CORRESPONDENCE.

THE GLACIATION OF IRELAND.

SIR,—Professor Kendall's criticisms of my paper on Irish Eskers include various specific objections.

(1) He complains of lack of deliberation. The first of my many visits to Irish eskers was in 1887. At my time of life I cannot hope for equal further deliberation.

(2) He objects that the paper contains very few new field observations. I should have been less surprised by the complaint that the paper quoted too lengthily from my field note-books; seventeen pages out of thirty-five are occupied by detailed descriptions of representative eskers and mainly of their internal structures; and I justified this length on the ground that the previous literature "deals mainly with their distribution and general structure" (p. 116), and but little with their intimate structure.

(3) Apparently I should have quoted my notes at still greater length, for compression has led to misunderstanding. In accordance with Professor Kendall's habit of regarding as simple mistakes views he does not accept, he so dismisses the attribution of the crescentic series of eskers around the northern end of the Slieve Bloom Mountains to ice from those hills. He says I "might have been spared this mistake" had I consulted G.S.I. Memoir, No. 117–8. I had not only consulted it but quoted it, for its account of some "anastomosing eskers". Professor Kendall's quotation from that Memoir supports my conclusion; for the rarity of Galway granite on the northern in contrast to its abundance on the southern slopes of the Slieve Bloom Mountains, is most easily explained by its entrance having been hindered or by it having been subsequently swept away by local ice. The existence of this local drift is recognized in the Memoir, No. 127, p. 26.

As regards the position of Roscrea, my sentence: "The Roscrea, Clonaslee, Mountmellick, and Maryboro Eskers were probably formed by ice which flowed down the northern slopes of the Slieve Bloom Mountains"—was an attempt to indicate in two lines the relations of over 30 miles of esker. They are part of one crescentic series around the northern end of the range. Moreover, the term Slieve Bloom Mountains is sometimes used (e.g. Phillips, *Atlas of Comparative Geography*, and the map used in Carvill Lewis' *Glac. Geol. Gt. B. and I.*, 1894, opp. p. 83), to include the geological continuation of the range south-west of the Roscrea Gap, and in that sense the south-western or Roscrea end of the series is north of the range.

I referred so briefly to this series because unusual detail had been given of its internal structure (e.g. Memoir, No. 126, p. 22).

(4) The origin of the boulders is but exceptionally referred to in my paper because it is so seldom that the last direction of movement can be inferred from the nature of the included rocks. Thus, the granite in the kame near Barony Bridge, Tyrone, gives

no clue to the movement of the ice which made the kame. No doubt the area was invaded by ice from the north-west; but the movement at the time of the kame formation is not shown by the original home of the boulders. According to Professor Cole (*Reg. Geol.*, iii, p. 329, received after my MS. had been sent to the Royal Society) the materials of the Tyrone eskers were distributed from south to north. I am sorry to differ from Dr. Charlesworth regarding an Ulster kame, and if he will publish his evidence I will carefully consider it; but I find it difficult to realize how this kame, steeply descending the hill to the north and with its curves concave to the south-east, can be due to ice from the north-west, although much of its material originally came from that direction.

Professor Kendall claims that by insertion on Fig. 9 of two arrows from Professor Sollas's map I have unintentionally contradicted the statements in the text that the ice flowed in the opposite direction. I described the Dunmore eskers in reference to the claim that they were deposited within ice which was moving at right angles to their trend. For, if so, and if glaciers have any power of erosion, it would appear clear that the eskers must have been formed after the ice had ceased to flow across their sites. I agree with Professor Sollas that the course of the ice was along a line trending north-west and south-east; and to show that Professor Sollas adopted that course I inserted the two arrows from his map. The legend of the figure quotes them as from Professor Sollas, and as marking "the course of the ice movement", not its direction along that course. It is made clear in the text that I consider that the direction adopted should be reversed; but I left the arrows to prevent any possible suspicion that I was claiming Professor Sollas's agreement with the direction of movement as well as with its course. Whether the ice moved from or towards the north-west being immaterial to the formation of these kames, I mentioned my conclusion and the nature of the evidence, but did not give it in detail.

(6) Professor Kendall also objects to my insertion on a sketch map (Fig. 11), showing the relations of the eskers to the 300 foot contour of some arrows indicating the ice movement according to Mr. W. B. Wright. The first objection raised is that Mr. Wright's map refers to the maximum extension of the ice and not to the stage to which I assign the eskers. But the theory which I was proposing to amend is that the eskers were formed by rivers within the great ice-sheet; and one fact I hoped to show by the figure was that many of the chief eskers trend across the line of the movement of the ice, and were formed on its margin during its retreat, and not within it. To that argument the map is relevant. The second objection is that I have unduly magnified the lines. In order to prevent attaching to Mr. Wright's lines a significance as to details greater than the scale of his map would warrant, I ended the lines to the south against the hill country of Slieve Bloom, between

Lough Derg and South Kildare. I inserted only thirteen out of the thirty-eight lines marked by Mr. Wright, as they were sufficient to show the general view as to the ice-movement in Mid-West Ireland. Eleven out of the thirteen lines follow the originals precisely ; the two easternmost are rather generalized to show the movement east of Lough Ree and trend rather farther to the west than the nearest corresponding of Mr. Wright's lines ; but this difference does not affect the argument, as their direction in the original is athwart the chief eskers, and is inconsistent with the formation of the eskers by intra-glacial rivers.

Origin of Boulder Clay.—Passing to the general question of the origin of boulder clay, Professor Kendall states that I intend " a general assertion of the marine origin of all boulder clay ". No such assertion is intended. I briefly stated my view of its origin in *Geology of To-day* (p. 227). So far from claiming all boulder clay as marine, I described its formation in an ice-blocked depression at the head of the Fulmar Valley in Spitsbergen. It is, however, sometimes subaqueous, deposited either in lakes or in quiet arms of the sea as off the Sefstrom Glacier. Each case must be determined by the local evidence, and where the boulder clay contains contemporary foraminifera, the possibility of its origin as marine mud must be considered. The foraminifera cannot be simply dismissed as derived from older rocks when they have been determined by Mr. Joseph Wright. I once sent him some Essex boulder clay, and he reported a number of species as derived from the Chalk and a list of others as indigenous to the clay. Mr. J. Wright is not likely to make the mistake suggested.

That the belief in the marine origin of boulder clay has been and is rejected by the majority of British glacial geologists is fully admitted in my paper. Professor Kendall asks to whom I referred as upholders of the marine origin of boulder clay. Professor Bonney's Presidential Address to the British Association in 1910 shows that the marine theory has been consistently supported by high authorities. My own partial acceptance of the view has been by no means consistent, for I at first regarded all boulder clay as terrestrial, and was only gradually led to the view that some of it is marine. Amongst men with an intimate knowledge of the boulder clay of the south-west of Scotland, and who regard it as a marine deposit, may be mentioned Mr. J. Neilson, for the Glasgow district, and Mr. John Smith, after his detailed study of the Ayrshire Drifts. The increasing faith in isostatic oscillations has also encouraged the probability of a glacial subsidence. In recent years there seems to have been a decided trend toward the opinions that the boulder clay has not yet been satisfactorily explained, and that some of it is marine. The consequences of that conclusion are not so startling as Professor Kendall suggests, for until the shell beds at 1,300 feet on Three Rock Mountain, near Dublin ; at Moel Tryfaen, Oswestry,

and Clava are adequately explained without submergence, there is nothing improbable in a 400 foot submergence of the Central Plain of Ireland. Carvill Lewis adopted a 400 foot Irish submergence during the advance of the ice (*Glac. Geol.*, p. 148).

The chief difficulty in the marine origin of Irish boulder clay is its poverty in marine fossils; but the references quoted in my paper show that marine fossils are widely scattered in the Irish drifts. They are rare, and to explain their rarity I quoted from men so experienced in polar biology as Dr. Nansen and Mr. J. Murray to show that under some conditions life is absent from the Polar seas. Dr. Nansen's statements that the floor of parts of the Arctic Sea are lifeless are not refuted by Gran having found the opposite in "samples taken later during the expedition". Similarly, in the Antarctic, Murray's statement that the shore deposits at Cape Royds contain no vestige of life is not refuted by the occurrence of shells elsewhere and in beds which, owing to the scarcity of life along the shore, Hedley and Priestly reject as beaches and attribute to upheaval and upthrust. Even in the Swedish drifts, though shells are usually abundant, the clays are sometimes sterile over large areas.

The marine origin of the Irish boulder clay is a subsidiary issue; the object of my paper was to show by a description of the internal structure and field relations of representative Irish eskers, that the most important were not formed along intra-glacial rivers, but on the margin of the ice, where it ended in a sheet of water. Most of the eskers in fact are kames, not osar. I fully recognize that the evidence for the sheet of water being the sea is less clear than that as to the nature of the eskers. I only advance the view that it was the sea as being more probable than that it was a series of glacial lakes; and there is nothing in Professor Kendall's note to modify that opinion. I regret his adoption of a tone of discussion which seems to me as out of date as the view that all boulder clay may be simply explained as *moraine profonde*.

J. W. Gregory.

THE AGE OF THE SHENLEY LIMESTONE.

Sir,—It is fortunate that the Shenley echinoderms have received expert examination, and our thanks are due to Professor H. L. Hawkins for his note on the subject in your February issue (p. 57). I will, however, ask for temporary suspension of judgment in respect to his deductions as to the age of the deposit.

As the result of recent work, I shall be able to communicate to the Geological Society during the present session a paper containing much new evidence, both stratigraphical and palæontological, to prove that the limestone is in its proper position below the Gault, and that the Gault of the section belongs to the Lower and not, as supposed, to the Upper Gault.

G. W. Lamplugh.

St. Albans.
February 10, 1921.