VI.—MR. HUDLESTON "ON THE EASTERN MARGIN OF THE NORTH ATLANTIC BASIN."¹

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THE delay of several months, in replying to Mr. Hudleston's communication relative to the above-named subject, has been due to absence in the field in a distant region, where I was not in touch with the current literature.

In the study of suboceanic topography, it is to be regretted that most of the writers use the hydrographic unit of measure—the fathom—where the investigations have for their object the comparison of the submarine features with those of the land where the unit is in feet. This use of two units dims the comprehension, unless the differential measurements are translated into the same standard.

On nautical charts, the 10- and 100-fathom contours are generally drawn for navigation purposes, and the latter is the only contour-line that usually approaches the suboceanic continental margins. Hence this line is most commonly spoken of as if identical with the submerged margin of the continent. This inaccuracy would scarcely obtain were the measurements in feet adopted. The edges of the submarine platforms vary all the way from 300 to 1,200 feet, though the margins of the outer shelves often reach to 3,000 feet or more.

In the study of the peculiarities of the suboceanic continental margins, contours of 1,000 or 500 fathoms apart are not sufficiently close to bring out the features in an intelligent way, although they may be all that are practicable on a map of small scale. By drawing them at 200 or 300 feet apart, I have found that many difficult features became easily explicable. If Mr. Hudleston had taken the fullest data and contoured the slopes from 100 to 1,000 fathoms on his map of the "British Biscayan Atlantic," with isobars of 200 or 300 feet, it is possible that he would not have found that the soundings "in certain localities round the Bay of Biscay are curious and perplexing." It is such curious and perplexing features as these, made known to a better degree in more favoured regions, which I have studied in large numbers, and which Professor Hull has found and more fully studied off the European coast. But Mr. Hudleston, in his really valuable paper, does not seem to have recognized their importance.

The first striking feature in Mr. Hudleston's paper is his study of the gradients of the suboceanic continental slopes. He has shown a very large number of cases where the declivities at different points amount to from only 1 in 188 to 1 in 7 or 8, except in the Bay of Biscay, where they may be as much as 1 in 4 or 5, and in one case so precipitous as to reach a deepening of one foot in two. His averages are those extending usually over considerable distances of 20 miles or more. The conclusion drawn from them is that the great continental slopes are not

¹ GEOL. MAG., Dec. IV, Vol. VI (1899), pp. 97-105 and 145-157.

vertical or nearly vertical precipices, which neither Professor Hull nor I ever stated, and, therefore, much of Mr. Hudleston's labour for the apparent purpose of refutation is lost. But on analyzing his method, it is found superficial only, as it does not give us a true idea of the variable forms and pitch of the continental slope. In other regions, notably the Floridian channel, leading to the Gulf of Mexico, the analyses of the soundings show that the mean slope in no way conveys an impression of the actual form of the deepening of the channel. The floor of the submerged valley is in reality made up of a series of very gentle gradients, or almost horizontal steps, separated by precipitous declivities. The margin of the Mexican plateau—back of Vera Cruz—has an elevation of somewhat more than 8,000 feet above the sea, or 6,500 feet above the inner margin of the coastal plain. Following up one indentation, the average declivity is only 1 in 30, while at another point it is 1 in 10. But these averages do not suggest the declivity of the great escarpment, for the slope is made up of a series of steep gradients, often 1 in 3 or 4, or perhaps in places 1 in 2, separated by zones of comparatively small pitch. The soundings along the Atlantic margin, which Mr. Hudleston has studied, may not have been close enough to reveal the steps, although he has shown that in the Bay of Biscay the declivities are precipitous. It appears to me that his citations of the mean declivity of the continental slope scarcely become a criticism of the characteristics of the great drowned valleys, as pointed out by Professor Hull and myself.

A second feature made by his studies of the suboceanic continental slope is that it is continuous over great distances (which is perfectly correct), and that the Norwegian basin is an extension of that of the North Atlantic; or, as expressed by him, "it is obvious from an inspection of the accompanying map that the eastern part of the North Atlantic, the Norwegian Atlantic, and a portion of the North Polar ocean, all belong to one and the same great geosynclinal depression, which has been partly interrupted by volcanic extravasation, more especially in the region between Shetland and south-east Greenland." This view cannot pass unchallenged. The ("eruptive") barrier extends *practically* from Europe to Greenland; and between points where the depth of even the valley-like extensions of the basins is 12,000 feet the breadth of the barrier is 600 miles or more, and it rises to within 2,000 feet of the surface of the ocean. Such a barrier, rising 10,000 feet out of 12,000 feet, is certainly too stupendous to be considered as only a partial interruption in one oceanic basin. As a matter of fact there are two basins (and the more northern is apparently continuous with that of the great Polar Sea), just as the Caribbean and Mediterranean basins are distinct from that of the Atlantic. If we could regard this Icelandic plateau in the manner that Mr. Hudleston does, as a volcanic extravasation, interrupting the continuity of the Atlantic basin, it might obviate the necessity of explaining the valley-like features of the ridge. He further says: "to the westward, beyond the centre of Lightning channel ('which is that eastward of Faroe

Islands') all the known land and presumably most of the submerged land is of igneous origin." This last conclusion, applied to a barrier nearly a thousand miles long and hundreds of miles in width, seems to me a more extravagant conclusion than the amount of changes of level postulated by Professor Hull and myself. The central American barrier, between the Caribbean Sea and the Pacific Ocean, is of even smaller proportion, and those parts of it which are above water show a prevalence of volcanic rocks of comparatively recent date; yet from beneath these rocks old crystallines are occasionally exposed, and remains of the earlier Tertiary formations are widely distributed. So, too, it is hardly justifiable to suppose that there is no diversity of formations on the Icelandic plateau, which would suggest continental features; accordingly, the student of submarine topography is forced to analyze the land-like forms of the ridge, all the more so as the valleys and their tributaries, cirques, and embayments suggest that they were fashioned in the same manner as other submarine and subaërial valleys, far away from great plateau eruptions.

A third feature noticeable in the paper is a general overlooking of the detailed form of the edge of the platform, where the soundings "are very irregular and puzzling, so much so that it is not easy to deduce any mean angle of inclination." There is no special reason why a *mean* slope should be found. It is the study of such "curious and perplexing" features which has given rise to the bold hypothesis of tremendous changes of level of land and sea. Therefore, so long as Mr. Hudleston overlooks the amphitheatres and valley-like indentations of thousands of feet in depth, dissecting the continental margins, he has not the facts before him to warrant the *ex cathedra* conclusion—that stupendous changes of level have not occurred.

However, Mr. Hudleston has spoken of the Norwegian channel from the Skager Rack, which has long been known and written upon. Its breadth seems to be greater than he is prepared to accept as that of a valley, although it is a strong feature from Christiania Fjord, rounding the point of Norway, and extending 500 miles to the oceanic abyss. Its breadth should be no difficulty, for it is not as great as that of the lower St. Lawrence merging into the Gulf of the same name; and the flood plains of the land-valley of the lower 500 miles of the Mississippi are from 30 to 80 miles wide, with its lower portion now buried by sediments to a depth of 1,000 feet. This Norwegian channel has a depth, near its head, of 2,658 feet, but farther seaward, as it is crossing the subcoastal plain, its depth is reduced to 960 feet, unless the soundings do not reveal the deepest portions of the channel. He considers this feature "unfortunate" for the theory of its being the course which drained the Baltic valley, and says: "the river of the Baltic, therefore, must have had some difficulty draining towards the Atlantic under these circumstances." Professor Hull and Professor Bonney have answered these points. As shown by them, it is in a region of glacial distribution; so that an easy explanation is seen in the unequal filling of the old

valley, such as is known to have frequently occurred. As an illustration of such obstruction, the ancient Laurentian valley between Georgian Bay (eastern portion of Lake Huron) and Lake Ontario is entirely obliterated, not merely by being filled with drift, but also by high transverse ridges. These Pleistocene accumulations have a depth of 500 to 1,000 feet. Yet the portions of this ancient valley, beyond both sides of the drift filling, are still open channels beneath the waters of the lakes. Again, the differential elevation may in part have raised the barrier across the Norwegian channel. Such a result has been the case in mostly closing the outlet of the basin of Lake Ontario, which is 738 feet deep; this differential elevation being recorded in the tilting of the beaches adjacent to the lake, but now raised far above it. As the development of this submerged channel is a feature very much greater than its partial obstruction, the hypothesis of its subaërial origin has in no wise been overthrown by Mr. Hudleston's superficially-formed conclusion, touched with sarcasm: a conclusion based upon one feature, namely, obstruction, which is much easier of explanation than the occurrence of the submerged Norwegian valley itself, if it were not of subaërial origin. Accordingly, the grand old valley is not yet placed without the pale of the laws of uniformity, which prevail in both subaërial and submarine valleys.

My conception of the proper treatment of the 'irregularities' in the continental margin, beneath the waters of the Bay of Biscay, differs greatly from that of Mr. Hudleston. For instance, at a point 100 miles south-east of La Chapelle bank, his statement is:—"For at one spot 600 fathoms is marked just outside the 100-fathom line; while at another, further out to sea, a distance of about seven miles, separate soundings of 880 fathoms and 2,100 fathoms respectively, thus giving a slope of 1 in 5, or 11°. In the latter case, the irregularity is not connected with the edge of the 100-fathom platform, but may be said to belong to abyssal depths." His explanation bears only upon the question of the irregularity of the slopes, and does not convey to me any impression that he was making any study of the cirque-like indentation. I should describe the phenomenon as shown upon his own map as follows:—There is a sounding of 3,600 feet (600 fathoms), located a few miles within the line connecting the 600-foot depths on either side, suggesting a cirque in the great continental slope. The trend of this slope on the eastern side of the cirque (or, more correctly speaking, amphitheatre) appears to turn so as to form an embayment due south of the 3,600-foot point, reaching to a depth of 6,420 feet, with a promontory to the east of it rising 860 feet above its floor at this point; thus showing a great amphitheatre-like valley continuing outward from the 600-foot platform. Beyond this point the 12,000-foot contour also shows a marked indentation, corresponding to a continuation of this valley, noted at depths of 3,600 feet and 6,420 feet, although Mr. Hudleston forms a contrary conclusion. At the head of the amphitheatre, there appears to be a precipitous declivity of about 3,000 feet, but from the foot of this declivity the mean gradient of the valley is about

1 in 30; while if I went farther and compared the gradient of the floor of the indentation with the pitch from the promontory upon its eastern side, I should find it 1 in 5 as given by Mr. Hudleston. He has studied only the angle of descent of the continental slopes: I have studied the sculpturing of the slopes, with their valleys and gradients.

This amphitheatre-like indentation, by itself, would not be a strong point, as the data are scanty; but when the feature is one of constant recurrence, and fuller information concerning such features is often obtainable elsewhere, as Professor Hull has shown to be the case at numerous points off the coast of France, Spain, and Portugal, experience makes easy the interpretation of the fragments of drowned valleys which have been recovered.

Turning now to the Fosse de Cap Breton, at the angle of France and Spain, the data are very much fuller, and Mr. Hudleston's treatment is happier, but still falls far short of the facts, as shown upon the chart, which he has reproduced, and therefrom my conclusions reach farther, and fully support the investigations of Professor Hull.

The Fosse dissects the submarine platform, limited by the 600-foot contour, which has a breadth of about 30 miles. It has a depth of 1,260 feet near its head. Farther seaward, the next sounding is 180 feet less, as also two other soundings are less, but these are not in the centre of the channel, as illustrated. Only fuller soundings would prove a barrier, but even then an obstruction by river deposits is what might occur. Another chart,¹ which I have seen, gives more conclusive data than the illustration here analyzed. A little farther outward, the depth has increased to 2,694 feet (on the side of the channel), then to 3,660 feet near the centre, and still farther to 3,750 feet. From this section, Mr. Hudleston passes to "35 miles" farther seaward, and finds soundings of 1,500 fathoms in an outer platform submerged to about 900 or 1,000 fathoms. He then ascertains the mean slope of the side (1 in 7, which is about 750 feet per mile). He cites an example, given by Professor Milne, of the submarine cables being buried by the fall of the vertical walls overhanging them. He concludes as follows: "there are several indications that this depth of 1,500 fathoms represents a hole rather than a channel, for the next sounding seaward shows only 1,250 fathoms." But this last sounding is eight or ten miles from the 1,000-fathom line adjacent to the coast, leaving plenty of room for the intervening channel to reach an additional depth of 250 fathoms. Indeed, to the north-west of the 1,500-fathom hole, and landward of it, the lateral depth is 1,370 fathoms. Between this hole and the Fosse de Cap Breton, already established, there is a chain of soundings of 1,170, 1,107 (this is midway between the 1,500-fathom "hole" and the 625-fathom depth of the fosse), and 915 fathoms, between which and the 625-fathom sounding there is one of only 500 fathoms. In the application of these soundings, it must be

¹ "Submarine Gullies," etc., by Henry Benest: *Geogr. Journ.*, October, 1899, p. 405. The soundings on the Admiralty Chart are very numerous.

borne in mind that the locations are not accurate, and that in off-shore hydrography, errors of location up to five miles are permissible (though usually much less) even in good work, when the soundings are made at different times. Consequently, the windings of the valleys are only approximately located, and there may even appear interruptions, but their depths at various points are correct. Under all these conditions, I am forced to give value to the intermediate soundings passed over by Mr. Hudleston, and reach only one conclusion, which is Professor Hull's—that the Fosse de Cap Breton is a continuous valley to beyond the depth of 1,500 fathoms, and from the character of the steep sides it appears to partake of the nature of a cañon. From the soundings, the floor of the fosse, as thus extended, seem to be characterized by three steps, separated by long intervals where the slope is reduced to a few feet per mile, while the mean declivity of the intervening steps is 200 feet per mile. If fuller soundings were made, the steps might be found more abrupt, and there might be a larger number revealed. Mr. Hudleston's explanation of the 1,500-fathom sounding as being a "hole rather than a channel"—an expression used similarly elsewhere—would appear to be a refuge from the valley hypothesis, unless it is shown how such are formed, especially when there is a succession of them.¹

I wish to correct a criticism. He says, "that Professor Spencer, having long studied the Caribbean Sea—perhaps the most unstable region of the earth—appears to have an exaggerated notion as to epeirogenic uplifts on both sides of the Atlantic"; and he further objects to the hypothesis that the Rockall embayment, west of Ireland, has been fashioned by atmospheric agents, and suggests that both it and the Lightning channels, and the deeps of the Norwegian Atlantic were due to tectonic causes. Whatever were the causes of the basin, the claims of Professor Hull and myself are only that the barriers were once land, which were moulded by atmospheric agents, producing the valleys dissecting them.

I fear that my critic's impression of the instability of the Caribbean region is very much magnified. It is now certainly established that the principal changes of level, since the early Tertiary period, over that region—vastly larger than the Norwegian basin—have occurred with remarkable persistency, though varied in magnitude. And these changes have extended to the North American continent, and continued northward to regions considered the most stable. I suspect that the most of these changes are represented in the eastern Atlantic, which the evidence brought out indicates. At no time have I stated that the same individual oscillations extended to both sides of the Atlantic, as the oscillations may not prove to have been absolutely synchronous. They may yet be found to have alternated. The changes may also have been more complete in one region than another.

¹ For a restoration of this submerged river channel as determined by Prof. Hull, see Geogr. Journ. for March, 1899, with map.

As to the Rockall embayment and Lightning channels, it may be said that only a brief notice was taken of these by me in my preliminary paper,¹ cited by Mr. Hudleston. In a general way I called attention to the deep valley-like depressions dissecting the submarine Icelandic plateau in both directions. These and the Rockall embayment, with their tributary amphitheatre-like valleys, were regarded as only the more advanced forms of atmospheric sculpturing of the margins of an elevated plateau, similar to land features, of which plenty of examples can be given. But the occurrence of the submarine plateau is explained by Mr. Hudleston as an igneous extravasation, while the dissection of the drowned barrier by deep valleys he attributes to tectonic causes. But he does not even compare the adjacent submarine features (as, for instance, the fjords and their tributary amphitheatres) with like forms of the basaltic plateau of the Faroes, dissected into islands by valleys and fjords, with cirques of 2,000 feet in depth. Neither the hypothesis of the general igneous origin of the Icelandic plateau barrier, nor that of the tectonic origin of the valleys, subsidiary to the plateau formation, does he support by evidence, and in substituting his explanations for the hypothesis of subaërial sculpturing of a former land, he brings back upon himself the *onus probandi* of sustaining his theory.

Mr. Hudleston sums up eight conclusions, and these by themselves would have been mostly accepted by Professor Hull and myself. But the details of the paper appear more strongly to have for their object the discrediting of the hypothesis of great changes of level of land and sea. I thank him for the trouble he has undertaken in bringing out so many points, and producing such good illustrations. It has enabled me to point out some of the methods of investigation adopted by Professor Hull and myself, and I trust that it may be of use to other students. But in nothing that he has shown does there appear testimony against the correctness of conclusions by Professor Hull concerning the great changes of level of western Europe in late geological times. The evidence makes me believe with Professor Hull that the submerged plateaux, with gently undulating topography, or rounded and flattened forms, were old land surfaces, largely brought into being by the laws governing the base-level of erosion during the very long Mio-Pliocene period. Some of the broader valley features may have been produced during some portion of the same period, but, generally speaking, our theory is that the deep channels dissecting the drowned plateaux were formed during a subsequent epoch of high continental elevation, or were reopened and enlarged in it. The evidence in America places the great elevation after the time of the establishment of the modern molluscan life, but before the epoch of the accumulation of the glacial deposits: approximately, about the commencement of the Pleistocene Period. Then the older valleys were being enlarged, and the bounding slopes were being dissected by receding cañons and amphitheatres, which

¹ Read before the Meeting of the British Association, 1897: GEOL. MAG., Dec. IV, Vol. V (1898), pp. 32-38.

were slowly developed into great and deep channels. We may not have sufficiently distinguished all the older from the newer features. As the amount of erosion performed may seem too great for the supposed period of high continental elevation, we are willing to concede that some of the continuations and enlargements of the more restricted channels, or the embayments terminating them, may be older features, even possibly due in part to tectonic causes, whatever such may be; but this is a question beyond the confines of our present investigations. On account of the great slopes being indented by the newer physiographic forms, we point out the evidence of the subsequent stupendous elevation of the land. We may have to concede to the Pleistocene period a really longer duration than has been popularly supposed. The growth of the valleys, under conditions of variable rainfalls on the elevated plateaux, does give rise to such features as we now find dissecting the submarine platforms; and the denudation under conditions of stupendous changes of level would accelerate the excavation of the valley features, or modify them, as described by Professor Hull and myself.

In place of pointing out the remarkable submarine holes and burying the observations in the tomes of some learned society, we have collected the phenomena and bound them together by a working hypothesis based upon the grammar of geomorphy; a new science as yet not generally mastered, with results at first startling, although they only give expression to some of the changes of land and sea, as taught by Lyell. Our imperfect studies have been met by observations from a different standpoint, as if we did not accept them in the common way; consequently, I do not see that any unanswerable objection has yet been raised to our conclusions. Hardly even enough to spur us on to seek for fresh confirmatory facts. Mr. Hudleston would doubtless drift along with us if he would give the subject the attention which it deserves; and already he says that he is not prepared to deny the connection between Europe and Greenland in mid-Tertiary times, though he fails to see any necessity for it. It was in the mid-Tertiary times that we suppose the great undulating features of the Icelandic bridge began to be formed. I take it that further studies must lead him to distinguish between these features and the deep channels with their tributaries, cirques, etc. But until this differential interpretation of the forms is used, the great changes of level of land and sea in late geological times will doubtless appear startling. Whatever agnosticism there may be concerning the correctness of our conclusions, so long as we are met by hypotheses substituting that of subaërial erosion, it falls upon the advocates of such hypotheses to sustain them with facts. While the changes of level of 3,000 feet would perhaps meet with almost general acceptance, we have not been shown any reason why this limitation should be made, when the valley-like features extend to depths of 9,000 or 12,000 feet, as found by both Professor Hull and myself.