mitted by meteorologists, that in the latitudes 0°–6° the deflection is also too small to admit of cyclones; and really I know of none. And even outside India, and the seas around it, there are scarcely cyclones in latitudes lower than 10°.

Thus, what Mr. Davis calls 'equatorial' should be called 'tropical' cyclones. If anybody wishes to mention 'equatorial cyclones,' let him first prove their existence. So long as this is not done, meteorologists having a mind for exact scientific terms will hold to my opinion. A. WOEIKOF.

St. Petersburg, Jan. 7, 1884.

I shall be well pleased if so distinguished a meteorologist as Dr. Woeikof finds no other points needing correction in my papers on storms than this one. That I fully agree, as to the facts, with him and with Dr. Taylor, who first, so far as I know, states this matter in connection with its cause,[1] is shown in my seventh paper (this volume, p. 40); but, while my use of the objectionable term was accidental rather than deliberate, there is, perhaps, little to choose between 'equatorial' and 'tropical,' both of which occur in this connection in my papers: for, if the first apply in strictness only to points in latitude 0°, the second is equally limited in its exact meaning to points in latitude 23½°; and if 'tropical' has come to mean 'within or between the tropics,' so 'equatorial' may mean 'near the equator.' *Tropenzone* of the Germans is not to be translated 'tropical zone,' but 'torrid zone;' and in English, 'tropical' should not be applied in an exact nomenclature to the equatorial low pressures of the doldrums, as in Buchan's writings, but rather to the high pressures of the horse-latitudes, as Ferrel uses it; and 'tropics,' when properly rendered into German, would be *wendekreisen*, or it might be paraphrased into *die polargrenzen der passate*. Inasmuch, then, as the truly tropical belts of the ocean are best characterized by regions of high pressure, free from cyclonic conditions, except where storms from lower latitudes cross them near their western shores; and as the *inter*-tropical rains of the doldrums are not called 'tropical,' but 'equatorial,' even when off of the equator, and by Dr. Woeikof himself, — it can hardly be considered a serious error to speak of the cyclones, which begin in the doldrums, as equatorial also.

Cambridge, Jan. 30, 1884. W. M. DAVIS.

Osteology of the cormorant.

Mr. Jeffries' answer in *Science* (iii. 59), to my letter in a former number of this paper (ii. 822), caused me genuine surprise. His suggestion that the occipital style of the cormorant 'is the ossified tendon of some of the extensor muscles of the neck,' made in a former communication (ii. 739), is here, apparently, announced as his conviction, and Selenka is introduced to sustain the statement. Now, I am informed by Mr. Jeffries, that, "in view of such eminent authority, it would seem that something more than simple denial is required to upset a statement accepted by anatomists for many years;" and a few lines farther on, I am said to acknowledge my mistake, because I ignored the point. Permit me to say, that nothing of the kind has been accepted by anatomists for many years. I met this statement by a simple denial, in order to save space in the columns of *Science;* but, if Mr. Jeffries must be informed as to what the occipital style of the cormorant is, I would inform him that this bone is not an ossification in any tendon of the extensors of the neck, because it is situated, as we know, in the median plane of the skeleton, at a mid-point on the occipital ridge. The tendons of the extensors in a bird's neck, which are inserted at the occiput, are in pairs, their insertion being bilateral; and their tendons are *never* inserted in the median plane: consequently this style cannot be an ossification of any of them. On the contrary, *it is* an ossification of the fascia between the extensors of the neck and what may be compared to the ligamentum nuchae.

As Mr. Jeffries seems to be anxious about the position in which I drew this occipital style, I would call his attention to the fact that it is shown as occupying its *proper site*, only tipped up somewhat, as it was on my dried skull. Such license is perfectly permissible in anatomical delineation, and is seen in the illustrations throughout the literature of anatomy. It often shows the parts to better advantage; and, in structures as well known as this style is, no explanation is necessary. Acquainted, as I am, with the *anatomy* of this 'nuchal style' and its anatomical relations, I must again acknowledge that I am still ignorant of the *physiology*, or really the function, of this style, or why it should occur in a cormorant and not in other birds nearly related.

As to Mr. Jeffries' concern at my not being, to his mind, thoroughly informed upon the homologies of the patella in birds, I would invite his attention to a paper of mine written some time before my 'Osteology of the cormorant' appeared. To show that I have always agreed with the eminent authorities he alludes to for my benefit, in the co-existence of a patella and an elongated cnemial process of the tibia in most divers, I refer to my article entitled 'The number of bones at present known in the pectoral and pelvic limbs of birds,' in which I say, "I know of but two free bones that occur about the knee-joint. The first of these is the patella; and this may co-exist with the cnemial ridge of tibia, as in Colymbus (Owen). The other is a free sesamoid found in some birds in a notch at the head of the fibula (Speotyto)" (*Amer. nat.*, November, 1882, 894). I repeat, that 'I find myself misquoted' by Mr. Jeffries, in his remarks upon my paper, 'more than once;' that is to say, he has failed to include statements falsely attributed to my article in the customary quotation-marks. I do not say, (1) that I figure this style '*in situ*,' nor (2) positively affirm that it has never been figured before (ii. 739), but do say, "I do not believe we have a figure showing the site of this bonelet" (ii. 640). Selenka's and Eyton's figures had slipped my mind for the moment, as their works had not been available for a year or more. Furthermore (3), I do not refer to Professor Owen to have him authorize any thing in regard to Podiceps, but only to the patella of the loon, as any one accustomed to anatomical reading can see by referring to my article on the 'Osteology of the cormorant' (ii. 640).

R. W. SHUFELDT.

Upperglow of the skies in relation to halos and coronas.

These striking and beautiful atmospheric phenomena, which have manifested themselves over the entire globe, have attracted much attention, and been minutely described by correspondents in various countries. But there is one feature, which, although incidentally noticed by some writers, has attracted but little attention. I allude to the fact, that, wherever

[1] On tropical hurricanes (Brit. assoc. report, 1852, pt. 2, 31). Herschel used this in his Meteorology, but failed to do justice to Taylor's explanation of how a deflective force arises from the earth's rotation, and omits mention of the effect of the conservation of areas, which Taylor recognizes as of essential importance.