and general sub-angularity in the form of these ironstone concretions, and may explain also the analogous peculiarities of many Chalk-flints. These latter occur in thin plates and tabular masses following lines of bedding, joints and fissures, in spheroidal and elongated forms, or in a mixture of both kinds, presenting odd corners and angles scarcely compatible with chemical aggregation around a central nucleus. The hollow portions are frequently filled with loose powdery chalk, and, except that they have not been observed in actual stages of transition, the Chalk-flints present characters similar to those of the Lower Greensand concretions.

NOTICES OF MEMOIRS.

I.—*Report on the Superinduced Divisional Structure of Rocks, called Jointing; and its Relation to Slaty Cleavage.* By William King, Sc.D., Professor of Mineralogy and Geology, Queen's University in Ireland, and Queen's College, Galway. Being Article XX. of Vol. XXV. (Science) of the Transactions of the Royal Irish Academy, 4to., pp. 605–662, plates 34–38. (Dublin, 1875.)

THE "Character of Jointing" is first treated of (pp. 606–612), several examples being mentioned; the "cleat" of coal, chert, and other rocks, and especially of the coaly cuticle of Lepidodendron and other fossil plants, is taken as a familiar instance, and is regarded by the author as the analogue of mineral cleavage rather than of the prismatic jointing of basalt. Further examples of rock-jointing are referred to in the treatment of the "Extensions of Jointing" (pp. 612–618). The shifting, or dislocation of jointings in some beds which have been vertically jointed, among others destitute of divisional planes, is described in detail (pp. 620–626), with examples. "Jointing developed in different Geological Periods," and "The Origin of Jointing," occupy pages 626–642; and Part II., "Jointing in its relation to Slaty Cleavage," with some notes, concludes the Memoir. A lithographic view of a wide area of bare jointed limestone, on the south side of Galway Bay (pl. 34), and numerous diagrams of the stratal conditions referred to (pls. 35–38), illustrate the Memoir.

The author alludes to the former promulgation of his views on the relation of joints and cleavage, in 1857–8; and now treats of his further researches, in which he has been aided by a grant from the Royal Irish Academy. Observations and opinions of others on the interesting subjects of jointing and cleavage are carefully alluded to, and often given in full.

In this Memoir, Prof. W. King, of Galway, urges the existence of universally distributed divisional planes, or joints, in a great variety of rocks constituting the earth's surface. He regards the joints as having been originally, and still often remaining, vertical; and as having either a more or less "meridional,"[1] or a more or less

[1] Spelt "meridianal" throughout in the original; but this modification, like "stratial" also throughout the memoir, we regard as neither necessary nor correct.

"equatorial," bearing; the intermediate bearings, which seem to have wide ranges, being termed "E. and W. meridionals," and "N. and S. equatorials." The meridional joints appear to be the most frequent and important. The cause of the jointing is not known: little is allowed to contraction and tension; and the less definite agencies of chemistry, electricity, and magnetism are referred to as probably concerned in its production.

It is also implied (p. 642) that the known secular variation of the magnetic meridian may have existed in very early times, and that "the magnetic polar currents," thus changing their direction, may have produced more or less divergent meridional jointings in course of ages. The author is "not acquainted with *true* jointing in rocks of later periods than the Jurassic in the British Isles"; but accepts the divisional structure of the Gypsum at Montmartre, and of the Nagelfluhe of the Rhigi, as probably Tertiary jointing.

The partial occurrence of such vertical jointing in one set of beds, over- and under-lain by jointed strata, is not only pointed out as a natural phenomenon, but the fact of such a set of joints having been laterally shifted into an oblique position (like a set of sloping books between two parallel shelves) by lateral thrusts, and here termed "dislocated jointing," is clearly expounded as a real cause of what is sometimes regarded as a local cleavage limited to one stratum or stratal series. It is shown also that jointings may really have determined the direction of dislocations, although the uppermost (latest and visible) set of joints may not coincide with the faults; for the lower strata may be cut up by older, deep-seated, and more widely pervading joints, giving direction to the main movements (p. 638).

"We have had," says the author, "examples before us of jointing both unlimited (as in the papery variety), and intermittent. Mineral cleavage I hold to be a superinduced divisional structure, its planes having no relation to incrementation or depositional growth: so it is also with jointing" (p. 640). He admits, however, that slaty cleavage in some rocks may be the result of pressure, rearranging and parallelizing the constituent particles and contained cavities, to produce planes of weakness; but unless the cleavage be practically unlimited or papery,—that is, if it be intermittent with intervals of even less than an inch, he prefers to regard the divisional planes as having been due to the jointing, whether in original vertical position, or modified by disturbance of the stratal mass, with or without lateral pressure. The welding together of joint faces is mentioned as a frequent condition, accounting for what is sometimes described as imperfect cleavage; and what has been treated of by others as secondary cleavage, Prof. King thinks may be also accounted for by modifications of joint-structure under pressure. The difficulty of explaining the discrepancy between the strike of bedding and the strike of cleavage in several known cases is met by the supposition of the lateral crush having been subsequent and oblique to one or more sets of original jointings. Thus, after stating that the *mechanical* theory of cleavage, as advanced by Sharpe, determined by Sorby,

and further elucidated by Tyndall and Haughton, necessitates a strictly coincident strike of beds and cleavage,—and after pointing out that in North Devon, Charnwood Forest, the Lake District, Ireland, and elsewhere, differences have been found to exist between the two strikes, Dr. King remarks: "The hypothesis proposed by myself does not limit the action in force to the direction just stated [*perpendicularly* to the resultant planes]: it admits of the force being applied in various directions *against* pre-existing divisional planes: therefore, cases answering to those noticed in the last paragraph do not stand out as antagonistic to it, but as coming within the category of its resultant phenomena. Obviously a force acting in a line deviating to some extent from perpendicularity to joint-planes will bring them into proximity, and weld cognate planes together; though, perhaps, not so effectively as if it had been exerted perpendicularly to them. Moreover, as noticed at p. 638 [limestone near Cork, with complex jointings], jointings of both sets forming the meridional system, one striking east of north, and the other west of north, may occur separately in a great thickness of superimposed rocks, representing two different geological periods—say Devonian and Silurian; therefore pressure exerted uniformly and simultaneously throughout the mass, while acting perpendicularly to the strike of the east-of-north joint-planes in the Silurian rocks, would also be acting obliquely to the strike of the west-of-north jointings in the Devonians: yet it may be consistently held that in both instances slaty cleavage, though necessarily of two discordant strikes, could result from one and the same movement. It appears to me that the above points not only elucidate cases of non-coincidence between the strikes of cleavage and bedding, but they afford a fair and full explanation as to why it is that 'cleavage is symmetrically related to axes of movement of the strata' [Phillips]. So much cannot be said of the purely mechanical hypothesis." (pages 651–2.)

The author states his view of the nature of jointing as follows: "Fully believing that jointing is in the main a physical phenomenon, the particular hypothesis which most strongly suggests itself to my mind, to account for it, is, that it is the product of divisional forces akin to those which give rise to mineral cleavage. The difference between the two phenomena arises, perhaps, from the forces, in the case of jointing, being subordinated to terrestrial magnetism—to laws pervading the crust of the earth; while those productive of mineral cleavage are obedient to crystalline polarity—to laws limited to specific mineral substances or solids. Or, to disarm adverse criticism, usually evoked when anything is ascribed to the mysterious agents that have been introduced (though I wish it to be understood that the circumstances indicate what my leaning is), it may be safer, expressing myself as I did in 1857, to regard both divisional structures as the result of different manifestations of one and the same force." (p. 632.)

The "electro-chemical agents" recognized by the late Evan Hopkins as always silently at work "dissolving, recomposing, and cleaving" both the deep-seated and the superficial strata of the

earth's crust, producing what he termed a "primary structure," and "showing the effect of a polar crystallizing force" (Quart. Journ. Geol. Soc. vol. vi. p. 364), were certainly analogous to, if not identical with Dr. King's world-wide magnetic action producing meridional and other joints. Mr. Evan Hopkins' hypothesis of "cleavage-planes," however, was associated with many untenable views; and certainly did not tend to elucidate the geological structure of either South America or South Australia, to which he applied it. On the contrary, the folded and crushed strata, cleaved by lateral pressure, and showing their obscured bedding-planes only to the instructed geologist, were unravelled on the *mechanical* hypothesis of cleavage alone. So in other countries also; and if this theory be sufficient for such great results, why should we ask for the intervention of an additional divisional structure, of universal range and of enigmatical origin? Dr. King's reply would probably be—at least to account for the above-mentioned cases of imperfect, secondary, and intercalated cleavage-sets, and for some discrepancies in the strike of bedding and cleavage. Good! Jointing is not an uncommon phenomenon, and it must have often been modified by pressure; and hence the explanation, now offered by our author, of some or all of these peculiar stratal conditions. But, as the jointings or other divisional structures, without pressure, will not explain the squeezed, contorted, or elongated conditions of fossils, the parallelized arrangement of particles, and the crumpling of seams, in cleaved rocks, the *mechanical* theory must be accepted, we believe, in most cases, over vast areas of the earth's crust. T. R. J.

II.—On a new Method of determining the Felspars contained in Rocks. By Dr. J. Szabó, Professor of Mineralogy and Geology in the University of Buda-Pesth, Hungary; Corr. Memb. Geol. Soc. With 11 Woodcuts and 5 Coloured Plates, pp. 88. (German edition, 1876.)

THIS method is based on those flame-reactions, for the first description of which science is indebted to Bunsen's classical papers, entitled "Löthrohrversuche," and "Flammenreactionen," published in Poggendorf's *Annalen* in the years 1859 and 1866.

The author of the present Memoir has found it necessary to establish a more precise means for determining the fusibility of the non-metallic minerals in general, and especially of the felspars; and he finds that at the same time all the other characters necessary for the exact determination of the species of felspar can also be observed in the flame.

The whole method is exhibited in a series of tables, in which the degrees of fusibility, the peculiar characteristics of the fused products, and the soda and potash reactions of the different species and varieties of the felspar family are very clearly exhibited.

In carrying out this method for determining the species of a felspar, a fragment of the size of a grain of mustard-seed is held in a loop of platinum wire and subjected to the three following experiments. 1.

It is held in the flame of a Bunsen lamp at a height of 5 millimetres from the base for the space of one minute, and its fusibility noted, its soda reaction being observed directly at the same time, and its potash-reaction through cobalt-blue glass or a solution of indigo. 2. The second experiment is in every respect similar to the first, except in the circumstance of the chimney being placed over the Bunsen lamp, and the assay held at the point of fusion (Schmelzraum) of the flame. 3. In the third experiment the assay is first dipped in distilled water, then in powdered gypsum, and lastly placed at the same point of the flame as in the second experiment.

As the lime cannot be directly determined in the presence of soda and potash, the author infers the quantity of this ingredient from the degree of fusibility and the characters of the fused product of the assay. By digesting the assay in hydrochloric acid, however, for twenty-four hours, the lime when present passes into solution, while the alkalies remain almost wholly undissolved. The author bases on this property of the felspars a fourth experiment for confirming the results of the other three.

The success of this method is of course largely dependent on careful manipulation, the details of which are given with great minuteness in the Memoir. Proofs of the accuracy of the results obtained by it are given in a series of comparisons of the numbers yielded by it with those afforded by the ordinary methods of analysis as applied by well-known chemists to some of the best-known species and varieties of felspar. J. W. J.

III.—On the Causes of Glacial Epochs. [Ein Beitrag zur Frage Ueber die Ursache der Eiszeiten.] By Dr. G. Pilar. (Agram, 1876.)

FROM a study of the phenomena presented by the whole Alpine system, and from the observations of Favre in the Caucasus, and our own geologists in Wales and Scotland, the author concludes that we have indubitable evidence of a former general lowering of the temperature, and a great extension of the glacial systems throughout Europe; and although no moraines or other indications of ice-action have been observed in the Altai Mountains, he considers this is merely due to the moisture from the Indian Ocean having been intercepted by the higher ranges of the Himalayas.

The author further adduces the occurrence of huge erratic blocks scattered over Patagonia and the deep fiords of the South-west Coast of South America, and also the physical features of New Zealand, as proofs of the former extension of cold in both hemispheres.

Dr. Pilar also alludes to the former extension through France, Belgium and England of a subarctic fauna and flora in prehistoric times, as evidenced by the remains of the Marmot, Reindeer and other subarctic animals in the caves of these countries.

The author cites the abandonment in the fifteenth century of the earlier Danish settlements in Greenland (founded in the tenth century), also the cessation of the cultivation of rye in Iceland, and

the destruction of all the larger trees in that island, together with the closing of many historically well-known passes in the Alps, as evidence of recent modifications in the climate, together with various meteorological and botanical observations tending to prove that oscillations more or less extensive in the climate of Europe have taken place in comparatively modern times. Dr. Pilar quotes the earlier observations of Charpentier, in which he suggests, as the probable explanation of the former greater extension of the glaciers of the Alps, that in the Quaternary period they stood at a far greater elevation than at present, thus affording a greater gathering-ground for the névé, and that they have subsequently been lowered by subaerial denudation, the detritus forming the great deltas and alluvial valleys of the Rhine, the Rhone, and the Danube, etc.

The author further gives a résumé of the views of Escher von der Linth, Dove, Albert Mousson, Sir John Herschel, Sir W. Thomson, Lyell, etc.

Dr. Pilar concludes that any theory which proposes to account for such phenomena by local variation of climate will not avail, and that only cosmical causes can have produced them. He cites the opinions of Godwin-Austen, Ramsay and others, as to the former evidences of glacial action in the New Red and Permian formations; he also gives Adhemar's views, formed on Humboldt's observations on the alternation of cycles of cold in the northern and southern hemispheres caused by the greater or lesser eccentricity of the earth's orbit, as worthy of consideration. But he concludes that Mr. Croll's theory is that which most fully meets the requirements of the case. A rival theory to that of Mr. Croll has been promulgated in Germany by Dr. H. Schmick, which seems to resemble that of Col. Drayson, quoted by Mr. Belt.[1]

Dr. Schmick believes that the attraction of the sun, alternately exercised on either hemisphere to a greater or less degree, would cause alternations in the volume of the oceans of the globe in the two hemispheres, resulting in periodic floods and glacial epochs.

One point of interest mentioned by Dr. Pilar is the greater force of the south-west trade-winds, which carry a greater amount of vapour to the Antarctic Continent than the Arctic Sea, thus causing greater relative condensation.

The chief aim of Dr. Pilar's pamphlet seems to be, first, to give a résumé of the various views of writers on climatic variations in the temperature of the globe; second, to combat the views of Dr. Schmick; and, lastly, to cause those of Mr. Croll to be more generally known on the Continent.

[1] See "An Examination of the Theories that have been proposed to account for the Climate of the Glacial Period," by Thos. Belt, F.G.S., Quart. Journ. Science, 1874, No. xliv. See also H. Woodward's Presidential Address to the Geologists' Association, Nov. 1874. And Anniversary Address by Mr. John Evans, F.R.S., President Geol. Soc., London, 18th Feb., 1876.