NO. XXVII.—ON THE ALLEGED PROOFS OF SUBMERGENCE IN
SCOTLAND DURING THE GLACIAL EPOCH.—I. CHAPELHALL,
NEAR AIRDRIE. By DUGALD BELL, F.G.S. With a Map.

[Read 10th March, 1892.]

ONE of the most interesting questions in recent geology relates to the degree of submergence which took place in Britain during the Glacial epoch. That some submergence did occur is admitted, and seems clearly proved; but *how much*, has been matter of prolonged discussion and difference of opinion among geologists. Till recently, a submergence of from 1500 to 2000 feet was freely affirmed, with regard to parts, at least, of the British Isles. This affirmation rested on certain "high-level shelly gravels" found in Wales and on the borders of Wales, and on one or two Irish mountains. It was assumed, of course, that these shelly gravels, as well as similar gravels without shells extending several hundred feet higher, had all been deposited at these heights by the sea. Gradually, however, this conclusion came to be doubted. First, the shell-less gravels were discarded as devoid of proof—the mere fact of their being gravels, more or less evenly stratified, being far from conclusive as to their being laid down by the sea. The shelly gravels themselves were next called in question, and were found to present so many doubtful features—in their detached and sporadic character, in their being largely made up of far-travelled and striated materials, and in the mixed and broken nature of their shelly contents—that the conviction grew up that they were not really in place, but had been transported somehow, probably by ice, from a former bed of the sea at a lower level. As similar instances of the transport of marine organisms by ice became known in increasing numbers this conviction strengthened, and it was corroborated by observations on the glaciation of the surrounding neighbourhood and of the country generally, as well as by comparison with what is known regarding the glaciation of other parts of the northern hemisphere. The researches and observations of Belt, Goodchild, Clement Reid, Carvill Lewis, and others,* have tended powerfully to establish the conclusion that these "high-level shelly drifts" are really of glacial origin,

* *Nature*, May, 1874; *Geol. Mag.*, 1874, p. 496; *Ib.*, 1881, p. 235; *Reports, Brit. Assoc.*, 1886-87; *Transactions*, vol. ix., part 1, p. 100.

SKETCH MAP OF DISTRICT, GLASGOW TO CHAPELHALL.

Figures indicate feet above sea-level. Dotted line = 500 feet. Arrows indicate direction of ice-flow.

so far as their present position is concerned. Hardly any geologist of mark now stands up for this "great submergence." Here and there, it is true, some doughty champion, unaware apparently that the position has been abandoned, steps bravely forward to defend it. But, speaking generally, we may say that this "great submergence" is a day-dream of the past, and "lives no longer in the faith of reason."

But there are other degrees of submergence—minor, yet very considerable—usually admitted and taught by geologists, which we still venture to think are far from being satisfactorily proved.

The first of these is the submergence of between 500 and 600 feet supposed to be indicated by a bed of shelly clay recorded to have been found at Chapelhall, near Airdrie, about 11 miles east of Glasgow. There is certainly a great difference between a submergence of 1360 and one of 526 feet; and it is quite possible there may be good reasons for doubting or disbelieving in the one which do not apply to the other. But it appears to us that even this second or much smaller submergence is by no means clearly made out.

I. *Accounts imperfect and doubtful.*—Our first remark on this instance at Chapelhall is that very little is really known of it—far too little to found so much upon it as has been done. May we ask attention to a brief narrative of what has been recorded regarding it?

(A) This "deposit" was first brought into notice by Mr. Smith of Jordanhill, in a paper read to the Geological Society in 1850, and re-published in his well-known little volume of "Researches in Newer Pliocene and Post-Tertiary Geology," in 1862. "The shells," Mr. Smith states, "were discovered by Mr. James Russell, an operative miner, in digging a well." Mr. Russell informed Mr. Smith that "at the depth of 14 feet from the surface he came to a bed of brick clay containing the shells." The surface of the ground here Mr. Smith found to be 524 feet, and, therefore, the shelly clay was 510 feet above the sea. At the time of his visit Mr. Smith saw part of the "superincumbent matter which had been thrown out, lying at the mouth of the well;" and this, he says, was "unquestionably true till," and contained, among other stones, "a small granite boulder," which must have been brought from a distance of about 60 miles to the N.W. He adds, "Mr.

Russell states that after passing through the shelly bed of brick clay he came again to the till"; proving that there had been "more than one deposition of the till or Boulder-clay."

It does not appear, then, that Mr. Smith either saw the shelly clay or took the shells from it himself; the latter he seems to have got either from Mr. Russell, or from Mr. John Craig, a mining engineer, whom Mr. Russell had first seen, and who informed Mr. Smith of the "deposit," and accompanied him to the locality. These shells—their number is not stated—seem all to have been of one species, *Tellina calcarea* (or *T. proxima*)—"an Arctic species," remarks Mr. Smith, "extremely abundant in the Clyde Pleistocene beds." The deposit was accordingly described by Mr. Smith as "a bed of the *Tellina*."

This is really all that can be gathered from Mr. Smith's paper. It rests throughout on the authority of Mr. Russell.*

(B) Sir Archibald Geikie, in the preparation of his valuable Memoir on "The Phenomena of the Glacial Drift of Scotland," † visited the spot, also under Mr. Russell's guidance. He describes it as situated "on the crest of a ridge which, rising high above the surrounding country, commands an extensive view across the lower part of the basin of the Clyde. On the water-shed of this high-lying ridge a well was sunk some years ago, and while the excavations were in progress the shells were found." Sir A. Geikie then gives particulars corresponding with those stated by Mr. Smith, adding that the lower till was here "about 24 feet thick, and lay directly on the Carboniferous strata of the district. The brick clay, at its thickest part," he continues, "measured 2 feet 1 inch in depth, but thinned away rapidly on every side so as to allow the

* "Researches," pp. 139-142. Of course in our remarks we do not wish to cast the slightest doubt on Mr. Russell's testimony as to the fact of the shells being found where he stated; all we say is that he does not seem to have examined the clay very minutely—certainly not with the care and minuteness now considered desirable, in view of the many questions which have arisen regarding these shelly clays; and that, so far as appears, neither Mr. Smith nor any other geologist had an opportunity of doing so personally. It is incredible that Mr. Smith, if he had seen the clay itself, should not have said so, and only mentioned the "superincumbent matter"; incredible also that, if he had had the opportunity, he should not have examined the clay for other organisms besides the *Tellina*, and stated that he had done so.

† *Transactions*, vol. i., part 2, 1863.

upper and lower till to come together. From a number of additional wells, sunk on purpose, Mr. Russell ascertained that the clay lay in a hollow of the undermost till, and that this hollow measured about 19 feet long by about 5 feet broad. Pits which were dug beyond the boundary of this little trough showed a great depth of the usual till, but without a trace of brick clay. The shells," adds Sir Archibald, "consisted entirely, I believe, of *Tellina proxima*. Usually the specimens were broken, but a good many were taken out entire, with both valves together."*

Sir Archibald thus adds several particulars to Mr. Smith's account; but, excepting his observations on the surface, which are interesting and valuable, these particulars are still all derived from Mr. Russell. The well was built up; the clay was not visible; but all that could be learned regarding it confirmed its local and limited character.

(C) About the same time as Sir A. Geikie's visit, or apparently shortly before it (though their account was later in being published), Dr. Crosskey and the late Dr. Bryce visited the locality and made some observations, the results of which were communicated by Dr. Crosskey in a paper to the Geological Society in January, 1865.† Dr. Crosskey alluded to the perplexing nature of the deposit as occurring "intercalated between two masses of Boulder-clay," and his more immediate object was to examine "whether the superincumbent matter was without doubt the true till," as Mr. Smith had pronounced it to be. He thought it was not, but that the upper clay might have been "the wash from a ridge of true Boulder-clay which rises above it, and against the slope of which it rests." This view, however, seems to be untenable. But, leaving this in the meantime, we note that at Dr. Crosskey's and Dr. Bryce's suggestion Mr. Russell "sank a fresh well, 7 yards from the old one," the section in which was entirely different, and showed *no* shelly clay.

OLD WELL.		NEW WELL.	
Upper Boulder-clay, - -	14 ft.,	-	5 ft. 3 in.
Clay with Shells, - -	2 ft.		
Boulder-clay (thickness not stated), -			9 ft. 6 in.
(not pierced through.)			

We may take it that this well was sunk before Sir A. Geikie's

* *Op. cit.*, pp. 58-9.

† *Quarterly Journal of the Geological Society*, vol. xxi.

visit; for, by that time, the information was that "a number of wells" had been sunk, and that the shelly clay had been ascertained to be "about 19 feet long by 5 broad." Of course, had this been known when Drs. Bryce and Crosskey were on the spot, there was no use in sinking a new well "7 yards from the old one."

The thickness of the lower Boulder-clay, also, which was quite indefinite at the time of Dr. Crosskey's visit, seems by the time of Sir A. Geikie's to have been ascertained to be "about 24 feet thick."

The only point on which Dr. Crosskey's account goes beyond either Mr. Smith's or Sir A. Geikie's, is in stating that besides the *Tellina calcarea*, "*Cyprina Islandica*, and a large *Balanus* are found in the clay." In one sense the occurrence or non-occurrence of these organisms in the clay is of no great consequence, as the *Tellina* by itself is sufficient to mark the deposit as glacial. But it is a curious point how, about fourteen years after Mr. Smith's account was first written, and a year or two after it was republished in book form, these additions came to be made to the organic contents of the clay. The well does not appear to have been re-opened in the interval; none of the clay seems to have been retained for further examination; new wells were sunk in vain to find it again, and not one of the observers we have been quoting from states he ever saw it. Whence, then, came the *Cyprina Islandica* and the *Balanus*? *

* We understand Dr. Crosskey received the particulars given in his paper from Mr. Russell. How, then, was Mr. Russell able to say so much more about the clay to Dr. Crosskey than he had said to Mr. Smith? As already said, we make no imputation against Mr. Russell's good faith and general accuracy; but mistakes will sometimes happen; and it is, at least, a coincidence that Mr. Smith in his paper (which, at the date of Dr. Crosskey's visit, had lately been republished), a few lines after describing the Chapelhall till, goes on to speak of organic remains sometimes found in the till generally, and adds, "upon one occasion I found broken and water-worn fragments of shells irregularly dispersed in it, amongst which I recognised the massive hinge of *Cyprina Islandica*, and the stem of a large *Balanus*" ("Researches," p. 141). A hasty, or not very exact, or, for the moment, inattentive reader, might make the mistake of supposing that here Mr. Smith was still referring to the Chapelhall clay, instead of the till in general; though, of course, Mr. Russell would have less excuse than most men in making such a mistake, seeing it was he who first supplied Mr. Smith with his information. In whatsoever way these additions to the fauna of the Chapelhall clay came to be made, they are to be found repeated, generally on Dr. Crosskey's authority, by various writers down to the present time (see, for example, Dr. James Geikie's "Great Ice Age," p. 163).

We have entered into these particulars simply to show how imperfect and unsatisfactory, in many respects, is all we know of this Chapelhall deposit.*

From our own observation we can corroborate Sir A. Geikie's description of the locality. The well is situated at the upper end of the village of Chapelhall, behind the cottage once occupied by Mr. Russell, who died about twelve years ago. Its position is on one of the highest of those long ridges of Boulder-clay which abound all over the district, extending approximately in a direction from N.W. to S.E., and it is not far from the summit of the ridge. It seems impossible, therefore, that the upper till could be a "wash down" from a higher ridge of "true Boulder-clay," as Dr. Crosskey suggested. Had it been in the hollow between two such ridges, this would have been more probable. But the fact is as stated, its position is high, on the very back or crest of the ridge, conspicuous all round; and there is not the slightest evidence of any "slip" of the Boulder-clay near the spot.

VIEW OF CHAPELHALL.

* Cottage with Well.

But we need not dwell longer on this, seeing that the upper is generally acknowledged to be as true till or Boulder-clay as the lower.

At present the well is about 16 feet deep (it was originally 2 feet deeper, but has been filled in at the bottom); and, as might be expected from its situation, the supply of water is not very copious, and in a continuance of dry weather almost ceases, or falls very low. At such a season, if the well were run dry, an effort might be made to reach the "shelly clay" again, and bring

* In a note received lately from the veteran geologist, Mr. David Robertson, he says, "It is a remarkable fact that no other shells were found associated with the *Tellina* in the Chapelhall deposit. I have never found it without finding others with it, and often it was far outnumbered by other species."

up a portion of it for further examination.* This is, of course, desirable, seeing that so little is known of it; that though mentioned in almost every geological work for the last forty years, it would seem that no geologist since Mr. Russell's day has set eyes upon it; and that though carefully searched for all round the neighbourhood—in railway cuttings, shafts, quarries, and other openings,—it has not been met with since Mr. Russell dug his well, and built it round, forty-two years ago.

II. *Difficulties in regard to it and other High-level Deposits.*—But even supposing the clay were found again in the same spot, however satisfactory this might be in some respects, it could hardly alter, or add to, the fact that here, at the very utmost, we have a limited patch or strip of shelly clay imbedded in the till or Boulder-clay at this height above the sea. The question then arises, Is this sufficient to sustain the immense pile of inferences that has been built upon it—first, a period of extreme glaciation, when “the great central valley of Scotland brimmed with glacier ice;” then a period of “milder conditions,” and a submergence of the land to a depth of between 500 and 600 feet beneath its present level; then a re-elevation of the land, and “another mighty ice-sheet again overflowing it?”† Restricting ourselves at present to the submergence, we ask, Is this patch of shelly clay sufficient to prove a submergence of between 500 and 600 feet? Some have thought so. “Evidently,” says one esteemed writer, “the shelly clay at that place marks an old sea-bottom, when the sea rose not less (indeed, certainly more) than 526 feet above its present level.”‡ We venture to think it is not sufficient without further confirmatory evidence, which, if the conclusion were true, might reasonably be looked for, but of which there is a very striking lack and deficiency; also that, as in the case of Moel Tryfaen, the theory of a purely marine origin for the deposit entails more and greater difficulties than it solves.

(1) *Its Limited Extent.*—If this Chapelhall clay were in the true sense a shell-bed, marking the former level of the sea, might we not expect to find remains of it in many localities all over the district and neighbourhood, and in other parts of the country as well as here?

* We are in hopes of this being done.

† “Great Ice Age,” pp. 175-6.

‡ *Ib.*, p. 175.

Dr. J. Geikie justly points out that "when a free ocean flowed over the site of the district in question"—supposing it did so—"a very large part of the Scottish lowlands would be submerged. The sea would wash the base of the Kilsyth hills on the north, and towards the south would extend up the Clyde valley as far at least as Hazelbank, a few miles below Lanark."*

In short, the sea would rise to a considerable height along the Campsie and Kilsyth hills on the one hand, and the Ayrshire, Renfrewshire, and Lanarkshire hills on the other; as well as along every range of hills all over the kingdom, unless the submergence were restricted to this district, which is not maintained.

Now, if there was such a submergence, the reason commonly assigned for the paucity and disappearance of all traces of it is the "clearing out" effected by the "second" or subsequent glaciation. But, as we have tried to show elsewhere,† this is a most inadequate reason, and might account for the absence of such traces in a few localities, but not for their disappearance all over the country. There are special features in this district which make it extremely unlikely. These hills to which we have referred as bounding the midland valley are characterised by numerous bends and inlets, and are intersected by transverse valleys, in which marine deposits would be most likely to accumulate, and where they would most likely be preserved. As Dr. Geikie well states, when referring to the beds of sand and gravel—old stream-deposits—often found intercalated in the Boulder-clay, "During the glacial period, the ice-sheet which followed the lines of the principal valleys must frequently have crossed the lateral and subsidiary valleys nearly at right angles. In the main valleys the glacier would exert its full influence, but it would not be able to do so in the narrow lateral valleys and ravines; the ice and till would merely topple into the glens referred to and gradually choke them up, and the main mass of the glacier would then pass on over the whole. In such narrow glens, then, any silt, sand, or gravel that had gathered during the temporary absence of the ice-sheet would not be so likely to be ploughed out when the ice returned as the similar materials which had accumulated in those broader and more open valleys where the ice would have full freedom to move and exert its erosive power."‡

* *Op. cit.*, p. 175.   † *Transactions*, vol. ix., part 1, p. 100.

‡ "Great Ice Age," pp. 133-4.

Why, then, if there was a submergence to the extent alleged, are such high-level shell-beds not found in scores of the lateral glens, valleys, and ravines which were comparatively sheltered from "the erosive power of the old glaciers," and all evidence of such submergence left, not in a sheltered position, but in a most unlikely place—on the summit of a high exposed knoll or ridge "nearly in the centre of that broad tract of lowland country" which it is admitted was swept by the old glaciers, and formed one of their main pathways and outlets? That is one difficulty.

It may be objected that this is an argument from a negative, which is always inconclusive. But in one sense there is a positive element in it—in a part of the country so much cut up and dug into, if such traces have not been found, we may safely conclude it is because they are not there.

(2) *Absence of Marine Organisms in the Overlying Deposits.*—Another difficulty is, if the "second glaciation" ploughed up and removed the shell-beds formed during the submergence—as it must have done to a great extent—why do we not find remains of these scattered throughout the upper Boulder-clay? We have instances of this in the "shelly tills" that are found in various parts of the kingdom, and in other parts of the world—wherever the ice-sheet is known to have passed over what had been a sea-bottom.

When the prolonged period of submergence, during which the broad midland valley was an ocean sound, passed away, and the "mighty ice-sheet again overflowed the land," should we not find abundant traces of the sea incorporated with the overlying Boulder-clay, its latest work? "During the last cold period," says Dr. Geikie, "the confluent glaciers cleared out the marine deposits laid down in preceding interglacial times, and incorporated them with their *moraine profonde*. This is the origin of most [all?] of our shelly tills or Boulder-clays."*

Again, "We know that the latest ice-sheet which overflowed Scotland did enter the sea and mix up marine deposits with its moraine. Thus it was that the silt and shells lying on the bed of the Minch were pushed up over the north end of Lewis, and so also was it that Caithness received the spoils of the Moray Firth."† Why, then, is this "upper Boulder-clay" of Lanarkshire—the ice

* *Op. cit.*, p. 329.

† *Ib.*, p. 390.

forming which must have passed over miles and miles of former sea-bottom (if the alleged submergence were a fact)—why is it not charged with marine remains?

On this point we cannot do better than quote Dr. Geikie's own words, "While there is proof to show that immediately before the appearance of the last ice-sheet, Scotland was submerged to a depth of more than 500 feet, yet the upper Boulder-clay of that country rarely contains broken shells, save in maritime districts. Why, we may ask, does not the upper Boulder-clay of the Central Lowlands (a great proportion of which must have been submerged before the last ice-sheet overflowed Scotland) contain broken shells, and afford other indications of the latest confluent glaciers having occupied the bed of the sea?"

That is the question, as Dr. Geikie well puts it. What is the reply? "That is a very hard question to answer, and I fear we must wait some time yet for an adequate solution of the difficulty."* We think it will be admitted that this is not satisfactory. If we adopt a theory, and find that it lands us in an inexplicable difficulty, to say simply that "we must wait some time yet for a solution" is surely very unsatisfactory, and should lead us at least to re-examine the theory, or inquire whether we are really shut up to its adoption.

(3) *Highly Arctic character of Shells found in the Clay.*—Another difficulty presents itself. It has been supposed that these shells were deposited in a "mild interglacial period"—in one of such periods when "the great ice-sheet disappeared from the low grounds and shrank into a series of local glaciers among the mountain regions;" nay, "which there is no evidence to show might not have been warm enough at times to cause all the ice and snow to disappear from the country."† Dr. Geikie, for example, dwells on this point, impressing upon the minds of his readers the long periods implied in such great climatic changes, and stating that it seems "not unreasonable to infer that the comparatively mild and genial periods, of which the interglacial beds are memorials, may have endured as long as these Arctic or glacial conditions which preceded and followed them."‡ We must not enter here upon the discussion of these so-called "mild and genial interglacial periods." We only remark that if such a submergence did take

* "Great Ice Age," p. 390.   † *Ib.*, pp. 155, 157.   ‡ *Ib.*, p. 158.

place, it would certainly, so far as we can see, be accompanied by a great amelioration of the climate. It seems clear that the greater or less height of the land would tell powerfully on the climatal conditions by increasing or diminishing the extent of high land, and by shutting off, or more freely admitting, the ocean currents, which are the great distributors of warmth and equalisers of temperature. In short, there seems no reason to doubt that any submergence of the land is, *pro tanto*, in favour of a mild and equable climate.*

Now, we should naturally expect that the organic remains found in deposits laid down during these "long, mild, and genial" interglacial periods—especially if accompanied by physical changes favourable to the maintenance and diffusion of such mild conditions—would indicate more or less clearly the great change of temperature which had taken place, and would present a distinctly more southern *facies* than those which had been laid down under colder conditions. I need not remind my present audience of the close connection there is between temperature and species, and that this is especially the case with regard to the mollusca. Some species are so intolerant of cold that they barely touch the southern coast of Britain; and others are so averse to warmth that they scarcely reach our northern shores,—so nice is their dependence on conditions of temperature, and their restriction to certain climatal areas.† Shells have thus been taken to be most sensitive and accurate indicators of the prevailing temperature. In illustration of this, several geological writers have shown that some shell-beds on our eastern coast indicate colder conditions than those of the Clyde valley; and they account for this by saying that "the North Sea, cumbered with ice from Scandinavia as well as from Scotland itself, would in all probability have a lower temperature than the freer ocean"—though it was certainly far from being free of ice—"that washed the western sea-board." If "therefore it is not surprising that this difference of temperature should have taken effect and influenced the distribution of the fauna,"‡—much more,

* Those geologists, however, who most favour submergences (for they often suppose *several* of them) don't always observe this connection in their scheme of events; they frequently put the milder condition and the retreat of the ice *first*, and *then*, after a considerable interval it may be, the submergence.—"Great Ice Age," pp. 184, 329, 397, 410, 563.

† See, *e.g.*, Miller's "Sketch-Book of Popular Geology," pp. 329, 330.

‡ "Great Ice Age," p. 265, note.

as it seems to us, should we expect this difference to appear in beds deposited during a long, mild interglacial period, when all the land of the country was lowered by 500 or 600 feet, and when a broad ocean sound filled the central valley.

What, then, is the nature of these shells found on this supposed sea-bottom near Airdrie, laid down at the very climax of such an interglacial period? Keeping by all we know—the *Tellina calcarea*—it is one of the most distinctively Arctic shells found anywhere in our glacial deposits, a shell marking extremely cold conditions, “not yet found living in the British seas, but abundant on the coasts of Greenland, boreal America, &c.” (Miller), “living in the Arctic seas, not known as a living British shell” (A. Geikie). Is there really any value in shells as indicators of the prevailing temperature in any formation, if a “bed” of this *Tellina calcarea* is to be taken as marking a “mild interglacial period?”*

So long as anything like the present distribution of sea and land prevailed (and it is not contended that there was any material

* Dr. James Geikie alludes to this difficulty. He says, “I have spoken of these shell-beds [Chapelhall and others] as interglacial deposits, although their fossils indicate the pressure of a much colder sea than now flows round the British coast, simply because they are links in the chain of evidence which goes to show that the intense cold of the glacial epoch was interrupted by more or less prolonged intervals, during which milder conditions of climate prevailed.” We confess ourselves unable to see how this Chapelhall bed, of strongly Arctic character, though deposited during a time of great submergence, can in any way serve to show the prevalence of “milder conditions of climate.” Its evidence seems to be quite the other way. Dr. Geikie continues, “A very little consideration will suffice to convince us that, however cold the climate may then have been, Scotland could not by any possibility have presented such an intensely Arctic aspect as when the ice-sheet was at its thickest” (“Great Ice Age,” p. 175). But was not this simply because there was less of it left to present any aspect at all? Judging from what is known of the habitats of this shell, the lands adjoining the seas in which it lived would be covered with ice and snow, and the seas themselves almost constantly laden with floating ice. And if this was the case in our country, even when it was submerged to a depth of between 500 and 600 feet, we fail to see the proof of an amelioration of climate.

[We trust our numerous references to Dr. Geikie’s work will be excused, as it is the leading work on the subject. But fifteen years is a long time in Glacial geology (we mean in the study of it) and the numerous points of divergence that are now cropping up suggest whether it is not time for a *third* and *revised* edition?]

difference in that respect during the Glacial epoch) there must always have been more or less of a warm ocean current from the S.W., reaching the shores of northern Europe in exchange for the cold current from the N.E., flowing southern along the shores of North America. This is in accordance with the great system of atmospheric and oceanic circulation, which again is a result of the greater heat of the tropics, and of the earth's rotation on its axis.*

It seems incredible, therefore, that there should be a submergence of the British Isles to the amount of between 500 and 600 feet—all the high grounds lowered and circumscribed to that extent, and the sea so much more freely intersecting the land—without a far greater amelioration of climate than is indicated by these Chapelhall shells. In other words, the nature of the organisms seem to be a further difficulty in the way of believing that this clay was actually deposited by the sea during a period of submergence. And these remarks, of course, apply to all so-called "high-level Arctic shell-beds." The higher the level, the greater the double improbability of their being (a) true "shell-beds," and (b) of Arctic species.†

* Nay, so similar in some respects to those now existing were the conditions during the Glacial epoch—the distribution of sea and land, the general configuration of the surface, and the prevailings winds and currents—that the areas of maximum precipitation seem to have been virtually the same as at present,—so that "the marks of ancient glaciers are most decided where the rainfall is now heaviest" (Jamieson, *Quar. Jour. Geol. Soc.*, 1874).

† Since this was read we have been reminded of the following passage in a paper by Sir William Thomson (Lord Kelvin) on "Geological Climate":—"A thousand feet of depression would submerge the continents of Europe, Asia, and America for thousands of miles from their present northern coast-lines; and would give, instead of the present land-locked and therefore ice-bound Arctic sea, an open iceless ocean, with only a number of small steep islands to obstruct the free interchange of water between the North Pole and temperate and tropical regions. . . . Now what must be the climate of a small island in an open iceless circumpolar sea? Temperate and quite free from frost except in hollows, we may, I think, safely answer" (*Transactions*, vol. v., part 2).

Of course, what his lordship here says of a depression of 1000 feet (which was taken simply by way of illustration) is true also in measure of a depression of 500 feet. Indeed, it is to be remembered that the principal change in the extent and contour of the land would be caused by the submergence of the *first* 500 feet.—We have great pleasure in citing this passage as strongly corroborative of the argument just stated.

(4) *Was the Submergence caused by Subsidence?*—These observations appear to hold good whatever theory we adopt as to the cause of the supposed submergence; and each theory as to that cause, we shall find, is attended with special difficulties of its own. Take the first and most obvious account of it, that there was an actual subsidence of the land, or depression of the earth's crust to this extent, in this and neighbouring parts of the northern hemisphere. This was the theory adopted by Sir Charles Lyell and all the earlier writers on the subject. Proceeding chiefly upon the changes of level that have been observed in Scandinavia (though there is no proof that these go beyond very narrow limits), Sir Charles supposed a gradual subsidence at the rate of about $2\frac{1}{2}$ feet in a century, and that this might go on for nearly 100,000 years, till there was a submergence of over 2000 feet.* And, of course, this must have been followed by a re-emergence, in all probability equally gradual, and occupying a similarly long period of time. Passing over the improbability of such great oscillations of the earth's crust happening within comparatively recent times—while all through the Secondary formations and down to the later Tertiary deposits, the changes of level, as Sir Andrew Ramsay has shown, seem to have been comparatively insignificant†—passing also the improbability of such a great submergence (occupying this immense interval of time), leaving only such few and doubtful traces behind it; and merely noting that there is no proof of anything like such a submergence in other parts of the northern hemisphere,‡—let us take the much smaller submergence of between 500 and 600 feet, which is at present under our consideration. At the assumed rate of movement, a submergence of 550 feet would occupy 22,000 years, and the re-emergence an equal period, or 44,000 years in all—allowing the movement to have been constant and uninterrupted by any periods of repose. There seems to be ample time here—especially under the conditions supposed—for the accumulation and redistribution of considerable bodies of sediment, and, in short, for changes of no small extent in the aspect and physical geography of the country. Now, the theory is, that the re-elevation of the

* “Antiquity of Man,” 3rd ed., chap. xiv.

† “Physical History of the Dee,” *Quar. Jour. Geol. Soc.*, vol. xxxii.

‡ See paper already referred to, *Transactions*, vol. ix., part 1, p. 100.

land was followed by a return of Arctic conditions, and a second period of glaciation. And it is to be noted that this second glaciation seems to have been very much a repetition or duplicate of the first. It is true that several authors speak of it as "milder," "less intense," "on a smaller scale," than the first; but when, according to their own showing, the Firth of Clyde again became so choked with ice that a wide-spreading sheet of it found its only available outlet across the midland valley to the German Ocean; when the Irish Sea was again filled from side to side with ice from the Southern Uplands, and the Cumberland and Irish mountains; when the ice from the N.W. Highlands again extended across the Minch, and over the northern part of the Island of Lewis; and when the British and Scandinavian ice once more coalesced in the German Ocean*—it seems a waste of words to say that this second glaciation was appreciably less than the first.

But if it be hard to account for a subsidence or depression of the earth's crust at that particular time, and equally hard to account for its subsequent re-elevation, it seems to add very much to the difficulty that, after these great changes, occupying several tens or scores of thousands of years, the land should be supposed so nearly to resume its former aspect; that the temperature and all the climatal conditions,—the configuration of the country, the height of the mountains, and the direction and inclination of the valleys,—everything should be so nearly the same over again as to produce that highly complex result, a second glaciation, in all respects so closely resembling the first.

It is this consideration which has led many eminent geologists, both in Europe and in America, to deny a second distinct period of glaciation altogether—believing rather in the oneness of the Glacial epoch, with, of course, considerable oscillations of the ice, than in two or more distinct periods of glaciation, distinct and yet so nearly alike.†

(5) *Was the Submergence caused by a Rise in the Sea-level?*—According to the other way of accounting for the submergence, "the sea may rise upon the land, without any movement of the land itself." As Mr. Adhémar and Dr. Croll pointed out, a great

* "Great Ice Age," pp. 159, 562, 564.

† Falsan, "*La Période Glaciare*," p. 211, &c. Wright, "*Ice Age in North America*," pp. 500-1.

mass of ice in either hemisphere such as that which covered so many northern regions during the Glacial epoch would, of itself, by displacing to some extent the earth's centre of gravity, cause a rise of the sea in the hemisphere where the ice was in force. To what extent this would take place has been matter of considerable discussion and difference of opinion. But Dr. Croll's calculation was that the transference of the "ice-cap" (as it was then called) from the Antarctic to the Arctic regions might produce "a rise of the sea-level at the North Pole of 285 feet, and in the latitude of Edinburgh of 234 feet."* Dr. Croll thought there might be a considerable additional rise, resulting from the melted ice—assuming that the ice would melt on the one hemisphere more rapidly than it would form on the other—which seems to be doubtful, and may therefore be set aside. Here then, as Dr. Geikie remarks, we have "a *vera causa* of changes in the relative level of land and sea; and certainly it is remarkable that evidences of submergence should so often accompany marks of strong glaciation, as if the one were somehow dependent upon the other."† Let it be borne in mind, too, that this explains not only the submergence and its coincidence with the Glacial period, but what is equally important and equally difficult to explain on any other hypothesis, the re-emergence of the land when the ice-sheet diminished or passed away. So that it seems in all respects a more probable hypothesis than actual depressions and re-elevations of the earth's crust, happening, in some inexplicable way, at that particular time.

But if we adopt this as in all likelihood the cause of the submergence, then there must be some proportion between the cause and the effect. It is surely inconsistent to premise submergences of 1400 to 2000 feet, bringing in "actual subsidences of the land itself" to account for these, and leaving the more moderate ones to be accounted for by the ice-sheet alone.

Even as regards a submergence of between 500 and 600 feet in this country, it seems to be more than we are warranted in attributing to this cause. We have seen that Dr. Croll—who took larger data as to the mass of the ice than most physicists will allow—only estimated a rise of the sea from this cause, in the

* "Great Ice Age," p. 487.

† *Ib.*, p. 488.

latitude of Edinburgh, at 234 feet. In North America, where an area of about 4,000,000 square miles was covered by ice (more than double the area so covered in Europe) with an average thickness, at its maximum, of a half or two-thirds of a mile—yet with this enormous mass of ice the American geologists compute that portions of the North Atlantic, and of the Arctic sea, would only be lifted “perhaps, on an average, two or three hundred feet higher than now.”* And a most interesting point is that the traces of submergence increase to a certain point northwards, as might be expected if due to this cause. Thus, near Boston, and north-east to Cape Ann, the coast seems to have been submerged to a slight depth, probably not exceeding 10 to 25 feet. Proceeding towards the north and north-west, the elevation of the marine beds lying on the glacial drift increases to about 225 feet in Maine, about 520 feet in the St. Lawrence valley at Montreal, and 440 feet at a distance of 130 miles W.S.W. of Montreal; but eastward, along the St. Lawrence, it decreases to 375 feet opposite the Saguenay, and does not exceed 250 feet in the basin of the Bay of Chaleurs. Along the east main coast of Hudson Bay, and on Hudson Strait, and in the region S.W. from James Bay, the submergence indicated is about 300 feet. On the other side of the continent, in British Columbia and Queen Charlotte Islands, the evidence points to a submergence of only between 200 and 300 feet, which corresponds with the smaller mass of the ice in that direction.†

If these are the figures for the North American continent, with its enormous ice-sheets during the Glacial period; and if in Scandinavia, where there is evidence that the ice attained to at least double the dimensions which it had in this country, the greatest submergence appears to have been barely 600 feet—(and some geologists doubt if it was so much)—it is difficult to believe that in this country, with a far smaller and more limited ice-sheet, the submergence (if due to this cause) should be equal to, or even exceed, the maximum shown by these other countries. Let us try a little simple calculation on this point.

* Warren Upham; see “Ice Age in North America,” p. 579. “Among American geologists the belief in a great submergence of the land in late glacial and post-glacial times is passing away” (“Great Ice Age,” p. 480).

† “Ice Age in North America,” pp. 580-1, &c.

In America, as has been remarked, the phenomena are "writ large," they are presented with great clearness, and on an immense scale. Now, we find there that from latitude 40° to 51° N., or about 750 miles, the increase in submergence, or the rise in the sea level, seems to have been about 450 feet, or $\cdot 60$ of a foot per mile ($7\frac{1}{5}$ -inches).

Here, owing to our smaller area, our proximity to the sea, and the coalescing of ice-sheets from different quarters, the phenomena are perhaps less distinct and easily traceable.

But let us take two fixed points. Take England, south of the Thames and the Bristol Channel, to begin with. That district, according to Sir Charles Lyell, "seems to have remained land during the whole of the glacial period, at a time when the northern area was under water."* And Sir Andrew Ramsay concurs that there is no trace of glaciation—"no recognised boulder-beds in England anywhere in or south of the valley of the Thames."† Some geologists, however, more recently recognise a small submergence of that region—from 20 or 30 up to 50 to 60—at all events under 100 feet. If we say 100 feet in the latitude of the Thames valley, or $51\frac{1}{2}^{\circ}$ N., then our next fixed point will be on the coast of Norway, 65° N., where the submergence appears to have been about 600 feet. Assuming that there was a gradual uniform rise in the level of the sea from the south of England to the coast of Norway, which is the most probable hypothesis we can reason upon, and it is so far borne out by the facts of a similar kind in North America; we have here a rise of 500 feet in a distance of about 900 miles, which is equal to $\cdot 55$ of a foot (or $6\frac{3}{5}$ -inches) per mile. This is not far from the result we arrived at as regards America, a little less, as perhaps might be expected. Taking, then, our own country at this rate, 350 miles north of the Thames would give only $192\frac{1}{2}$ feet (or say 200) of a submergence; 400 miles, 220 feet, and even the extreme north of Scotland would only reach about 250 feet. Now this is very far short—much less than half—of what is required in the centre of Scotland for the supposed submergence under consideration. ‡

* "Antiquity of Man," 3rd ed., p. 275.

† "Physical Geology and Geography of Great Britain," 3rd ed., p. 239.

‡ It is sufficient for our purpose if these results, from a fair use of the known facts, are at all approximately correct.

Thus, if we adopt the attraction of the ice as the probable cause of the submergence (or rise in the waters of the sea), we must reason from what we know, or can observe, of its effects in other parts of the world, and over the northern hemisphere generally; and all the facts we know regarding these seem quite opposed to a submergence of anything like 500 or 600 feet being possible in this country from this cause.

Tested by what is admitted regarding the "Second Glaciation."—We may note here, in passing, that the submergence which can be shown to have occurred during what is called the "second glaciation,"—which, from what has been admitted as to the extent of the ice and the effects it produced, must have been, as we have remarked, very much a repetition of the "first glaciation"—the submergence which occurred in the last cold period, and which, we may conclude, should be more clearly ascertainable than that which occurred in the first, seems to have been only about 100 or 125 feet.* This was the case, when the ice again attained such huge dimensions—when the whole country, except, perhaps, the highest peaks, lay buried under it, and great ice-sheets or systems of confluent glaciers extended all around it as before. Taking what is said of it by those who advocate it, it is hard to suggest any reason why the submergence which occurred during this so-called "second period of glaciation" should be only about one-fifth part of that which is claimed to have occurred during the first period, to which in all other respects it seems to have borne so striking a resemblance. I say it is hard to suggest any reason for this, if we proceed on the theory which we are arguing on at present, that the ice itself was the cause of the submergence.

Tested again by what is known of other Countries.—Referring again for a moment to other countries, we may note that, instead of the numerous oscillations and repeated submergences supposed by some geologists to have taken place in this country,† it seems there was in Norway just one partial submergence (which, as we have shown, there is every reason to believe, was greater than that

* Sir A. Geikie, "Text Book," p. 903. "Great Ice Age," pp. 254, 262.

† "How many submergences there were, we cannot at present, and perhaps never may be able to say."—*Op. cit.*, p. 188. We gather, however, that at least *three* or *four* distinct submergences are (or were) believed to be made out (pp. 184-5, &c.), which seems pretty well.

which took place in this country); and that this was not followed by another glaciation, but by the retreat of the ice and the re-elevation of the land.*

To the same effect, Professor Wright states that the only changes of level connected with the Ice Age in North America of which there is satisfactory evidence are the following:—(1) A pre-glacial elevation of a few hundred feet; (2) a depression varying in different localities, but as a rule increasing northward, from 150 up to 520 feet—the greater number of instances observed being between 300 and 400 feet; (3) a re-elevation towards the close of the period, of the regions that were thus submerged. These, he says, “seem to be all the changes of level which are absolutely required to explain the facts in hand.”†

Is it not worth considering whether (with a narrower limit certainly as regards the submergence, corresponding with the smaller scale of the other phenomena) these would not also explain all the facts in this country? We may reasonably assume that the phenomena of the Glacial epoch were exceedingly uniform all over the northern hemisphere; and any scheme of interpretation which holds good over the greater part of the area is *ipso facto* more probable than a scheme which is good for one locality only—for Lewis, or Wales, or the east of England, or the North of Scotland—and differs entirely from that proposed to account for the corresponding facts in other localities.

Recapitulation.—For all these reasons, which we leave to have such weight as may appear fairly to belong to them, we find it impossible to believe that this clay at Chapelhall is a true shell-bed marking a former level of the sea.

Passing over the preliminary objections that might be raised on account of the imperfection of the evidence on which it rests, and the little that is really known of it; and taking all we do know, we have touched upon these points:—

(1) Its local and limited character, and the absence of all traces of it in a number of other localities, not only in the neighbourhood, but on both sides of the central valley where these might be expected, and in other parts of the country generally.

* *Op. cit.*, p. 410.

† “Ice Age in North America,” pp. 414-5,

(2) The absence of marine organisms in the overlying Boulder-clay, which, if the sea had reached this limit, must have been largely formed of materials derived from the old sea-bed.

(3) If deposited during a period of great submergence and milder conditions, as had been alleged, it seems reasonable to expect, from all we know regarding the laws which regulate the distribution of the mollusca, that the marine organisms it contains would be of species indicating a more genial temperature than do those actually found at Chapelhall.

(4) If there was an actual depression and re-elevation of the land, occupying, according to all known data concerning such great crustal movements, scores of thousands of years, it seems highly improbable that the land should so nearly regain its former level just at the recurrence of another cold period, and so exactly resume its former condition, that an ice-sheet capable of depositing another great thickness of Boulder-clay composed of materials largely derived from the Western Highlands should again overspread the central valley of Scotland.

(5) If, on the other hand, the submergence was due to the mass and attraction of the ice itself, then we are met by the difficulties (a) that it is greatly in excess of the submergence traceable in other parts of the northern hemisphere, where the ice is known to have been in much greater force; (b) that the submergence traceable in these other parts seems not to have taken place at the same stage of the glacial epoch as this alleged great submergence in Scotland, *i.e.*, seems not to have been succeeded by a second glaciation; but to have taken place just prior to the final retreat of the ice and the "last elevation of the land;" and (c) that there is no degree of proportion discernible between this alleged submergence of 500 or 600 feet during the "first glaciation" and that which can be shown to have accompanied what is called the "second glaciation," which, however, from its extent and effects, seems to have been scarcely, if at all, inferior to the first.

Some of these reasons will doubtless appear more weighty than others; but taken altogether, we think they form tolerably strong ground against accepting this Chapelhall deposit as a true "shell-bed," and the submergence of "526 feet or more," which it has so frequently been taken to indicate, as proved.

III. *The Alternative Supposition—Transport by Land Ice.*—But if this patch of shelly clay was not laid down at this level by the sea—if there seem to be so many and such varied objections to concluding that it was so deposited—then the only alternative we can see is that it was somehow transported to its present position from a distance, and from a lower level; and the only agency known to be capable of doing this, and to have in many other instances actually done so, is land ice. I need hardly remind you of the numerous shelly tills to which I have alluded in an earlier part of this paper—such as those of Caithness, Lewis, the Endrick Valley, the south of Ayrshire, Arran, Wigtonshire, in our own country; the Isle of Man, Cromer, Holderness, Bridlington, in England; and in North America (not to multiply examples), near Boston and along the lower reaches of the St. Lawrence on the east coast, and at Vancouver on the west. We may say, in short, wherever the ice can be shown (from the direction of the striæ and the nature of the rocky fragments in the “drift”) to have passed over a part of the sea-bed in its progress, there shells or fragments of shells have been found in the glacial deposits.*

If, however, it be objected that this is not, strictly speaking, a shelly till, but a patch or strip of shelly clay imbedded in the till, still we may reply such cases are not uncommon. Thus, in the Boulder-clay at Bridlington, as is well known, there occur patches of clay, or “argillaceous greensands, of small thickness, containing a considerable number of shells in a fair state of preservation.” These patches, as Mr. Lamplugh has shown, are “not *in situ*, as at first supposed, but are masses torn off from the original bed, and re-deposited in the Boulder-clay.” †

Such strips and patches of shelly sands and clays, it has been suggested, may have been in some instances frozen into a hard mass ere they were taken up by the ice-sheet, and so transported by it without much injury to the shells enclosed. ‡

* “The presence of such shells in the Boulder-clay of the lower reaches of the St. Lawrence is really no proof that the clay in question was deposited by the sea. The shells may quite well have been introduced into the ground moraine of the ice sheet which passed up the Gulf of St. Lawrence” (“Great Ice Age,” p. 456).

† Prestwich, “Geology,” vol. ii., p. 447. *Quar. Jour. Geol. Soc.*, vol. xxiv., p. 146.

‡ Peach and Horne “On the Glaciation of Caithness,” *Proceedings, Roy. Phys. Soc. Edin.*, vol. vi.

A similar suggestion has been made with regard to strips and patches of clay containing plant remains and thin layers of peaty matter intercalated in the Boulder-clay of North America.*

Now, undoubtedly this locality, Chapelhall, lay in the very track of the ice, which (as shown by the striæ and by the pieces of granite, quartz, mica-schist, &c., in the Boulder-clay) originated among the Highland mountains to the north-west, and moved in a great sheet or *mer de glace* eastward across this part of the country to the German Ocean. The striations on the rocks everywhere in the neighbourhood, of which there are many clear and striking examples, as well as the stony contents of the Boulder-clay, demonstrate this. And the reason, of course, is that the choked and blocked condition of the Firth of Clyde, by ice from these mountainous regions, rendered egress for the whole of it in that direction impossible. Now, if in Loch Lomond and Loch Long, for example, there is evidence that the ice was not less than about 3000 feet thick, here, around Airdrie, it may still have been over 2000 feet in thickness. We may take for granted, as sufficiently obvious, that the ice must have moved across part of the sea-bed on its way hither. And it is quite credible, for we know it has done so in a multitude of similar cases, that it should take up and carry forward with it shells and pieces of clay charged with shells from that sea-bed, and deposit them elsewhere in its progress.

As a subsidiary point, it may be worth noting that the locality in which these shells were found is not only in the main track of the ice-sheet across the country, but immediately in front of the highest ground or watershed of the district, and where the ice was most likely (as some one has remarked in a similar case) to make a "long pause" either in advancing or retreating. It is immediately in front of one of those great masses of basalt which in this district rise through the Coal Measures and partly overflow them, and which, from their greater hardness, have more effectually resisted the denuding agencies. Accordingly, the level of the country rises rapidly here; while it is only 300 feet at Coatbridge, the 500 feet line runs close to Chapelhall (it is 526, as has been said, on the surface at the well); and a little farther east it rises to 928 feet at Torrance and 959 at the Kirk of Shotts. It seems to be just the sort of locality where anything being carried forward

* "Ice Age in North America," p. 287.

by the ice would be most likely to be deposited; and the conspicuous ridges of Boulder-clay all around, to which I have referred, corroborate this statement. It seems to be where the ice meets with some decided obstruction, that such things are most apt to be deposited by it—of which illustrations might be adduced from Caithness and elsewhere.* In short, this case at Chapelhall seems to present on a smaller scale what happened at Moel Tryfaen, and also on the Dublin and Wicklow hills, where such deposits of transported materials were left on prominent outstanding obstructions in the path of the ice.†

I need not pursue the subject further at present. The other “high-level shell-beds” in Scotland—those of Aberdeenshire and Nairnshire—I purpose considering on another occasion. Meantime, I am confirmed in the view I have taken of this subject, by finding that Sir Archibald Geikie, in the last edition of his “Text-Book,” expresses himself with great caution and reserve regarding these “high-level shelly deposits.” He says, “they are probably not conclusive proofs of submergence,” for “they may have been picked up from the sea-floor by moving ice,” as in the various instances to which we have already referred. He dismisses the shells at Moel Tryfaen as “probably not really in place;” and with regard to this instance at Chapelhall, he admits that “the layer containing them may have been transported by the ice-sheet,”—which is virtually giving up the case for submergence.‡

I venture to think that the more the matter is considered, and the difficulties against any other hypothesis fairly weighed, the more will this explanation commend itself to the minds of geologists. I accordingly submit that this instance of Chapelhall ought no longer to be cited as a proof of submergence.

* See Peach and Horne, *Proc. Roy. Phys. Soc. Edin.*, vol. vi.

† We may also notice in passing the colour of the clay—red, or, as Mr. Russell described it to Sir A. Geikie, “a fine reddish brick-clay.” The clay proper to the Carboniferous rocks is, we need hardly say, of a much darker tint, “bluish-black, dull blue, or leaden.” So that from this description, the shelly clay would certainly appear to be of an origin foreign to the district.

‡ *Op. cit.*, pp. 897-902, and “Scenery of Scotland,” 2nd ed., p. 366.