

the red spot formerly was is now very white; it passed over the central meridian of the planet this morning at 4h. 36m. (M.T. at Palermo), which gives for this place the Jovicentric longitude 63° , plainly corresponding to the longitude that Mr. Marth assigned to the red spot at present, if visible. This proves that the neighbourhood of the red spot had followed the particular motion of the spot itself. This place is well characterised by the permanent depression in the great reddish band of the planet.

A. RICCO

Royal Observatory, Palermo, September 10

"Elevation and Subsidence"

MR. O. FISHER has been so good as to offer a reply to my "remark with a query," his answer being (allowing for an obvious printer's error) that it is "an open question whether the melting temperature of rocky matter is, or is not, raised by pressure."

I cannot for a moment pretend to the same familiarity with the results either of experiment or of calculation as is doubtless possessed by Mr. Fisher. I only claim to speak as representing the class whose knowledge on these subjects is essentially second-hand; but, speaking as such, I think that Mr. Fisher's reply will not generally be regarded as satisfactory. I should, therefore, like to repeat my question with a little extension:—

1. Do not the "rigidity" calculations incontestably show that the earth is extremely rigid, *i.e.* solid? Are not, therefore, all theories which disregard this result (such as that the nucleus may be above its own critical temperature) put out of count?

2. Are not the phenomena of metamorphic and hypogene rocks on too large a scale to be accounted for by heat of merely local origin, whether produced by chemical or mechanical action, such as has been suggested in connection with volcanoes?

3. Do not all reasonable views of the origin of the earth, *i.e.* any form of the nebular hypothesis, point to the same conclusion as (2), viz. that the earth's heat is the residuum of a much greater amount formerly possessed, and not yet entirely lost by radiation?

4. Does not (3), taken in connection with the known laws of conduction, involve a continuous increase of temperature, whether rapid or slow, as we descend below the surface?

5. Although we may have no *direct* evidence as to the "temperature at depths bearing considerable ratios to the radius," is there not ample evidence that at comparatively insignificant depths the temperature is such as would melt not only "rocky matter," but far more refractory substances, if there were no counteracting influence? Even allowing a very slow increase, provided the increase is always positive, as 4 points out, should we not sooner or later almost certainly reach the melting temperature of the most refractory substances with which we are acquainted?

6. Can we then escape the conclusion, either that the nucleus consists of matter of a totally different kind from anything with which we are familiar, or that pressure raises its melting temperature? But does not every fact bearing on the question discredit the former hypothesis?

7. Should we not then accept the view that pressure does raise the melting-point of nucleus stuff, at least as a working hypothesis, only to be overthrown by direct evidence to the contrary, if direct evidence on the subject is ever forthcoming?

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F. YOUNG

IN a paper I read before a full meeting of the Geological Association on March 2 last, of which a brief notice is given in *NATURE*, vol. xxvii. p. 523, I discussed the probability of subsidence of land, in certain cases, being due to *loading* by local accumulations of terrestrial matter acting upon a deflectible crust supported upon a viscous interior. The greatest effects, I imagined, from this cause, were due to local accumulations of ice past and present, particularly about the poles of the earth; but that secondary and important effects were due to the weight of accumulations of solid mineral matter from denudations being carried by oceanic currents and winds, from coral deposition, and the reaction of volcanic outflows. One illustration I proposed was that the sinking of the coast of Greenland was probably due to the weight of inland accumulation of ice, which proposition I thought was original, but Mr. Gardner (*NATURE*, vol. xxviii. p. 324) says—"It has often been supposed that the sinking of the coast of Greenland is similarly due to its icecap." I should

feel obliged if Mr. Gardner would point out references where this has been proposed, as I thought I had read the literature of the subject, and I fear that this part of my paper is less original than I assumed.

W. F. STANLEY

THAT there is a connection between sedimentation and subsidence on the one hand and between denudation and elevation on the other is a fact now admitted by most geologists. The real question to be answered, however, is:—Are these directly connected as cause and effect? or are they simply concomitant effects of the same cause? If the first be true, we should expect cause and effect to vary together, that is, that subsidence should keep an even pace with sedimentation. That this has not been exceptionally the case is proved by the sections of the carboniferous system in the central valley of Scotland, where the facts point to a continuous subsidence, accompanied by a very irregular sedimentation, with the result that now subsidence gained on sedimentation, now sedimentation on subsidence. Again, once the process commenced—and it is not very evident how on an originally even surface it could have commenced at all—we should expect it to be continuous. Sedimentation causes subsidence, subsidence gives rise to fresh sedimentation, and that again to renewed subsidence, and so on and on. Consequently we should expect that when once an area of sedimentation and subsidence was formed, it would continue an area of sedimentation and subsidence through all geological time.

It appears rather, I think, that the connection between them arises from their being concomitant effects of lateral pressure in the earth's crust (for notwithstanding the Rev. O. Fisher's masterly exposition of the inadequacy of this cause to produce the observed inequalities of the earth's surface, I still believe that, with the exception of the ocean basins, which must be otherwise accounted for, it is quite competent to account for the facts). We may suppose the action to take place so:—

A certain portion of the earth's crust is first thickened and strengthened by volcanic outburst or other accumulation on the surface. This part, when the tangential thrust comes, offers, by reason of its increased weight and thickness, a greater resistance to the elevating force than the parts around, and as a consequence these are raised around the thickened part, while it is at the same time depressed in a corresponding degree; in other words it becomes the centre of a syncline, while the strata around are raised into anticlines. Depression naturally leads to sedimentation, and this still more thickens the part, and enables it to offer greater resistance to the tangential thrust, with the result that it continues to be depressed as the strata around are elevated. The converse is also true. Denudation means the thinning and consequent weakening of the crust, and hence when the thrust comes the denuded part is the more likely to be elevated into the anticline.

This theory provides for the cessation of the phenomena, since the tension of the crust is after a time relieved. It also accounts for the fact that strata around volcanoes and volcanic necks, as also along the base of mountain chains, so frequently appear to dip below them. The rate of subsidence, too, would vary with the intensity of the exciting force, though the consequent sedimentation need not vary with it in the same absolute degree.

Perth, September 3

WILLIAM MACKIE

MY article on elevation and subsidence has provoked considerable and, on the whole, friendly criticism, a so far satisfactory result, though but few points have been raised requiring reply. Dr. Ricketts objects, and very properly, that I have not alluded to his many writings on the subject; and to this I can only plead want of space, that I have not entered at all into its already voluminous bibliography, and that my article was written and in type before his recent contributions to the *Geological Magazine* had appeared. Beyond this I had sufficiently indicated that there were many observers in the field, and every geologist must be aware that the subject has for a long while past excited attention not only in England but in France and America.

The fundamental error in my article is pointed out by the Rev. Mr. Fisher and by Mr. Young, and the assumption that inert pressure induces heat must be abandoned. As I had read the "Physics of the Earth's Crust," I expected that this would be challenged, but I let it stand, as the fallacy has been shared by a large number of geologists, comprising some of the most distinguished, and has even escaped the correction of physicists. But this rectification, while very important, by no means affects the results, and on the contrary facilitates an appreciation of the