figuration of shores. Our paleozoic ocean was too broad to hurry its currents by crowding them. There is no probability that differences of *ocean* temperature in the past have been great enough seriously to increase the currents; and the little that is known of past aerial temperatures is not enough to insure steeper barometric gradients for stronger winds. As to the velocity of the winds being proportional to the rotation or size of their planet, I must venture to differ from Mr. Darwin (*Nature*, xxv. 1882, 213): for barometric gradients would be steeper on a small planet than on a large one; and the deflecting force, coming from the planet's rotation, depends, not on its size, but on its angular velocity. Moreover, this force does not significantly affect the wind's velocity, but only its direction; and if the earth turned faster, as it may have formerly, the course of the trade winds would be *flattened* (made more oblique to the meridians), but their velocity would not be materially changed, as has been shown by Ferrel. It does not, therefore, seem safe to count on stronger ocean-currents in the past, until it can be shown that the difference between polar and equatorial temperatures was formerly greater than it now is.

But with tides the case is different. There has been found a mechanism by which the tides have decreased automatically from a former greater strength, and I feel that such a contribution to former greater activity in the ocean is to be welcomed in physical geology. It is not a question of six hundred foot tides, by whose devastating strength Mr. Ball has weakened his argument, but of paleozoic marine transportation along the open shores of the ocean, of greater force than is now found; and to this end the old tides promise effective aid. W. M. DAVIS.

Cambridge, April 8.

Transmission of long or inaudible sound-waves.

A simple method of testing whether the atmospheric wave (which, it is claimed, passed around the earth in less than thirty-six hours) had its origin at, and was due to an explosion of, the volcano Krakatoa, would be to examine the previous records of the self-recording instruments for those particular times at which the waves caused by the explosions of some of the larger powder-mines would reach a given locality.

That explosions of this kind cause disturbances which are made manifest (without the aid of any delicate instruments) at localities many miles from the place of disaster is a well-known fact. S.

Tornado in western North Carolina.

On Tuesday, March 25, about five P.M., a tornado passed through portions of Catawba and Iredell counties, extending in a due east course for twenty-five miles.

The first evidence of a destructive storm is two miles and three-fourths west of the town of Newton, the highest point of land east of Baker's Ridge, which is twelve miles to the west. The fallen trees showed two distinct currents of wind, — the one from a few degrees north of west, the other south-west. No evidence of a rotary motion was observed until within three-fourths of a mile of Newton, which, however, was only in a limited area. In the town, and east of it, the rotary motion was decided and destructive.

A very extended and severe hail-storm extended all along the track of the tornado on the north or left side, slowly moving south, reaching the path of the storm. The day had been unusually warm; wind south, shifting to south-west. Several persons witnessed the meeting of the rapidly moving clouds from the south-west with the hail-cloud; also the formation of the descending tornado-cloud. Before it reached the earth, portions became detached, and descended to the earth, afterwards united, and moved forward unbroken. While passing through Newton, the form of the cloud was that of an hourglass, the lower end considerably retarded, the middle portion waving. Immediately east of the town there is a valley; and, when the cloud passed over it, it became erect and funnel-shaped. The surface of the country over which the storm passed is quite diversified. Valleys nearly in the direction of the storm's path were able to deflect its course slightly. The highest points showed evidence of greatest force, though frequently the trees were felled in the lowest parts of the valleys.

The after-wind was but slight. Several houses were lifted from the lower floor and carried away, leaving the occupants unhurt, and not blown along by an after-wind.

The left side of the track is quite sharply defined, while the right extends to a much greater distance, and gradually all trace disappears. The width of the path is from five hundred yards to a mile, though the more destructive part is from a hundred and fifty to five hundred yards.

The damage to houses, barns, timber, and fencing, was very great; nothing being able to withstand the force of the storm except the small trees.

J. W. GORE.

University of North Carolina,
April 8.

Osteology of the cormorant.

If Dr. Gill had read the literature on the cormorant before writing to *Science*, he would have learned that I was following Selenka, and that my reference was all-sufficient for the purpose; namely, a reference to a previous figure. Dr. Gill might as easily have referred the committee to the other references found in Carus and Engelmann's *Bibliotheca zoologica*. Those interested in the subject will find my last remarks on the point in dispute in the *Auk* for April.

J. AMORY JEFFRIES.

The remarks of Dr. Gill, which are contained in his letter to *Science*, No. 61, have just been read by me. As one of the persons designated by your correspondent, permit me to thank him for the information he has so timely tendered.

A certain amount of reprehension always attaches to a laborer in any field of science if he is found not to be thoroughly acquainted with the literature of his subject. This censure is well deserved, particularly if no good excuse exists for such ignorance. The language used by Dr. Gill in his letter seems to bear with it this charge; and, in simple justice to myself, I feel that a few words are demanded from me in answer to it. In my first paper upon the 'Osteology of the cormorant' (ii. 640), I distinctly said that the occipital style is alluded to by Professor Owen, in his 'Anatomy of vertebrates.' That was equivalent to stating the fact that it was universally known to anatomists. The libraries were not available at the time that that article was penned, and I candidly stated in it my ignorance of any figures of the bone in question.

At the time my second notice of this bone was written, the views of other scientific men and the libraries were available; and in a few lines I simply refuted Mr. Jeffries' notion that it was an ossified tendon (ii. 822). Nothing further than this was called for. In my third and last notice (iii. 143) the manner in which the muscles attached to the occipital style are inserted was alluded to, and it was compared with an ossified ligamentum nuchal. All of this I still maintain. At that time, for lack of material, I had not especially looked into its physiology; and my discussion with Mr. Jeffries closed (Feb. 8, 1884). Since, both through my reading and observation, much has come to my notice of interest with regard to it. Garrod's dissections of Plotus anhinga are very suggestive. Dr. Gill had kindly called my attention to Yarrell's paper, before his notice in *Science* appeared, which he had unexpectedly come across while searching for facts to illustrate another subject. Finally, in one of the most useful and reliable of books, Coues' 'Ornithological bibliography,' I had noticed Rudolphi's article; but other matters were engaging my attention then, and reference was not made to it. There are still others. I have already cited Eyton's figure (iii. 143), and believe, at the time Dr. Gill's review of my work appeared, I was hardly entitled to the charge he brings against me in it. I am more and more convinced, every day of my life, that *good illustrations* of such common facts in anatomy are most urgently demanded. R. W. Shufeldt.

A singular optical phenomenon.

I think it would well repay almost any one to study the beautiful phenomenon so clearly described by 'F. J. S.' (*Science*, No. 57, p. 275), and so suggestively discussed by Professor LeConte (No. 61, p. 404). My own theory of it involves no inverting action, as in the camera, and no *primary* dependence upon binocular vision, but, rather, it resembles the theory of watered silks, or of chords and beats in music. It seems to me geometrically demonstrable; and it includes the phantom meshes' gigantic size, their bewildering motions, their conspicuousness even to eyes out of focus for the actual wires, and the non-appearance in them of objects attached to those wires.

Before the observer are two parallel screens of square-meshed wire netting. The coarser one is seen through the finer, and *the two are at distances from him nearly proportional to the diameters of their meshes, measured from centre to centre of the wires.* To fix the ideas, suppose that he looks with only one eye, seeing the nearer wires black and the farther ones bright: then, if the above proportionality be exact, all the bright wires can be simultaneously eclipsed, each by a separate dark wire; or, upon moving the eye very slightly to the right and upward, all the bright wires will flash into view at once. Now let the observer advance or retire a few inches from this first position, so that the dark wires may subtend visual angles a little larger or smaller than do the corresponding bright ones: several successive bright wires will thus be in view, then one or more will be eclipsed, then several others will be seen, and so on; that is, the phantom screen will be formed, with its great square meshes and shadowy bars.

Next let the observer move slightly to the right: the phantom also moves, but more, and to the right or the left, according as he is in front of or behind his first position. Indeed, the motions of the phantom bars, and the visual angles they subtend, are as if the observer viewed a virtual image whose plane passed through his first position, but imagined it to be some feet in front of him. The size of the virtual image would be very nearly such, that, in it and the farther screen together, there would be as many bars to the foot as in the nearer screen. Its colors would appear to be those of the farther screen, but weaker and oppositely arranged. It would *not* be upside down. Indeed, if 'F. J. S.' will paint the upper wires of the farther screen vermilion, or will hang behind them a blue curtain, then I think that the upper meshes, but not the bars, of the phantom, will be reddened; or the upper bars, and more slightly the meshes, of the phantom, will be bluish. Or, if he will paint the vertical wires red and the horizontal wires yellow, probably the phantom meshes will incline to orange, the vertical phantom bars to yellow, and the horizontal ones to red.

Suppose that two-thirds of the light coming from within the boundary of the farther screen be from the bright wires: then the phantom meshes will be three times as bright as the phantom bars; but at their edges they may blend into one another, the eclipses there being less complete. Thus no lines appear in the phantom whose pictures on the retina are not much broader than the picture of a point, even when out of focus, and hence the phantom is seen by near-sighted and far-sighted alike.

Phantoms often less simple and conspicuous may be got when the visual angles subtended by single spaces in the two screens are not approximately equal, but are approximately in a simple numerical ratio. The screens may also be of lattice-work, or pale fences, not necessarily parallel, seen two or three deep against the sky; and the effects are sometimes very beautiful.

Undoubtedly, when the screens are fine, binocular vision, with the stereoscopic matching of patterns, comes in, as suggested by Professor LeConte; making the phantom seem real and solid, and fixing its assumed distance from the observer. But I leave this part of the discussion to him, because he can treat it far better than I can. James Edward Oliver.

Cornell university, April 8.

I was gratified to find that the phenomenon described in No. 57 proved of interest to Professor Joseph LeConte. He states that my explanation of the cause of the phenomenon is erroneous, and I am in no wise qualified to dispute him. Nevertheless, a careful repetition of the experiment would indicate that his explanation is not the correct one. The phantom image is as readily seen with one eye as with two; and I still think I am correct in saying it is inverted and magnified. I hope Professor LeConte will make the experiment himself, and give us his explanation of the phenomenon. In the mean time, allow me to state the facts as they occurred in an experiment made after reading his letter.

Standing about twelve feet from an ordinary fly-screen, and looking through it at the blinds of a house about one hundred and fifty feet distant, phantom lines, alternately a light one and a dark one, are seen crossing so much of the field of view in which the blinds lie, but not continued beyond their limits. The lines remain visible, although one eye be closed.

The image rises as I bow my head, and sinks as I lift it. Is not this evidence of inversion?

I can readily count the lines that lie across a blind, twelve light and twelve dark ones; but, in order to correctly count the actual slats in the blind, I am obliged, on account of the distance, to have recourse to a telescope. My wife, who is short-sighted, can only distinguish the mere outline of the actual blind; but the phantom lines are plainly visible to her. The number of slats in a blind is thirty, which would give sixty alternating dark and light lines. Is not this evidence of magnification? F. J. S.