June 7, 1855.

The LORD WROTTESLEY, President, in the Chair.

The Annual General Meeting for the Election of Fellows was held.

The Statutes respecting the election of Fellows having been read, the Rev. Dr. Booth and William Tooke, Esq., were, with the consent of the Society, appointed Scrutators to assist the Secretaries in examining the lists.

The votes of the Fellows present having been collected, the following gentlemen were declared duly elected:—

Arthur Connell, Esq.
William Farr, Esq.
William Lewis Ferdinand Fischer, Esq.
Isaac Fletcher, Esq.
William John Hamilton, Esq.
John Hawkshaw, Esq.
John Hippisley, Esq.
James Luke, Esq.
A. Follett Osler, Esq.
Thomas Thomson, M.D.
Charles B. Vignoles, Esq.
Charles Vincent Walker, Esq.
Robert Wight, M.D.
Alexander William Williamson, Esq.
George Fergusson Wilson, Esq.

The Society then adjourned.

June 14, 1855.

The LORD WROTTESLEY, President, in the Chair.

The following nobleman and gentlemen were admitted into the Society:—

His Grace the Duke of Argyll.
Arthur Connell, Esq.
William Farr, Esq.
William Lewis Ferdinand Fischer, Esq.
Isaac Fletcher, Esq.
John Hawkshaw, Esq.
John Hippisley, Esq.
James Luke, Esq.
Charles B. Vignoles, Esq.
Alexander W. Williamson, Esq

The Royal Society is collaborating with JSTOR to digitize, preserve, and extend access to *Proceedings of the Royal Society of London.* www.jstor.org

The following gentlemen were recommended by the Council for election as Foreign Members:—

Gustav Lejeune Dirichlet.	Heinrich Rathke.
Julius Plücker.	Carl Rümker.

The President announced that Edward Tuson, Esq., who at last Anniversary had ceased to be a Fellow of the Society in consequence of the non-payment of his subscription, had applied to the Council to be reinstated, alleging that unforeseen circumstances had prevented him from paying the annual contribution. The President therefore, in accordance with the Statutes, gave notice that the question of Mr. Tuson's readmission would be put to the vote at the ensuing ordinary meeting.

The following communications were read:—

I. "Remarks on the Rev. H. Moseley's Theory of the Descent of Glaciers." By James D. Forbes, D.C.L., F.R.S., Corr. Inst. France, and Professor of Natural Philosophy in the University of Edinburgh. Received May 22, 1855.

In a paper "On the Descent of Glaciers," communicated to the Royal Society on the 19th of April, 1855, and printed in their Proceedings, the Rev. Henry Moseley has proposed an explanation of that phenomenon.

The first part of his paper contains a lucid description of the gradual motion of a sheet of lead covering the roof of Bristol Cathedral, which he ascribes (I have no doubt justly) to the successive expansions and contractions of the lead by atmospheric temperature. He explains the influence of the slope of the roof and of the measure of friction upon the motion with his customary precision and clearness. He also finds for the probable measure of the effect or creeping motion of the lead, a quantity which, considering the imperfect nature of the data with regard to temperature, agrees sufficiently well with observation.

In the latter and shorter part of the paper is a transition to the

case of glaciers, whose motion over their beds may, he thinks, be accounted for in the same way, namely, by the alternate contraction and expansion of the ice by diurnal changes of temperature, and he then enters into certain calculations founded principally on data contained in my 'Travels in the Alps of Savoy' in confirmation of this view.

Entertaining as I do the highest respect for Mr. Moseley's eminent attainments as a theoretical mechanician, it is with extreme regret that I find it necessary, in maintenance of the views regarding glacier motion which I have elsewhere advanced, and in the interest of scientific truth, to show (as I believe I can) that Mr. Moseley has been led, apparently by a sudden inadvertency, to uphold an opinion completely indefensible.

I must first object to Mr. Moseley's description or definition of a glacier, as calculated to mislead the inquirer: he says (p. 339), "glaciers are, on an increased scale" [compared to the sheet lead covering of a roof], "sheets of ice placed upon the slopes of mountains." There are certainly some inconsiderable glaciers of the second order to which this description might possibly apply, with the exception of the small thickness inferred by the word "sheet;" but the true glaciers, whose theory has been so often discussed (which theory must evidently likewise include that of glaciers of the second order), cannot fairly be called either *sheets* of ice nor be accurately described as lying *on* the slopes of mountains. They are vast icy accumulations whose depth bears a considerable proportion to their breadth, and which fill mountain ravines or valleys.

Glaciers are very generally hemmed in by precipitous rocks which determine their *contour* or ground plan; they have often to make their way through contracted gorges where the ice occupies (as in the case of the Mer de Glace of Chamouni), within a short distance, a channel but half as wide as it did before. Yet the glacier, preserving its continuity as a whole, expands or contracts in conformity with the irregularities, not only of its lateral walls, but of its bed, forcing itself over obstacles, or even occasionally allowing itself to be cleft into two branches by them, and closing again into a united mass after the insular obstruction has been past. To speak of such resistances of the channel to the progress of the ice as mere *friction*, or of a glacier considered as a solid body and in its whole extent

(or in any considerable part of it) as having an *angle of repose*, as in the case of a substance with a flat base resting on an inclined plane, is evidently inadmissible and tends to mislead. The valley of the Mer de Glace might have almost any possible inclination before the ice would tend to slide out of it *en masse*, for it is moulded to every sinuosity or protuberance of the bed, whether vertical or horizontal. Let Mr. Moseley imagine a sheet of lead having the ground plan of the *Mer de Glace* and confined by margins of wood accurately adapted to it, and he will see that unless lead were so ductile as to be entitled to the appellation of a semifluid, no motion could possibly result, however great might be the slope on which it lay.

I am sorry to find that Mr. Moseley denies entirely (p. 341) the viscous or plastic structure of a glacier as "not consistent with the fact that no viscosity can be traced in its parts when separated." The answer to this objection seems to be merely this; that the viscosity, though it cannot be "traced" in the parts, *if very minute*, nevertheless *exists* there, as unequivocally proved by experiment on the large scale, or even on spaces several yards or fathoms in extent*. The plastic condition of a glacier is, as I have repeatedly stated, no longer an hypothesis, but a *fact*, since I have in many places demonstrated that, account for it as we may, different portions of the same continuous mass of ice are moving at the same moment with different velocities. That a small piece of ice is not sensibly plastic, is not more strange than that the fine blue colour so perceptible in the glacier totally vanishes in its constituent fragments. That *ductility* and *fragility* are not incompatible qualities, is shown by the fact, that sealing-wax at moderate atmospheric temperatures will mould itself (*with time*) to the most delicate inequalities of the surface on which it rests, under a pressure of not more than half an inch of its substance, but may at the same time be shivered to atoms by a blow with a hammer.

The question of plasticity, however, affects only *mediately* Mr. Moseley's theory of the primary cause of motion by dilatation and contraction. According to the views I support, the dilatation and contraction of the ice of glaciers (assuming it to exist) would be inefficient to move the mass unless it moved plastically; and if it moves plastically, the supposition of its thermal expansion is, at all

* See Phil. Trans. 1846, p. 162, and Phil. Magazine (1845), xxvi. p. 414.

events, superfluous, since gravity is in that case a sufficient moving force.

But, it will be argued, if the ice be really acted on by heat and cold as Mr. Moseley supposes, it is a *vera causa* of motion and cannot be neglected. And here we join issue respecting the physical theory proposed.

Mr. Moseley's explanation of the descent of the lead on a roof at an angle much below that at which motion could take place by gravity, friction being allowed for (the *angle of repose*), amounts to this, that every increase of temperature of the mass by the heat of the day expanding it, pushes the lower end downwards *more* than it pushes the upper end upwards; whilst the cold of the night retracts a little the lower end, but (being favoured by the slope) it pulls down the upper end more than it had been pushed up during the heat of the day, and thus by a species of vermicular motion impels the body down the inclined plane. The motion is calculated from a formula including the absolute expansibility of lead, the slope of the roof, the angle of repose, and the *diurnal range* of temperature. Taking then corresponding data for the Mer de Glace of Chamouni, assuming 30° to be the angle of repose of a glacier upon its bed, taking the expansion of ice to be nearly double that of lead (according to experiments made at St. Petersburgh), and the daily range of temperature *of the ice* to be the same as that *of the air* observed by De Saussure on the Col du Géant in the month of July, Mr. Moseley calculates the daily descent of the glacier opposite the Montanvert and compares it with my observations.

Waiving for the moment all other objections, *can we possibly attribute to the ice of the entire mass of this vast glacier an average daily range of temperature of* $4\frac{1}{4}°$ *of Reaumur or* $9\frac{1}{2}°$ *of Fahrenheit*? The idea seems to me to be perfectly untenable.

The expansion and contraction of ice by heat and cold can of course only take place *below* the freezing-point, or 32°. Let it be percolated by water as it may, it cannot rise *above* that temperature nor expand in the smallest degree. But it is a matter perfectly notorious, that, at least in summer, and throughout the whole extent of the Glacier Proper, and even far into the region of the *névé*, the glacier is charged with percolating water derived from superficial fusion. Mr. Moseley admits this, and even attributes the diurnal

oscillation of temperature which he assumes, to the action of water, as in the following passage: "Glaciers are, on an increased scale, sheets of ice placed upon the slopes of mountains, and subjected to atmospheric variations of temperature throughout their masses by variations in the quantity and the temperature of the water, which flowing from the surface everywhere percolates them" (p. 339). This action therefore clearly brings the temperature of the ice up to 32° during the day. But how is the cold of the night to operate in reducing the temperature of a mass of ice certainly from 300 to 600 or more feet in thickness through the enormous average depression of $9\frac{1}{2}$ degrees? The water so efficient by its percolation in *raising* the temperature (if necessary) to 32°, being frozen, is now powerless. Cold can be conveyed downwards, or to speak more correctly, Heat can be transmitted upwards through the ice only by the slow process of conduction, and this on the supposition that the depression of superficial temperature is all that the theory might require. But how stands the fact? Mr. Moseley quotes from De Saussure the following *daily ranges* of the temperature of the air in the month of July at the Col du Géant and at Chamouni, between which points the glacier lies.

At the Col du Géant 4°·257 Reaumur.
At Chamouni ..... 10°·092 Reaumur.

And he assumes "the same mean daily variation of temperature to obtain throughout the length" [and depth?] "of the Glacier du Géant which De Saussure observed in July at the Col du Géant." But between what limits does the temperature of the air oscillate? We find, by referring to the third volume of De Saussure's Travels, that the mean temperature of the coldest hour* (4 A.M.) during his stay at the Col du Géant was 0°·457 Reaumur, or 33°·03 Fahrenheit, and of the warmest (2 P.M.) 4°·714 Reaumur, or 42°·61 Fahrenheit†. So that even upon that exposed ridge, between 2000 and 3000 feet above where the glacier can be properly said to commence, the air does not, on an average of the month of July, reach the freezing-point at any hour of the night. Consequently the *range of temperature attributed to the glacier is between limits absolutely incapable of*

* The observations were made every two hours day and night.

† The corresponding extremes at Chamouni are 53°·25 and 75°·96 Fahr.

effecting the expansion of the ice in the smallest degree. This would of course be still more applicable if we take the mean of the temperatures at Chamouni and the Col du Géant to present the general atmospheric conditions to which the glacier is exposed.

It is in summer that the glacier moves fastest: it is with my observations of motion *in July* that Mr. Moseley compares the results of his theory: and therefore it is of no avail to say that there are periods of the year when congelation penetrates at night some inches, or even it may be some feet into the ice, and when therefore the sensible heat of the glacier may be considered to vary, though, if regard be had to its vast thickness, it must be on an average and in the most extreme circumstances to an absolutely inappreciable degree.

Lastly, Mr. Moseley, whilst condemning in the following passage the theory of glacier motion by the dilatation of water in the interstices of the ice, clearly passes sentence on his own, which could not come into action until the other had already produced its effects: "The theory of Charpentier, which attributes the descent of the glacier to the daily congelation of the water which percolates it, and the expansion of its mass consequent thereon, whilst it assigns a cause which, so far as it operates, cannot, as I have shown, but cause a glacier to descend, appears to me to assign one inadequate to the result; for the congelation of the water which percolates the glacier does not, according to the observations of Professor Forbes, take place at all in summer more than a few inches from the surface. Nevertheless it is in summer that the daily motion of the glacier is greatest." (Moseley, Proc. R.S. vol. vii. p. 341.)

II. "Researches on the Foraminifera.—Part I. General Introduction, and Monograph of the Genus *Orbitolites*." By William B. Carpenter, M.D., F.R.S., F.G.S. &c. Received May 21, 1855.

The group of *Foraminifera* being one as to the structure and physiology of which our knowledge is confessedly very imperfect,