and the Geological Survey of the range has been executed by my colleague Mr. H. H. Howell."

From this it will be seen that the age of these rocks was determined by no less an authority than Professor Sedgwick, the founder of the Cambrian system in Wales, supported by his friend and pupil Professor Jukes, and the ground of this determination is the resemblance of the Forest beds to the Cambrian beds of North Wales. Who amongst men should have been better able to recognize this resemblance than the distinguished founder of the Cambrian system? Nor was the opinion of Professor Jukes less entitled to weight, as Jukes spent several years of his Survey life amongst the old rocks of North Wales. For six months of my own Survey life I was his companion and pupil in the same region; and when a few years afterwards I had opportunities of examining the rocks of Charnwood Forest I was able to recognize the similarity of physical character, upon which Sedgwick and Jukes to a great degree relied in determining the geological age of the rocks; nor, notwithstanding what has been written by more recent authors, am I able to see good reason to alter the opinion I then held.

On reading over again the brief description of these rocks (as far as regards their non-plutonic masses), written at a time when their characters were vividly impressed on my mind, and when there was no dispute regarding their age, I see how similar is the description to one which might be applied to the Lower Cambrian beds of Merionethshire. "Crossing the axis we find the beds as far as Upper Black Brook, consisting of bluish-purple and green slates, of a coarse character, alternating with fine grits." The eruptive masses are then described, and their resemblance to the contemporaneous and intrusive masses of North Wales is pointed out (p. 12). Since these words were written much new light has been thrown on the composition and structure of the igneous masses by Professor Bonney and other petrologists. In 1860, the application of the microscope to thin sections of rock was scarcely begun; but the determination of the geological age of the whole series of the Charnwood rocks is not affected by microscopic definitions of the eruptive beds.

I have still much hope that fossils may yet be discovered in these old rocks. After the discovery of a Trilobite in the slates of Llanberis no one need despair; and any young geologist who desires to break fresh ground and win his spurs would do well to turn his attention to the slates of Charnwood Forest.

EDWARD HULL.

THE MAKING OF MOUNTAINS—A REPLY TO MR. MELLARD READE.

SIR,—May I ask you to allow me space to answer, as briefly as possible, a letter, in the GEOLOGICAL MAGAZINE for December, 1894, in which Mr. Reade attacks my theory on the making of mountains.

I must in the first place object to a statement implying that I have, to a certain extent, changed front. Mr. Reade remarks: "Mr. Vaughan *now* says, that he does not rely upon decrease of volume due to pressure, but upon the transference of material," etc. I *never did* rely on decrease of volume, as Mr. Reade will see if he reads my original article more carefully, whereas, again and again, I insisted upon the main principle of transference of material.

Further on I read, that Mr. Reade has found out that the maximum elevation which could possibly be produced, according to my theory, is 1000 feet. Absurd though I believe this result to be, from my own calculations, I shall be most grateful if he will indicate his method of analysis, as the problem seems to present exceptional mathematical difficulties. Looking at the question in its simplest aspect the analysis employed must take account of each of the following phenomena:—

(1) The gradual contraction of a spherical cap with the consequent production of tension at its boundaries and frictional stress beneath.

(2) The inbending of the crust, and consequent outshear of material from beneath, with the necessary production of frictional stress. For this outsheared material must set up tensions and compressions in the superincumbent crust; the tensions being directed from the centre of the area outwards, and the compressions producing folding and ridging, principally at the boundaries, in the regions immediately preceding those of upheaval.

(3) The rate of contraction beneath the area, which will necessarily be made more rapid by the extension of material, and the consequent exposure of hotter layers to the cooling influence.

In fact, we have to deal with elevation due to distortion of the inner nucleus, and this cannot be limited to the amount by which the suboceanic shell can contract; for the viscous stresses due to outshear of subjacent material will compress the bounding parts of the area into folds and ridges. The only rough limit which can be assigned is, that the volume of the elevations above sea-level cannot exceed the volume of the ocean.

Another objection which my critic urges is that, since my theory demands that the surrounding continental crust should yield to the intrusion of rock underneath, the area could not bend inwards, because it has no anchorage. Here he has totally neglected the friction beneath the sinking area, arising from the outshear of material, which it seems to me will be powerful enough, not only to hold the area, but to fold and ridge its boundaries.

Lastly, Mr. Reade seems to argue that, granted the anchorage, the tension of rock would not be sufficient to allow of enough pressure to produce any tending inwards. In this reasoning he has entirely omitted the vastly important time factor; for, in reasoning on all geological phenomena, the length of time available is the most important point in the problem; since there are few geologists who will not grant that the making of a mountain range is spread over a vast number of years.

Now, it is well known that any practically solid body, acted upon by differential stresses, however small, will yield to their influence, if the time be sufficiently prolonged. Hence there is no need for the tension to exceed or even approach the breaking tension of steel.

To illustrate the great importance of the time factor, I will assume the extreme case, that entire rupture is immediately produced all round an isolated suboceanic area, in spite of the natural effect of the viscous friction beneath. The mechanical problem is then reduced to the following: An inner nucleus of highly heated material is surrounded by a spherical shell of rock. From this outer shell a spherical cap is cut out, reduced in area, and replaced, so that it does not quite fit and is in consequence entirely separated from the rest of the shell; every particle of shell and cap being subjected to the attraction of gravity tending towards the centre of the sphere.

The forces acting upon the non-fitting cap are simply its weight and the pressure of the subjacent nucleus; and it is to be noticed that, in the actual case, the weight of the suboceanic crust is almost certainly greater than that of the continental (see Pratt, "Figure of the Earth").

In the case of any portion of the rest of the shell the forces are: weight, resistance of nucleus, *together with* the supporting compression at its boundaries, arising from the fact that the area forms part of a continuous shell.

Hence the pressure on the nucleus will be greater under the suboceanic than under the continental crust. Thus, remembering that there is practically no limit to the time factor, transfer of material will take place slowly from beneath the oceanic to beneath the continental regions, and this the more easily on account of the high temperature at which the subjacent rocks exist.

I believe that I have now answered Mr. Reade's objections, which seem to me somewhat hastily conceived and dogmatically expressed; but I must confess to being unable to understand how the last sentence in his letter has any bearing on the points at issue. He says, alluding to my theory: "It seems to me *very like* the case of a man trying to lift himself up by pulling at the chair he is sitting upon." I cannot answer this objection, because I cannot imagine which is the man, nor which the chair beneath him; nor can I see how one is attempting to pull the other. As Mr. Reade has never made any reply to my criticism of the theory which he advocates, may I definitely ask him how he explains the under-mentioned geological facts?

It is well known that, during many periods, great thickness of sediment have continuously accumulated, every part of which must have been laid down in shallow water; and that contemporaneously the area of denudation has been rising.

Does it not follow that, if two miles (say) of rock has been deposited continuously under shallow-water conditions, that the sea-flow must have been depressed through that distance, and that, consequently, an equivalent amount of subjacent rock has been squeezed out? I entirely fail to see how this is in accordance with Mr. Reade's theory, that, as sediment accumulates, the area of sedimentation rises; for, considering the slow rate at which deposition goes on, the elevational effects (if any) due to accession of heat must be contemporaneous with sedimentation.

Again, referring to the Himalayas and the plains south of that range, Mr. Osmond Fisher says (Ch. x., "Physics of the Earth's Crust): "The conclusion seems irresistible that, corresponding to the long though occasionally interrupted depression of the plains, a correlative elevation of the great range, which has supplied the deposits, has been going on." This seems to me totally at variance with Mr. Reade's theory of contraction by denudation.

9, PEMBROKE VALE, CLIFTON. ARTHUR VAUGHAN.

OBITUARY.

PROFESSOR ALLEN HARKER, F.L.S.

BORN 1848. DIED DECEMBER 19TH, 1894.

BY the early death of Allen Harker, the Royal Agricultural College at Cirencester has lost one of its most active and popular Professors. Appointed in succession to Dr. Fream, in August, 1881, Prof. Harker soon gained the esteem and affection of all his students by his admirable courses of instruction, both in the field and in the lecture-room, and by his genial character. It is no easy task now-a-days to teach Botany, Zoology, and Geology; and even when lessons are restricted to their special applications to Agriculture, the work of the Professor is necessarily of an arduous nature. Prof. Harker, however, carried on his labours with much enthusiasm, and devoted all the time he could to researches on those subjects with which he had to deal in his lectures. He made known, through the Proceedings of the Cotteswold Club, of which he was an active member, many new facts in the local geology. He first drew attention to the fine exposure of the Kellaways Beds in a new railway cutting at South Cerney, where many large "doggers" or concretions of calcareous sandstone were opened up, and where a number of fossils were obtained. Other sections of Cornbrash, Forest Marble, and Great Oolite displayed in cuttings along the same railway between Cirencester and Chedworth, were also described by Prof. Harker, and he discovered in the Great Oolite traces of the organism determined to be *Solenopora* by Prof. H. A. Nicholson, and subsequently described as *S. jurassica* by Dr. Alexander Brown.[1]

In Cirencester itself the records of various well-borings occupied Prof. Harker's attention, and he was enabled by the account of the strata and their fossils, to determine the presence of a small faulted tract of Oxford Clay and Kellaways Beds that had previously escaped notice.

When, in 1887, during the Presidency of Mr. F. W. Rudler, the Geologists' Association paid a visit to Cirencester and its neighbourhood, Prof. Harker acted as guide in the excursions made to Birdlip, to the Royal Agricultural College, and to South Cerney.

He died, after a painful illness, on December 19th, 1894, aged 46.

[1] See Harker, Proc. Cotteswold Club, vol. x. p. 89; H. B. Woodward, Memoir on the Lower Oolitic Rocks of England, p. 290; and Brown, GEOL. MAG. Dec. IV. Vol. I. 1894, p. 150.