

as well as Greenland and the Eastern and Western coasts of North America (Jeffreys, *op. cit.* vol. iv. p. 321).

XII. *Fusus Fabricii*.—Jeffreys only gives Greenland and the White Sea as the habitat of this shell (*op. cit.* vol. iv. p. 322).

XIII. *Littorina expansa*.—This is apparently the *Turbo expansus* of other writers, and the only habitat I can find for it is that given by Forbes as Arctic Seas, and coasts of America.

XIV. *Margarita inflata*.—The *Turbo inflatus* and *Turbo Groenlandicus* of other writers. It lives, says Jeffreys, in every part of the Arctic Ocean, and on the coasts of the White Sea, Scandinavia, Iceland, Canada, and the States of Maine and Massachusetts, and he mentions also its occurrence as local, but not uncommon, about the West of Scotland, the Orkneys and Shetlands, in Dunnet Bay, Caithness, etc. (*op. cit.* vol. iii. p. 299).

XV. *Velutina undata*.—This shell occurs in the seas of Boreal America (Forbes, p. 422).

XVI. *Natica clausa*.—This shell occurs in Scandinavia as far south as the Christianiafiord<sup>1</sup> (Jeffreys, vol. iv. p. 229).

This examination is assuredly very impressive. Of the various shells enumerated by Forbes and others as proving glacial conditions to have surrounded them when alive, there is hardly one which is not still living in the North Atlantic at the present moment under conditions completely different to those we describe as glacial. No doubt these shells do live and thrive in very high latitudes, and doubtless also have their focus there at this moment; but it is no less true that they extend down into the temperate regions of our own seas and those of the United States and of Southern Scandinavia. If the whole of the mollusca of the beds we are dealing with belonged to the same facies, we should be bound no doubt to conclude that the conditions of South Greenland or of Iceland prevailed here when these shells were living; but the fact is this class forms a small proportion of the whole. Here we must at once say that Forbes's method of tabulating the results of his examination of all the beds together will no longer be deemed satisfactory. We have now the clearest evidence both in Scandinavia and in Britain that some of the beds have a much more arctic facies than others. We have in both areas beds which apparently represent the old sea-bottom of mud or sand with the shells still remaining *in situ*, bivalves standing erect with both valves intact, and with all the appearance of being undisturbed. These are the lower beds. For the most part, so far as I know, they occur at low levels and are characterized largely by shells of an arctic type.

(To be continued.)

#### VII.—THE PERMIAN AND TRIAS—REJOINDER TO PROF. HULL.

By REV. A. IRVING, B.A., B.Sc., F.G.S.;  
of Wellington College.

PROFESSOR HULL'S criticisms (pp. 491–5 of this volume) on a former paper of mine on the "Classification of the Permian and Trias" lay upon me the burden of a brief reply. I may say

<sup>1</sup> Dr. Van Geuns discovered it in a Pliocene bed near Palermo.

that Prof. Hull seems to suppose that the task which I had set myself was more ambitious in its scope and aim than I was conscious of; it was *not* my purpose "to review the whole question of the relations of British Permian and Trias" in a paper which was limited by its title to the European rocks, and from which these systems in other parts of the world were intentionally omitted. My idea was rather to give to readers of the *GEOL. MAG.* an opportunity of forming some judgment on the results which might be fairly gathered from a digest of the opinions collected from the various members of the "Sub-Committee on Permian and Trias," taken along with the information which exists in the literature of this subject in England and on the Continent, more especially in Germany; and to raise discussion thereupon before the assembly of the Berlin Congress in 1884. My starting-point in this investigation was the elaborate table of the Permian and Triassic rocks of England and the Continent, which Professor Hull himself was good enough to send in to the Committee, before the duty was entrusted to me, in conjunction with Mr. De Rance, of doing the work of 'Reporters' of the Sub-Committee. I had read and seen enough of Continental geology to perceive at once how inadequately Professor Hull had represented that side of the question in his table; and it was with much surprise that I found so high an authority giving a reference (and his only reference) for information on the Muschelkalk Formation to a statement contained in about half a dozen lines of De la Beche's *Manual of Geology*, more than fifty years old. It was somewhat incomprehensible to me that he should go back so far for information, and thereby ignore the labours of a whole generation of such workers as the two Credners, Quenstedt, von Hauer, Gümbel, Zittel, Geinitz, and a host of others, 'men of renown' wherever these formations are intelligently studied. Here I may be permitted to thank Prof. Hull for the great services which his otherwise copious list of references has rendered to me; I assure him that I have worked them out carefully and honestly, and that had he been good enough to add to them a reference to the "Survey Memoirs of South Lancashire Coalfield, etc.," these too would have been carefully studied. As it is, I must confess that I have overlooked them, though it still remains possible for me to consult them. This point therefore I must beg to waive for the present.

Had I started in the investigation committed to any very definitely expressed opinions, it might have been suspected by some, who shirked the trouble of following me in wading through the mass of literature which I have had to consult, that I was not quite proof against the temptation to twist things to answer my own ends. But apart from the folly of such a course, when the literature of the subject is open to the whole world of critics, it may suffice, in order to allay even Prof. Hull's suspicions, to mention that at the outset I was certainly far more inclined (in common with several geologists of no mean standing) to think much less of the distinction between Permian and Trias than the investigation of the subject has led me

to do, as any impartial and unprejudiced reader of my paper can see for himself. I can assure Prof. Hull that I did not begin with any dire design of smashing to pieces the beautiful tripod image of a "Palæozoic Trias," which the great Murchison set up for the veneration of geologists, and for which the learned Professor seems to have a lingering affection; the conviction, not only of its meaninglessness, but of its mischievous fertility in confusion of ideas, grew upon me gradually as I went on with the subject. As it is, one cannot fail to read with some satisfaction Prof. Hull's own admission that he is "almost inclined to concur that there is but little evidence to support the view of a threefold division of the Permian beds" (p. 493). So much for the general drift of my paper.

I now come to matters more of detail which Professor Hull has touched upon. In the first place he says that I "quote his name as a supporter of the view that it (*i.e.* the Sandstone of the Vale of Eden) is of Permian age." My reply is that *I have not quoted Prof. Hull's name in this way*; nor do I fear to challenge him to show a single passage in my paper in which I have connected his name directly with the "Sandstone of the Vale of Eden." The only three occasions in which his name is mentioned in any such connexion are in the two paragraphs, in the earlier part of the paper, the one beginning, "It is no wonder that such special pleading . . .," the other, "In *Siluria* there is much vague talk . . . ." In these three cases I have been studiously guarded in the use of language, out of consideration both for Prof. Hull's susceptibilities and for my own wish to put things in the truest possible light. What I do say is (*a*) that "It (*i.e.* the triple classification of the Permian System as put forth by Murchison) was adopted by Prof. Hull in a paper read by him before the Geological Society of London," and I give the reference to this paper, Q.J.G.S. vol. xiv. This statement any one can verify therefore for himself by a simple act of reference. (*b*) Nearly a page further on I say, "The classification put forward by Murchison still finds favour in some quarters; and quite recently so high an authority as Prof. Hull has proposed it for the acceptance of the British Committee." Prof. Hull has surely been caught here "reading the spirit of the commentator into the text." The readers of the *GEOL. MAG.* are doubtless capable of giving to language its natural construction, and will hold me responsible only for my *ipsissima verba*, not for another person's version of my statements. On this latter point the question therefore narrows itself to the categorical answer to be given to the question: "Did Prof. Hull, or did he not, propose the threefold classification (after Murchison) of the Permian System for the acceptance of the British Committee?"<sup>1</sup>

Prof. Hull states "that the evidence (as to the particular horizon of the Sandstones of the Vale of Eden upon which Murchison rested his claim) was rather of an inferential and indirect nature;" and so

<sup>1</sup> On referring to the original document I find that the "Upper Sandstone of St. Bee's Head" is mentioned expressly by Professor Hull as a part of the "Upper Permian."

far he admits substantially the force of the reasoning adopted by Mr. Goodchild, to which I have referred in that part of my paper in which Prof. Hull's name occurs. He then goes on to say that he is quite prepared to "fall back upon Prof. Sedgwick's views as soon as the officers of the Geological Survey shall have definitely pronounced in their favour;" yet in the sequel he does not hesitate to reject the testimony of two several officers (not certainly speaking *ex cathedra*) of that much respected body in favour of such views: the evidence of two others (some of which is most to the point) he ignores altogether.

As regards the joint paper of Murchison and Harkness on the Edenside rocks, what Prof. Hull urges goes certainly to show Harkness's modesty in surrendering so soon the views he had previously expressed. Those who know anything of the contests which Murchison waged with Sedgwick on the one hand, and with M. Jules Marcou on the other, will reserve to themselves the right of forming a judgment on this matter.

Two points raised by Prof. Hull remain to be specially dealt with:

1. Mr. Strahan's view, which has been summarized in the Report of the Proceedings of Section C. of the British Association, 1881. Will Professor Hull be good enough to show by reference to this how I have misrepresented Mr. Strahan? If he cannot show this, the quarrel remains with Mr. Strahan, who, I dare say, can take care of himself. I may add though, that I was present at the meeting of the Section, when the said paper was read, and with the sections before the meeting had a better opportunity of judging of the paper than the mere summary of it affords. It is a pity that Prof. Hull, who was then in York, should not have refuted Mr. Strahan's arguments at the time. The palæontological evidence cited by Prof. Hull certainly shows the presence of the Magnesian Limestone series in South Lancashire, which no one ever called in question: I fail utterly to see how Prof. Hull can regard the strata which furnish such evidence as the "equivalents of those referred to by Mr. Strahan," without at the same time surrendering the question of these latter being "Upper Permian."

2. In a footnote Prof. Hull shows himself very incredulous of evidence which he does not like. I presume that he is prepared to allow considerable weight to any views which Mr. De Rance may express on the Permian strata of the Lancashire area; and in this note he is indirectly bearing his testimony to the value of opinions from such a quarter. Whether or not Prof. Hull's incredulity may be overcome, I know not; though he could easily have obtained from his colleague an affirmation or a denial of what had been attributed to him by me, if he had cared very much to know what Mr. De Rance's views really were. At any rate the readers of the *GEOL. MAG.* will be able to judge for themselves, by comparing the following literal transcript from a MS. in Mr. De Rance's own handwriting with the statement made in my paper:

PERMIANS OF LANCASHIRE.

<i>North Lancashire.</i>	Furness Abbey and Hawcoat.
Upper Permians.	(St. Bee's Sandstones).
Middle Permian.	Magnesian Limestone of Stauk (Schizodus).
Lower Permian.	(Penrith Sandstone, absent.)
<i>Lancashire Fylde.</i>	
Upper Permian?	(Garstang Sandstones.)
<i>South-West Lancashire.</i>	
Upper Permian.	(Absent.)
Middle "	Magnesian Limestone, 6 feet at Skillaw Clough.
Lower "	{ Marls, 30 feet.
	{ Sandstone, 40 feet.
<i>South Lancashire.</i>	
Middle Permian.	Marls and Limestones (Schizodus) 300 feet.
Lower "	{ Collyhurst Sandstones 300 feet at Manchester
	{ thickening north-east to 1000 feet.
.....	Unconformity.

It is obvious that I could not give a reference to an unpublished paper; but my own statement was: "He (Mr. De R.) considers the 'Upper Permian' of the Survey to be wanting in South Lancashire, and is doubtful if the Garstang Sandstones ought to be so considered. Mr. De Rance also considers the so-called 'Upper Permian' of North Lancashire to occupy the same horizon as the St. Bee's Sandstone." Further comment is needless.

VIII.—ON THE DISCOVERY OF CAMBRIAN ROCKS IN THE NEIGHBOURHOOD OF BIRMINGHAM.<sup>1</sup>

By CHAS. LAPWORTH, F.G.S., ETC.,

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THE geology of the neighbourhood of Birmingham is one of more than ordinary interest, owing to the great variety of geological formations exposed within its area. Its rocky structure was investigated about thirty years since by the officers of the Geological Survey of Great Britain, and its several geological formations laid down upon their maps, and described in detail in their various explanatory memoirs. Some of these publications—notably the maps and descriptions of the South Staffordshire Coalfield—have subsequently become classic in the literature of Geology. Since these publications were issued, however, the science of Geology has made great advances, more accurate and detailed methods of research among the older rocks have been developed, and their application to the study of the strata of the Birmingham district has recently resulted in the detection of several most interesting facts which escaped the notice of the earlier investigators.

In the geological history of the Birmingham district three grand historical periods have long been recognized—the deep-water period of the Silurian, the Estuarine period of the Carboniferous, and the Continental or inland-lake period of the Permian and Triassic.

The oldest strata of the region have hitherto been supposed to be of Upper Silurian age. They compose the rocks of the Lower

<sup>1</sup> Reprinted from the Proceedings of the Birmingham Philosophical Society. vol. iii. page 234.