pretation of the history of these phenomena as sudden and cataclysmic. Sir Henry goes on to produce evidence of his postulated concurrent elevations of mountain chains, which, as he says, were very rapidly, if not suddenly, elevated at the same period, viz. the close of the Mammoth age.

Again, he observes:—"Just as I ventured to argue that the upheaval of the Ural Mountains, of the Altai, and of the American Cordillera was rapid, if not sudden, and marked by every sign of cataclysmic change, so also do I hold the corresponding subsidences to have been rapid, if not sudden." No ambiguity of expression here; Sir Henry seems to outdo his "old masters" in his appeal to catastrophism to explain his difficulties. He, however, naively says, "Evidence on such a point is not always easy to find"; just so, because the facts are all the other way. Mountain ranges were not formed in a day, but are the result of long-continued upward movements, of periods of repose, and also of depression.

It may be asked, what would be the effect of such sudden and violent terrestrial disturbances on the organic life of the globe? Undoubtedly great climatic changes would be induced, unfavourable and destructive to any unfortunate animal or plant that might be then existing; as no time would be given to the organism to adapt itself to its altered environment. Where are the signs and evidences of such destruction of life?

But to return to our mountain-uplifts—the very structure of mountain ranges, their various strata of different geological ages, their successive lines of cliff and other evidences of long erosion, all requiring time for their production, prove that their movements, as with subsidences, have, as a rule, been very slow and intermittent.

There was undoubtedly a very pronounced upward movement of a large area of North America at the close of the Tertiary period, to which American Geologists, with great show of probability, ascribe that severe climatic change which resulted in what is known as the "Glacial age"; but, if this continental uplift be the *cause* of the Glacial age, it does not square with Sir Henry's postulate as to the geological date of the changes of level which resulted, as Sir Henry assumes, in the destruction of the Mammoth at the *close* of that period.

It will be needless, I think, to pursue this subject farther, or to attempt to lighten "the burden of the parable which Sir Henry has been trying to preach" (I would rather say paradox). His recent pleadings in favour of his pet postulate of the "Mammoth and the Flood" appear to me as unreliable and unconvincing as those of his many previous contributions to the pages of the GEOLOGICAL MAGAZINE on the same subject.

VIII.—PROBLEMS CONNECTED WITH A COOLING EARTH.

By ARTHUR VAUGHAN, B.A., B.Sc.

IN the GEOLOGICAL MAGAZINE for June and July of this year, I stated a new theory to account for the inequalities of the earth's surface, and, at the same time, offered a criticism of Mr.

Reade's theory, as epitomised by him in the May Number of the same Journal. In a short reply, contributed by Mr. Reade to the September Number, he shows so clearly that he has totally misunderstood the physical reasoning upon which my theory is based, that some rejoinder is absolutely demanded.

In criticising my theory, he asks: "How then could the underlayers, by shrinking, exert such a huge pressure on the interior as to actually *compress the materials of the earth into a smaller volume?*" and, a few lines further on, he proposes that we should "*work out the decrease of volume which would result from a given contraction of a shell of steel, 30 miles thick, acting on a sphere of the size and composition of our earth.*"

Did I rely upon actual decrease of volume due to pressure, my theory would, of course, be absurdly untenable; but my reasoning is based upon the *transference* of material from beneath a surface of great pressure to below a surface upon which the pressure is much less. This I have so frequently reiterated in my paper that I could quote from almost any page; perhaps the following extract, taken from my first paper, will suffice: "The underlying material will be, so to speak, squeezed out, and this will cause a real transfer from under the sinking area to beneath the surrounding regions."

To my criticism of his own theory Mr. Reade has not replied, but he has, however, objected to my attempt to show that the reasoning he employs is hardly sufficient to disprove the old contraction theory. Mr. Reade takes exception to a calculation, based upon his own figures, as to the elevation which could be produced from an outer shell too large, by a certain amount, for the interior sphere. I took a special case, and supposed the outer shell to be drawn out over the interior sphere, somewhat in the shape of a balloon, so as to produce over a certain large area a vast hollow cone-shaped elevation. I cannot understand how the very simple calculation necessary can be called in question, and I can only suppose that Mr. Reade has assumed that any elevation must be solid. If, however, we accept the theory of elevation by the folding due to lateral pressure, it is very difficult to see how all vacuities could be avoided, and yet the known dips of anticlines produced. It is, consequently, necessary in considering any elevation, which is not actually a maximum, to assume that elevation to be hollow.

The last point raised by Mr. Reade is one of much greater interest, and it directly introduces the main object of this paper. Stated in its simplest form, the physical problem is:—What would be the behaviour of a contracting shell closely fitted round a sphere unalterable in volume? (This is, in reality, the same as supposing such a shell to surround a less rapidly contracting interior.) Here we can easily distinguish two cases.

First, the shell may be contracting unequally at points whose distance from the centre is the same. This is the actual problem in the case of the earth, as I have shown in a former paper; and the results I believe to follow may be briefly recapitulated.

Any contracting area must attempt to become smaller, and this

object could be attained in two ways; either by splitting, that is, by overcoming the forces of cohesion between separate particles, or by moving bodily inwards, so as to become flatter and therefore smaller. The resistance to this last-named effect will arise from the pressure of the interior sphere, and in the case we are considering, of an unequally contracting shell, the effect will be to subject the interior sphere to pressure at all points, but to much greater pressure under the more rapidly contracting areas than under those whose contraction is less. The result seems clear, that the interior sphere will become deformed; material being transferred from under the areas of great pressure to beneath those of less, with the after effects I have stated in a former paper. The mechanism by which the solid interior sphere would thus become deformed can be illustrated by the consideration of two layers of closely touching spherical balls. If we suppose pressure to be applied over any area of the upper layer, the lower layer of balls beneath that area will be pushed asunder, and those which lie on the borders of the area of pressure will wedge themselves between the layers of balls surrounding the area, and thus produce a vertical elevation around the area of pressure.

Secondly, suppose an ideal case, that the shell contracts equally at all points equally distant from the centre. It follows that the pressure exerted upon the interior sphere will now be the same at all points of its surface, and, consequently, no transfer of material from areas of high to areas of low pressure can take place. The only relief to the forces of contraction can now be found in rupture or deformation.

Either of two effects may ensue. In the first place, the shell may, in contracting, split and each separate portion then contract normally, so that its contraction in any direction is linear. It is to be remarked that any want of homogeneity, or, in other words, any slight differences in contractibility, which may exist among the materials of which the shell is made, will aid in producing this effect. Secondly, the shell may stretch without splitting. This is a case of great interest as being the one usually assumed in problems dealing with the contraction of the earth's crust.

This assumption amounts to supposing any shell to contract by splitting, and then the separate portions to be pressed together again by the pressure of the overlying mass; much as if a very heavy roller had been pushed over their surface.

Now this second process will obviously evolve heat by the mutual rubbing of particle over particle; so that each shell must be regarded as an origin of heat, of which no account is taken in using the ordinary differential equation of conduction. The general effect will be to increase the time from first consolidation.

For, let us suppose the two processes to take place perfectly independently. First, let us suppose the shells to contract by splitting, so that no portion is in any way deformed, but decreases in volume, always remaining similar to itself. If, then, we further assume the conduction of heat to be unaffected by the vacuities so

formed, that is to say, if we suppose the heat to be uniformly conducted through the mass, and not to flow mainly along the separation surfaces (as is the case in the flow of electricity through a solid cylinder), we shall have the conditions which are assumed in the equation employed for solution.

Now let us imagine all the separate pieces of each shell rolled together; the further any shell is from the centre the greater will be the volume per unit area to be filled up, and, consequently, the greater the amount of heat evolved by viscous friction. Hence the difference between the temperatures of two consecutive shells, at any time, will be, in reality, less than that, had the first process alone come into play. Thus it would really take longer to establish any given temperature gradient; which amounts to saying that the time from consolidation would be increased. To make allowance for this new factor we should require some data as to the heat evolved by viscous friction in the rolling out of sheets of rock material.

TIME FROM CONSOLIDATION.

Since physicists and geologists differ considerably in the number of years which they would allow for the world's age, it is a matter of great interest to see whether the numerical results arrived at by physicists should not be considerably increased.

I need scarcely remark that, in what follows, I am in no way concerned with the effect which any of the alterations suggested below may have upon the position of the theoretical level of no-strain; for I have already expressed my belief that the main elevations and depressions of the earth's surface have been caused by differential contraction.

In the determination of the time from first solidification, heat is supposed lost solely by conduction from within outwards, and no mechanical deformation or change of physical state is accounted for. Now, as I have shown above, heat must be generated in that settling together, which is considered to be the probable outcome of contraction, and this extra heat will, as I have shown, tend to increase the time from consolidation. So far there is a correction to be applied on account of the insufficiency of the equation employed to express all that actually takes place, for this equation takes account only of heat actually conducted from point to point, and does not recognise any heat originated within the cooling mass.

Again, the final result arrived at is, that the time varies as the square of the temperature of solidification directly, and as the co-efficient of thermometric conductivity inversely. The temperature of solidification is assumed as 7000° F., and the coefficient of conductivity as 400. Both these quantities are supposed constant throughout the mass considered, and hence must be taken to have their average values throughout that mass.

But when the Earth was molten, the pressure at any point within a few hundred miles of the surface, was practically that due to the weight of the overlying mass; hence it follows that the molten

rock would solidify at a very much higher temperature than at the surface. The average value assumed must represent the temperature at a great depth; in fact, the constant multiplier of the integral, which occurs in the equation connecting temperature, depth, and time, is found by assuming that the temperature of solidification is the average value which it has at a very great depth. It is, then, quite beside the question to employ the temperature of solidification of slag or other molten rock at the surface, for we really require to know at what temperature the rock would solidify under a pressure of, say, a hundred miles of rock. At the surface molten slag solidifies at about 3000° F.; it seems then, that the assumption of 7000° F. for use in the problem under consideration, leans very much on the side of under-estimation.

Again, the conductivity or, in other words, the number which represents the amount of heat which escapes through a given thickness of rock in a given time is assumed as 400° , a number obtained by experiments on rocks at the surface. But the greater the temperature, the greater must be the instability of the mean centre of oscillation for each molecule, and this instability must increase until the fusion point is reached, when the molecules have free motion over each other. Hence, the hotter the rock, the more heat will be absorbed in molecular action, and, consequently, the less will be the heat conducted through in any given time; or, in other words, it seems probable that the conductivity must diminish as we reach a greater depth beneath the surface. It follows, that the average value of the conductivity must be much less than its surface value. But any decrease in this value increases the time in the same ratio. A further cause acting in the direction of increasing the time has already been pointed out, though probably of no great weight and not worthy of consideration, beside the corrections suggested above. This consists in the conduction being diminished when heat has to traverse rocks stratified or laminated transversely to the flow of heat. Since the rocks near the surface have their separation planes, as a general rule, inclined at a low angle, the stratified rocks of the crust must conserve the heat passing through by conduction; though this will be, to a small extent, counteracted by the existence of vertical joints which allow a more rapid loss by a kind of surface creep.

In fine, the geologist and biologist have a right to demand of the physicist first, that all losses of heat, other than those considered in his calculations, should be shown to have a negligible effect on the result; and, secondly, that the numerical data he assumes should be at least greater than the probability, not less. Results obtained by the substitution of values, formed on considerations of phenomena taking place under atmospheric pressure, and, as in the case of the determination of conductivity, at low temperatures, can lead to nothing but misconception. There seems no reason why the estimate of 100 million years should not be increased to 1000 million, which would almost allow as much time as the evolutionist desires.