem generally to resist well the salt water, whereas most Leguminosae resist much worse, as I have shown in your number of the 26th of May, than other orders. I have lately tested this conclusion with quite fresh seeds of Trifolium incarnatum and

Kidney Beans. Indeed with respect to some Leguminosae I have reason to believe that a short immersion in plain water will kill them. So with respect to the subject lately discussed in your columns, namely how long seeds will remain alive when buried in damp earth, I do not see that any safe conclusion can be drawn from the length of time during which the same seeds can retain their vitality whilst dry.

1. *Gardeners' Chronicle and Agricultural Gazette*, no. 48, 1 December 1855, p. 789.†

Longevity of Seeds[1]

As you have lately published such full and interesting details on the case of the long entombed Raspberry seeds, you may like to hear that a somewhat similar instance has been observed on the Continent. Gaertner (Versuche über die Bastarderzeugung, s. 157) states on the authority of Jouannot that seeds from the graves of ancient Gauls, of the date of the introduction of Christianity (probably at the time of Clodowig in the third or fourth century A.D.) germinated and produced Heliotropium vulgare, Centaurea cyanus, and Trifolium minimum. Gaertner gives, as reference, Froriep Notizen, B. XLIII., No. 946, p. 348. It seems that no known botanist looked to the correctness of these names.

1. *Gardeners' Chronicle and Agricultural Gazette*, no. 52, 29 December 1855, p. 854.†

Seedling Fruit Trees[1]

As several different statements have been published on how far the different varieties of our fruit trees produce seedlings like their parents, I think very interesting information might be given by some few of your correspondents who may have carefully sown named seeds and have noted the result. Jourdan (in the "Mémoires de l'Acad. de Lyons," vol. ii., p. 94, 114) states most positively that he has tried repeatedly, and that all the many seedlings which he raised from the same variety of fruit tree resembled each other in foliage and general manner of growth as perfectly as do the young plants of any wild species whatever; and therefore that they differed from the seedlings of every other variety of the same fruit tree. Hence, also, as he asserts, the seedlings of one variety can never be confounded by an experienced eye with those of another variety, being as distinct as were their parents. Moreover, he states that the fruit of seedling Pears and Apples, though differing greatly in size, succulency, and flavour from those of their parents, yet resemble them in

the more important characters of form and in the nature of their seeds. On the other hand Van Mons[2] asserts that he sometimes raised from the seed of one variety of Pear a quite distinct kind; but it now appears that Van Mons was careless in marking the varieties sown. If any one can give accurate information on this curious subject, I hope that he will be so kind as to take the trouble to do so; and will give, as far as he can, some idea what proportion of seedlings are produced which resemble their parents in foliage and general habit; for if seedlings differ from their parents only in a few rare instances, this might perhaps be attributed to an accidental cross from some neighbouring tree. Is it known whether some varieties of Pears and Apples tend to produce truer offspring than other varieties? Plums are said to come very true. Mr. Rivers, and possibly others, could probably give very interesting details on this head.

1. *Gardeners' Chronicle and Agricultural Gazette*, no. 52, 29 December 1855, p. 854.†

2. Jean Baptiste van Mons, *Arbres Frutiers* . . ., 2 vols. (Louvain: n.p., 1835–36); see Charles Darwin, *Variation of Animals and Plants* . . . (New York: Appleton, 1899, vol. 2, p. 324, n. 29).†

Cross Breeding[1]

I have been lately collecting all the evidence which I can get from the observation of others and my own, on the natural crossing of varieties of plants. The evidence in regard to Leguminous plants is curiously conflicting, but preponderates against their ever crossing without artificial aid. I should esteem it a singular favour if any of your correspondents would give in your paper or send me any evidence showing either that Leguminous crops, when grown close together, do sometimes cross; or, on the other hand, that they may invariably be grown close together without any chance of deterioration.

1. *Gardeners' Chronicle and Agricultural Gazette*, no. 49, 6 December 1856, p. 806.†

On the Action of Sea-Water on the Germination of Seeds[1]

During the spring of last year it occurred to me that it would be worth while, in relation to the distribution of plants, to test how long seeds could endure immersion in sea-water, and yet retain their vitality. As far as I knew, this had not been tried by botanists, who would have been far more capable of doing it efficiently than myself; and I now find that M. Alph. DeCandolle,[2] in his admirable work, "Géographie Botanique,"

regrets that such experiments have not been tried; I think, that had he known even the few facts here to be recorded, some of his opinions on the means of distribution of particular families would have been slightly modified. The Rev. M. J. Berkeley has likewise tested fifty-three different kinds of seeds, and has published a report in the "Gardener's Chronicle,"[3] to which periodical I have also sent two brief notices on the same subject.[4] I intend here to give, with Mr. Berkeley's kind permission, an account of our joint experiments. I may premise, that not knowing, at first, whether the seeds would endure even a week's immersion, I selected a few by simple chance, taking, however, the seeds of different families; subsequently I have been aided by suggestions from Dr. Hooker.

I must briefly describe how my experiments were tried: the seeds were placed in small bottles, each holding two or three ounces of salt water, carefully made according to Schweitzer's analysis: as both *algae* and marine animals have, as is well known, long survived in water thus made, there can be no doubt that the experiment was thus fairly tried. Mr. Berkeley sent his seeds to Ramsgate, tied up in little bags and placed in the sea-water, daily renewed; and they were thus immersed for three weeks, and when partially dried, but still damp, were sent off, but by accident were not unpacked for four days subsequently, so that their total immersion "was equivalent to one of more than a month." Some of my bottles were put out of doors in the shade, and were exposed to an average weekly temperature of from 35° to 57°; the other bottles were kept in my cellar, and were exposed to much less variation of temperature, viz. to a daily mean average of from 46° to 56°. Further, to test the effect of temperature, I immersed eighteen different sorts of seeds in salt water, in a tank, which, from containing much snow, was for six weeks at a temperature of 32°, slowly rising for the next six weeks to 44°; but the seeds thus tested did not seem to withstand the injurious effect of the salt water better than those exposed to a higher but variable temperature. I may remark, that amongst the eighteen kinds of seeds immersed in the cold salt water, there were seeds of a somewhat tender constitution, as capsicum and vegetable marrow, but the exposure to the cold in no degree injured their germination. In the case of some of the seeds which I first tried, and which were put out of doors, I did not change the salt water for fifty-six days, and it became putrid and smelt offensively to a quite surprising degree, especially the water with the cabbage, radish, cress and onion seed, which also gave out strongly the odour of each kind; so that I thought the putridity would infallibly have been communicated to the seeds; but judging from the seeds of some of the same plants (but not actually from the same lot of seed) placed in salt water often renewed, and likewise kept in the cellar under a less variable temperature, neither the putridity of the water nor the changing temperature had any marked effect on their vitality. Cress seed (*Lepidium*

sativum) and that of *Phalaris Canariensis*, after twenty-two days' immersion, were thoroughly dried for a week and then planted; they germinated pretty well, but the seeds themselves of this particular lot were not very good. At first I tried the seeds after each successive week's immersion, and they germinated at the same period as did seeds of the same kind which had not been salted; celery and rhubarb seed, however, were somewhat accelerated in their germination. Some kinds of seeds, as of *Trifolium incarnatum*, *Sinapis nigra*,[5] peas, kidney and common beans, swelled much in the salt water, and they generally were killed by a short immersion; but the swollen seeds of *Lupinus polyphyllus* germinated better than those which did not swell. I was surprised to observe that most of the seeds of *Convolvulus tricolor*[6] germinated after seven days under the salt water and lived for some time in it; as did likewise the fresh seed of *Tussilago farfara*[7] after 9 days; after 25 days I took out some of the young plants of the *Tussilago* and planted them, and one of them grew: some of the seeds of the garden orache (*Atriplex*) also germinated under water after 56 days' immersion, but I failed in raising the seedlings; the other seeds of the same lot of the orache germinated excellently after 100 days' immersion.

The total number of seeds tried by Mr. Berkeley and myself amount only to 87, for unfortunately we happened to select some of the same kinds; in one respect, however, this has been fortunate, for we thus have tested each other's results, and they accord perfectly as far as they go; the seed of the tomato, however, germinated better after a month's immersion with Mr. Berkeley than after only 22 days with me; but my seed appeared to be old. And this leads me to remark, that I suspect that fresh seed withstands the salt water better than old, but yet good seed; this was the case with *Trifolium incarnatum*, *Phlox Drummondii*, and I believe with *Sinapis nigra*. Of the genus *Godetia*,[8] Mr. Berkeley found one species was killed by, and another survived, a month's immersion: but a far more curious case is presented by the varieties of the cabbage; for I found that good seed of the "Mammoth white broccoli" germinated after 11 days' immersion, but was killed by 22 days; seed of the "early cauliflower" survived 22 days; but was killed by 36 days; "Cattell's cabbage" germinated excellently after 36 days, but was killed by 50 days; and lastly, fresh seed of the wild cabbage from Tenby germinated excellently after 50 days, very well after 110 days, and two seeds out of some hundreds germinated after 133 days' immersion.

Of the 87 kinds of seeds tried, 23 or more than one quarter did not endure 28 days' immersion: capsicum has endured the trial best, for 30 out of 56 seeds germinated well after 137 days' immersion: of celery seed after the same period of 137 days, only 6 out of several hundreds germinated. The worst germinators have been dwarf kidney beans and *Hibiscus manihot*, both killed by 11 days' immersion; common peas were

killed by 14 days'; *Tussilago farfara* germinated under water after 9 days, but the young plants kept alive for some time: the next worse germinators have been *Phlox Drummondii, Trifolium incarnatum, Linum usitatissimum,* and *Sinapis nigra,* very few of which survived 15 days' immersion.

From such scanty materials it is, perhaps, rash to draw any sort of deduction in regard to the power of resistance to salt water in the different divisions of the vegetable kingdom; but a few remarks may be permitted. Three out of the 17 Endogens[9] and 20 out of the 70 Exogens[10] were killed by a month or 28 days' immersion: this fact, together with marked power of endurance in the *Atriplex, Beta, Spinacea,* and *Rheum,*[11] lowly organized exogens, accords with, and is perhaps connected with, the fact, insisted on so much by M. A. DeCandolle, of the wider range of the Endogens and of the lowly organized Exogens, than of the higher Exogens.[12] The four *Solanaceae* and two *Umbelliferae*[13] endured the salt water very well, and each included the longest survivor of all the species tried. Ten *Compositae* were tried, and only one was killed by a month's immersion, that is excepting the *Tussilago* which germinated under water. Eight *Cruciferae* were tried, and all withstood the influence well, excepting *Sinapis nigra,* which was killed by 25 days' immersion; three of the *Cruciferae* survived 85 days: this power of endurance in the seeds of this family is, perhaps, surprising, considering the oil in their seeds. Nine *Leguminosae* were tried; these all resisted the salt water badly, with the exception of the hard thin seeds of *Mimosa sensitiva,* which germinated pretty well after 50 days; three species of Lupine seemed just able occasionally to withstand about 36 days' immersion; the seeds of the other *Leguminosae* having all been killed by much shorter periods. I suspect that it is the water, and not the salt, which kills the *Leguminosae;* at least I found that a lot of fresh "Thurston Reliance" peas were all killed by 13 days' immersion in pure water[14]; and I have been assured that a much shorter immersion will kill kidney beans. Lastly, seven species of the allied familes of *Hydrophyllaceae*[15] and *Polemoniaceae*[16] (six having been selected by Mr. Berkeley) were killed by a month's immersion, and so great a proportion can hardly be accidental.

From the great difference in the powers of resistance to the sea-water in the different families just specified, and even in the varieties of the same species; and from the *Leguminosae* being apparently in this respect the tenderest, whereas they are generally believed to keep longer than any other seeds in a dry state, I think we may learn a lesson of caution, not to infer with too much certainty which seeds will endure longest when naturally buried in damp earth, from knowing what kinds will keep best in an artificial state.

I had intended trying many more seeds, as I at one time thought that

these experiments would have thrown more light on the dispersal of plants than I now think they do. I soon became aware that most seeds, in accordance with the common experience of gardeners, sink in water; at least I have found this to be the case, after a few days, with the 51 kinds of seeds which I have myself tried; so that such seeds could not possibly be transported by sea-currents beyond a very short distance. Some few seeds, however, do float, as I have tried with some of those cast by the Gulf Stream on the coast of Norway. From knowing that timber is often cast on the shores of oceanic islands far from the mainland, and from having met with accounts of floating vegetable rubbish off estuaries, I assumed that plants, with ripe seeds, washed into the sea by rivers, landslips, &c., might be drifted by sea-currents during a period of some weeks. The closing of the capsules, pods, and heads of the *Compositae*, &c., when wetted, and their re-opening when cast on shore and dried, the seeds being thus allowed to be driven inland by the first stormy winds, seemed to favour such means of transport. But in putting 34 plants of different orders, with ripe fruit, into salt water, one alone, the *Euonymus*, floated for a month, being buoyed up by its fruit; the others all sunk in 21 days, some in 5, and several in 7, 9, and 11 days. But I am not sure that I have made the trial fairly, for I kept the floating plants in too warm and dark a place, which might have favoured their decay. Finally I may remark, that the seeds of very few species are, as far as we yet know, all killed by 10 days' immersion,—that some plants will float for this period,—that the average rate of the ten currents in the Atlantic Ocean, given in Johnston's "Physical Atlas," is 33 miles per diem (the main Equatorial current running at the rate of 60 miles, and the Cape Stream at 80 miles per diem); and therefore I conclude, under the existing extremely scanty materials for forming any opinion, that some plants might under favourable conditions be transported over arms of the sea 300 or even more miles in breadth; and if cast on the shore of an island not well stocked with species, might become naturalized.

In the following list, to save repetition, I have marked the plants tried by Mr. Berkeley, and which germinated after a month's immersion, with *; when they did not germinate, this is expressly stated. The "cold water" refers to the seeds placed in salt water in the tank with snow.

I have arranged the families in accordance with Lindley's "Vegetable Kingdom."[17]

Endogens (Gramineae)

Avena (common oats): after 85 days' immersion germinated excellently; after 100 days some germinated; after 120 days some half-germinated.

Hordeum (common barley): germinated well after 28 days, but none after 42 days; in the cold water well after 30 days (*).

**Triticum* (wheat).

Phalaris Canariensis: after 70 days nearly all germinated; in another lot after 85, most of the seeds germinated, but the seedlings died off; after 100 and likewise after 120 days' immersion, in each case, a single seedling came up.

Holcus saccharatus: after 36 days germinated fairly; after 50 days all died.

**Zea Mays:* none germinated after a month's immersion.

**Arum maculatum.*

**Anomatheca cruenta.*

**Babiana plicata.*

**Trichonema pudicum.*

**Sisyrinchium iridifolium.*

Canna Indica: after 50 days several germinated, but not very strongly.

**Colchicum autumnale:* did not germinate.

Allium cepa: after 56 days' immersion, 3 out of 15 germinated; after 82 days in the cold water, most of the seeds grew well; after 100 days, 2 or 3 grew out of about 25 planted (*).

**Bulbine annua.*

**Asphodelus luteus.*

**Uropetalum serotinum:* did not germinate.

Exogens

Ricinus communis (*var. major* and *minor*): both germinated after 36 days.

Cucurbita Melopepo (vegetable marrow): germinated after 100 days; of 4 seeds immersed in the cold water for 82 days, 2 germinated.

**Cucumis Melo* (melon).

Cistus (mixed shrubby garden varieties): germinated well after 36 days, and some germinated after 70 days.

(Cruciferae)

Lepidium sativum: after 85 days' immersion only one out of many germinated; after 56 days 6/57 grew: in the cold water, after 65 days, 4/60 grew. (**var.*, golden cress.) These seeds gave out an astonishing quantity of slime in the salt water.

Brassica oleracea, var. "Mammoth white Broccoli:" germinated after 11 days' immersion, but after 22 days all died.

——— ———, *var.* "Early Cauliflower:" after 22 days 5 out of 100 germinated; after 36 days all dead.

Brassica oleracea, var. "Cattell's Cabbage:" germinated excellently after 36 days; all dead after 50 days.

——— ———, *var.* growing wild on the Castle Rocks of Tenby; fresh seeds, after 50 days germinated excellently; after 110 days germinated very well; after 133 days only two out of some hundreds germinated (*).

**Brassica Rapa* (*var.* yellow turnip).

Raphanus sativus: after 85 days, 2/30 germinated; the cold water seemed to be injurious to these seeds, for after only 30 or 50 days all the seeds were dead (*var.* black radish) (*).

Erysimum Perowskianum: after 36 days germinated well; after 50 only one seed; after 70 days all dead (*).

Matthiola annua: germinated after 28 days; all dead after 54 days.

Sinapis nigra: seeds much swollen; germinated after 11 days; all dead after 22 days: *fresh* seed germinated pretty well after 15 days, but were all killed by 25 days' immersion.

Crambe maritima: after 37 days germinated well.

Tropaeolum majus: after 37 days nearly all germinated, but after 50 days none did.

**Limnanthes Douglasii.*

Hibiscus Manihot: all were killed by 11 days' immersion (*).

**Malope grandiflora.*

Papaver somniferum: germinated well after 28 days; was killed by 54 days.

Argemone Mexicana: came up excellently after 50 days, and pretty well after 70 days.

**Chryseis crocea* (germinated very imperfectly after the month).

Linum usitatissimum: after 7 and after 14 days only two or three seeds, out of very many, germinated; after 28 only one seed came up; after 42 days not one germinated. These seeds gave out much slime.

**Silene compacta.*

Rheum Rhaponticum: germinated well after 82 days.

Atriplex (garden orache): some of the seed germinated under water after 56 days' immersion; the remaining seed germinated excellently after 100 days.

Beta vulgaris: excellently after 100 days (*).

Spinacea oleracea: excellently after 70 days; a few after 120 days; all killed by 137 days (*).

(Leguminosae)

Vicia Faba (*var.* "Johnston's Wonder"): two out of six lived after 11 days' immersion; one half-germinated after 14 days; after 22 days all dead: many of these beans swelled greatly. I tried sixty after 28 days and found all dead. None survived 30 days in the cold water.

Pisum sativum: after 11 days some germinated; none survived 14 days;

none survived 30 days in the cold water. Another lot of *fresh* seed ("Thurston's Reliance") all died after 12 days; none survived 30 days in the cold water. I found 13 days' immersion in pure water killed these latter fresh peas. (*None germinated.)

Phascolus vulgaris (*var.* "early frame dwarf"): all died after 11 days' immersion; after 28 days' immersion, 80 were planted, but all dead. I tried another lot of *fresh* seed, but none of them resisted even 10 days' immersion; nor did they resist 30 days in the cold water: many of these seeds swelled much (*).

Trifolium incarnatum: all died after 11 days' immersion, and after 30 in the cold water. *Fresh* seed germinated excellently after 5 days' immersion, well after 12 days, and one single seed out of some hundreds germinated after 20 days. These seeds swelled much.

Ulex europaeus: after 11 days germinated well; after 14 days two germinated; after 28 days all dead.

Lupinus polyphyllus: after 22 days, out of seven swollen seeds three germinated; seven others did not swell and were all dead; after 36 days' immersion one began to germinate and then died.

Lupinus luteus (pale *var.*): after 22 days 4/12 lived; after 36 days' immersion 3/18 germinated; after 50 days all dead.

**Lupinus pubescens* germinated after a month, but Mr. Berkeley says the greater number were rotten.

Mimosa sensitiva: germinated excellently after 36 days' immersion, and pretty well after 50 days.

Geum coccineum (*var. splendens*): after 36 days germinated well, and after 70 days one single seed germinated.

Saxifraga incurvifolia: did not germinate after 30 days' immersion.

——— *aizoides,* nor did this species, but the seed was not very good.

(Solanaceae)

Capsicum annuum: after 137 days' immersion, 30, out of 56 planted, germinated well (*).

Solanum tuberosum: germinated excellently after 70 days, well after 100; all dead after 120 days.

——— *lycopersicum* (common tomato): one seed germinated after 22 days' immersion, the rest were killed by 36 and 50 days' immersion. (*But Mr. Berkeley found that they germinated after a month.)

*——— *melongena.*

Convolvulus tricolor: after having been 7 days in the salt water, many of the seeds germinated, and the embryos came out of the husks: of those which did not germinate under water, one germinated after 36 days' immersion.

(Polemoniaceae and Hydrophyllaceae)

Gilia tricolor (*was killed by a month's immersion).

Phlox Drummondii: of old seed none germinated after 11 days; but of fresh seed, 3 out of many germinated after 15 days, and none after 25 days' immersion.

Eutoca viscida.
Nemophila insignis.
——— *atomaria.*
——— *maculata.*
——— *discoidalis.*

*None of these were found by Mr. Berkeley to germinate after a month's immersion.

Borago officinalis: a few came up after 14 days' immersion, one after 28 days, and none after 42 days.

**Nolana grandiflora.*

Satureja (common savory): after 42 days, 3 seeds out of many germinated.

Campanula Pentagonia (*did not germinate after a month's immersion).

**Fedia graciliflora.*

**Fedia* (corn salad).

(Compositae)

Lactuca sativa (common lettuce): after 56 days' immersion 7/20 of the seed came up; after 85 days only one out of several germinated. Cold water had no marked effect, but after 65 days they germinated rather better than the others (*).

**Cichorium Endivia.*

Galinsoga trilobata: germinated after 22 days.

Aster Chinensis (mixed German varieties): germinated after 28 days; all dead after 54 days' immersion.

Ageratum Mexicanum: after 100 days, one seed out of many germinated; at much shorter periods these seeds did not germinate well.

Leontodon Taraxacum: germinated excellently after 61 days' immersion; the seeds were fresh.

Tussilago Farfara: fresh seeds being placed in the salt water, after 9 days, many of them germinated under water. After 25 days, I took out some of the young plants and planted them: one grew. The germination of these seeds is the more remarkable, as this is not a sea-side plant.

**Monolopia Californica.*

**Cenia turbinata.*

**Cosmos luteus:* did not germinate after a month's immersion.

Clarkia pulchella: germinated well after 28 days; was killed by 54 days' immersion.

**Godetia rubicunda.*

*——— *Lindleyana* was killed by a month's immersion.

Apium graveolens (*var.* "Cattell's white"): after 137 days only 6 seeds out of some hundreds germinated; after 85 days the seeds germinated excellently; they did not appear to germinate quite so well after 82 days in the cold water (*).

Daucus carota: a very few germinated after 85 days; after only 56 days 3/30 grew (*).

1. [Read 6 May 1856.] *Journal of the Proceedings of the Linnean Society (Botany)* 1(1857):130–40.†

2. Alphonse de Candolle, *Géographie Botanique Raisonnée, ou, Exposition des faits Principaux et des lois Concernant la Distribution Géographique des Plantes de l'époque Actuelle*, 2 vols. (Paris: Masson, 1855).†

3. Sept. 1st, 1855.

4. May 26th and Nov. 24th, 1855.

5. *Sinapis:* a small, Old World genus, including the white mustard.†

6. *Convolvulus:* a genus of climbing herbs, for example, morning glory, bindweed.†

7. *Tussilago:* an asteraceous herb.†

8. Genus of ornamental flowers from western America.†

9. Stem develops inwardly; obsolete class of plants of Lindley; included monocotyledons.†

10. Stem grows by addition of rings on the outside; obsolete class of plants of Lindley; included dicotyledons.†

11. Rhubarb.†

12. Godron in his "Florula Juvenalis," p. 16, states that the seeds of some plants, as of *Atriplex* and certain *Gramineae*, germinate perfectly in salt-marshes, where they have been immersed during all the winter under salt water.

13. Mostly herbs, hollow internodes, stout stems, flowers in umbels.†

14. Loiseleur-Deslongchamps says (Consid. sur les Céréales, Part ii. p. 234) that in wheat put into water the embryo comes out in the course of two days; as Mr. Berkeley's wheat survived after 30 days' immersion in sea-water, one may suspect that in this case, the seed would survive longer under sea-water than under fresh water.

15. Waterleaf family; includes *Phacelia.*†

16. Family mostly of herbaceous plants with handsome flowers; includes *Phlox* and *Gilia.*†

17. John Lindley, *The Vegetable Kingdom: Or the Structure, Classification, and Uses of Plants, Illustrated upon the Natural System*, 3d ed. (London: Bradbury, Evans, 1853).†

Hybrid Dianths[1]

As you have noticed hybrid Dianths, you may like to hear that the summer before last I fertilised a poor single pale red Carnation with the pollen of a crimson Spanish Pink; and likewise a Spanish Pink with the pollen of the same Carnation. I got seed from both crosses in fair number; namely, 77 seed from two pods of the Spanish Pink, and raised

plenty of seedlings. In the eyes of a florist they would be, I presume, quite worthless from their straggling habit; but they were showy, and like most hybrids produced during a long time an extraordinary abundance of flowers. They varied somewhat in colour, but in no other respect; and one variety was of a really beautiful pale crimson. Taken in a mass there was no difference between the reciprocal crosses. Not one plant of either lot set a single seed. One plant came up identical with the Spanish Pink; no doubt owing to a few grains of the pollen of the Spanish Pink not having been removed; for Gaertner has shown that this is sometimes the result when a flower is fertilised with mixed pollen. I may add that Gaertner raised many hybrids between various species of Dianthus.

1. *Gardeners' Chronicle and Agricultural Gazette*, no. 10, 7 March 1857, p. 155.†

Mouse-coloured Breed of Ponies[1]

Mr. Charles Darwin asks if any of our readers will kindly inform him how dun or mouse-coloured ponies with a dark stripe down their back are bred. He says:—"The breed is common in Norway, on the banks of the Indus, and in the Malayan Archipelago; and in some respects very interesting in relation to the origin of the domestic horse. Is the peculiar colour thrown from ponies of any other colour, or must one or both parents be dun? Occasionally ponies of this colour have a cross stripe on the shoulder like that on the ass, and likewise bars on the legs. If any one who has bred ponies of this colour would inform me whether these stripes are more distinct in the colt than in aged ponies I should be much obliged. The transverse bars sometimes seen on the legs of the ass are said to be plainest during growth."

1. *Gardeners' Chronicle and Agricultural Gazette*, no. 24, 13 June 1857, p. 427.†

The Subject of Deep Wells[1]

The subject of deep wells has been sometimes discussed in your columns. I have a well 325 feet deep, and the 12-gallon bucket actually weighs 40 lbs. For many years I used a chain weighing 232 lbs; this, with the water, itself 96 lbs., amounts to 481 lbs.[2] I have made an enormous saving of labour by using for the last half year Newall's patent wire rope. Now will anyone have the charity to say from experience whether there could not be a great saving in the weight of the bucket. Would zinc, or gutta percha, or leather serve? The bucket must be strong enough to withstand

being occasionally dashed against the side of the well. Or must I stick to my old substantial oaken friend?

1. *Gardeners' Chronicle and Agricultural Gazette*, no. 30, 25 July 1857, p. 518.†
2. Darwin's figures actually add up to 368 pounds.†

Bees and Fertilisation of Kidney Beans[1]

Mr. Swayne[2] in the 5th volume of the Horticultural Transactions incidentally speaks of the advantage of artificially fertilising the early Bean. Can you tell me to what sort of Bean he refers? (We presume to the Early Mazagan; but we have no special information.)[3] and who has followed this plan, and how has it been effected? My motive for asking is as follows: every one who has looked at the flower of the Kidney Bean must have noticed in how curious a manner the pistil with its tubular keel-pistil curls like a French horn to the left side—the flower being viewed in front. Bees, owing to the greater ease with which they can reach the copious nectar from the left side, invariably stand on the left wing-petal; their weight and the effort of sucking depresses this petal, which, for its attachment to the keel-petal, causes the pistil to protrude. On the pistil beneath the stigma there is a brush of fine hairs, which when the pistil is moved backwards and forwards, sweeps the pollen already shed out of the tubular and curled keel-petal, and gradually pushes it on to the stigma. I have repeatedly tried this by gently moving the wing petals of a lately expanded flower. Hence the movement of the pistil indirectly caused by the bees would appear to aid in the fertilisation of the flower by its own pollen; but besides this, pollen from the other flowers of the Kidney Bean sometimes adheres to the right side of the head and body of the bees, and this can scarcely fail occasionally to be left on the humid stigma, quite close to which, on the left side, the bees invariably insert their proboscis. Believing that the brush on the pistil, its backward and forward curling movement, its protrusion on the left side, and the constant alighting of the bees on the same side, were not accidental coincidences, but were connected with, perhaps necessary to, the fertilisation of the flower, I examined the flowers just before their expansion. The pollen is then already shed; but from its position just beneath the stigma, and from its coherence, I doubt whether it could get on the stigma, without some movement of the wing petals; and I further doubt whether any movement, which the wind might cause, would suffice. I may add that all which I have here described occurs in a lesser degree with Lathyrus grandiflorus.[4] To test the agency of the bees, I put

on three occasions a few flowers within bottles and under gauze: half of these I left quite undisturbed; of the other half I daily moved the left wing-petal, exactly as a bee would have done whilst sucking. Not one of the undisturbed flowers set a pod, whereas the greater number (but not all) of those which I moved, and which were treated in no other respect differently, set fine pods with good seeds. I am aware that this little experiment ought to have been repeated many times; and I may be greatly mistaken, but my belief at present is, that if every bee in Britain were destroyed, we should not again see a pod on our Kidney Beans. These facts make me curious to know the meaning of Mr. Swayne's allusion to the good arising from the artificial fertilisation of early Beans. I am also astonished that the varieties of the Kidney Bean can be raised true when grown near each other. I should have expected that they would have been crossed by the bees bringing pollen from other varieties; and I should be infinitely obliged for any information on this head from any of your correspondents. As I have mentioned bees, a little fact which surprised me may be worth giving:—One day I saw for the first time several large humble-bees visiting my rows of the tall scarlet Kidney Bean; they were not sucking at the mouth of the flower, but cutting holes through the calyx, and thus extracting the nectar. I watched this with some attention, for though it is a common thing in many kinds of flowers to see humble-bees sucking through a hole already made, I have not very often seen them in the act of cutting. As these humble-bees had to cut a hole in almost every flower, it was clear that this was the first day on which they had visited my Kidney Beans. I had previously watched every day for some weeks, and often several times daily, the hive-bees, and had seen them always sucking at the mouth of the flower. And here comes the curious point: the very next day after the humble-bees had cut the holes, every single hive bee, without exception, instead of alighting on the left wing-petal, flew straight to the calyx and sucked through the cut hole; and so they continued to do for many following days. Now how did the hive-bees find out that the holes had been made? Instinct seems to be here out of the question, as the Kidney Bean is an exotic. The holes could scarcely be seen from any point, and not all from the mouth of the flower, where the hive-bees hitherto had invariably alighted. I doubt whether they were guided by a stronger odour of the nectar escaping through the cut holes; for I have found in the case of the little blue Lobelia, which is a prime favourite of the hive-bee, that cutting off the lower striped petals deceived them; they seem to think the mutilated flowers are withered, and they pass them over unnoticed. Hence I am strongly inclined to believe that the hive-bees saw the humble-bees at work, and well understanding what they were at, rationally took immediate advantage of the shorter path thus made to the nectar.

1. *Gardeners' Chronicle and Agricultural Gazette,* no. 43, 24 October 1857, p. 725.†
2. George Swayne, "On Fertilizing the Blossoms of Pear Trees," *Transactions of the Horticultural Society of London* 5(1824):208–13.†
3. Parenthetical phrase by editor of the *Gardeners' Chronicle.*†
4. *Lathyrus:* sweet peas.†

Productiveness of Foreign Seed[1]

Will the writer of the highly remarkable article on weeds in your last Number have the kindness to state why he supposes that "there is too much reason to believe that foreign seed of an indigenous species is often more prolific than that grown at home?"[2] Is it meant that the plant produced from the foreign seed actually produces more seed, or merely that the introduced stock is more vigorous than the native stock? I have no doubt that so acute an observer has some good reason for his belief. The point seems to me of considerable interest in regard to the great battle for life which is perpetually going on all around us. The great American botanist, Dr. Asa Gray, believes that in the United States there are several plants now naturalised in abundance from imported seed, which are likewise indigenous; and my impression is (but writing from home I cannot refer to his letter to me) that the imported stock prevails over the aboriginal. So again, Dr. Hooker in his admirable Flora of New Zealand[3] has told us that the common Sonchus has spread extensively from imported seed, whilst the same species is likewise an aboriginal; the natives in this instance being able from trifling differences to distinguish the two stocks. Might I further ask whether it is now some years since the seed of Sinapis nigra was accidentally introduced on the farm described; and if so, whether the common Charlock still remains in lessened numbers owing to the presence of the invader, and without, as far as known, fresh seed of the invading S. nigra having been introduced?—whether, in short, it was a fair fight between the two species, ending in the victory of the Black Mustard? Would it be trespassing too much on the kindness of the writer of the article to ask whether he knows of any other analogous cases of a weed introduced from other land beating out, to a greater or lesser extent, a weed previously common in any particular field or farm?

1. *Gardeners' Chronicle and Agricultural Gazette,* no. 46, 14 November 1857, p. 779.†
2. See *Gardeners' Chronicle,* 7 November 1857, pp. 761–62, and 31 October 1857, p. 746, regarding a paper by Mr. Wood on weeds presented to the Croydon Farmers' Club.†
3. Joseph Dalton Hooker, *Handbook of the New Zealand Flora: A Systematic Description of the Native Plants of New Zealand and the Chatham, Kermadec's, Lord Auckland's, Campbell's, and Macquarrie's Islands,* 2 vols. (London: Reeve, 1864–67, p. 165).†