

which have been deposited in the areas surrounding the reefs. These dolomitic reefs are mostly poor in fossils, but the author attributes their deficiency in this respect to the same causes which have brought about the obliteration of organic remains in recent coral reefs. The numerous sections figured and described, in which mechanical deposits of tuff and other materials rest on, and occasionally dove-tail into, the steep slope of the reef-like walls of pure dolomite, are strong evidence of what the author puts forward as the main object of this work to prove—the contemporary formation of these deposits of such different characters in the period of the Trias.

The concluding chapter treats of the periods at which the different dynamical forces have operated to produce the present mountain ranges, and the principal direction of the various dykes and faults.

The maps accompanying the book are drawn to the scale of  $\frac{1}{75000}$ , and no fewer than 47 different geological divisions are indicated on them in different tints. The photographs are produced by the Albertotype process, and give an excellent idea of the physical characters of this Alpine region. This work merits the study not only of those who purpose themselves to visit the region described, but of every student of geology. H.

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#### CORRESPONDENCE.

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##### RECENT PUBLICATIONS OF THE GEOLOGICAL SURVEY OF INDIA.

Manual of the Geology of India, Introduction, chapters xx. and xxi., by W. T. Blanford, Esq., F.R.S., etc.

Palæontologia Indica, Series XIII., Salt Range Fossils, by Dr. W. Waagen.

SIR,—Being the person to whom the geological examination of the Upper Punjab, as well as the Salt Range, has been entrusted, I would point out that in the publications above mentioned many of my statements, as recorded in my Salt Range Memoir (*Geol. Surv. Ind. Mem. vol. xiv.*) and other papers, have not been accurately reproduced.

In the Manual, although chapters xx. and xxi. are said in a footnote (p. 480) to be “chiefly compiled from data furnished by Mr. Wynne’s papers, except where the contrary is stated,” there are very numerous instances, unaffected by the last clause of this passage, in which the published statements of my memoir are replaced by others with which I cannot coincide.

So far as mere speculations are concerned, opinions may of course differ widely, but as to statements of fact I adhere to the views presented in my various papers regarding the geology of the Upper Punjab, including the structure of the Salt Range; with which no other officer of the Survey is more fully acquainted than I am myself, while the writer in the Manual has not even seen the ground.

Throughout the introduction to the lately issued part of the *Palæontologia Indica*, Dr. Waagen repeatedly attacks my classification of the Salt Range Series, giving an even less definite one of his own, and while condemning that which I adopted, never makes the least allusion to the facts; that mine was based upon the general determinations of our Survey palæontologists (and others), of whom he was

then one; also that this classification was made in his own presence, in consultation with himself, without his offering a single objection to it at the time, when specially deputed to assist in the interpretation of the ground. He implies that I treat these local divisions as "real formations equal in importance to 'Silurian, Devonian,' etc. (p. 2)," and says (p. 6) that I appear to have considered the older division of the series "as equivalent to the whole Palæozoic series as it has been defined in Europe and elsewhere."

So far from these statements being correct, I referred the recognizable divisions of the series, on the best palæontological evidence available, to their general ages as "rock groups"<sup>1</sup> merely—and not as "formations" with the definite sense Dr. Waagen would attribute to my words,—leaving the ages of the unfossiliferous zones uncertain, distinctly stating, at p. 281, there was no reason to assert the presence of a Devonian group, and pointing out at p. 118 that considerable intervals were left unrepresented.

Dr. Waagen now classes the *Obolus* beds, or Silurian zone of my series, with the *Productus* limestone of his list in one division; entirely omitting to mention that this Silurian zone was originally founded upon Stoliczka's determination of the fossils I obtained from it, confirmed by himself.

His statement (foot-note to p. 3) that Dr. Fleming and I considered *Terebratula* (*Waldheimia*) *Flemingi* (belonging to the upper part of the series) to be Carboniferous, is simply without foundation, so far as I am concerned (see my Memoir, p. 104, where it is mentioned as coming from a higher stage; and section, p. 190, No. 11, where a *Terebratula* supposed to be the same is noticed on the evidence of Dr. Waagen's own notes). I was quite aware of Mr. Davidson's remarks regarding this fossil, but left the settlement of such a point as a matter of course to the Survey palæontologists.

Dr. Waagen further accuses me of wrongly grouping his sections, which in all cases I copied from his own dictation in English; but in one to which he refers at p. 30, owing to his own omission of the groups, I had to supply these from comparison with others. If there are errors in these sections, the fault is his own, and the grouping into which they were thrown at the time is proof of Dr. Waagen's assent to the classification then adopted without reservation or condition on his part.<sup>2</sup>

With regard to this classification generally, now altered by Dr. Waagen, it will be observed he gives no reason at present for questioning the accuracy of Stoliczka's (and his own) determination of the *Obolus* or *Siphonotreta*, on which the Silurian age of one division was based, preferring to attribute error to myself alone.

And further, in referring to the peculiar fish teeth from Chel Hill

<sup>1</sup> See chap. iii. of my Memoir referred to.

<sup>2</sup> See his subsequently written paper, Mem. Geol. Surv. Ind. vol. ix. p. 351, where, notwithstanding some uncertainty stated in the text, he uses the words "*Carboniferous deposits of the Salt Range*" in the title, and indicates elsewhere the Carboniferous affinities of certain fossils of the Range. This shows that the Lower Salt Range limestone was then referred to the Carboniferous period by others on the Indian Survey, as well as by myself.

(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.

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#### 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