

action. I would suggest that in all probability, even if these materials did sink into the ground, its loose constitution would prevent the formation of any deposit as rich as Black Earth. Owing to the matting of the roots above-mentioned, it is always necessary to remove the upper layer of black soil round any spot where trees are to be planted. Mr. Christy has maintained that trees on the treeless plains grow readily. Mr. Rodway admits this to be true in part. For Russia, I may venture to go further, and say that Black Earth is in many respects absolutely inimical to the growth of wood or forest.

*(To be continued in our next Number.)*

VII.—REMARKS ON MR. MELLARD READE'S ARTICLE "ON THE RESULTS OF UNSYMMETRICAL COOLING AND REDISTRIBUTION OF TEMPERATURE IN A SHRINKING GLOBE AS APPLIED TO THE ORIGIN OF MOUNTAIN RANGES."

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

*[Addendum to paper on Corrugations of Earth's Surface.]*

**A**FTER I had written the above dissertation and sent it for publication my attention was kindly drawn to a paper, dealing with the same subject, communicated by Mr. T. Mellard Reade, C.E., F.G.S., to the Liverpool Physical Society, and since republished in the May number of the *GEOL. MAG.* for this year (p. 203).

As I was not aware, when engaged upon my own paper, of the views which Mr. Reade had expressed, I feel that some discussion of the theory put forward by that writer is of necessity demanded. Since, however, Mr. Reade's views are extremely antagonistic to those which I have already endeavoured to explain, such discussion will find its most convenient place as a brief addendum to what I have already written.

Mr. Reade's paper opens with a statement of some results arrived at by Lord Kelvin in his classical paper on the "Cooling of the Earth"; since the utmost importance is necessarily attached to the conclusions arrived at by so eminent a physicist, it is very important that such results should be quoted correctly, and also that their statement should be accompanied by a short exposition of the assumptions on which they are based. I propose then, in the first place, to point out, as plainly and briefly as possible, the method of reasoning employed and the data assumed in Lord Kelvin's essay; this I regard as especially necessary, because Mr. Reade not only mis-states some of the most important results, but also invests them with a rigidity to which the writer expressly lays no claim.

The essence of the employment of Fourier's equations depends upon assuming that we are dealing with nothing but the *conduction* of heat from point to point. Thus, no heat must be supposed to be used up or evolved in changing the physical state of the material under consideration; for example, we cannot suppose that any large portion of the mass has, during the time we are considering,

passed from the molten to the solid state; for this would involve the liberation of that amount of heat which would be necessary to convert the solid rock into a molten state at the same temperature. If applied to any period during which such changes of physical state took place our equations of conduction would simply be talking nonsense; so that the first important point assumed by Lord Kelvin is that, during the whole period to which he applies his reasoning, the mass under consideration is supposed to be in the same physical state, and therefore, of course, at all times solid.

Another point to be especially noticed is that there is supposed to be nothing in the nature of discontinuity throughout the mass, or, in other words, that heat is freely conducted from layer to layer. In consequence, any portion of the mass exhibiting such phenomena would not satisfy the only condition on which our equations can be used; so that when applied to such portions our equations cannot be regarded as telling us the absolute truth. Now, as far as the interior of the earth is concerned, we can no doubt assume perfect continuity with sufficient exactness; but when we approach the surface we come upon very heterogeneous rocks, separated by bedding and other separation planes, so that we introduce an element of discontinuity.

This new element will not, to any appreciable extent, affect the general considerations put forward by Lord Kelvin, but will, on the other hand, render results deduced from our equations in the neighbourhood of the surface necessarily fallacious.

I attach some considerable importance to this condition as tending to vitiate the practical value of results concerning a level of no-strain to which I shall refer later, and also as exerting a conservative influence in the matter of the outflow of heat, and, in consequence, as tending to lengthen the time from first consolidation.

Lastly, for purposes of calculation we require to know a certain average value of the coefficient of thermometric conductivity, and this involves the two considerations of conductivity and specific heat. Of the difficulties attending such a determination, it is only necessary to point out that the variations of these two physical quantities with pressure and temperature are as yet very imperfectly known, and that, consequently, the assumption of any such coefficient for the unknown rocks of the interior must necessarily be an extremely tentative one. But, supposing such an average coefficient to have been found, it obviously would not agree at all closely with that for rocks near the surface; for, in its very character as an average coefficient, it must essentially refer to the vast bulk of the interior rocks, and not to the comparatively insignificant crust.

Thus we see another reason why, though our equations may give us exact results with regard to the interior, they cannot, for that very reason, apply rigidly to rocks near the surface. In fine we are not justified in applying Fourier's equations of conduction to rocks near the surface, or, at all events, not equations involving the same constant of conductivity as we have used for the interior.

In the next place it is important to notice the data which are assumed. Lord Kelvin assumes that, at the epoch of consolidation, the earth, or at least an outer shell of a thickness of 100 or 200 miles, was at an uniform temperature, and that the temperature of the surface was subjected to a loss of  $7000^{\circ}$  F. and then maintained at this lower temperature.

The temperature at the very start was, in consequence, uniform downwards, not a rise of  $1^{\circ}$  F. in 10 feet as, by some very curious oversight, is stated by Mr. Reade. This latter temperature gradient, which is given as that of first consolidation, was not established, on Lord Kelvin's assumptions, until 4,000,000 years later.

In this reduction of  $7000^{\circ}$  F. at the surface we have, at the best, a probable hypothesis with no pretence at exactness. The value is, of course, mainly based upon the heat set free when the earth, or a very thick outer shell, passed from the molten to the solid condition; in other words, upon the latent heat of fusion for the earth rocks.

Starting from this datum, and assuming the applicability of Fourier's equations, the temperature which at any given time existed at a given distance below the surface is easily calculated, and it is found that, after 100,000,000 years (not 96,000,000 as quoted by Mr. Reade), the temperature gradient would be  $1^{\circ}$  F. in 50 feet, which Lord Kelvin assumes as the present temperature gradient very near the surface. Since, also, this gradient varies inversely as the square root of the time from consolidation, it is easy to obtain the number of years which must have elapsed to establish any particular rate of rise of temperature with depth. For example, for a gradient of  $1^{\circ}$  in 64 feet, the time would be 160,000,000 years for the same initial reduction, and the depth to which loss of temperature would have penetrated would be 180 miles, as opposed to 150 miles on the assumption of  $1^{\circ}$  in 50.

To emphasize how broadly these results must be looked at, it is only necessary to point out that doubling the initial reduction of surface temperature would multiply the number of years before the establishment of any stated gradient by four, and that halving the coefficient of conductivity would double the time. For example, if the initial reduction were  $10,000^{\circ}$  F. the number of years would amount to about 300,000,000. This is in accordance with the wide general limits assigned by Lord Kelvin in his essay, in which he places the time from first consolidation as somewhere between 20,000,000 and 400,000,000 years.

Again, owing to the partial inapplicability of our equations to surface rocks, it is questionable whether we can, with justice, assume that  $1^{\circ}$  in 50 feet expresses the surface gradient which would have been established were the whole globe continuous. In short, the effect of discontinuity in surface rocks would seem to point to a retardation in the loss of heat and a consequent increase in time from consolidation.

To pass on now to consider the value of the assumption of a shell of no-strain: The contraction taking place in one year at any point

within the earth, if unresisted, would no doubt be proportional to the loss of temperature at that point in that time, and this, assuming the truth of our old equations, would be proportional to the rate of increase of the temperature gradient at the point. We thus obtain the simple conception that the contraction will increase from the surface inwards to a certain depth and then diminish, until the point is reached at which temperature becomes uniform downwards.

The distance of such a shell of maximum contraction below the surface, estimated in the way already indicated, will be equal to  $\sqrt{kt}$ , where  $k$  is the coefficient of thermometric conductivity, and  $t$  the time from consolidation in years. This gives, assuming the same data as before, a depth of 38 miles. (Mr. Reade states that the value is 50, which would be the depth after about 160,000,000 years.)

From the fact of the existence of such a maximum-contracting shell, Mr. Reade has deduced with perfect theoretical correctness, so far as a homogeneous sphere is concerned, the necessary existence of a level of no-strain. In other words, there should, theoretically, be a shell, between the surface and this shell of maximum-contraction, which could contract with loss of temperature so as not to exert any pressure on the underlying mass; whilst all shells below it would, in contracting, squeeze the interior, and all shells above it would, owing to less rapid contraction, attempt to stand off from lower shells, and would, in consequence of the force of gravity, be put into a state of compression.

It becomes then a very important problem to find the depth below the surface at which this level of no-strain lies; and this appears to be easily attacked in the following manner.

Choosing any shell at a given distance from the centre of the earth, we can easily find, by the foregoing analysis, the radial contraction which has taken place in one year in any of the shells below it which suffer loss of temperature, and, by summing all these separate contractions, we obtain the whole amount by which the interior has attempted to draw itself away from our chosen shell in that time. If we equate this amount to what would be the radial contraction in one year of a sphere with radius equal to the distance of the selected shell from the centre, and further suppose the whole of this sphere to be contracting at the same rate as the given shell, we shall find the distance of such a shell from the surface. Working this problem out with our preceding assumptions I obtain a distance of a little over a mile from the surface. I have felt it necessary to give the reasoning employed, as this result differs from that found by Professor Darwin, as quoted in Mr. Reade's paper, and to whose high authority I should naturally attach great weight; but I have not, unfortunately, had the opportunity of referring to his paper.

Assuming, however, that Professor Darwin is correct in estimating the depth of this shell of no-strain at two miles below the surface, I cannot think that the result has any but a theoretical interest.

For here we arrive at a point to which, as I have shown above, the method of analysis cannot rigidly apply, not only on account of the physical discontinuity of surface rocks, but also on account of the irregular distribution of the large land and ocean areas which renders a shell at the depth of two or three miles a purely theoretical conception.

In fine I adhere to the conception, so fully explained in the beginning of my paper, that, owing to the existence of separation planes, this imaginary shell of compression settles down upon the interior rocks; so that in reality we are dealing with a vast series of shells, contracting upon and consequently squeezing each other, surrounded by a thin layer of surface rocks, which settles down upon them by closer application.

Before criticizing the theory put forward by Mr. Reade, I should like to point out the questionable strength of his disproof of the older theory.

Assuming that contraction has only affected a shell about 150 miles in thickness, he supposed this shell to have lost an average of 1000° F. since Cambrian times, and proceeds to calculate its radial contraction.

To quote his own words:—

“The linear contraction of 150 miles of rock cooled 1000°, using this coefficient, would be 4,125 feet, but, as the contraction of the shell 150 miles deep is voluminal, the contraction in thickness of the shell would be three times this, or 12,375 feet or=2·344 miles.”

I fail to see how the contraction in thickness, *i.e.* the contraction of a certain line of material, can by any possibility be voluminal. But, accepting this result for the sake of argument, Mr. Reade finds that the girth of the interior shrinking sphere would be 15 miles less than that of the thin imaginary outer shell of compression; so that this represents, so to speak, the amount of looseness which has been used up in mountain-making since Cambrian times.

Here Mr. Reade leaves this part of his argument, but if we apply the above result we shall find that the elevation which could thus be produced is a very large one indeed. For suppose, to take a case far from the maximum possible, that the extra length of 15 miles in each great circle is used up in forming a cone tangential to the interior sphere. We should thus be able to form a mountain covering nearly a million square miles and rising to a height of 60 miles above the surface of the sphere. But, as Mr. Reade says, his assumptions are generous in the extreme, and I feel sure that, as referred to above, he has gratuitously multiplied them by three.

In the very simple calculation presented early in this paper, I took a coefficient not differing much from the one adopted by Mr. Reade, and made allowance for a loss of 10° C., which I believe would more than cover any one period of mountain-making. By this line of reasoning we seem to obtain a better means of comparison with actually known facts than if we estimate the total available volume which the old theory would allow for mountain-making since Cambrian times, and then attempt to compare it

with the very unknown volume which has, in reality, been employed in constructing all the mountains since that period.

I now wish to examine Mr. Reade's theory, and to compare it with that put forward in this paper.

In the first place, Mr. Reade imagines a great depth of sediment to be laid down over a large area, and, to fix ideas, suggests an average depth of 4 miles of sediment deposited over an area 2000 miles by 1000 miles.

No doubt it follows, with perfect accuracy, that, as the layers accumulate with great slowness, the temperature gradient at any time during deposition will simultaneously attain its average value, and be roughly, say,  $1^{\circ}$  F. in 50 feet downwards; so that, as any layer becomes buried deeper and deeper beneath successive deposits, its temperature will continually rise.

For the same reasons, the rocks which constitute the floor upon which sediment was first deposited will suffer a continual increase of temperature. After a certain accumulation of sediment, the expansion produced by this rise of temperature will cause the underlying rocks to curve upwards, so that the curvature of any layer, which was at first approximately that of the surface of the earth, becomes increased and allows of expansion, much in the same way that a bar of iron, bent into an arc and fixed at each end, will, if heated in the middle, bend into a steeper curve.

I have taken great care to state exactly what I understand to be Mr. Reade's views, in order that my criticism may be comprehended better. I may, perhaps, just point out that this extension has, of course, the effect of a vertical lift upon the overlying mass, and that it is to this vertical motion that Mr. Reade attributes the inception of mountain ranges.

Now, it is a fair deduction from the very slow rate at which sediment accumulates, that the full effect of extension proper to any given depth of sediment will have been brought about at the time at which that depth is established. Such a process would necessarily go on until the central portion of the region was raised to the sea-level and the deposition of sediment in consequence ceased, and no further rise could take place, at least so far as due solely to expansion of underlying rocks.

The mechanism by which the fold thus formed is further compressed and raised by lateral pressure, I fail to understand; especially in the light of Mr. Reade's insistence that expansion is internal and shading off to zero at the boundaries of an area of deposition, so that any new area of sedimentation which may be established on the sides of the old fold would produce a new central fold, and not exert lateral compression on the earlier one. It is also hard to see how such an area of deposition could exist outside the first fold, except as derived by marine denudation of that fold itself.

Again, the conception that any fold is formed must necessarily imply that the rocks which form it are drawn away from the underlying mass; for it is upon the amount of lateral expansion, and not upon the comparatively small vertical expansion, that the theory



rests. Hence it follows that an absolute vacuity must be formed under the fold, since in no way could a shell of expanding material exert a squeezing force on the rocks beneath. Thus the undermost of the layers which constitute the fold must support the whole weight of the overlying mass. This result does not seem to accord well with the potent effects which are attributed to the weight of overlying rocks in the direction of causing, what Mr. Reade calls, compressive extension. But, in fact, I believe that there is an element of unreality which renders the whole conception invalid.

In the first place, so far as the effect of heat upon the actual sediment is concerned, this sediment is deposited, not as solid rock, every component particle of which is in closest union with its fellows, but rather as a great number of small particles, each of which has, so to speak, plenty of elbow room. It would seem then that the effects of increase in pressure and temperature would be mostly used up in welding the rock into a dense mass; each particle expanding into the interspaces surrounding it, and the greater the resistance to general expansion the more closely would each particle be forced into union with its neighbours. This, surely, is in agreement with the actual facts observed in the lowest of a great series of conformable sedimentary strata. But, as regards the actual floor and the rocks which lie beneath it, which may be supposed to be in a dense state and not to admit of any great expansion of separate particles, I believe that the effect of expansion will only tend to minimize the contraction which, I now attempt to show, must be in progress.

It will, assuredly, be readily granted that, to allow of the accumulation of the great thickness of sediment here conceived, there must be an approximately proportional depression of the floor, and such depression must imply the curving inwards of that zone of the earth which forms the area of depression. Such inward motion must result in the reduction of curvature, and the consequent diminution of area; in other words, the floor must be contracting and must continue doing so approximately as long as sedimentation proceeds. Thus the only effect of expansion, in such an area, must be in the direction of minimizing the rate of shrinking, and, should the effects of expansion balance those of contraction, the area ceases to sink and deposition soon ends.

I will now briefly point out what I conceive to be the true phenomena which cause and accompany the deposition of a great thickness of sediment near land.

Imagine an area, such as I have conceived in the statement of my theory, to be bordered by a large land region. Owing to the more rapid contraction of our depressed area, its floor bends inwards, or the area sinks; over that portion of the area which borders our supposed land region sediment will be deposited and will accumulate to a greater and greater thickness as the area sinks. This sinking must obviously be accompanied by the actual displacement of material from underneath, which will be forced below the surrounding regions, and increase their relative height. Thus, as long as

sinking continues, there is a transfer of material, squeezed out from under the depressed area, and forced below the bordering land region.

As a great thickness of sediment accumulates on the margins of the main area of depression (which is itself far from land), owing to the establishment of a temperature gradient throughout this sediment, the contraction of this border region becomes continually less and less than that of the main area of depression; and at last the region of sedimentation ceases to sink and begins to rise owing to the introduction of material, squeezed out from under the more rapidly contracting area.

To sum up, the sequence of events will be briefly as follows:—

First, when sediment is accumulating, the neighbouring land area is rising, owing to actual transference, from under the sinking area to beneath the neighbouring land region.

Secondly, this continues until, owing to the thickness of sediment accumulated, the area of deposition contracts at a much slower rate than the main area of depression, and, in consequence, is forced up by the introduction of material squeezed out from beneath that area. Thus the tendency is, as I stated above, to narrow the main area of depression.

The last point in Mr. Reade's paper, to which I wish to call attention, has reference to the effects of denudation.

As far as I am able to understand the very brief statement contained in his paper, Mr. Reade argues that, as foot after foot of rock is removed by denudation, the temperature of any point beneath is gradually lowered, so that contraction sets in, which doubtless tends to draw the rocks in such an area of denudation away from those surrounding it.

The actual results which Mr. Reade considers would follow are best given in his own words:—

"The weight of the crust itself squeezes up all vacuities, and the area adjusts itself to its decrease of volume by normal faulting and keying up in a wedge-like manner and by compressive extension."

Now to bring about normal faulting the rocks must not only split apart, which is the necessary result of contraction, but this must be accompanied by vertical displacement, and I can see no possible cause contained in the theory which accounts for the vertical sinking of one part of the area of denudation relative to another.

Again, as regards what Mr. Reade calls compressive extension, I think it is a sufficient answer to point out that contraction is greatest nearest the surface; for it is always there that the reduction of temperature must originate. In other words, the first layers to contract are the outer ones, or those under the least overlying weight; and further, in contracting, each layer must tend to assist the contraction of one lower. To bring to a close a paper perhaps already inexcusably long I will briefly recapitulate my own views on the results to be expected in such a contracting area of denudation.

In so far as contraction is general over the area, the tendency will be for the whole area to attempt to bend inwards, and the result of such a tendency is to check the introduction of material which,



as I have attempted to show, is being squeezed out from beneath the neighbouring area of depression. Thus, such material will be more and more used up in raising the bordering one of sedimentation, with the ultimate production of a parallel elevation.

In so far as contraction actually produces splitting, this will, over a region with no especial lines of weakness, produce a very complicated system of joints throughout the entire mass; and, by thus slowly relieving pressure, bring about the formation of holocrystalline igneous rocks beneath.

#### VIII.—HOW CHLORITE IS CONVERTED INTO BIOTITE.

By C. CALLAWAY, D.Sc., F.G.S.

GENERAL McMAHON'S paper in the GEOLOGICAL MAGAZINE for June attacks my conclusion that the Malvern biotite is formed from chlorite, but leaves my evidence untouched. Indeed, a large part of the article is occupied in discussing a theory which I do not hold. I have never said that the chlorite was converted into biotite by contact-action *only*. That there may be no further mistake, let me repeat that, in the Malvern rocks, chlorite is changed to biotite by contact-action *plus* dynamic deformation.

General McMahon, in rejecting my theory, has rightly felt that he was bound to offer an alternative explanation. He is aware that the materials for the manufacture of the mica must come from somewhere, and he suggests that it may have been produced by "direct impregnation" from the granite. That the granite has impregnated the encasing diorite with one at least of its constituents, viz. potash, is one of my own points; but that it has bodily produced the biotite is simply impossible. This granite, it must be remembered, consists almost exclusively of quartz and potash felspar. An analysis of it by Mr. Player gives 1.1 per cent. of iron-oxides, and 0.3 per cent. of magnesia. Three analyses of the same rock by Mr. Timins yield about the same results, but one of them contains no magnesia. You cannot get a black mica without iron or magnesia, and it is certain that the granite could not have supplied enough of these bases for its manufacture. The encasing diorite, on the other hand, contains plenty of both bases.

General McMahon objects to my statement that the conversion of chlorite into biotite is an "observed fact." He writes that "the fact actually observed is the existence of chlorite and biotite in the same rock." So my critic believes that, merely because the two minerals lie side by side, I have inferred that the one was formed out of the other! I might as well have argued that the sugar in my tea-cup was evolved out of tea.

I will give General McMahon one or two facts, and allow him to judge whether they support his suggestion. Near the Wych, there is a very clear case of contact between diorite and granite. Both are sheared, and more or less altered. Near the contact, the hornblende of the diorite is decomposed; chlorite and iron-oxide being most conspicuous amongst the resulting products. Slides of the rock taken at the exact contact, show the decomposed diorite side by side