

in the Dartmoor granite. Two groups of associated but as yet unrelated facts were recorded concerning the sector-twinned cordierite in the garnetiferous granite of Sweltor: (1) an aggregate of cordierite grains is separable into fractions varying in composition; in particular, the molecular ratio FeO/MgO varies from 0.37 to 1.28 in six intermediate fractions analysed, the ratio for the aggregate being 1.52. (2) all sectors are optically negative, but the value of $2V$ varies between 56° and 72° . Centrally paired sectors give the same $2V$ value, whereas adjacent sectors often give different values, the maximum difference observed being 12° .

CORRESPONDENCE.

MECHANISM OF EARTH MOVEMENTS.

SIR,—Mr. R. G. Lewis's paper "A Search for the Mechanism of Earth Movements" (GEOLOGICAL MAGAZINE, November, 1933) has fairly astonished me. I will not discuss his hypotheses, as they are too vaguely put and have not been worked out well enough to allow any discussion. I may refer to the remarks of Mr. H. Jeffreys on the requirements of a theory in general (*The Earth*, Appendix C 4, p. 324). There are further, in this case, some special requirements to be satisfied. Whether the factor 1.7 and the equality of terrace intervals between different sites really exist must be proved by a thorough analysis with the calculation of probabilities. If then their existence is proved any explanation given must also explain these points, which is not done by Mr. Lewis's hypotheses. But his method of making use of the facts may be discussed. And here I have some serious objections:—

(1) In the case of the Dancala Depression, he omits two steps in his Fig. 5: the step at -20 to -30 and the one at -62 m. (See p. 501.)

(2) On p. 504 he says that "each successive deficiency is almost exactly three times the last". This is not true. But every two deficiencies are almost exactly three times the next two, which makes the relation more complicated than probable. But may we speak of a relation at all, when only five steps of the twelve found show this relation? And last of all the coincidence has been brought about by Mr. Lewis himself, by taking 1.79 for his factor. He could even have done better by taking 747.1 for the highest terrace and 1.1642 as a constant. Then all the terraces fit in with a departure from the theoretical curve of less than 5 per cent, excepting the terraces II–VI, which give deficiencies with a constant ratio of -1.4029 , with only five terraces being eroded away and the one at -20 to -30 m. being neglected. [747.1; (-641.8); 551.2; -473.5 ; 406.7; -349.4 ; 300.1; -257.8 ; (221.4); (-190.2); 163.4; -140.4 ; (120.3); -103.6 ; (88.9); (-76.4); 65.6.] But then, of course, we can get as perfect a fit as we like by taking the constant as near to unity as is necessary.

(3) (Fig. 8, first period and Fig. 10). When we apply his theory to the case of the Oxford river terraces we would expect that the Old Wolvercote channel would have been cut more deeply than the sunken channel as, according to Fig. 10, there was more time. But even if we take a constant wavelength, the fall of the river would have been greater. So the maximum height for this channel would be 17 feet below present sea-level, the minimum being of course 58.5 feet below present sea-level. This is inconsistent with his Fig. 8. Moreover his two figures do not agree, as what is the Summer-town-Radley channel in the first instance is taken for the Buried Channel in Fig. 10.

(4) The most astonishing thing, however, is that, when trying to account for the Bilbao profile (Fig. 1), which is inconsistent with his theory, he proves the Nive profile to be theoretically at fault, whereas the Nive profile is one of those on which his theory was based. In doing thus he weakens his own theory, leaving the Bilbao profile unaccounted for.

(5) When, moreover, he speaks of a rising of the land between the Pyrénées and Armorica towards the end of the beachforming period, he is quite wrong and in contrast with his own data. Let us make the same assumptions as he does, viz. (a) that of two terraces the higher is always the older; (b) that of each terrace the higher margin was formed earlier than the lower; (c) that the whole range of land, where the terraces occur, moved *en bloc*; (d) his theoretical heights for these terraces and the deficiencies resulting from them. We must bear in mind that the movement that we are in search of is the differential movement superposed on the oscillating movement. Now let us denote by a, c, e, g, i the movements of the land during the formation of the first (highest), 2nd, 3rd, 4th, and 5th terrace respectively; and by b, d, f, h the movements of the land between the formation of two terraces, and by j , the movement afterwards.

According to assumption (b), a, c, e, g , and i will be positive. According to assumption (c) each terrace margin will be found now at a height, which is expressed by the sum of all the movements occurring after its formation. We then find: $a = 0$; $b = -15$; $c = +15$; $d = -17$; $e = +17$; $f = -19$; $g = +8$; $h = -7$; $i = +2$; $j = -10$. If we reverse assumption (b)—the only supposition we may change without discarding the whole theory—we get: $a = 0$; $b = 0$; $c = -15$; $d = +15$; $e = -17$; $f = +6$; $g = -8$; $h = +3$; $i = -2$; $j = -8$. The result in both cases is an irregular movement with the general character of a subsidence.

(6) Perhaps I may be permitted to make some more positive remarks too. The fact still remains that, if we take Mr. Lewis's graphic method consistently, most of the profiles mentioned still show a convexity towards the X- and Y-axes. This again may be due to chance, as the number of profiles is rather small. Two factors, however, can be indicated that might tend to cause this

convexity. First, it may be more probable for higher terraces or beaches to be eroded away than for lower ones. Secondly, if a set of beaches little differing in height is raised to some altitude, they will more often than not be taken together as one beach, as might be indicated by the wide margins given for some of the raised beaches. Both factors will make the left side of the curve steeper than it would originally have been, resulting in giving the curve a tendency to convexity towards the X- and Y-axes.

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THE GLACIAL SUCCESSION ON THE NORTH COAST OF NORFOLK.

SIR,—By the publication of his paper (*Proc. Geol. Assoc.*, xliii, pt. 3, 1932, 241–271), Dr. Solomon has made another and a valuable contribution to our knowledge of the very complex problem of the glacial succession on the north-east coast of Norfolk. During the last fourteen years I have made very numerous visits to the area dealt with by Dr. Solomon and have carried out a close examination of most of the deposits with which he deals. The object of my excursions to the Norfolk coast has been the excavation and collection of flint implements, and I suppose I am one of those responsible for the finding of these specimens in situ in various beds, of which Dr. Solomon speaks. I fear, also, that I am to be numbered among the archæologists who have indulged, though I trust not, in my case, in an orgiastic manner in the correlating of certain types of implements with these deposits. However, having always proceeded with due care and caution along this difficult path, I am hopeful that my tentative efforts will not be included among those which are condemned by Dr. Solomon as “ill-informed”. There is, of course, not much doubt that some of the correlations which have appeared in various publications are of small value. But the archæologists may perhaps be forgiven for this because, after all, if it had not been for their investigations, carried out quite frequently in the face of determined opposition by geologists, the latter to say the least would have experienced considerable difficulty in extricating themselves from the monoglacial morass, and the slough of despond of the post-glacial age of man.

I hope my geological friends will forgive me for reminding them of this historical fact, which appears at times to be in grave danger of being overlooked and forgotten.

There are, however, many things for which archæologists should be thankful; and I trust I am duly appreciative of Dr. Solomon’s statement that, while the implements I have found in the Stone Bed, near Cromer, are not “universally accepted”, nevertheless “the bulk of archæological opinion is inclined to accept their