

organic remains. A post-glacial forest-bed occurs at Holm Scarf, off the Norfolk Coast, and may plainly be seen at low water. It is a bed of peat in which trunks of trees are imbedded. It was in one of these trunks that Mr. Edwards found a flint implement sticking.

Within the last few days I have come upon the remains of another submerged forest or peat-bed at Bawdsey, near Felixstowe. It is only visible and accessible at low-water spring-tides, and even then it is seen sloping down into the sea. The cliffs at Bawdsey are formed of London Clay, capped by Red Crag, and they do not waste so rapidly as many other parts of this coast. The London Clay forms the bed of the sea, except near the northern side of the estuary of the Deben. There we find the peat-bed, resting directly on the London Clay. It is about four or five feet thick at its thickest part, but it has evidently been very much denuded, and is now merely a relic of what it once was. Remains of trees are not plentiful in it and the peat contains an abundance of fresh-water and marsh plants, but I found no fresh-water shells. The only animal remains I obtained are the upper part of the skull and horn-cores of *Bos longifrons*, but I was told that bones had frequently been washed out of it. Among the plants a species of *Cyperus* was abundant, and *Sphagnum* was also plentiful. Indeed, the nature of the peat-bed indicates its formation under just such marshy conditions as geologists have assumed the bed of the German Ocean to have been in before the submergence took place which brought the sea-water over it, and so converted England into an island.

The discovery of this remnant of a once extensive peat-bed uncovered only in part even at extreme low-water spring-tides, is therefore interesting as confirming the geological speculations concerning the old marshy plain over which the German Ocean now extends.

J. E. TAYLOR.

REPLY TO MR. ALFRED TYLOR.

Sir,—Mr. Alfred Tylor complains that being dissatisfied with certain views of some prominent geologists, I have “ready a theory of my own to meet all the difficulties of the student of Quaternary Geology.” My rôle I am afraid is much more humble. It is true that I have spent much time in trying to unravel the difficulties of the surface beds of Western Europe, and have found, as Mr. Tylor no doubt has, that almost every student of them has a different theory. It is true also that, disagreeing with the many and contradictory views that have been propounded, I have tried (I hope in deferential language) to show why they seem incompetent to explain the facts, and having done so have propounded another view; but I neither claim for this conclusion that it explains all possible difficulties, nor that it is necessarily a final view. I do not believe in final views in Science. Every one of us is as a fly on a plate in view of the advancing tide of Knowledge, and we can do no more than frame an hypothesis that shall meet the facts accumulated up to the time when we write. To-morrow a child may find a fresh fact which will not fit our theory. That theory must thereupon go

under; but it will have done its work. All I claim to have done is to have framed an hypothesis which will meet *all* the facts known to me. I have not shirked or wilfully evaded any, and I hope I have accumulated a very considerable number as the basis of every step in the argument. Further, the position I am fighting for is only partially mine. I have merely tried to extend to *all* the beds which are confessedly on the same horizon, however different in texture, an explanatory cause which such great authorities as Murchison, Belgrand, and Prestwich, have agreed in assigning to *particular* cases, and to show that the evidence is convergent and cumulative. If I have misstated or misread facts, there is nothing will be more grateful to me than to have my slips pointed out; and if the position is shown to be untenable, it will be surrendered at once, for there is not time in seventy years, which is our portion here, to fight for prestige. Of course when we take a new departure we must adopt the position of Ishmael. We are bound to struggle with those who already monopolize the ground, and I know of few better examples to point the moral than Mr. Tylor himself. He has defended his Pluvial period with ingenuity and skill. He has written about it in a way which has been a delight to myself to read, and although he has not convinced many people that he is right, he has not done the cause of science any the less good service by presenting certain neglected aspects of a difficult problem in a striking light. Mr. Tylor's second and third paragraphs I do not quite understand the appositeness of. In answer to his arguments that the denuding influence of a river upon its channel increases many fold with the increase of its water, I urged that this does not follow, because the motion of a river, especially of a deep full river, is largely limited to the upper and central portions of its current, that it decreases as we get nearer the bottom, while that portion in contact with its channel is nearly quiescent. Do I understand Mr. Tylor to dispute this elementary hydrostatical position, which is not a theoretical one, but has been amply proved experimentally by a succession of observers, and especially by Defontaine and Raucourt? If not, I do not understand the drift of his remarks. I may add by the way that I have found since I published the suggestion that the quiescence of the fundamental layers of water in a river may account for northern rivers so frequently first freezing at the bottom, that the same suggestion had been previously made, unknown to me, by Arago, and I willingly shelter behind his ægis.

Mr. Tylor's claim to have suggested as early as 1853 that the crumpled gravels in the valleys of some of the French rivers were produced by the mouths of these valleys having been gorged with ice, is quite just; but in quoting Mr. Belt I was looking not at this local fact, but at the important theory he formulated which proposed to explain the valley terraces, etc., as lacustrine deposits, the European lake being embanked and pounded back by ice.

DERBY HOUSE, ECCLES, MANCHESTER,
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