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