would have too little contact with ascertained fact to be of much value.

I will take this opportunity of mentioning an alternative theory, which has the advantage of being amenable to experimental test. If we suppose that the rate of transformation of uranium is much diminished by increase of temperature, the quantity of radium and of all the other products will be diminished too, and with it the general rate of heat production inside the earth.

The effect of heat on radium and its products has no direct bearing on the problem. Everything is governed by the primary slow transformation—that of uranium.

There is no experimental evidence on this question so far as I am aware. It could probably be best attacked by comparing the ratio of formation of uranium X at various temperatures. The amount of uranium X which had grown in the course of a few days could be determined by β-ray measurements, which might be made after cooling. R. J. STRUTT.

Sunnyside, Cambridge, February 13.

Ground Ice.

I SEE in your issue of January 30, p. 295, a letter from the Rev. John J. Hampson asking some questions on the subject of ground ice. I should like to say that my father, the late Prof. James Thomson, read a paper on this subject at the Natural History and Philosophical Society of Belfast on May 7, 1862, and I think his paper answers most of the questions. Thus he writes, after reviewing and setting aside several older theories:—"My own view is that the crystals of ice are frozen from the water at any part of the depth of the stream: whether the top, the middle, or the bottom, where cold may be introduced, either by contact or radiation; and that they may also be supplied in part by snow or otherwise: and that they are whirled about in currents and eddies until they come in contact with some fixed objects to which they can adhere, and which may perhaps be rocks or stones or may be pieces of ice accidentally jammed in crevices of the rocks or stones: or may be ground ice already grown from such a beginning.

"That pieces of ice under water have the property of adhering to one another with a continually increasing firmness, and this even when the surrounding water is above the freezing temperature, has been shown in a set of very interesting experiments by Prof. Faraday. I think too that the ready adhesion to the bottom, or to ice already anchored there, may possibly be increased by the effects of radiation, but I am confident that the anchor ice is not formed by crystallisation at the place where it is found adhering."

This paper has never been printed *in extenso*, but I hope soon to bring it out in a volume of collected papers written by my father. JAMES THOMSON.

22 Wentworth Place, Newcastle-on-Tyne,
February 11.

The Stresses in Masonry Dams.

MR. MARTIN at first asserted that my reasoning was wrong on some general principle which I failed to grasp, whereas he has now fallen back on the order of the approximation, and appeals to what he terms an axiom of practical mathematics, which he illustrates by the statement that between o and π a parabola can be found "differing but little from sin x." If by the method of least squares a parabola be fitted to sin x, it will be found to differ by more than 30 per cent. from the ordinate of sin x when $x=5^{\circ}$; whether that difference is material or not depends entirely on what use is to be served by the correspondence.

In the memoir which has led to this controversy I showed that the equation for the stress function V, *i.e.* $\nabla^2\nabla^2 V=0$, was the same for a thin[1] slab and an actual dam. Since writing the paper I noticed that the third equation for the stresses was *apparently* not the same. I now see that this is only in appearance, for the terms which have a coefficient involving different functions of Poisson's ratio for the two cases are

$$\left(\frac{d^2}{dx^2}+\frac{d^2}{dz^2}\right)\left(\widehat{xx}+\widehat{zz}\right)$$

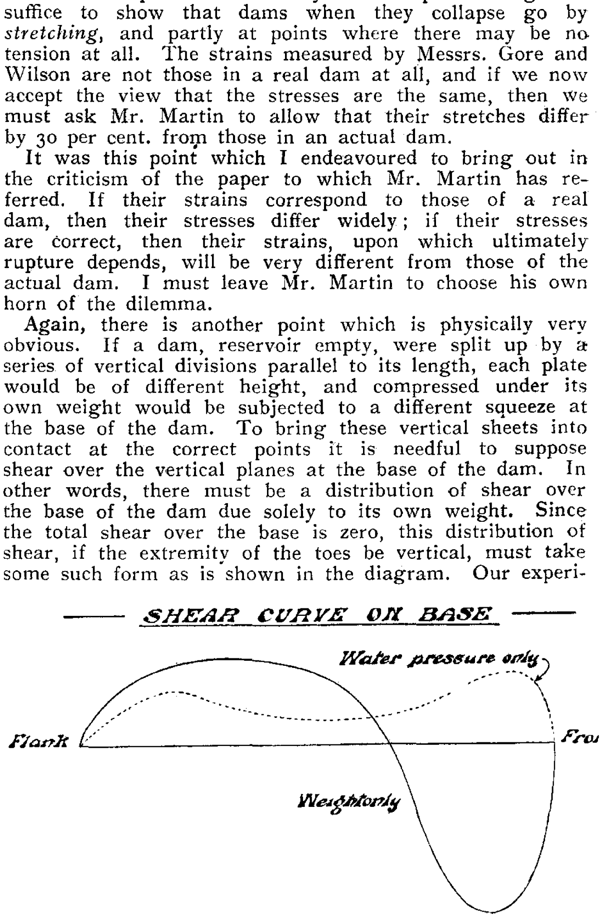
and I find that this vanishes by means of the differential equation for V. Hence, as I stated in my memoir, thin plates can be used to find experimentally the stresses. Mr. Martin is therefore quite correct in his views on this point, although I cannot still agree with his demonstration of the principle.

There are, however, far more vital criticisms to be made of the memoirs recently read before the Institution of Civil Engineers than the mere question of whether the stresses in a slab and an indefinitely long dam differ by 10 per cent. or 20 per cent. A very little experimenting will suffice to show that dams when they collapse go by *stretching*, and partly at points where there may be no tension at all. The strains measured by Messrs. Gore and Wilson are not those in a real dam at all, and if we now accept the view that the stresses are the same, then we must ask Mr. Martin to allow that their stretches differ by 30 per cent. from those in an actual dam.

It was this point which I endeavoured to bring out in the criticism of the paper to which Mr. Martin has referred. If their strains correspond to those of a real dam, then their stresses differ widely; if their stresses are correct, then their strains, upon which ultimately rupture depends, will be very different from those of the actual dam. I must leave Mr. Martin to choose his own horn of the dilemma.

Again, there is another point which is physically very obvious. If a dam, reservoir empty, were split up by a series of vertical divisions parallel to its length, each plate would be of different height, and compressed under its own weight would be subjected to a different squeeze at the base of the dam. To bring these vertical sheets into contact at the correct points it is needful to suppose shear over the vertical planes at the base of the dam. In other words, there must be a distribution of shear over the base of the dam due solely to its own weight. Since the total shear over the base is zero, this distribution of shear, if the extremity of the toes be vertical, must take some such form as is shown in the diagram. Our experiments at University College showed that this base shear due to the weight of the dam only was as important as, and probably more important than, the distribution of shear due to the water pressure.

SHEAR CURVE ON BASE



There is no evidence at all that I can see in Sir John W. Ottley and Dr. Brightmore's recent paper that they have paid attention to this point. They speak of the "original vertical lines on the model," and of measuring the displacement of these lines from "vertical lines on the glass." They speak of the return of the vertical lines on the model to the vertical lines on the glass on the removal of the water pressure. It would appear, therefore, that they have only measured the slide due to water pressure. But to deduce the stresses in the dam they must have the total shear, that due to the weight as well as that due to water pressure. I can find no evidence in their paper of any determination of the shear due to the weight of the dam. They say that the shear along the base is uniformly distributed. This, as Mr. Pollard and I showed in our memoir of last July, is *roughly*, but only roughly,

[1] Only thick plates can be properly used in dam experiments, for thin plates buckle and require a side support which destroys accuracy of experimental result. Even Messrs. Wilson and Gore's plates were at the toe as thick as they were broad.