relative order of the sub-faunas, the record agrees, in general, with that of the series exposed along the same meridian, farther north, in New-York state. The principal difference which strikes one familiar with the New-York section is the appearance of S. disjuncta and O. Tioga lower down in the faunas in the southern sections.

But although heretofore S. disjuncta has been met with in America only in the middle and upper parts of the upper Devonian, in Devonshire we find it reported from the middle Devonian, with corals and trilobites in abundance; and in northern Europe it begins at least as early as the base of the upper Devonian.

While it is beyond doubt that even in New-York state the three spirifers mentioned appear mingled at various zones in the upper Devonian, we do not question the fact that the periods of abundance for each species are in separate zones, and assume a regular sequence relative to each other.

HENRY S. WILLIAMS.

Cornell university.

The use of the method of limits in mathematical teaching.

Rice and Johnson's 'Method of rates' is especially to be commended for the scholarly manner in which they developed the subject; but there is the same difficulty in the fundamental conception as in the infinitesimal method. One may assume to understand an expression with which he is familiar until closely questioned. A student learns to repeat with ease, 'Velocity is rate of motion,' and thinks he understands it; but I have had many such ask, 'In a mathematically perfect engine, does the piston stop at the end of the stroke?' 'Does it remain at rest at any time?' 'How can it reverse its motion, if it does not stop?' 'How can it cease going in one direction, and move in the opposite direction, without stopping between the two motions?' These are critical questions, lying at the very foundation of all change of motion. Does change in the rate of motion take place *at* an instant, or *during* an instant?

The method of limits leads the mind towards a result the conclusions of which it is impossible to escape: hence, as a system of philosophy, it retains its strong hold.

DE VOLSON WOOD.

Hoboken, March 16.

Ropes of ice.

On Saturday, March 8, while traversing several counties of southern Ohio by railroad, I observed an illustration of the viscosity of ice, that seems deserving of mention.

For a number of hours, rain had been falling, much of it freezing as it fell; but through the day the temperature rose slightly, remaining, however, close to the freezing-point. All exposed objects were coated with ice. In particular, telegraph-wires and the strands of wire fences were heavily loaded. In the afternoon the ice broke loose from the wires at innumerable points, hanging from them in depending curves, the fixed points of which were sometimes as much as six or eight feet apart, and the lowest points of the curves from two to twelve inches below the wires. Occasionally the curves would break, and the ends of the ice rope, two or three feet in length, would project downwards from the wires at an angle of forty-five degrees or more.

The best examples were passed without opportunity to make examination, but all of the facts were illustrated at the stations where the train stopped.

E. O.

Illusive memory.

I merely intended, in my letter of March 7, to present two of the most prevalent theories which have been advanced for these illusions. The 'race memory' theory, kindly brought out by W. B. T., should perhaps have been mentioned, as well as the theory of Lewes and Ribot, that these deceptions arise from the retrojection or false location of a *present* mental image as a recollection. The inheritance of the actual *cerebral impressions* of a former generation rests upon no scientific basis. We do inherit the brain structure, and, in so far as brain functions are dependent upon structure, we may be said to inherit certain functional disposition and powers; but this structure, and the impressions made upon it by sense-perception, are essentially different facts.

The correspondence invited should be addressed to Princeton, *N.J.*, instead of Princeton, N.Y., as as wrongly given in *Science*, No. 57.

HENRY F. OSBORN.

Princeton, N.J., March 21.

Ripple-marks.

Professor Wooster's note in No. 57, on ripple-marked limestones in Kansas, recalls an observation of my own in Utah. In the south part of that territory the Jurassic formation includes a sectile limestone fifteen to twenty-five feet in thickness, containing remains of Camptonectes and Pentacrinus. Some of the surfaces of the layers exhibit coarse ripple-marks, the wave-lengths ranging from six inches to one foot. The associated fossils cannot be regarded in this case as indicative of quiet conditions, for in neighboring districts the same forms are found in argillaceous sandstones. In the sandstones the shells and crinoid segments exhibit wear from rolling, but in the limestone their angles are unimpaired. While, however, there is no evidence in the limestone of violence, there is evidence of motion. The crinoids have not been found entire, and all their segments are usually detached. Moreover, the structure of some of the limestone layers is oölitic.

I conceive that the association of ripple-marks with shallow water, while usual, is not invariable. The most important condition for the formation of ripple-marks is motion; and any thing competent to produce motion at the bottom of deep water may form them. Wind-waves on the Atlantic are said to have brought sand to the surface from a depth of five hundred feet, and they must be supposed to produce at a still greater depth the gentler agitation necessary for the formation of ripple-marks.

The association of the Kansas ripple-marks with fine argillaceous rocks is perhaps unprecedented, but there seems no theoretic reason to regard it with wonder. Fine sediment does not usually come to rest in spots where the water is subject to agitation, but exceptionally it does; and the centre of every shallow pond with a muddy bottom affords an illustration. Some years ago I observed ripple-marks on a surface of fine river-silt at the bottom of a pool which had communication with a rushing river. The pulsation of the torrent communicated agitation to the pool, but no current; and I inferred that the pulsatory agitation caused the rippling. The pool shared to some extent the muddiness of the river, and the silt on its bottom was evidently a forming deposit. Not far away the bank of the same river exhibited in section