become incorporated with the post-cretaceous continent,* it is with much satisfaction that I find views for the most part so similar developed by Professor Huxley with the ability which marks all his work. Nevertheless, I venture to submit that the view I then advanced as to the period of the origin of the Eocene types is more in accordance with the facts, as far as we know them, than the hypothesis of their origin in a detached province *during* the Mesozoic period.

The view I advanced was that great changes in the distribution of the continents and seas took place at the close of the Palæozoic, and again at the close of the Mesozoic epoch; and so far I am at one with Professor Huxley: but I inferred that the geographical changes taking place at the close of the Mesozoic epoch were accompanied by the formation of a continent extending over all the *geologically known* parts of the globe, whose endurance was so prolonged as to have afforded the necessary time for the evolution upon it of the Eocene types.

In support of this inference I dwelt upon the entire disappearance of the orders *Pterosauria*, *Enaliosauria*, and *Dinosauria* among vertebrates, and of the *Ammonitidæ* among invertebrates; as well as upon the great extinction that took place in various other forms of life. Such a process as the one suggested by Mr. Huxley would lead us to look in Eocene strata for an intermingling of these distantly evolved types with forms belonging to the several orders just named; however much these forms might in their specific, or even in their generic characters, have been changed during the interval in which these distantly evolved types were introduced. But instead of this we find an absolute disappearance of several important orders of life, of which, from their *habitat*, some—especially the *Pterosauria*—would seem to have been independent of geographical changes simply.

Mr. Huxley intimates that he is led to his view by arguments which he had previously used to demonstrate the necessity of the existence of all the Eocene types in some period antecedent to the Eocene; but may we not suppose that the interval thus marked by the disappearance of so many great orders was vast enough even for this evolution? Indeed so much did this great extinction weigh upon me that even the intervention of a vast lapse of time seemed scarcely sufficient; and I felt driven to suppose that these geographical changes in some degree altered the general conditions under which life had previously existed; and that this alteration, while stimulating evolution on the newly-formed continent, contributed to the great extinction which marks the intra-cretaceous and Tertiary epoch.

Subtle as are the causes which have brought into existence the various types of being, those which have produced their extinction are not less so; though they have not yet received that attention which has been directed to the origin of species. I feel how crude were the suggestions I offered in 1860 to explain this great extinction, and how wide a field of conjecture upon the subject is left open; for these orders of life were not only various in their *habitat*, but equally various in their food. We may imagine the extinction of a species to take place from failure of its food, from destruction by enemies, or—and I think this may be a cause more potent than any other, especially with forms possessing great fecundity—by a failure of the reproductive function; just as among men families diminish and eventually leave no descendants. Be the causes, however, what they may, this great extinction requires us, I submit, to suppose the occurrence of an interval of time as great, and accompanied by changes of conditions as complete, as any that we can urge as necessary for the evolution of the Eocene types. Moreover, the cretaceous period itself, whose terrestrial fauna is as yet unknown, may, so far as we yet know to the contrary, have witnessed in the European area the commencement of, or even some progress in, the evolution of the Eocene types.

Brentwood, March 10 SEARLES V. WOOD, Jun.

Transactions of the Royal Society of Edinburgh

WITH reference to G.'s letter in the last number of NATURE, I have merely to observe that (as you will see by the accompanying list) the Transactions of the Royal Society of Edinburgh are *regularly* sent to no less than twenty-three different societies, institutions, or museums in London alone—besides being sent to many Honorary and Ordinary Fellows residing there. As regards the special case of the British Museum, I have in my possession at this moment their acknowledgments of receipt of the successive parts of our Transactions up to March 1869, and expect immediately to hear that they have received our last published Part. J. H. BALFOUR, *Sec. R. S. Edin.*

Euclid as a Text-Book

"THE first four books of Euclid: or the principal properties of triangles, and of squares and other parallelograms treated geometrically: the principal properties of the circle and its inscribed and circumscribed figures treated geometrically." Such is the wording of the programme put forth by the University of London, of the Mathematical portion of the examination for matriculation candidates. Whether the papers have ever been drawn up in accordance with it I cannot say, but certainly my experience for the last four or five years has led me to believe that the alternative side has, of late, at least, been altogether ignored.

The slightest inspection of recent papers will show that they are constructed on the Euclidean type, and so long as Euclid was generally taught in schools, I think rightly so. But that such a course should now be persisted in (with such latitude as the programme provides) is hard upon those establishments which have taken up the modern views of the subject, such as those so ably advocated by Professor Hirst,* and Mr. J. M. Wilson of Rugby.† It can hardly be thought that so advanced an examining body as the London University will continue to act as an obstructive—for non-encouragement is almost tantamount to tabooing the subject; and the practical result of persistence, I fear, will be this, that the course pursued will press unfairly upon those schools in which (as in University College School, where Wright's Geometry is now the text-book) Euclid has been almost ‡ discarded. Boys are required to study in their school work this modern geometry, founded on French mathematical works; and yet, seeing what value is set upon the same in the examination papers I am discussing, feel themselves constrained to read Euclid that their prospects of good places may be enhanced.

I am disposed to believe that "something will shortly be done," but the reform, though it ought rightly to commence here, ought not to stop here. Every examining body, if a fair field is to be given to the students of modern geometry, should put forth a scheme similar to that which heads my letter, and not merely put it forth "as a sop to Cerberus," but act upon it and let it be a reality.

University College School R. TUCKER

MECHANICAL PROPERTIES OF ICE, AND THEIR RELATION TO GLACIER MOTION

A FEW weeks ago I prepared for the February number of the *Alpine Journal* a review of the contributions made by the Rev. Canon Moseley to the theory of glacier motion, which have appeared at various times during the last fifteen years in the *Proceedings of the Royal Society* and the *Philosophical Magazine*. Some new facts having come to my knowledge since the publication of my paper, I venture to recur to the subject, and to invite discussion upon those memoirs of Canon Moseley in which he endeavours to prove that the descent of glaciers by their weight alone is a mechanical impossibility.§ The arguments he advances in support of this conclusion may be epitomised as follows:—

If a transverse section of a glacier were to be made, the ice would be found to be moving differently at every point of it. The velocity is greater at the surface than deeper down, at the centre of the surface than the edges. There is a constant displacement of the particles of ice over one another, and alongside one another, to which is opposed the resistance known as *shearing force*. By the property of ice called regelation, where a surface so sheared is

* "On the probable events which succeeded the close of the Cretaceous period;" read before the Geological Society on February 1st, 1860. The publication of the paper, beyond a brief abstract, having been denied by the Council, the desideratum was kindly supplied by Dr. Francis; and the paper in extenso is given in the *Philosophical Magazine* of March, April, and May, 1862; the title having been changed to "The form and distribution of the Land Tracts during the Secondary and Tertiary periods, &c."

* In his college lectures, and lectures to ladies at St. George's Hall, &c.

† "Euclid as a text-book of Elementary Geometry" (read before the London Mathematical Society, and printed in the *Educational Times*, Sept. 1868), and in his "Elementary Geometry."

‡ *Almost.* In consequence of pressure from without, arising from the circumstances with which my letter deals, Euclid is again read in one class.

§ Proceedings of the Royal Society, Jan. 7, 1869. Philos. Mag., May 1869. Philos. Mag., Jan. 1870.

brought into contact with a similar surface, it unites with it so as to form one continuous mass. Between the resistance to shearing and the force which tends to bring the glacier down there must be a mechanical relation, so that if the shearing resistance were greater, the force would be insufficient to cause the descent. By a series of experiments upon cylinders of ice inserted in a cylindrical hole bored through two pieces of wood perpendicularly to the surface along which the one was made to slide upon the other, it was found that the force necessary to part the ice along the sliding surface varied from 75 to 119 lbs. per square inch. Canon Moseley has calculated that for the Mer de Glace to descend by its own weight, its shear per square inch cannot exceed 1·3193 lbs., and that to produce the actual motion with a shear of 75 lbs. per square inch, a force in aid of the weight and thirty-four times as great must be called into existence, and applied in the direction of motion. For such a force to be produced by the weight of the glacier alone the density of ice would require to be increased more than 400 times.

In this reasoning Canon Moseley has neglected, as it appears to me, the capability of ice when in a state of deliquescence to slide along a surface of small inclination, as demonstrated by the well-known experiment of William Hopkins. It is, however, not the motion of a block of ice as a whole, but the differential motions of its particles that we have now to consider. It occurred to me that the Canon's arguments upon this branch of the question might be put to an easy practical test by subjecting a block of ice to a strain produced by its own gravitation, and observing its behaviour under this condition, and I was fortunate in obtaining the assistance of my friend Mr. A. F. Osler, F.R.S., in carrying out the experiment.

A plank of ice 6 inches in width and $2\frac{3}{8}$ inches in thickness was sawn from the frozen surface of a pond, and supported at each end by bearers exactly six feet apart. From the moment it was placed in position it began to sink and continued to do so until it touched the surface over which it was supported, drawing the bearers with it, so as to make their upper ends converge. At its lowest point it appeared bent at a sharp angle, and it was rigid in its altered form. The total deflection was 7 inches, which had been effected in about as many hours under the influence of a thaw, during which the plank diminished slightly in width and thickness. On observing the under surface of the plank near the point of flexure, I noticed a number of very minute fissures extending a short distance into the ice, but they certainly were not sufficient to account for the flexure of the plank.

The question at once suggested itself, was the change of form in the ice plank due to fracture and regelation? I did not think it was, but the experiment was not decisive. Some weeks afterwards an opportunity occurred of trying it under other conditions. During the last frost we cut out another ice-plank. Its length was 6 feet $9\frac{1}{2}$ inches, its width varied from $6\frac{1}{4}$ to $6\frac{1}{2}$ inches, and its thickness was $1\frac{7}{8}$ inches. Two large bricks, of a width exceeding that of the plank, were set up on end, on a horizontal surface, exactly 6 feet apart, and the plank was laid upon them at five p.m. on the 12th of February. At 3·15 p.m. on the 13th it was continuously curved from end to end, so that it only rested on the edges of the bearers, and the middle point of its upper surface was deflected $1\frac{3}{4}$ inches below the line joining its two extremities. The temperature was 26° F. The curved plank was perfectly rigid, as was proved by taking it off the bearers and inverting it. I examined it again on the two subsequent days with the following results :—

Feb. 14th, 9.30 A.M. Temp. 29° 5 F.

Deflection of upper surface below chord . $2\frac{3}{8}$ inches
,, of lower surface below its original horizontal position . . $2\frac{1}{4}$,,

Feb. 15th, 9.30 A.M. Temp. 30° 0 F.

Deflection of upper surface below chord . $3\frac{5}{8}$ inches
,, of lower surface below its original horizontal position . . $3\frac{1}{8}$,,

During the whole of this interval, in which the temperature never rose above the freezing point, there was no indication of fracture in the plank, nor did the optical continuity of the ice suffer the slightest interruption. On the 15th it began to thaw, and the bearers having become frozen to the ground, and the plank to the bearers, the suspended portion was unable to yield to the strain produced by its gravitation; and when I re-visited the plank on the afternoon of the 15th, it was broken into half-a-dozen pieces.

These experiments were very rough and imperfect; we intend to renew them on some future occasion, and to conduct them with much greater care and proper mechanical appliances, when we hope to be able to bend an ice-plank double, without destroying its continuity.

The following conclusions may fairly be drawn from them :—

1.—A mass of ice may change its form under strains produced by the gravitation of its particles, without becoming fractured, and without returning to its original form when the strain ceases.

2.—The change of form takes place at temperatures both below and above the freezing point, but is greatly accelerated in the latter case.

I shall not now attempt to discuss the nature of the molecular displacements to which the change of form is due. Their occurrence is indisputable; whether or not they are to be dignified by the name of shearing is a mere verbal question of little moment. In a very able paper in the *Philosophical Magazine* for March 1869, Mr. James Croll adduces good reasons for believing that when a mass of ice has a deliquescent surface, its molecules may experience repeated momentary losses of their shearing force. While, therefore, he admits the conclusiveness of Canon Moseley's reasonings for temperatures below freezing, he conceives that ice at all higher temperatures may shear by its own gravitation. It is evident that the former concession in Canon Moseley's favour cannot now be maintained, and that the point to which our experimental researches should be directed is not what amount of force will suddenly rend asunder the molecules of ice beyond the sphere of their mutual attractions, but what amount of force will produce molecular displacement within that sphere, with time allowed for its operation.

If we conceive an ice-plank, instead of being placed horizontally between bearers, to be laid with its narrowest face upon a plane of small inclination, with its upper edge horizontal, and its ends confined between vertical walls converging in the direction of motion, with its under surface deliquescent, so that friction would almost be annihilated; and if we further imagine the diminution of gravity due to resolution along the plane to be compensated by increasing the length or diminishing the thickness of the plank, the plank would alter its form in a way presenting a striking resemblance to the actual movement of a glacier. Its central portions would move more rapidly than its lateral ones; its surface more rapidly than its base; and when the strain upon its particles exceeded their cohesive power, it would fracture obliquely to the axis of the channel.

If the conclusions drawn from the experiments above described are legitimate, plasticity must be admitted by the side of sliding, and fracture and regelation as one of the constituent elements of the theory of glacier motion, and a more important place in that theory must be assigned to the views of the late Principal Forbes than has for some years been conceded to them.

WM. MATHEWS, Jun.