caskets are designed to preserve the body in nearly a natural state by excluding the air. The body is surrounded with ground cork, and the lid of the casket is carefully cemented with white lead; it is then wrapped in a layer of thick felt, and placed in a tightly constructed pine case, which is completely filled with the ground cork. The seams of the pine box are carefully covered with white lead, and the whole is enveloped in another thick wrapping of felt; over the latter is a covering of burlap, secured by stout cords; outside is a pine crate. These caskets are believed to be the best ever made for the preservation of the dead; and the great success achieved in the transportation of the remains of De Long and his companions would seem to indicate their entire feasibility for general use in similar instances, or where bodies are to be transported long distances through many climatic changes.

— The herbarium of the national museum at Washington now embraces over 25,000 specimens, representing 17,000 species, and is established upon a broad basis, which admits of almost unlimited expansion. The North American flora is represented by about 7,000 species, contributed by Ward, Canby, Havard, and others, and is constantly increasing. The herbarium is also rich in European species, the gift for the most part of the authorities at Kew, and chiefly from the collections of George Curling Joad and J. Gay. This material, however. represents only a small portion of the national herbarium, the greater part of which is yet at the department of agriculture, where the government collections were formerly deposited before the erection of the national museum building. Case-room is provided, and the specimens are permanently mounted and systematically arranged according to the system adopted by Bentham and Hooker in their 'Genera plantarum.' The collection is rendered easily accessible by means of a card catalogue, and Roman and Arabic label numbers for order and genus on each genus-cover. The herbarium is placed in immediate connection with the department of fossil plants, and under the same curatorship. It is intended that all duplicate material shall represent either additional parts of plants or widely different localities, as illustrating their geographical range, local variation, etc. Other duplicates will, however. be utilized in effecting exchanges for species not represented.

— The *Berichte der deutschen botanischen gesellschaft* contains the interesting results of a number of experiments recently made by Strasburg upon the grafting of solanaceous plants. Jimson-weed (Datura stramonium) and 'wintercherry' (Physalis alkengi) were ingrafted upon potato-stocks, with immediate union; and with the tobacco-plant less speedy though equally successful results were derived. Grafting deadly nightshade (Atropa belladonna) and henbane (Hyoscyamus niger) was accomplished with more difficulty. Other attempts also succeeded in ingrafting the potato upon the nightshade (Solanum nigrum), tobacco, and wintercherry, though with less ease. Not only were union and growth secured between these different solanaceous plants, but also between the potato and Schizanthus Grahami, a Chilian scrophularian plant, upon which the potato-fungus grows. The development in this last, however, was feeble. In none of these experiments did there appear to result any modifying influence upon the stock. The potato produced tubers as usual, though there appears to have been a greater number of irregular forms. With the jimson-weed the tubers were well developed, but no seeds were produced. On the other hand, tobacco-plants fructified abundantly, with only a sparse growth of tubers. Reserve material does not seem to be sufficient to admit of both seeds and tubers together. Potato-plants grafted on others seemed to possess a superabundance of reserve material, however, resulting in the growth of tubers of the size of a walnut, in the axils of the leaves. The 'eyes' of these tubers, it is interesting to state, developed leaves of considerable size. This growth of tubers above ground has been previously observed in the potato-plant, where the stem had been crushed close to the surface.

LETTERS TO THE EDITOR.

*** *Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

Sea level and ocean-currents.

ACCORDING to Zöppritz, the winds were thousands of years in overcoming the inertia of the water, and causing the present ocean-currents. Of course, during the latter part of this long period, after their effect had extended down to the bottom of the ocean, a part of their force was spent in overcoming the friction over the bottom, and toward the last a very small part only in accelerating the motion. But according to the same authority, after 239 years, while the whole force of the winds was spent upon the inertia of the water, only one-half the surface velocity was communicated to the stratum at the depth of 100 metres; and so at the depth of a few hundred metres there was yet very little velocity. The greatest surface velocities in the open sea, supposed to be due to the winds, are, on the average, not more than ten miles per day. The whole amount of momentum, therefore, caused by the action of the winds, is only about equal to that of a stratum 100 metres in depth, with a velocity of ten miles per day, the amount of momentum below 100 metres in depth being about necessary to reduce that above 100 metres to the momentum, corresponding to that of a uniform velocity of ten miles per day for all the strata. We can only judge of the force of the winds, as exerted upon the surface of the ocean, by the amount of momentum produced in a given time; and, from the small amount of momentum produced in so long a time, this force must be very small.

Let us now examine the effects of gravity as called into play by the gradients of the strata of equal pressure, arising from unequal upward expansions due to differences of temperature. Referring to my notes upon this subject, I make the following extracts from a larger table, in which the temperatures and the upward expansions are given for three stations at the given depths in the first column: —

DEPTHS IN FATHOMS.	EQUATOR.		LAT. 23°.2 N. LONG. 38°.7 W.		LAT. 37°.9 N. LONG. 41°.7 W.	
	Temp.	Expan.	Temp.	Expan.	Temp.	Expan.
0	25°.5C.	5.1ft.	22°.2C.	7.8ft.	21°.1C.	8.5ft.
50	17 .7	3.9	—	—	—	—
100	13 .1	3.2	19 .4	5.8	17 .5	6.7
200	8 .1	2.8	14 .8	4.6	15 .9	5.2
300	5 .7	2.4	11 .4	3.6	15 .6	4.0
400	4 .6	2.0	8 .7	2.8	12 .7	3.0
500	3 .8	1.8	6 .5	2.3	8 .2	2.3
600	4 .0	1.6	5 .4	2.0	5 .3	1.9
700	3 .9	1.4	4 .8	1.8	4 .8	1.7
800	3 .9	1.2	4 .1	1.6	3 .4	1.6
900	3 .4	1.1	4 .0	1.5	3 .2	1.5
1000	2 .7	1.0	3 .5	1.4	3 .2	1.4
1500	2 .3	0.6	2 .6	0.9	2 .7	0.9

The temperatures are the means of six soundings of the Challenger expedition, as given by Dr. Croll; and the upward expansion, computed from Dr. Hann's formula for the density of sea-water, is that arising from the differences of temperature at the different depths, and that of the maximum density of sea-water in the polar regions. The temperatures at the bottom of the stations, ranging in depth from 2,500 to 2,700 fathoms, were a little less than 2°. The upward expansion of the surface at the equator is a little greater than that of Dr. Croll (4.5 feet), obtained by means of Muncke's tables, but the difference is of no consequence.

It is seen that the temperatures and upward expansions diminish rapidly near the surface, and that the latter are small in the lower depths. Supposing, for simplicity, that the gradients are uniform from the equator to the latitude of maximum density, say 5,000 miles from the equator, then the average gradient of the whole mass of the ocean, down to the depth of 2,500 fathoms, is about 1.5 feet in 5,000 miles, instead of 5.1 feet, as at the surface. The force, therefore, down this average gradient, of the whole mass, is to that of gravity about as unity is to 18,000,000. It is readily found, from computation, that this force down this small gradient would give to the whole mass, in four days, a velocity of ten miles per day. According to Zöppritz, the whole action of the winds in 239 years produced only this amount of velocity on a surface stratum of 100 metres in depth, say one-fiftieth part of the whole depth. To produce an amount of momentum, therefore, equal to that of the whole ocean, with a velocity of ten miles per day, would require nearly 12,000 years. Comparing, now, four days with 12,000 years, we get an approximate idea of the relative strengths of the two forces, for these must be inversely as the times required to produce a given amount of momentum. The force of the winds upon the ocean, therefore, in comparison with the gravitation force, is almost infinitely small, if Zöppritz's results are to be accepted. But I have never accepted these, and therefore regard this simply as a very strong *argumentum ad hominem* on the subject to anyone who accepts them, and also maintains that the winds have any sensible effect in causing ocean-currents. Of course, a very small force, with time enough, will produce any given amount of momentum; and so the winds, in time, could have caused an amount of motion equal to that observed in the ocean, if no other forces had been in operation; but with other forces many times greater, causing both vertical and horizontal circulations, of course the effects of the infinitely small force would be entirely lost.

In the flowing of rivers down a gradient, knowing the gradient and the mass, we have a measure of the force required to overcome the friction; and thus, from the known depth and velocity, it is easy to obtain approximately the value of the friction-constant. From any considerations of this kind I have never been able to obtain a friction-constant nearly so small as that assumed by Zöppritz, and therefore think it is many times too small as applied to rivers or ocean-currents.

If we assume that the winds can cause the given amount of momentum in one year, instead of 12,000, we still have their force upon the ocean nearly 100 times less than the gravitation force; and I think good judgment in the matter would decide that a year, at least, would be required for the slight action of the gentle winds blowing over the ocean to give an amount of momentum equal to that of the whole mass, with a velocity of ten miles per hour. I cannot think, therefore, that the effect of the winds is more than one-hundredth part of that of the gravitation force.

Professor Davis seems to think that the gravitation force is too small, even allowing it a long time to act, to move the whole mass of the ocean. But the greatest tidal gradients with reference to the resultants of gravitation and lunar forces, are little, if any, greater than that of fifteen feet in 5,000 miles; yet these move the whole mass of the ocean to the bottom back and forth twice a day, causing regular elevations and depressions of the surface, now high water, and six hours after, low water. The maximum tidal velocities for all depths amount to a velocity of nearly a mile per day. I do not think a quarter-diurnal reversal of the directions of the winds would give rise to reversed velocities of that amount to a stratum of the depth of ten metres; and so the effect of the winds would be about 150 times less than that of the tidal forces, which are about the same as those of the gradients arising from the differences of temperature.

The regular gradients from the equator to the polar regions must be regarded as the initial ones, and consequently the forces arising from them, as the forces which overcome the inertia of the water before the final motions have been fully established. But the directions of the initial motions are very much modified by the deflecting forces of the earth's rotation, and the distribution of the temperature disturbances somewhat changed. An interesting example of this kind is indicated by the temperatures of the last two stations of the preceding table, from which it is seen, that, in the region of the Sargossa Sea, the high temperatures extend down to greater 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. 18.

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