and refers to the Vale of Eden, South Wales, and other distant areas in connection with the Vale of Clwyd. In my paper, however, I relied on the same system of faulting in South-West Lancashire and West Cheshire—about Liverpool and Birkenhead—where there are many such north-and-south faults, dislocating the Trias, and varying from a throw of a few up to 1,000 and even 2,000 feet,[1] equal to any in the Vale of Clwyd and all indisputably of post-Triassic age. This area is only 25 miles from the Vale, so that the conclusion is irresistible that the faults in both areas were produced at the same time. The post-Triassic faults in South-West Lancashire and West Cheshire are as great dislocations as those which traverse the Coal-measures in the country to the east, the only difference being that the Trias has been denuded from off the older strata, after the faulting had been completed.

With the exception of the Carboniferous Limestone, there are few really important exposures in the Vale of Clwyd, and most of them afford considerable scope for the use of the imagination, and it is surprising when anyone has a theory to uphold how facts crop up to support it. I have my theory, and appear to see faults where Mr. Strahan does not, while he thinks he can see evidences of the Trias overlapping the Carboniferous Limestone and perhaps the Wenlock Shale where I do not. It seems to me that it is the absence of good sections that is the cause of the difficulty.

I have been indebted to Mr. Strahan for much information, and for the position of exposures which, however, I always examined for myself, while on the other hand I constantly informed him of the progress of my work in many areas in North Wales. No geologist is a more careful observer than Mr. Strahan, and I much regret that he left the Vale of Clwyd before I began to examine it about seven years ago. Still, he has only completed a portion of it, so that when he has finished there may be little difference of opinion between us.

G. H. Morton.

Liverpool, *March* 20, 1899.

THE EASTERN MARGIN OF THE NORTH ATLANTIC BASIN.

Sir,—Will you allow me briefly to reply to the communications from Admiral Sir William Wharton and Mr. Hudleston which appear in the April number of the Geological Magazine, so far as they concern myself.

Presuming from the context that the question to which Admiral Wharton was invited to reply refers to some supposed statement of mine, I have to say that I am not aware of ever having asserted that there are "submarine vertical precipices 7,000 feet or so in height," and, therefore, as far as regards anything I have written I might leave the matter to others. But I can scarcely conceal from myself that the words have been put into my mouth, and I have reason to complain that no reference is given to which I can refer. I have, it is true, called the sub-oceanic 'slope,' along which the Continental

[1] The boundary fault (2,000 feet) has the Trias only on the downthrow side, but before the country was denuded the Triassic strata were on both sides.

platform terminates, 'an escarpment' or 'declivity' of 7,000 feet or more, but that is a very different thing from a 'vertical precipice' of that height. The nearest approach to the above quotation I can find in what I have written is in my paper published in the Transactions of the Victorian Institute for 1896–7, and subsequently in the GEOLOGICAL MAGAZINE, August, 1898, in which I state, and state truly, if the soundings are correct, that the escarpment off the Porcupine Bank of 7,800 feet "is quite precipitous" (p. 354); also that there is "a sheer precipice of 5,000 feet just south of La Rochelle Bank"; but in neither case do I use the word 'vertical,' although, in *some parts* of their descent, the cliffs no doubt *are* vertical. I draw a distinction between a 'precipice' and a 'vertical precipice.' It is very rarely that precipices of a thousand feet and upwards are vertical; but they do not cease to be precipitous at any angle less than (say) about 45° to 50° from the vertical. That some faces of the outer declivity approach these angles, or even exceed them, throughout a portion of their descent from the edge of the British-Continental platform, is fully borne out by the soundings, but it will be observed that this is a very different statement from that which has drawn forth the emphatic reply of the gallant Admiral.

But that there are precipices, in the sense I have explained above, of 6,000 or 7,000 feet in some of the submerged river valleys, such as those (presumably) of the Mondego (lat. 40° 30′ N.) and that which lies off Cave Carveiro (lat. 39° 30′ N.), is clearly shown by the soundings. I cannot expect Admiral Wharton to have recognized them; for even in tracing by a light dotted line the 100-fathom contour on the Admiralty Charts the draughtsman has sometimes lightly skipped across these indentations, which he probably considered of no consequence; still, there they are, when the isobaths are accurately worked out. I cannot, however, but feel obliged to Admiral Wharton for his quotation from the report of Captain Hoskyn when he says, regarding the form of the great declivity: "The intermediate soundings give no evidence [off the coast of Iceland] of a precipice, but a mountain of this height on the land would present an imposing appearance, with perhaps some steep escarpments." This is exactly my own view; and if the reader will examine the sub-oceanic sections given with the map in my original paper in the Transactions of the Victoria Institute, quoted above, he will see that this is so.

Referring now to Mr. Hudleston's important contribution in the March and April numbers of the GEOLOGICAL MAGAZINE, I have nothing to complain of the manner in which he has dealt with my own views; and I am glad that, once and for all, he has given his powerful aid in favour of the view that the British-Continental shelf was at a former time a land surface, and that "so long as Professor Hull confines himself to tracing the old river-courses cut in the continental shelf he is pretty safe" (p. 153). It then only remains to be determined to what depth below the general level of the platform the channels may be carried down;

and without drawing in the least on that "scientific imagination" with which my distinguished friend is kind enough to credit me, and relying only on the use of my reasoning faculty, I maintain that they must be carried down to the lowest levels shown by the soundings, since rivers only flow at the bottom of their valleys! And, in this connection, let me ask Mr. Hudleston with reference to his plan of the Gulf of Gascony and his isobathic contours of 100 fathoms and 1,000 fathoms of the Fosse de Cap Breton (p. 151), why has he left the latter in so incomplete a state? The 1,000-fathom contour is broken in two just above the point where the narrow channel of 1,500 fathoms opens out on the abyssal floor, as will be seen by reference to the map itself. Surely with so many soundings there can have been no great difficulty in carrying the 1,000-fathom line eastwards to the point of crossing, which would indicate the form of this remarkable sub-oceanic ravine; and if contours of intermediate depths (say 750, 500, and 250 fathoms) had been traced, they would have thrown additional light on its form and character. As it stands, Mr. Hudleston's map is unintelligible, and reminds me of that of M. Elisée Reclus, who leaves the mystery of the Fosse de Cap Breton unsolved.

EDWARD HULL.

April 7, 1899.

THE ASSOCIATION OF *SCHLŒNBACHIA INFLATA* WITH *HOPLITES INTERRUPTUS*.

SIR,—In discussing "The Base of the Gault in Eastern England" (GEOLOGICAL MAGAZINE, April, p. 161), Mr. A. M. Davies refers to the mixture of Lower and Upper Gault species at Heath, near Leighton Buzzard, and remarks that the same mixture "appears to exist in the Isle of Wight, where *S. inflata* occurs in the Gault Clay along with *H. interruptus*," his authority for this statement being the Geological Survey Memoir on the Isle of Wight.

It is true that in the tabular list of fossils at the end of that Memoir (p. 279) *Am. rostratus* and *Am. interruptus* are entered in the Gault column with the indication that both were found at Compton Bay, but it is not stated in the text that they were found in association. It so happens that I have had occasion to investigate this very point, and discovered that Mr. Rhodes had only found *H. interruptus* in the lower 20 feet of Gault, that a specimen obtained between 73 and 93 feet from the base was *H. denarius*, and that the *S. rostrata* came from a still higher bed, namely, that given at 8 feet thick in Mr. Strahan's section on p. 63 of the Memoir.

Other specimens of *H. interruptus* have been found in other parts of the island, but always in the lowest part of the Gault and never in association with *S. rostrata*. There is no mixture of zonal species, but the upper part of what is referred to 'Gault' by Mr. Strahan and others belongs to the zone of *S. rostrata*.

At Heath, on the other hand, there is unquestionably a commingling of Lower and Upper Gault species, and I am quite unable to explain it unless the Upper Gault should turn out to be much