LETTERS TO THE EDITOR.

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The Royal Society.

THE article on the Royal Society, published in NATURE of June 8 by my friend Mr. Thiselton-Dyer, contains very little statement of fact to which I, or any one acquainted with the history and traditions of the Society, could wish to take exception.

It does, however, seem to me to be important to point out (a thing Mr. Thiselton-Dyer has omitted to do) that the tendency of the development of the Society has been to restrict its ordinary membership to those who have done valuable work in "the improvement of natural knowledge" either by the exercise of their own mental gifts, or by assisting in some marked way—by the wise application of money or other direct influence—the efforts of others to that end. When, some thirty-five years ago, the annual number of elections of ordinary Fellows was practically restricted to fifteen by the limitation to that number of the list recommended by the Council, the chance of admission to the Society for "a member of the legislature with the keenest sympathy for science" (to quote Mr. Thiselton-Dyer's words) became small; and as the years rolled on, and the number of serious workers in science increased in unexpected proportion, it became less and less. Accordingly, not many years ago, it was determined by the Society, in order to meet this undesirable state of things, that members of the Privy Council should be eligible at any time as Fellows of the Society without reference to the annual list of fifteen prepared by the Council. Apparently the intention of this measure was to relieve the ordinary annual list of fifteen candidates for Fellowship from the presence of a certain number of members of the legislature with keen sympathy for science, and other such aspirants, and to reserve it for those for whom it could be claimed that they had done something tangible for "the improvement of natural knowledge." It seems to me that the selections made by the Council since that date confirm this view. Mr. Thiselton-Dyer makes a mistake in confounding the real services to natural knowledge rendered by Sir John Kirk, Sir George Naes, and Sir Charles Warren, with the "sympathy for science" of amiable members of Parliament.

There is another aspect of the question recently discussed which seems to me to be important. Does the Royal Society propose, or does it not, to include in its annual elections persons eminent in historical study? If it does, surely Freeman, Stubbs and Gardiner would have been Fellows of the Society. The examination and exposition of documents when they relate to an Asiatic race cannot be regarded as more akin to the investigations of the improvers of natural knowledge than is the study of the inscriptions, camps, and pottery of European peoples. Does the Royal Society explicitly or implicitly recognise claims which would give their possessor a first place in an Academy of Inscriptions or of Historical Science? I should venture to reply to this question: "Certainly not; by most definitely expressed intention such studies as those of the historian were excluded by the founders of the Society from their scope. And further, were such studies to be embraced by the Society as a new departure, it would be necessary to make special provision for them by increasing the annual number of elections, and by securing seats on the Council for one or two persons acquainted with those studies and the merits of those who pursue them."

I believe that the Royal Society is honoured and trusted by the British public as being the leading society "for the improvement of natural knowledge." Its original and deliberately chosen motto, "Nullus in verba" is a distinct profession of its purpose to appeal to experiment and the observation of phenomena, rather than to encourage the disquisitions of the bookman and compiler of history.

Though it may well be urged that such a body as "the Royal Society for the Improvement of Natural Knowledge" is wise in offering a kind of honorary membership on special terms to those who are a power in the State, there seems to be no ground for maintaining that the Fellows (as Mr. Thiselton-Dyer declares) "display themselves as reasonable, if hard-headed, men of the world" when they sacrifice one of

their fifteen ordinary annual fellowships for the purpose of enrolling among their number an isolated example of the numerous body of historians and essayists who have attained some distinction in subjects and methods remote from those professedly pursued by the Society.¹

Were the Royal Academy of Arts to assign one of its Associateships to, let us say, a distinguished botanist who is known to have a keen sympathy for Art, the world would, I venture to maintain, consider that the Academicians had not "brought themselves into touch with another field of national life," nor "displayed themselves as reasonable or hard-headed men of the world," but had simply stultified themselves whilst conferring no real honour upon their nominee. The Royal Academy includes a small number of laymen as honorary members, but it is recognised that the Academicians shall only confer their regular membership upon those of whose work they are competent judges, and consequently upon those who are really honoured by the selection, namely, artists.

It seems to me that *mutatis mutandis* much the same is true of the Royal Society. The Society has gained in the past, and will retain in the future, public esteem, and increasing opportunities for usefulness by aiming with singleness of purpose at "the improvement of natural knowledge." To confer honour on those who have improved natural knowledge is its privilege and its duty. The appreciation of historians and of "sympathetic legislators" is a function which the Society is incapable of performing, and moreover one which few, if any, persons desire it to attempt, since it must lose dignity by assuming to adjudicate in matters in which it is incompetent.

Oxford, June 26.

E. RAY LANKESTER.

ALTHOUGH my friend Prof. Lankester finds that the "tendency of the development" of the Royal Society has been to restrict the area from which its members are selected—a conclusion in which I am not disposed to agree—I do not find that he seriously impugns the account which I attempted to give of what appeared to me to be the traditional practice of the Society in the matter.

In fact in one respect he goes much further than I should myself be inclined to do in admitting as a qualification for membership "the wise application of money." I must confess that I should be disposed to regard this, for obvious reasons, with very close scrutiny.

Apart, however, from this it is evident that Prof. Lankester and those who agree with him would like to make the Royal Society much more professionally scientific (for there are very few scientific men nowadays who are not in some sort or other professional). If they succeed I am disposed to think that it would be a very much less influential body than it is at present. And I find that no inconsiderable body of the existing Fellows are of the same opinion.

W. T. THISELTON-DYER.

Kew, July 1.

Ice as an Excavator of Lakes and a Transporter of Boulders.

I HAVE devoted a considerable space in a work I have recently published in which I have criticised the extreme glacial views of some writers to an issue which underlies a great deal of their reasoning, and which, it seems to me, it is absolutely necessary we should determine before we are entitled to make the deductions habitually made by them.

Before a geologist is justified in making gigantic demands upon the capacity and the power of ice as an excavator or as a distributor of erratics and other debris over level plains it is essential that he should first ascertain whether it is capable of the postulated work or not. It is not science, it is a reversion to scholasticism to invoke ice as the cause of certain phenomena unless and until we have justified the appeal by showing that it is competent to do the work demanded from it. This preliminary step is not a geological one at all. It is a question of physics, and must be determined by the same methods and the same processes as other physical questions. So far as we know the mechanical work done by ice is limited to one process. The ice of which glaciers are formed is shod with boulders and with pieces of rock which have fallen down their crevasses. These

¹ I have addressed my remarks mainly to the contentions of Mr. Dyer's article. I should wish to avoid discussing the merits of a particular election which in my opinion cannot now and never could legitimately be a subject for public comment. I wish, however, to state that I am not unacquainted with the interesting essays on the history of geological theory which we owe to the hero of that election.

pieces of rock abrade and polish and scratch the rocky bed in which they lie when they are dragged over it by the moving ice. Without this motion they can of course effect nothing either as burnishers or as excavators.

This motion has been shown by recent experiments to be very largely if not entirely a differential motion due to the viscous nature of ice, as Forbes long ago argued on *a priori* and other grounds that it was. The viscosity of ice is different at different temperatures. It differs also greatly when it is in the form of granulated ice, such as a glacier is composed of, from ice formed in a laboratory or directly frozen from water in a pond, but in any case it is slight, and it needs a considerable and a long-applied force to make it shear. The consequence is that when it rests on a level or nearly level surface, where gravity does not work, it ceases to move at all. In order that it should acquire motion sufficient to drag stones, &c., along, it is necessary that there should be some *vis a tergo*. Either the ice must rest on a slope sufficiently inclined to generate a gravitating movement in it, as a whole, or the slope of its upper surface must be sufficiently great to cause the movement of its surface layers to be continued down to and to remain effective in its nethermost parts. Every attempt made by Croll and others to invent for, and assign to, ice molecular movements capable of causing lateral motion in the stones beneath it other than those induced by gravity, seems to me to have utterly failed. The cause—the only cause which is competent to make it move is gravity acting either in one or the other way above specified.

This seems to be the inevitable conclusion whenever the problem is tested as it ought to be tested, by empirical tests. If so, it seems to put out of court the continual appeals made to ice as the distributor of debris over hundreds of miles of level plains, and as the excavator of basins and lakes at a considerable distance from mountain slopes.

In the first place, the modulus of cohesion of ice being what it is, it has been shown by Mallet, Oldham, and Irving that thrust cannot be conveyed through it for more than a short distance, since it must yield and eventually crush.

This *a priori* view is supported by the actual observation of glaciers in which we find that the rate of motion is very largely a function of the slope of the bed, and when a glacier leaves the slope on which it rests and gets on to level ground it very soon ceases to move altogether.

It has been argued that in the Ice Age the ice was piled up in dome-shaped ice sheets, and that the distribution of the boulders and the excavation of mountain lakes was due to the results of the efforts of the viscous mass to reach a state of equilibrium by hydrostatic movement, or by rolling over itself. But this ignores the very slight viscosity of ice which would require a very high slope in its upper layers to induce movement in its lower ones at all. It is impossible to see how this high slope could be secured, since the effort to restore equilibrium would be continuous, and the potential movement involved in every fresh fall of snow would at once be dissipated instead of being accumulated.

I cannot see, therefore, how under any circumstances it is possible for ice either to travel over long distances of level ground, or to excavate hollows such as the great majority of mountain lakes are.

I have not in this letter referred to the geological difficulties of such a hypothesis, which are manifold. I have limited myself to the physical difficulties alone. They seem to me to underlie the whole problem, and it is useless to discuss it until they have been solved, yet they are persistently ignored by the ardent champions of ice. That ice can do a good deal when allied with gravity is true enough, but the problem, as presented by Mr. Wallace, Prof. James Geikie and others requires that it should continue to do portentous work when no longer allied with gravity. Is it too much to ask that some justification should be offered (and nowhere better than in your catholic pages) for such an enormous unverified postulate?

Athenæum Club, July 1. HENRY H. HOWORTH.

Abnormal Weather in the Himalayas.

ON May 26 I walked from Changla Gali (about 9000 feet) to Dugar Gali (under 9000 feet) by the "pipe" road. On the way we passed (the road is cut along the side of the steep mountains) a narrow valley filled with snow to about a height of 100 feet. The width of the hard snow on the road was 20 feet. On the 28th I walked back to Changla Gali by the main road.

Here we saw a great deal of snow. A bridge spanned a narrow valley, a mass of flat snow, perhaps 15 feet thick, filled the valley to the bridge. No snow ran up the valley. Then we came on two valleys converging into one at the point where the road passed. Both valleys above and the valley below were filled with snow, and the road for 150 feet was cut on the face of the snow.

In the first week of May terrific storms burst over Murree; we had onstant storms at Dugar on the night of the 26th up to 12 a.m. on the day of our leaving, the 28th. On the 28th the last two miles of the road into Changla were simply carpeted with leaves and twigs broken off by a violent hailstorm. The sides of the road, sometimes the road itself (four hours after the storm), were covered with drift and massed hailstones of the size of big marbles (ice with the usual whitish centre).

This continuance of snow and this stormy weather is stated to be altogether abnormal.

F. C. CONSTABLE

Changla Gali, May 29.

Peculiar Hailstones.

A FRIEND of mine writes me from Peshawar about a very curious phenomenon which I think is worth notice in your columns. The monsoon has set in this season earlier than for some years past. A few days ago in a village named Daduzai (a tehsil in the Peshawar district) rain fell, preceded by a wind storm, and with the rain came a shower of hailstones which lasted for a few minutes. The most curious part of this occurrence is that the hailstones when touched *were not at all cold*, and when put in the mouth (as is the custom in this hot country) tasted like sugar. I am further told that these hailstones were extremely fragile, and as soon as they reached the ground they broke in pieces. These pieces when examined looked like broken sticks of crystallised nitre. My informant tasted them, and was struck with their purity and sweetness. A few pieces were also sent to the Deputy Commissioner of the district. The phenomenon has been duly reported in the leading newspapers of the province, and the *Akhbar-i-Am* has noted it in its leading columns.

KANHAIALAL.

Lahore, June 20.

Crocodile's Egg with Solid Shell.

DURING the year 1885 I was stationed at Trincomalee, when it was my luck to find a large crocodile's egg near Kuntalay tank. On showing the specimen to several friends who knew more about natural history than I did, they expressed their astonishment at seeing a hard-shelled egg, as the consensus of opinion was that such eggs were invariably surrounded with a soft parchment-like covering.

I made a hole in the top and bottom of the egg and blew out the contents. The shell is still in my possession, and resembles more the hard enamelled-like egg of the ostrich than anything else I have seen.

The above facts may interest those who take a pleasure in objects of natural history.

J. BATTERSBY.

Murree Hills, June 7.

UNIVERSITY AND EDUCATIONAL ENDOWMENT IN AMERICA.

THE statements in the following extract are so remarkable that I think they deserve a wider publicity than they will probably receive in the pages of a Parliamentary paper.

One may hope that the reconstructed University of London will make provision for post-graduate study and the advancement of knowledge in the greatest city of the world. It must be admitted that this cannot be done without the expenditure of a good deal of money. May one hope further that the cause of the higher education will find friends amongst us in London as munificent as university and technological studies have found in one of the newest of the world's cities?

Kew, June 30.

W. T. THISELTON-DYER.