

depths, and the consequent upward expansions are greater. This is caused by the gyratory motion of the water around this region. The deflecting force of the earth's rotation arising from this motion, being on all sides to the right of the direction of motion, drives the surface water, together with the seaweed from all sides, into this region; so that there is a little heaping-up of the water in this region above that caused by the greater upward expansion: and this causes a settling-down and a flowing-out at all sides below, where the gyratory velocity, on account of greater friction, is less, and the consequent inward pressure toward the central part less, than they are above. This carries the warm surface water downward, and makes the average temperature for all depths and the upward expansion greater here than in the surrounding parts; and this, together with the slight accumulation of the mass in the region of the Sargossa Sea, raises its level several feet.

Where wind drives the water against a barrier or shore, as in the case of Lake Ontario or the Atlantic Ocean, regular progressive currents from top to bottom in the same direction cannot be established; but the surface water which is driven forward must return below, or at the sides if the wind blows over the middle part only. In such cases the greatest change of sea-level takes place soon after the winds begin to blow in any given direction, while the whole force is spent upon a comparatively thin stratum. It is well known that winds blowing over a very shallow stratum of water, or along the length of a very shallow canal, may produce a considerable change of level; whereas, if the depth were considerable, the change would be but little. At first, while the whole force of the wind is spent upon the surface water of a lake or ocean, the great body of undisturbed water below is the same as so much solid matter. But after the surface water has been driven to one side, and the pressure there increased, which gives rise to the return current below,—when this has been fully established, the difference of sea-level at the two sides or ends, from and to which the wind blows, is less.

W. FERREL.

Washington, D.C., Feb. 13.

The Davenport tablets.

Please allow me to trouble you once more, and finally, in reference to the Davenport tablets.

Mr. Putnam says, "If Professor Thomas will take the Grave Creek tablet, or even the famous Rosetta stone, and sit down before them with his Webster's 'Unabridged,' he will find no end of similar resemblances." Very true, as the alphabets used on the Rosetta stone are some of those given by Webster, and the characters on the Grave Creek tablet have been taken from half a dozen different alphabets, which is one of the chief reasons why it is generally rejected by modern archeologists (see Dr. Wilson's scathing criticism in his 'Prehistoric man,' third edition, vol. ii. pp. 99-111).

Mr. Putnam's criticism of Mr. Tiffany's letter, on account of illiteracy, is in strange contrast with the records of the Davenport academy, which show that Mr. Tiffany was one of its four original organizers (Proc., vol. vi. p. 1), was a member of the museum committee, was one of the board of trustees named in the constitution and articles of incorporation, was a member of the committee on finance (Proc., vol. i. pp. 4, 7, and 8), was more than once selected as one

of a committee of three to draught resolutions (Proc., vol. i. pp. 23 and 71), was one of a committee of two appointed to take steps toward erecting a building, was for some years treasurer of the academy (Proc., vol. i. p. 67), and did considerable mound-exploring, for which special credit is given in the president's annual address of 1876.

It is true that in the letter, from which I quoted only so much as touched upon the points then under discussion, Mr. Tiffany expresses entire confidence in the shale tablets, which is proof that his expression of doubt in regard to the 'limestone tablet' was not for the purpose of 'defaming his old associates,' but because the evidence satisfied him it was a plant.

In answer to Mr. Putnam's singular philosophy respecting the entrance of water into the little vault where the limestone tablet was found, it is only necessary to refer to the figure and description of mound 11, heretofore given. As neither cement, plastering, clay, nor mortar was used, it would have been, as every mound-explorer knows, a miracle if water had failed to enter the vault, and, in the course of centuries, fill it with dirt. Moreover, in the course of time the superincumbent weight would have pressed the slab which covered the vault down upon the tablet.

Archeologists, so far as they have spoken, have, almost without exception, indicated in their published works a want of faith in these tablets. Short, in his 'North Americans of antiquity' (p. 40), says, "The above conjectures as to the significance of the representations on these tablets are based upon the supposition that they are genuine, and not the work of an impostor, of which we cannot refrain from expressing a slight suspicion." Rev. J. P. MacLean, speaking of the cremation scene, says, "Among the cabalistic characters, the word 'town' stands out in bold lines, and the figure '8' appears in rude shape among other marks. The picture of a face occurs in the sun, resembling the face of a European. The artist has overdone his work: it needs no further investigation" ('Mound-builders,' p. 116). Yet Mr. MacLean is one of two (Dr. Willis De Haas is the other), of whom Mr. Putnam remarks in his recent annual address to the academy, as published in the local papers, "There are thus no more competent archeologists in the country." Mr. Peet, in the *American antiquarian* of July, 1878, expresses the same opinion as Mr. MacLean. Prof. M. C. Read, in the *American antiquarian* of April-July, 1882, expresses a doubt as to their authenticity, based upon the characters they bear. Dr. E. Schmidt, in an article entitled 'The mound-builders and their relation to the historical Indians' (*Kosmos*, 1884, p. 146), remarks, "It is hardly necessary to be pointed out that none of the notorious tablets are without suspicion, and that all which have been subjected to earnest investigation have turned out to be gross forgeries." It appears from these notices that I am not alone in expressing doubt as to the authenticity of these tablets.

Notwithstanding the kind invitation of the academy to visit their museum and inspect the tablets, I preferred, for the present, to base my arguments on the publications of the academy (the albertypes included) and the statements of its members, as this avoided recourse to personal judgment, and appealed only to what is before the public. Even the extracts from Mr. Tiffany's and Mr. Pratt's letters were in

confirmation of Mr. Harrison's published account of the finding of the limestone tablet. If this evidence leads to the conclusion that these relics are modern productions, as I believe it does, there is no necessity for the present of 'further investigation,'—a conclusion Mr. MacLean seems to have reached while writing his 'Mound-builders.' CYRUS THOMAS.

The claimed wheat and rye hybrid.

In *Science* of Jan. 15 appears an article from Dr. Sturtevant, which, to save words, I will call a criticism of an account of my rye and wheat hybrids, published in the *Century magazine* of last January by Charles Barnard. Mr. Barnard, after an examination of the plants at my place last summer, gives their history, accepting, without question, their hybrid origin. Dr. Sturtevant, who also examined them last summer, begs to dissent. He considers the evidence adduced only 'sufficient to establish grave doubts.'

While we were on our way to the plots, Dr. Sturtevant remarked that he wanted me to know that he was 'incredulous as to the whole thing.' While we were returning, he said, "I am convinced that they are hybrids, but I question whether they will not be found to be distinctly either wheat or rye." In the *Science* article referred to, he next states that he has compared the pictures of a few of these heads which appeared in the *Rural New-Yorker* with those of five old varieties which he mentions, and finds them closely alike. Then he remarks that he does not question the 'attempt at a cross.' The 'variability effected is,' he admits, 'indicative of a foreign pollen.' This variability, which he believes not to be due to hybridization, the doctor explains by an 'hypothesis.' It is that under the stimulus of the rye pollen, atavism has resulted, whereby varieties dormant in the wheat (female) plant have made their appearance. Finally he expresses the hope that some one, expert in agricultural botany, may 'investigate a series of these specimens.'

Dr. Sturtevant, though he states that he has carefully studied the 'published claims,' has apparently overlooked the published fact that specimens of these hybrids have been sent to no less than six well-known botanists, several of whom have replied that they were evidently hybrids, while others replied to the effect that the hybridization was a most interesting fact, etc.

Now, if we emasculate the florets of a head of wheat while the anthers are immature, and repeatedly apply rye pollen, and thus succeed in attaining ten grains, from which, in three years, at least fifty different varieties appear, differing as widely as any known wheats differ from each other, while some of them resemble rye more than wheat, can anyone reasonably doubt that a hybridization was effected? Why assume anything else whatever? What does Dr. Sturtevant mean by ascribing such changes to the 'stimulus of foreign pollen' as something different from the sexual effect of foreign pollen? Suppose atavism is shown in some of these: does it not prove, all the same, that hybridization was effected? A hybrid may show all, some, or none of the characteristics of either parent, and still be a hybrid, as has often been revealed in the later seedling progeny.

In drawing resemblances between the pictures in the *Rural New-Yorker* and those of which he speaks,

the doctor, very likely, forgets an important fact; viz., that in many of the heads of the plants most resembling rye, the spikelets bear but two kernels, while many are wholly abortive. Again: the botanical relationship is marked not only by narrower glumes, by fewer florets and grains, but by the fact that the culms beneath the head for an inch or so are hairy,—a characteristic that never occurs on wheat culms. The color of both the culms and leaves is also distinctly lighter (more glaucous) than that of wheat, and the habit of the young plant is that of rye. E. S. CARMAN.

A recent ice-storm.

The trees in central Massachusetts, along the line of the Boston and Albany railroad from Worcester to Spencer, suffered severely from the weight of ice formed upon them during the storm of Feb. 11-13, that caused the recent destructive floods. It was noticeable that the trees which exposed the largest surface for the attachment of ice did not suffer most: the pines with their green needles, and the oaks with their dead leaves, generally escaped injury; and the slender birches were saved by bending instead of breaking. But from five to twenty per cent of the other deciduous trees were more or less hurt. The side limbs were not often broken: it was nearly always the vertical top-stems that sustained the most injury, apparently because their natural position was farthest from that into which the weight of the clinging ice forced them.

Can some of your readers furnish direct observational evidence to show why the pines and leafy oaks escaped, while the bare trees were so much damaged?

W. M. DAVIS.

Cambridge, Feb. 20.

Corrections of thermometers for pressure.

If any of your readers interpreted our reference in *Science*, Feb. 12, to a letter from the signal office, as your correspondent, Sig., feared they might do, we regret it, and are glad that the import of that letter has been fully explained. We are well aware that many of our text-books on heat refer to the effect of pressure on the thermometer, and state how to prevent it in some instances. The effect of appreciable changes of pressure on the thermometer seemed to us to be sufficient to demand correction in all accurate thermometric work. If such corrections are generally made, they are omitted in the report of experiments.

F. P. VENABLE.

J. W. GORE.

University of North Carolina, Feb. 22.

Is the dodo an extinct bird?

Referring to Dr. Shufeldt's article (*Science*, vii. 145) respecting the supposed present existence of the dodo, it may be desirable to state, for the benefit of those who are not already aware of the fact, that the so-called dodo from Samoa, mentioned in the clipping 'from an English newspaper,' is not the dodo at all, but the dodo-pigeon, *Didunculus strigirostris*, a living specimen of which was last year presented to the national museum by Dr. T. Canisius, ex-consul of the United States at Samoa. This specimen was, at latest account, thriving in the zoölogical garden at Philadelphia.

ROBERT RIDGWAY.

Smithson. inst., Feb. 15.