The method followed was to find the distinctly preglacial valleys tributary in the preglacial stream valley, now deepened by glacial erosion, and occupied by Lake Cayuga. There are several of these with the broad valley and gently sloping sides so characteristic of old valleys, and so different from the steep side post-glacial gorges. Their directions are at all angles with that of the ice motion. If the main valley (the present lake valley) has not been sensibly deepened by the ice, these tributaries should not be rock bottomed. In the present instance the entire valley is found to be underlain by the Devonian shale in place (nearly horizontal); and the bottom of the preglacial valleys are there, at the lake margin, found to be over 400 feet above the deepest point of the lake.

There are three possible explanations of this phenomenon: either (1) the rivers had a fall of over 400 feet in less than a mile, while above this the slope was only very slight, or (2) the lake valley has subsided 400 feet, or (3) it has been deepened by ice erosion. Few will, I think, consider the first two to be possible, and there is evidence that they are not. For fuller details reference may be made to my forthcoming paper.

It seems to me that we have here a reasonable and possible method of testing the value of the rock basin theory, and I believe that its application in other regions will show that ice can, where conditions were favourable, excavate lake basins of large size, and has done so. This conviction comes to me in spite of distinct preconception and prejudice against the theory.

R. S. TARR.

Cornell University, Ithaca, New York, January 15.

Glacial Erosion in Alaska.

REFERENCES in your recent correspondence to my estimate of the rate of erosion by the Muir Glacier in Alaska, call for a supplementary statement. The estimate was made in 1886 by determining the amount of sediment per gallon brought down by one of the sub-glacial streams, and calculating as best I could the area of the glacial basin, the amount of annual rainfall, and the probable waste by evaporation and by the formation of icebergs. The result obtained was the removal of one-third of an inch of rock per annum over the glaciated area.

Since my visit to the Muir Glacier, Prof. H. F. Reid has spent two summers on the same ground with more ample preparations for collecting the facts. His report may be found in the *National Geographic Magazine* (Washington), vol. iv. p. 51. According to his calculation, the erosion amounts to three-quarters of an inch, or nearly three times as much as I had estimated. I have little question that Prof. Reid's estimate is more nearly correct than mine, since my calculation was based upon the removal of sediment from the entire drainage area of the glacial amphitheatre. Prof. Reid, however, rightly concludes that this is full twice as large as the actual bed of the glacier to which the glacial erosion was practically limited. Making that correction, our estimates are in close agreement.

It should be observed, however, that these observations do not bear directly upon the question of the erosion of lake basins by glaciers; for the Muir Glacier, whose sediment was estimated as above, is moving down a slope of about 100 feet per mile. The erosion over this slope, therefore, may be quite different from that at the foot of the glacier as it descends below the water-level into the head of the tidal inlet, where, I should presume, the erosive power would be soon reduced to a small quantity. Still, the mechanical problem involved in calculating the distribution of the force of a descending glacier as it reaches the foot of the incline is too complicated for ready solution. That there is some scooping out of a rocky basin in such cases seems amply proved by the facts which I have quoted in my "Ice Age" (pp. 237-239), from Prof. I. C. Russell, concerning the formation of cirques in the Sierra Nevada Mountains.

In my own observations two or three years ago, however, upon Lake Geneva in Switzerland, I was led to believe that whatever might be true of glacial erosion, attention enough had not been given to the theory of a possible buried outlet leading past Mount Sion and Frangy to Seyssel. Certainly the course of the Rhone across the spur of the Jura Mountains at Fort De l'Ecluse is very suggestive of recent occupancy. Among the great lakes of America there can be but little doubt that Lake Erie and some others owe their existence almost wholly to the choking up of preglacial outlets of the valleys by glacial debris. The great depth of Lake Geneva, however, would render it improbable that it was wholly due to such a cause, and I do not

know that the conditions are such as to permit the supposition made above. I distinctly remember, however, that from the vicinity of Seyssel there was an unobstructed view between the mountains towards Geneva, and that the gravel deposits extend from Geneva far down towards those which appear about the head-waters of the small tributary to the Rhone which joins it at Seyssel. Perhaps this theory has been fully considered and refuted; if so, I have not seen the refutation.

G. FREDERICK WRIGHT.

Oberlin, Ohio, January 17.

On the Equilibrium of Vapour Pressure inside Foam.

It is known that the vapour pressure near a curved liquid surface is different from that near a flat surface, being less near a concave surface and greater near a convex surface than near a flat one. Now, inside foam bubbles the surfaces are approximately flat, except where three bubbles join to form an edge, and along these edges the surface inside any bubble is concave with a very small curvature. How does it happen that equilibrium can exist with the small pressure in these corners, and a larger pressure required near the flat surfaces? In the first place it may be that equilibrium cannot exist, and that all foam is essentially unstable; and it would be almost impossible to disprove this by a direct experiment. If, however, foam can be stable, it seems as if the only conclusion possible were that the flat surfaces will evaporate and thin down, the liquid condensing in the corners, until the flat parts are so thin that they are in equilibrium with a smaller vapour pressure than a thick liquid surface would require. In other words, that the vapour pressure near a very thin film may be less than it can be near a thick one at the same temperature. It is evident that inside or outside a solid box stability must be possible, so that the second alternative is the only solution.

We see a phenomenon of this latter kind in the hygroscopic films that cover glass. Being due to an attraction of the glass for water there results what I am describing, namely, that in an atmosphere incompletely saturated a film of such a thickness can exist that the vapour pressure near it is such as corresponds to the existing vapour pressure in the surrounding atmosphere.

If we knew the connection between the thickness of a film and the vapour pressure near it, it would be possible to calculate the shape of a bubble near a corner. The pressure at any point being that due to the thickness diminished by an amount proportional to the curvature. So that if $f(y)$ be the pressure due to a film of thickness y and r be the radius of curvature of the surface of the liquid near a corner, we get as the equation of the surface

$$f(y) - \frac{T}{r(\delta - \sigma)} = \text{constant},$$

when T is the superficial tension which may be a function of y and δ the density of the liquid and σ of the vapour of the liquid, which latter will depend on the vapour pressure inside the bubble. To determine this and the constant we should

know how much liquid we have at our disposal ($V = \int y dx$) to lie round the bubble and to supply vapour inside, and it would appear that inside a cubical box falling freely, for instance, a bubble would always be spherical unless the quantity of liquid were sufficiently small to require the sides of the bubble to be flattened against the sides of the box; i.e. unless the volume of liquid were less than $1 - \pi/6$ times the volume of the box.

In connection with my letter in last week's *NATURE*, I may suggest that a possible cause of warming of solid powders when mixed with liquids, is that the solids have already got a film of water over their surfaces, which being on the outside in contact with air, is at least on its outer surface in tension, and that this enormous area of air-liquid film disappears when the solid is immersed in liquid, and that the heat is due to the extinction of this great film. It would require very careful measurements of the heat evolved and estimates of the area of the film concerned, to decide whether this would account for the heating observed. In my former letter I assumed that liquids would soak up into dry powders, and that these latter warmed liquids when mixed with them. The suggestion I am now making would account for a warming being produced by damp powders, or by spray or cloud falling into a liquid.

G. F. FITZGERALD.

Trinity College, Dublin, January 29.