

(of which I certainly forwarded several specimens), my full account of their discovery (Memoir, p. 145) and locality has not been given, and my suggestion that they came from the horizon of this same *Obolus* zone, rather than that of the Magnesian sandstone, has been omitted.

My statement as to the absence of unconformity in the Salt Range is indorsed at p. 2 (wherein he differs from the views presented in the Manual), but he seeks to establish breaks in the perfectly consecutive and stratigraphically united series, both on palæontological and other grounds; overlooking the point that the perfectly united fossil-bearing groups distinguished as Carboniferous and Triassic, Jurassic and Cretaceous, in my list (Memoir, pp. 66, 96, and 277)—from one to another of which some genera at least pass upwards—must be considered more definitely related to one another than any of them are to the equally physically united but unfossiliferous groups beneath.

One of these unfossiliferous groups at a higher stage in my list, No. 8, the red Trias (?), is entirely omitted from Dr. Waagen's transcript of my classification at p. 3 of his paper. Though dealing with other Azoic groups, he seems to have been unable to find a place for this one, leaving it suspended in the anomalous position of Mahomet's coffin.

As a matter of fact, there is little choice as to which of the Salt Range groups are most closely associated or most distinctly divided by stratigraphic features; difference of colour and texture, more or less sudden change, or apparent local transition, being characters observable with varying intensity along most of the boundaries; still I had little difficulty, except in one or two cases, in identifying each group of the series as on a distinct horizon.

Should Dr. Waagen's palæontological labours improve the classification I adopted after consultation with him (as above stated), it will be a welcome result. I regret, however, that his indiscriminate imputations of error compel me to state the actual share taken by him in what had been done previously.

A. B. WYNNÉ,
Geological Survey of India.

JUKES'S THEORY OF RIVER VALLEYS.

SIR,—Mr. Kinahan's reply to my letter on this subject is so extraordinary that I must crave space for a few further remarks.

In his book on Valleys, Fissures, etc., he devotes several pages to the discussion of Jukes's explanation of the river valleys in South Ireland, and from one of these pages I quoted a statement referring to the limestone of that district; yet he now "explains" that the extract "refers to the formation of valleys in any country and in any kind of rocks."

I maintain that the passage cited has no sense unless it refers to the South of Ireland and to Jukes's theory.

Admitting that Professor Jukes in 1862 did believe that the Carboniferous Limestone was *originally deposited* over the whole of S.W. Ireland, yet the theory then enunciated by him in no way

depends upon the truth of this supposition. Jukes never supposed the limestone to have had this extension *at the period when the erosion of the valleys was commenced*; on the contrary, his theory is expressly based on the supposition that the surface over which the rivers originally ran was a plain of marine denudation, cutting across the folds and contortions of the rocks.

Mr. Kinahan now affirms that the Carboniferous Limestone thins out southward, and that its original thickness in the valleys of the Lee, Bride and Blackwater was not so great as Jukes had supposed; but, assuming this to be true, the argument which Mr. Kinahan founds upon this premiss is equally defective in logic and in grammar;—he thinks that “as the theory was founded in a county and on suppositions which were afterwards found to be erroneous,” he is justified in saying that it falls to the ground. I can only express my astonishment that Mr. Kinahan should consider any part of his own country to be erroneous,—the imputation is so utterly inconsistent with his patriotism that in a Dogberry sense it seems to be “flat burglary.” As regards the erroneous supposition (for there is only one), my answer is simply that Jukes’s theory was not founded upon it; Mr. Kinahan himself admits this in the passage I quoted from his earlier work, distinctly saying that it did not much affect the subject, “as some of *the* other rocks are nearly as easily denuded as limestone.”

It will be obvious to others, however, that, if the thickness of the limestone was never sufficient to fill up the troughs to the level of the original plain, their centre would be occupied by a strip of Coal-measures; and that Jukes’s reasoning would still remain the same, for his theory does not depend on the universal presence of limestone, but on the fact of the rocks in the synclinals being more easily denuded than those of the anticlinals.

Having entered the lists in defence of my late uncle’s views on this subject, I am glad of the opportunity of noticing a difficulty raised by Prof. Hull in his “Physical Geology of Ireland.” Prof. Hull accepts the theory in general, but dissents from its application to the Blackwater, because the point where the present river is deflected southward does not coincide with the influx of any stream from the north. But Jukes’s main object was to explain the formation of the transverse ravines, and he looked back to a time when the longitudinal part of the Blackwater Valley did not exist, and when the two brooks from the north “may have united their waters somewhere about the northern end of the Dromana Ravine.” I am quoting Jukes’s own words, and feel sure that were he now alive he would make a similar answer, and would add that the present actual point of deflection has been fixed by the subsequent changes in the river-course since the establishment of the Dromana Ravine. I trust that Prof. Hull will reconsider this point before bringing out a second edition of his work, and may see his way to accept the theory without excepting the Valley of the Blackwater.

GEOL. SURV. OF ENGLAND,
ALFORD, Aug. 8, 1879.

A. J. JUKES BROWNE.