

fauna showing the change to deeper-water conditions. Near Folkestone the change to the deeper water of the Grey-chalk sea is very plain, and is seen to have been a gradual one. The discovery of these Red Clays is of exceeding interest, but it is misleading to speak of them as analogous to the Gault.

J. S. GARDNER.

PARK HOUSE, ST. JOHN'S WOOD PARK, N.W.

May 17th, 1877.

#### DR. WILLIAM SMITH'S GEOLOGICAL MAPS.

SIR,—At a recent sale the copper-plates of William Smith's original folio atlas of geologically coloured maps of England, sixteen in number, including the index, published in 1821, came into the possession of Mr. Edward Stanford, of Charing Cross, who is willing to sell them at, as he writes to me, a trifling cost (for sixteen large coppers), if purchased for the Geological Society. It would not pay now-a-days to reprint maps only of historical interest; but I venture to think that the maps of the father of English Geology are worthy of being preserved from the melting-pot, the doom of superannuated copper-plates, and entrusted to the safe keeping of some chartered society. I write this, therefore, to obtain the opinion of geologists on the matter, and shall be glad to receive the names of gentlemen who will subscribe for their purchase, as I propose, for presentation to the Geological Society, which already possesses the original manuscript maps.

G. S. BOULGER, F.L.S., F.G.S.

SCIENTIFIC CLUB, 7, SAVILE ROW,

July 12, 1877.

#### PREMATURE CONCLUSIONS.

SIR,—The practice of the Geological Society, of publishing "abstracts" of papers read at the meetings, before the papers themselves are published, is sometimes of great service both to the authors and to the public; but it has this serious drawback, that the public generally found their conclusions regarding the value of the paper—and the correctness of the author's views—not on the paper, but on the "abstract," which necessarily contains but an imperfect statement of the data upon which the author has rested his arguments; and the probabilities are, that when the paper itself appears *in extenso* some months afterwards, the men who have based their conclusions upon the statements of the "abstract" will not care to make themselves acquainted with the details and arguments of the paper.

This drawback has come with great force to my mind (as no doubt it has done in the case of others) from the manner in which the paper I had the opportunity of bringing before the Society has been received and criticized in several quarters. One geologist, for whose opinion I entertain a high respect, wrote at once to intimate that he could not accept my conclusions; and when I naturally replied that he had not had an opportunity of reading the details upon which they had been founded, he replied that, "having seen the 'abstract,' he knew already quite enough to satisfy his own mind on the subject;" and I greatly fear my friend, who on a former

occasion has openly expressed his impatience of long papers, will consign my production to the shelf or waste-paper basket when it reaches his hands, as he probably considers he knows enough of the matter.

The paper of Mr. Dakyns in the *GEOL. MAG.* for this month (July, 1877), is another case in point. It contains a *critique* on my paper as contained and represented in the "abstract" only; and of this I complain. If it had been based on a perusal of the paper itself, I should have been perfectly satisfied, whatever the conclusions of my reviewer might have been, because I would have been aware that he had all the data before him; and if these did not bring him to the same conclusions as myself, I should conclude that this was owing to the fact that his mind and my own are constituted differently; but I deprecate conclusions drawn from a partial knowledge of the facts.

I cannot now go fully into Mr. Dakyns' objections—time and space forbidding. I ask him, however, to mark the force of the term "essentially"—as used by me—and to recollect that it does not mean *exclusively*.

Then as regards the difficulty of believing the Gannister beds to be marine *essentially*—notwithstanding the large number of marine mollusca, etc., they contain—because of the occurrence of beds of coal in Scotland. This is not so surprising as the occurrence of beds of coal in Scotland overlaid by marine limestones, which shows that Nature accomplishes results which man sometimes cannot conceive.

As regards the term "Yoredale," Mr. Dakyns, as an officer of the Geological Survey, might surely have concluded that I have adopted the term as it is used by the Survey itself, whatever its original signification may have been. It may not be strictly correct, but it would be hard to find a better for the great series of beds above the Mountain Limestone of Derbyshire.

As regards the latter part of Mr. Dakyns' paper, does he hold the opinion under which I myself was enthralled till lately, that the great limestone series of the north of England and Scotland is all of it the representative of the true Carboniferous Limestone of Derbyshire and Lancashire? If so, I believe this to be a popular delusion, which I have endeavoured to prove as such in my paper. The true Carboniferous Limestone is, I believe, represented in the north only by the bed (or group of beds) known as "the Scaur Limestone" of Phillips, and in Scotland, as the Lower (or Roman camp) Limestone. The series of beds, limestones, ironstones, coals, shales, etc., which overlie this, being the representatives of the "Yoredale" beds only. Lastly, let me ask how is it possible to believe the Carboniferous rocks to be "one indivisible formation," if by that term is meant a heterogeneous collection of beds of various mineral characters, and of various modes of formation, in the face of the great fact of the predominance of marine limestones in the lower part, and their entire absence in the upper? So far from this being the general conclusion to which a survey of the Carboniferous rocks of the British Islands and the West of Europe would lead us, I

have always found it rather difficult to prove to a student that the Lower and Upper Carboniferous beds really belong to one formation at all, so great is the contrast between the “essentially” marine aspect of the lower, and the essentially lacustrine aspect of the upper division. If this be so, is it not “philosophical” to suppose that there is a middle group, between these extremes, “essentially” marine, yet less oceanic than the lower stage of the Mountain Limestone?

Meanwhile, allow me to ask my colleague to defer his opinion on the views I have stated in my paper till he has had an opportunity of reading it.

EDWARD HULL.

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#### THE RELATION OF THE PERMIAN TO THE TRIAS.

SIR,—Mr. Irving appears to have quite mistaken the purport of my communication on the relation of the Permian to the Trias in the neighbourhood of Nottingham.<sup>1</sup> I understood it had been stated by geologists of Nottingham, that not only a perfect conformity existed between the Permian and the New Red Sandstone near that town, but there was a passage upwards from one formation into the other.

I merely wrote to say this could not be, for the reasons I gave. But I never intended to imply there was not a general conformity between the two formations, for this general conformity must be apparent to any one on looking at a good geological map, whereon these formations are laid down. Neither did I intend it to be understood that I considered that the break between the Permian and the New Red Sandstone was greater than between some of the subdivisions of these formations. As, for instance, the break between the Middle Marls and Lower Magnesian Limestone of the Permian, or that between the Keuper and the Bunter of the Trias. I gave no opinion one way or the other on these points.

The point of my communication was this. The relation of the Permian to the Trias I considered an important problem yet to be worked out. If a perfect passage from the one up into the other was found, it would go far to settle the question. As far as I know, that passage has not been found, and, I contend, it does not exist in the neighbourhood of Nottingham.

Some personal remarks in Mr. Irving's communication I shall not reply to, they have nothing to do with the question, and were wholly uncalled for. I do not consider the pages of a scientific magazine the place for that kind of bantering.

W. TALBOT AVELINE.

#### HURONIAN VOLCANIC ROCKS.

SIR,—In an able paper in your last issue, Mr. George M. Dawson publishes the results of his study of the “Porphyrite Formation” of British Columbia, and applies these results to the explanation of the origin of the Huronian series of Eastern North America. I am particularly pleased to find so good an observer as Mr. Dawson not

<sup>1</sup> GEOL. MAG. Dec. II. Vol. IV. p. 155.