others. Will we not, therefore, have to cut down very materially the great length of time generally believed to have elapsed in this region from the beginning of this lacustrine period to the present time, when we find that a great portion of the sediment that once filled the lakes is due, not to the products of erosion, as has hitherto been supposed, but to repeated showers of volcanic dust? Again, do not these volcanic materials, which must have fallen in showers over a large extent of country,—accumulating in some cases in beds forty to ninety feet thick,—account for the perfect preservation of the vertebrate remains which characterize the formations in so many parts of the west; and is there not also suggested one possible cause for the extinction of some of the many groups of animals which have at present no descendants in this region, and whose only remains are the bony fragments found in these lacustrine deposits?

A. C. PEALE.

U. S. geological survey.

Carnivorous prairie dogs.—Carnivorous orioles.

The statement of R. W. Shufeldt that his pair of young prairie dogs took kindly to a meat diet (*Science*, viii. p. 102) attracted my attention and interest, for it recalled to my mind an experience of my own in the summer of 1838. Having a pair of the marmots at this moment under observation here, I determined to try them with a piece of raw beef, and the eagerness with which they *plunged* at it (for their avidity cannot be characterized by any milder word) was certainly something very astonishing. Their ordinary vegetable food they take *quietly*, but the beef seemed to set them frantic. They acted as though they were famishing,—they seized it so fiercely, fighting with one another for it, and hastening back to ask for more. And so it has continued. Their owner fears to feed them with it exclusively, but gives them more or less daily, and the contrast between their eagerness for the meat and their quiet consumption of vegetables is a very instructive lesson. Their stomachs, out on the plains, always hold vegetable contents and nothing else. This was doubtless the first piece of meat ever tasted by either of these. Whence this craving appetite?

The experience of 1838 to which I referred was this: That was in the earlier days of my 'natural history,' three years before my first ichthyological paper was written. I had taken three young Baltimore orioles from their nest, but feared that I should lose them, for they refused every variety of food I offered them. At that time I was collecting birds zealously, and was skinning several of them daily. As I was preparing a specimen, one of the young orioles was sitting on my table, very stupid indeed, head drawn in, not life enough to utter a sound, thoroughly dumpish. Without knowing why, I picked up a bit of the bird's flesh and offered it to him. To my great surprise he swallowed it on the instant, and roused himself at once. That one mouthful had done him so much good that he wanted more. I took him on my finger and fed him piece after piece, till his throat was swelled out like an over-fed chicken's crop, and I feared to give him more. He settled himself down with great satisfaction, and went to sleep. I fed his brother and sister in the same way; and from that time till they were fully grown they had not a mouthful of food except the flesh of the birds I was skinning. Their eagerness for the meat was extreme. They learned the bird-skinning business to perfection. As soon as they saw me prepared for work, they all gathered about the specimen, ravenous for meat, and I almost always commenced to skin my bird, with an oriole sitting on each hand, and one on the specimen itself, and with three little heads down over the abdomen, where the first cut was to be made (they knew the point well enough); and the instant I opened the skin, in went three bills, digging and tearing fiercely for their food, and continuing at it as I continued my work, till their appetites were satisfied.

I do not know that this fact concerning the Baltimore oriole has ever been reported. I recollect mentioning it to Mr. Audubon, but it was after his account of the species had been published.

W. O. AYRES.

New London, Conn., Aug. 11.

Flooding the Sahara.

In our own country an evaporation of two feet per year is a small figure, and twice that amount has been recorded in some cases; so that it would seem to be safe to assume that it would exceed the latter value in the north of Africa. Taking Mr. LeConte's figures (*Science*, vol. viii. p. 35), and an evaporation of *two* feet per year, and the cubic feet evaporated, on an area of 3,100 square miles would be $2 \times 864.-230 \times 10^5$ cubic feet $= 1{,}728{,}460 \times 10^5$ cubic feet per year. But the inflow, according to his assumptions, would be $1{,}262{,}277 \times 10^5$ cubic feet per year; so that at the rate of two feet of evaporation per year, the amount evaporated would be 1.3 times the amount of the inflow. In other words, at the rate of inflow assumed, the depression to be flooded would *never* be so far filled as to make a surface of 3,100 square miles; and if the evaporation be four feet per year, the inflow would necessarily be nearly three times that assumed by Mr. LeConte.

DE VOLSON WOOD.

Hoboken, Aug. 14.

Barometer exposure.

The discussions in *Science* relating to the effect of high winds upon the indications of a barometer in a room, have been highly interesting. I only desire at this time to present a few facts that bear upon the problem, and to correct a few misconceptions. No one that has attempted making a fire in a very cold room, on a very windy day, with a refractory chimney in the fore ground, can be easily convinced that there is much of a draft up a cold chimney, even with a hurricane. Even if there were such draft, the air must flow in through all the cracks, especially on the windward side, and equilibrium would thus be kept up. It should be noted also that the wind does not blow steadily, but rather in gusts; consequently there can be no such thing as a permanent lower pressure inside than outside a room, but a momentary depression by a gust would be relieved almost immediately by the lull.

This is shown beautifully by a barograph properly arranged. All references will be to a barograph inclosed in a tight glass case, such as has been adopted by Mr. Hough of Albany. The fluctuations are so rapid that they cannot be seen on a sheet carried at the rate of one to two inches per day, but only upon

one carried from seven-tenths of an inch to one inch per hour. In the latter case, with a very high wind sometimes, but rather the exception, there will be seen fine serrations, at intervals of one or two minutes, having the appearance of a very fine saw. These serrations are quite regular, and are seen only during the high wind. The greatest fluctuation cannot be more than eight one-thousandths of an inch and seldom are above four one thousandths to six one-thousandths. It is probable that the wind influences these fluctuations, but it is very difficult to determine just how. That a high wind does not always produce them is quite remarkable. Returning to our drawing chimney, it would seem an interesting computation as to how long a gust would need to last in order to draw out of a chimney one foot square sufficient air to produce the supposed depression.

If we consider that the barograph is inclosed in an almost air-tight case, we have still another addition to our problem. Even if there were a withdrawal of air from the room, is it possible for the influence to reach the inside of the case before the lull has made a change? A partial answer to this question may be had by experimenting with the case. If the door be opened rather suddenly a partial vacuum is formed, or a jar occurs, which moves the float, and the pencil falls or rises according as the barometer has previously had a tendency down or up. This effect is only two one-thousandths of an inch; and it is very rare that an influence greater than that can be brought to bear upon the apparatus under these conditions. It would seem as though the effect produced by opening or closing the case may be many times greater than the utmost that can come from an intermittent wind.

If we turn to the original letter by Mr. Clayton (vol. vii. p. 484), we shall find these particular cases given by him: 1°. "On March 16 the wind's velocity rapidly rose from five to thirty-five miles, and the barometer suddenly fell five one hundredths of an inch;" 2°, "During a sudden gust attending a shower, last summer, the barometer fell a tenth of an inch, and immediately rose again as the gust ended;" 3°, "It [the pressure] fell as much as a tenth of an inch during a seventy-mile wind in February." It will be seen that each of these cases occurred under abnormal conditions, and just at the time when we would naturally expect such fluctuations; but they can hardly be due to the wind, as they are often noted when there is no high wind. The wind's action is intermittent, and there is no evidence whatever of this most important fact making itself known. It is a matter of regret that Mr. Clayton did not open and shut his trap-door at intervals of five or ten minutes, for an hour or so. He would have settled the question beyond doubt if he had done this.

Much has been written in regard to the evidence of observations on Mount Washington. Mr. H. A. Hazen has given a partial discussion of the Mount Washington records in the 'Annual report of the chief signal officer,' for 1882. He there has shown that the effect of the wind upon the computed elevation changes sign at a velocity of twenty-five to thirty miles per hour; i. e., instead of the effect being zero when there was no wind, it was really zero with a wind of twenty-five to thirty miles per hour. This is a fair indirect proof either that the wind does not cause the fluctuation, or, if it does, that another force is superposed upon it.

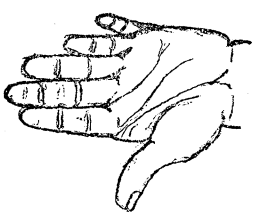
It is hazardous drawing conclusions upon the facts thus far developed. It may be that the wind can produce more than one effect, and that the serration effect above alluded to is not the only one to be considered. The weight of evidence seems to be rather against any great depression being produced. Mr. Clayton will do meteorology a great service by trying a few experiments. If his barograph, shut, is carried along only two inches a day, opening the trap-door ten minutes will give only one seventy-second of an inch for the pencil to move in. The difficulty can be obviated, however, by letting an attendant note the movement of the pencil (if there be any) and carefully take the time of the fluctuations, if the time of manipulating the trap-door be also taken, a comparison of times will settle the question.

GAN.

Aug. 10.

A case of inherited polydactylism.

In the spring of 1883 I saw and examined a case of inherited polydactylism, which I think worth recording. While enjoying the hospitality of a friend, in a charming ravine opening into Napa Valley from the mountains on the west side, my attention was drawn, by my intelligent hostess, to the hands of a German laborer at work in the garden. There were six well-formed, usable fingers on each hand. The metacarpals were of the normal number, but the fifth bore two fingers. The supernumerary little finger differed from the true little finger only in being much smaller.



I give a rude drawing of the left hand, made on the spot, showing the size and position of the supernumerary finger.

I inquired concerning his family history in this regard. His account is given in the following diagram, in which I have italicized those who are or were polydactylous:

Maternal Grandfather. *Maternal Grandmother.*
Father. *Mother.*
Peter Weitner, Sisters. *a*, b, c, d; Bro. *a*.
Children.

It is seen that the deformity was inherited from his mother's maternal grandmother; that, besides himself, it has affected one sister, out of four, and one brother, and has been transmitted to the children of the sister, thus affecting at least four generations.

JOSEPH LE CONTE.

Berkeley, Cal., Aug. 5.

"Thumb marks."

One of the anatomical characteristics recently brought within the area of anthropological investigation is the marking on the skin of the hand, espe-