This will lead to a modification of some of the views advanced in my census report above referred to, concerning the past geological history of the peninsula, and the origin of the high hummocks; for these hummocks, in part at least, are produced by the action of the *miocene* phosphatic limestone, and not the *oligocene*, upon the prevailing sandy soils.

And, similarly, the much wider distribution of these miocene rocks proves that a much larger proportion of the peninsula was submerged after the oligocene period than I at one time supposed.

We shall look with the very greatest interest for the results of Mr. Johnson's investigations of the rocks of the western coast of Florida, in Hernando and Hillsborough counties.

I may add that none of the specimens of the upper oligocene or Vicksburg limestone, either from Florida or Alabama, which I have examined, show more than a slight trace of phosphoric acid.

EUGENE A. SMITH.

University of Alabama, April 20.

It might have been hasty, without books, and without sufficient opportunity for comparison, to have pronounced the phosphatic rocks of Preston's Sink, Fort Harlee, miocene, or not older. I now think it later still; but always with the reservation that I may be permitted to change my mind upon a more careful study, under circumstances more favorable, and also deferring to the opinion of Dr. White, who already has such favorable opportunities, when he can get time to take up the subject, with all my collection before him.

The location of these phosphates is of more immediate import to you and me. But, on the question of the horizon, I ask the consideration of the facts and specimen already sent you. The 'Nigger Sink' at Downing's, in this vicinity alone, ought to set the question at rest.[1] There you find *in situ*, and exhibiting their due relations, the oligocene limestone at the base, and finally, after various intermediate deposits, a hundred and fifty feet above, the siliceous phosphatic rock, exactly similar to that sent you from the quarry at Gainesville, from Liveoak, and which is found in this oak and hickory region on the top of every hill.

There, also, you find two fossils,— the Ostrea, found also at Hawthorne and in the Wacahootie region, Marion county, always underlying the phosphates, and above the Orbitoides and Pecten of the limestone; and the other, the great coralline, of which I could mail but a fragment. This last is seen *in situ*, so far as I am now informed, nowhere but on the tops of these hills, overlooking the Natural Bridge of Santa Fé.

The Fort Harlee marl, near Waldo, is quite different from the phosphatic rock I have been sending you from so many points. It has all its shells, or casts of shells, intact. The vertebrate fossils, however, seem the same; that is, the sharks' teeth and saurian remains are alike. The phosphatic rock has lost all its fossil shells. That these once existed, is clear from the fact that occasionally a trace may be found. If not the same, then how are they related? The argument must be postponed; but to me the conclusion is clear that the Waldo bed is newer than the others. All the others, from the texture of the rock, the obscure traces of shells, the chemical constituents, and from the surroundings, may be classed as one. The great extent of the formation, and the uniformity of the rock, are still very remarkable.

Undoubtedly it is the same rock seen near Ocala, where the limestone is not visible, at Hawthorne, at Gainesville, at Newmanville, at two or three knolls in the vicinity of Liveoak, and on innumerable others all over this central region of oligocene sinks. Strangely, too, the knobs are uniformly of a height of about sixty feet above the surrounding flats and depressions marked by the cherty limestone. It would be interesting and valuable, if I had the means in my power, to locate and measure the extent of every one of these deposits. Your own census report, giving the extent of hummocks, and oak and hickory soils, east of the great chain of sand-dunes from Apopka northward, and west of the lake region, is the nearest means I can suggest for making an approximate estimate.

LAURENCE C. JOHNSON.

Newmanville, Fla., March 22.

[1] Three others, heretofore explained, — Simmons at Hawthorne, Sullivan old field, and the devil's mill-hopper, — sustain the same conclusions, and none contradict.

Do telegraph-wires foretell storms?

Probably some thousand Americans have noticed the automatic storm-signalling of wires by sound-vibration. I allowed a telephone-wire to remain for a long time attached to one corner of my (frame) house because of its practical utility as a weather-prophet. When not a leaf was stirring in the neighborhood, and not a breath to be felt, the deep undulations were audible in almost every room, although mufflers had been duly applied. Before that, some hours in advance of every severe storm, the upper story was hardly inhabitable on account of the unearthly uproar, which would have made a first-rate case for the Society for psychical research.

The warning that it gave varied from six to twelve hours, rarely exceeding the latter; and I do not think it ever warned in vain. When the storm actually came, the noise nearly always ceased. It never was noticeable in the warmer part of the year; and through the heat of midsummer it was silent. I cannot recall any exception to this. Its climax of clamor was reached some hours before the 'electric storm,' as it was called, of November, 1882. But all through two winters and the proximate parts of autumn and spring I found it a trustworthy and self-announcing storm-signaller, which left me abundant time to prepare. I had it removed, finally, because there was sickness in the house, and its doleful prophecies were not appreciated.

I explained the phenomenon, partly at least, by the effect of very distant air-impulses transmitted in sound-waves from wire to wire, after the manner of the acoustic or mechanical telephone. Yet this does not seem quite adequate, when one considers how far those vibrations must have travelled to outstrip a storm by hours; and yet how much energy and sonorousness they retained when they reached me!

WM. H. BABCOCK.

Washington, April 16.

[We have good authority for saying that the vibrations of the telephone and telegraph wires here referred to are certainly not due to electric currents, nor to the minute acoustic waves of the mechanical telephone, but are simple transverse vibrations and longitudinal waves such as occur on every stretched cord that gives out a musical note. These vibrations are ultimately caused by the wind. For any given wire stretched in a permanent location, there will undoubtedly be a certain direction and character of wind that will call forth its loudest tones. Our correspondent's wire may be specially influenced by the south-

erly winds that precede storms. Sometimes rapid alternations of sunshine and shade, by heating and cooling the wire, cause it to elongate and contract rapidly, and maintain an additional series of musical notes. Sometimes the length and tension of a wire stretched between two telephone supports is such that it can harmonically respond to several classes of waves transmitted from distant parts of the line. We thus obtain the very rich effects of the aeolian harp, which, as is well known, has often been said to ring out the finest notes before a storm, and whose action was also attributed to magnetism and other occult causes, until Chladni gave the correct explanation. — ED.]

An attempt to photograph the solar corona.

Mr. W. H. Pickering having called my attention to his letter entitled 'An attempt to photograph the solar corona,' printed in *Science* for April 3, may I ask you to insert the following lines in the next number of your journal.

The false coronal effects which Mr. Pickering describes are precisely those which might have been expected to result from his optical and instrumental methods. I have in my papers called special attention to the two principal sources of false effects which are present in the form of apparatus employed by Mr. Pickering; namely, the use of a lens, and the position of the drop-shutter which is said to have been 'attached to the lens.'

In some early attempts which I made with lenses, any true coronal effect which may possibly have been upon the plates was completely masked by very strong false coronal appearances and rays, similar to those obtained by Mr. Pickering. These were due, probably, in part to outstanding chromatic aberrations of the lenses, though corrected for photographic work, in part to reflections from the surfaces of the lenses, and in part to a diffraction annulus about the sun's image. It was on account of these, and some other probable sources of error when a lens is used, that I had recourse to reflection from a finely polished mirror of speculum metal. When the mirror was used, all these false effects disappeared.

It is scarcely necessary to remind your scientific readers that the only position in which the drop-shutter can be placed, when an object so bright as the sun is photographed, without introducing strong false coronal effects about the sun's image from diffraction, is in, or very near, the focal plane. 'Attached to the lens,' whether behind or in front of it, a strong diffraction effect is produced upon the plate at the beginning, and again towards the end, of the exposure. If Mr. Pickering will direct his apparatus to the sun, and observe the sun's image on the ground glass of the camera during the time that the drop-shutter is moved very slowly past the lens, he will be the spectator of a succession of fine diffraction effects, which in the aggregate, as far as they were bright enough, must have recorded themselves on his plates. In this way, with care and skill, the sources of other instrumental effects could, no doubt, be tracked out.

In one of my papers my words are, "The moving shutter, being placed very near the sensitive surface, and practically in the focal plane, could not give rise to effects of diffraction upon the plate." I may now add, that, even with the shutter near the plate, care has to be taken that no light is reflected from the edge of the moving plate of the shutter.

I state that with my apparatus, when the sky is free from clouds, but whitish from a strong scattering of the sun's light, "the sun is well defined upon a sensibly uniform surrounding of air-glare, but without any indication of the corona. It is only when the sky becomes clear and blue in color that coronal appearances present themselves with more or less distinctness." Any apparatus intended for photographing the corona must fulfil perfectly these conditions before any serious attempts are made to obtain the corona.

I stated, in a paper presented to the British association for the advancement of science in the summer of 1883, that I had discarded the use of colored glass (or cells of colored solutions) because of the danger of false appearances from imperfections in the surfaces or in the substance of the glass.

Mr. Pickering does not state that his sensitive plates were 'backed' with a solution of asphaltum, or other black medium, in optical contact with the glass, — an essential condition.

No tube, with suitable diaphragms inside, appears to have been used in front of the lens to prevent light falling upon the inside of the telescope tube or camera, and being thence reflected possibly upon the plate. The desirable precaution of using a metal disk, with a suitable surface, a little larger than the sun's image, and placed close in front of the sensitive plate, does not seem to have been taken.

Mr. Pickering says of the violet glass, "By its use, a negative image of the sun's disk was obtained; but without it, the plate gave a reversed image." I found no difficulty in obtaining a negative, or a reversed image, when violet glass was used, by a suitable change of the time of exposure; and therefore Mr. Pickering's time of exposure was in fault, if he wished a different result.

Mr. Pickering says, "Both bromide and chloride plates were provided; but, as with Mr. Huggins, the latter proved to give much the better coronal effects." And again, towards the end of the letter, he says that "chloride plates are more suitable than bromide ones for obtaining an atmospheric corona, just as Mr. Huggins has claimed that they are more suitable for taking a solar one: hence I think one must not rely too much on the ultra-violet sensitiveness of the chloride plate for the separation of the two." Passing by the use of the words 'atmospheric corona' for the false appearances which were due in great part, if not altogether, to diffraction and other instrumental effects, as I have already pointed out, and presuming that Mr. Pickering was not unfamiliar with the greater blackness of chloride plates, especially when developed with ferrous oxalate, he seems to infer some special suitability of the chloride plates to bring out the false effects upon his plates. It may be suggested that Mr. Pickering seems to have used the same length of exposure throughout, "giving an exposure which may be estimated at about a fifth of a second." Now, it is scarcely probable that the bromide and chloride plates possessed the same sensitiveness; and it may have been that the (probably) more sensitive bromide plates were thin from excessive exposure. It may even have occurred that his lens, if corrected for bromide plates, gave an outstanding aberration about H, or a little beyond. Anyway, until these and some other similar points are cleared, it does not seem to me that Mr. Pickering is justified in making the insinuation which seems to lie in the words which I have quoted.

In conclusion, I cannot refrain from expressing great surprise that Mr. Pickering should have mentioned my name in connection with experiments carried out in complete disregard of the conditions to which I had called attention, as essential in a matter