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.

the phenomena have been sufficiently pronounced, the sun is during the day encircled by a more or less distinct *colored halo or corona.* At this place the assumed supra-cirrus volcanic dust seems not to have been sufficiently dense to have developed the colored rings; and there was observed nothing more than a *whitish glare* extending over the sky from 20° to 25° from the centre of the sun. But the Rev. S. E. Bishop writes me from Honolulu, that this chromatic circle around the sun has been constantly observed in all of the Hawaiian Islands for several months. It has likewise been observed in England as a frequent accompaniment of a conspicuous manifestation of the upperglows of sunset and sunrise.

It is an interesting question, whether this more or less distinct colored zone encircling the sun is a true *ice-crystal halo,* or a *diffraction corona.* Its want of sharp definition, and the absence of the regular succession of prismatic tints due to refractive dispersion, would seem to point to diffraction as the true cause of the chromatic phenomena. On the other hand, the large size of the colored circle, having a radius of from 20° to 30°, would seem to connect it with the well-known ice-crystal halo of about 22° radius.

While I am disposed to regard this chromatic feature of the phenomena as mainly due to the diffractive action on light of the impalpable dust-particles suspended in the lofty supra-cirri regions of the atmosphere, yet it is by no means improbable that ice may be associated with the phenomena: for it appears from the experiments of M. Coulier, and more particularly from those of Mr. John Aitken, communicated to the Royal society of Edinburgh, Dec. 20, 1880 (*Nature*, vol. xxiii. pp. 195–197; also vol. xxiii. p. 384), that the presence of dust-particles in the air is essential to the formation of fogs and clouds; that, when aqueous vapor condenses in the atmosphere, it always does so on some solid nucleus; and that the dust-particles in the air form these nuclei. Now, it is evident that the presence of these attenuated dust-particles in the supra-cirri regions of the atmosphere would produce condensation of the rarefied aqueous vapor at these lofty altitudes. But inasmuch as this region must, even within the tropics, be far above the plane of perpetual congelation, the condensed vapor must necessarily assume the form of aggregations of ice around these nuclei: hence the diffractive coronas may be associated with imperfectly developed ice-crystal halos. JOHN LECONTE.

Berkeley, Cal., Jan. 25, 1884.

Inheritance of physical injuries.

Well-authenticated instances of the inheritance of a physical injury are so rare, that I wish to put upon record one which has recently fallen under my observation. A gentleman, when a boy about seven years of age, had the second toe of the right foot deformed by wearing a tight boot. The first and third toes were crowded together, forcing the second one under and backwards, and causing a curvature of the second joint, which, in time, became permanent. The joint, being somewhat elevated above those of the other toes, received the pressure of the shoe, and always after was more or less troublesome in consequence. The gentleman was twice married. By his first wife he had six children, the second of which was a daughter; the rest, sons. The daughter inherited the crooked toe; but the feet of all the sons were normal. The deformity appeared, however, in the son of one of these, — the brother next younger than the sister, — affecting the same foot and toe as on the grandfather. By his second wife the gentleman had only one child, a son, who also inherited the peculiarity; but in this instance it was the second toe of the *left* foot, instead of the right, that was affected.

Knowing that much doubt still exists whether the results of a slight physical injury, like the one I have described, are ever transmitted, I have taken pains to examine carefully all the evidence under my observation; and I feel assured of its correctness. All four having the deformed toes are now living, and all agree upon the facts. The gentleman is positive that his feet were normal until he was about seven years old, and says he remembers very distinctly wearing the boots which caused the deformity. An examination of the foot does not show any congenital peculiarity which might have been transmitted. The toe, when restored to its correct position, appeared normal in every way. No peculiarity of this kind has ever appeared in any other of the gentleman's relatives. I can see no way, then, of avoiding the conclusion that the injury, or rather its results, have been transmitted to two generations.

The case presents some features which render it especially interesting. The peculiarity's appearance in the children of both wives seems to eliminate altogether the element of the mother's influence in producing it. The recurrence of the variation in the grandchild, the father being normal, indicates how powerful was the tendency to perpetuate this slight deviation from nature's standard. In the other cases which I have studied personally, if a variation did not appear in a child, that child's children were free from it also. I should be glad to know if any one of your readers has observed this tendency toward reverting to the ancestral type under similar circumstances. IRVING P. BISHOP.

Perry, N.Y., Jan. 28, 1884.

Pumice from Krakatoa.

Capt. A. W. Newell, of the bark Amy Turner of Boston, has brought in some pumice which was washed aboard his vessel, Sept. 17, 1883, in latitude 7° 25′ south, longitude 103° 21′ east, about a hundred and sixty-five miles south-west from Krakatoa, Sunda Straits. It covered the sea in windrows, and was observed as fine ashes as far distant as thirteen hundred and fifty miles from its source.

A piece about seven inches by five, which came to my notice, is of a reddish-gray color, and very much inflated: it carries porphyritic crystals of plagioclase felspar, in many cases surrounded by dark-brown glass, forming small black spots in the gray mass, which might at first sight be mistaken for augite or hypersthene. There is, besides, dark-green augite and brown hypersthene, which is strongly pleochroic, and resembles closely that found in the lavas from the volcanoes of northern California and the Cascade Range (Notes on the volcanoes of northern California, Oregon, and Washington Territory, *Amer. journ. sc.*, September, 1883).

The percentage of silica for this pumice was found to be 62.53, and is almost identical with that of the hypersthene-bearing pumice from Mount Shasta, which is 62. It is undoubtedly the pumice of a hypersthene andesite, and is especially interesting because of its similarity to rocks found on the western coast of North America. The observations of Rénard on the ashes that fell in Batavia soon after the eruption of Krakatoa (*Nature*, Dec. 6, 1883) show the same component minerals, and have doubtless been made on similar material.

JOS. P. IDDINGS.

U. S. geological survey, New York,
Jan. 30, 1884.