

are of the utmost importance from a geological standpoint. That crystalline affinity is a definite molecular force accompanied by exothermic changes is doubtless correct, but whether this force can determine chemical changes of an endothermic character is a question involving an entirely new conception, and requires careful consideration before it can be accepted as a reasonable hypothesis. More especially is this the case when we have to deal with the constitution of hydrates, which afford such excellent examples of the application of the phase rule in chemical physics. Hydrates, as is well known, have a vapour pressure of their own, and only continue to exist when in equilibrium with the vapour pressure which they have to support. Thus the hydrates of copper sulphate can be successively decomposed under varying conditions of temperature or pressure. The instability of the aluminium hydrate,  $\text{Al}_2\text{O}_3 \cdot 3\text{H}_2\text{O}$ , at moderate temperatures, also, is a fact well known in chemistry; and it seems probable that the occurrence of any hydrate, either of aluminium or of iron, in nature will depend upon which happens to be the stable phase under the existing conditions of temperature and pressure.

In connection with the crystallization of alumina the researches of W. Spring, of Liège, appear to have some bearing. According to this observer, amorphous alumina or ferric oxide, if damp, can be rendered compact, presumably with the occurrence of an incipient crystallization, by pressure alone; and we are induced to consider whether the amorphous state in solids may not, in some cases at least, be comparable with the condition of superfused solutions and glasses. In fact, many of the distinctions between solids and liquids are gradually breaking down under the researches of modern physics.

There does not, therefore, appear to be any necessity for a new theory to explain the facts in this case. It appears rather that Mr. Holland has unnecessarily introduced a difficulty by presupposing that the molecules in laterite are isolated from extraneous energy. If this were really the case there could be no change of entropy such as he describes, and rightly, to be the result of the reactions involved.

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#### THE COLOUR OF GLASLYN AND OF LLYN LLYDAW.

SIR,—Glaslyn and Llydaw are the names of the two chief Snowdonian tarns. Glaslyn has been noted from time immemorial for the greenish colour of its water, as is implied by its name; but until the Summer of 1899 there was nothing peculiar about the colour of Llydaw. During that Summer, however, for the first time within the last fifty years at least, the water of Llyn Llydaw became as green as that of Glaslyn. The cause of this remarkable change of colour is not far to seek; for in the Spring of 1899, some time about March I am told, the company that works the Snowdon Copper Mine commenced crushing and washing their ore on the bank of Llydaw, so that a large quantity of greenish débris was

and is daily carried into the lake, whose water has thus become turbid and greenish in colour. The rock excavated along the copper veins is of a greenish colour, as may be seen by looking at the tips from the adit-levels.

This change of colour in Llydaw explains the colour of Glaslyn, about the cause of which there has hitherto been some doubt. For it cannot now be doubted that Glaslyn owes its green colour to the detritus of green rock washed into it from the adit-levels of the mines.

J. R. DAKYNS.

P.S.—I should say that the mines are situated immediately above Glaslyn.

SNOWDON VIEW, NANT GWYNANT, BEDDGELERT.  
January 21st, 1903.

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THE TERM 'HEMERA.'

SIR,—Mr. Buckman appears to think that stratigraphy is nothing but geological chronology i.e., that it is chiefly concerned with the days and weeks of geological time, and that the actual sequence of rocks is of less importance.

He will not admit that his definitions of the term *hemera*, or his correlation-table of zones and *hemeræ* in *Quart. Journ. Geol. Soc.*, vol. xlix, p. 519, are open to misconstruction, and yet he complains that most of those who have essayed to use his term have misunderstood the meaning he intended to give it. It now appears that in that table he was giving us a geological calendar, and not an ordinary correlation-table of rock-subdivisions.

The real fact is that Mr. Buckman gave a name to an abstract idea relating to a thing which had no definite name at the time when he wrote. His paper was a stratigraphical one, and he cannot deny that he was actually dealing with the subdivisions of zones, yet, instead of proposing a name for the small subdivisions which he recognized in the sequence of deposits, he gave a name to the time occupied in the formation of each subdivision; in other words, he saw no necessity to give a name to the thing itself, but only to the geological day or week in which it was formed.

He asserts that he was giving a name to the duration of a zone, but this assertion is inconsistent with his original definition of a *hemera*; he says, "successive *hemeræ* should mark the smallest consecutive divisions which the sequence of different species enables us to separate in the maximum developments of strata." Now, a zone is not the smallest possible subdivision of a series of beds, and Mr. Buckman's own tables show that he knew it was not, for they show that it took the time of two or three *hemeræ* to form one zone. Hence, if a *hemera* is anything at all it is not the duration of a zone, but of some subdivision of a zone.

The only point that Mr. Buckman has made quite clear is, that he will not have his term 'hemera' used as the name of a rock-division, but he has not clearly indicated with what recognized subdivision of a stage he wishes the term to be connected. If he makes any reply to this letter, let him state clearly whether he accepts the term *subzone*,