the Lowell Institute, Boston, in 1889, devoted to Alexander, Hannibal, Cæsar, Gustavus Adolphus, Frederick, Napoleon, and the record of their achievements and the analysis of what each of them contributed to military science; "Ancient and Modern Light-Houses," by Major D. P. Heap, Corps of Engineers, U.S.A.; a new edition of "Discourses on Architecture," by E.-E. Viollet-Le-Duc, richly and copiously illustrated with hundreds of steel engravings and woodcuts, translated from the French by Benjamin Bucknall; a new and cheaper edition of "A Hand-Book of Christian Symbols and Stories of the Saints, as illustrated in Art," by Clara Erskine Clement and Katherine E. Conway; and "His Two Wives," a novel, by Mary Clemmer, being No. 50 of Ticknor's Paper Series.

LETTERS TO THE EDITOR.

***Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

Twenty copies of the number containing his communication will be furnished free to any correspondent on request.

The editor will be glad to publish any queries consonant with the character of the journal.

Dew-Point and Predictions of Weather.

ONE of the most serious drawbacks to a discussion and utilization of humidity records has been the lack of proper observational methods, and also of tables of reduction. As late as 1884 we find in Guyot two distinct tables for reducing observations with the psychrometer (the usual instrument for determining humidity) which give results differing by more than sixty degrees at extreme dryness. Perhaps nothing can indicate better the hopelessness, as late as 1887, of nearly all attempts at solving the problem of the relation between the dry and wet thermometers and the dew-point, than the announced determination of the meteorological committee to omit a table for the psychrometer from their compendium of tables for international use. It will be generally admitted that such a table is the most important and most needed of any in meteorology. The most serious difficulty in nearly all investigations has been a lack of ventilation of the psychrometer.

In September, 1883, the sling psychrometer, which combines all the admirable qualities of perfect ventilation and accuracy, great speed of action, and extreme portability, was adopted in this country.[1]

With this the true relation between the quantities mentioned above was determined in 1884, and published in February, 1885; and this has been used in the latest tables, leaving nothing now to be desired except observations to check the formula at extreme dryness, such as does not occur east of the Rockies.

I propose to discuss a few recent observations with the sling psychrometer. It might be a question as to the best form in which to study the moisture of the air. The relative humidity, the difference between the dew-point and air temperature, the dew-point itself, the absolute humidity, and the vapor pressure, have all had advocates. It may be remarked that the second of these, being a deduction from two quantities which are often rapidly varying in opposite directions, seems a little uncertain. The fourth and fifth are similar to the third.

The following propositions regarding the dew-point are set forth: 1. The diurnal change in air temperature does not affect the dew-point; 2. The temperature change from day to day does not change the dew-point; 3. The air temperature is generally very near the dew-point at sunrise, and farthest from it at 2 or 3 P.M.; 4. The air temperature in its fluctuations from day to day follows the dew-point; 5. Direction and velocity of the wind do not in general affect the dew-point; 6. The same may be said of fluctuations in air-pressure; 7. The most marked rise in the dew-point occurs on the approach of a storm having an abundance of rain and during rain itself (the time of beginning and ending of rain cannot be foretold from the dew-point); 8. The most marked fall in the dew-point is caused by the advance of a high area, as was to be expected; 9. The most marked feature of the dew-point is its constancy, though at times it has a range in several days far greater than the air temperature, yet it quickly recovers from a fall or rise to a normal position, depending on the season and other general causes; 10. The dew-point is the same in all parts of a quite extended region.

The fourth of these is one of the more important, and seems to follow from the third. We have usually been taught that the air temperature on a clear night will continue to fall till the dew-point is reached, when there will be condensation of moisture, and liberation of latent heat, which will prevent the further fall in temperature; but it will be found, that, except after a long rain and in a fog, the air temperature never reaches the dew-point. Very often on clear nights the latter falls, and draws the former after it. If this proposition can be established, there may be a chance to predict changes in air temperature from the dew-point, though they are very close together.

On many accounts the seventh proposition is the most interesting of all. Does the atmosphere in this case gradually sink down? This usually would *increase* the dryness. The wind does not appear to carry the moisture, for this steady rise occurs in a calm. Moreover, the direction of the wind, as coming from the earth's surface, makes little or no difference. It is very evident that the dew-point cannot be used in predicting rain. Under the eighth proposition it should be noted that the fall in the dew-point ceases in a few hours, and long before the pressure has reached a maximum. The figures from which these propositions arise will shortly be published elsewhere. It would be gratifying if others are stimulated to make similar research. H. A. HAZEN.

Washington, Jan. 16.

[1] My attention has just been called to the use of a sling psychrometer by Espy in Philadelphia in 1834. His results, which were not entirely satisfactory, were far ahead of his time, and till quite recently exceeded in accuracy all others since. As is so often the case, they seem to have attracted little or no attention.

Horns of the Prong-Buck (Antilocapra).

THE other evening, while reading an article on the *Artiodactyla*, by Professor Cope, in the *American Naturalist* for December, 1888, I was much surprised at finding the following note: "Antilocapra is sometimes separated from the Bovidæ as the type of a family, because it is said to sometimes shed its horn-sheath. This character, *were it really normal*, has no significance sufficient for the establishment of a family division" (Italics mine).

This doubt as to the shedding of the horn-sheath was so entirely foreign to what I had been led to believe, both by observation and reading, that I took the pains to look over what little literature I possess touching the subject; and, finding it so uniformly in favor of the shedding theory, I write, asking if your readers can give any additional facts in the case.

Owen (*Anatomy of Vertebrates*, London, 1868, vol. iii. pp. 626, 627) gives a description of the shedding of the horns, and growth of new ones, noticed by Mr. Bartlett in the Zoölogical Gardens of London in 1865; also notes of Dr. Canfield at Monterey, Cal., from 1855 to 1857, on a young male in captivity. Dr. Canfield is also quoted: "In the months of December and January I have never killed a buck with large horns; and at that time of the year all the bucks appear to be young ones, because their horns are so small; whereas in the spring and summer months almost all the bucks appear to be old ones, for their horns are then large and noticeable." Dr. Canfield also states that "in the summer months the line of demarcation between the horn and skin from which it grows is very apparent and abrupt; whereas in winter there is no demarcation, the horn being very soft at its base, and passing insensibly into cuticular tissues, and the horny substance being covered thinly with hair."

Gill (*Arrangement of the Families of Mammals*, Washington, 1872, p. 72) says of *Antilocapridæ*: "Horns deciduous, peculiar to the rutting-season (in both sexes), developed as pseudocorneous sheaths, with agglutinated hairs on osseous cores originating from the frontal bones." Gray (*Hand-List of the Edentate, Thick-skinned, and Ruminant Mammals in the British Museum*, London, 1873, p. 135) evidently believes in this shedding, because he places *Antilocapra* under a separate sub-order, *Dicranocera*, instead of merely a separate family. Mivart (*Lessons in Elementary Anatomy*, London, 1883, pp. 245, 246), on ecderonic appendages, says, "and only in an anomalous form, the prong-buck (*Antilocapra*), are these horny structures shed at intervals;" Huxley (*A Manual of the Anatomy of Vertebrated Animals*, New York, 1883, p. 327), "But in the remarkable prong-horned antelope of North America (*Antilocapra*) the horny sheath is annually shed,

and replaced by a newly formed one." Caton (*The Antelope and Deer of America*, 2d ed., New York, 1881 [?]) gives a lengthy description of the shedding process as observed by him upon antelope in captivity; also quotes Audubon and Bachman (*Quadrupeds of America*) as saying, "It was supposed by the hunters of Fort Union that the prong-horned antelope dropped its horns," — a supposition that these naturalists thought they had disproved by merely showing that it had an osseous horn-core. E. R. Alston (*Biologia Centrali-Americana*, 1879–82, p. 112, article "Mammalia") says, "Although the fact that the prong-buck sheds its horns annually was long well known to hunters and backwoodsmen, and had been noted by one or two writers, yet it was generally disbelieved or ignored by zoölogists; and Mr. Bartlett, the observant superintendent of the Zoölogical Society's Gardens, was the first to demonstrate its truth and insist on its importance." Flower (*Encyclopædia Britannica*, 9th ed., p. 431, article "Mammalia") says, "The only existing species [of the *Bovidæ*] in which such a process [shedding] occurs regularly and periodically is the American prong-buck (*Antilocapra*), in which the horns also differ from all others in being bifurcated."

This evidence resolves itself into three separate cases of direct observation on animals in captivity, — the statement by Audubon and Bachman of the belief of the hunters of Fort Union; and the indefinite statement of Mr. Alston, that "the fact that the prong-buck sheds its horns annually was long well known to hunters and backwoodsmen."

My own observations are as follows. I have several times handled skins of this animal from the Western plains, from which the horn-sheath could easily be drawn, exposing to view a partially formed horn beneath. These, I have every reason to believe, were wild animals. I think, at the least, I have examined six or eight such cases; also I have noticed many cases in which the horn-sheath insensibly graded into skin, and was covered with hair for a considerable distance from its base, and many other cases where the demarcation was sharply drawn. Unfortunately I cannot recall at what seasons of the year these animals were killed. Again: in two or three cases have I known of taxidermists, uninformed that the phenomenon was known, coming to an independent conclusion that the antelope sheds its horns.

Now, let us see what the evidence amounts to. The generally accepted belief that confinement effects moultings must be taken into account; but, as far as I am aware, there is nothing in this evidence that would lend any support whatever to the idea that it could produce such a remarkable change as that of a permanently horned *Cavicornia* changing to a deciduous one. However, those antelope kept by Dr. Canfield and Judge Caton can hardly be strictly classed as animals in confinement. That of Dr. Canfield used to go hunting with him as far as twelve miles from home, we are told, and "hunted coyotes with tho dogs at night;" while those of Judge Caton had the run of a large park. Neither can a change of climate or natural food be called in to account for this moult as described by Dr. Canfield, for his buck was living in its native habitat.

The opinion of the hunters of Fort Union is of considerable value. While hunters, Indians, etc., are not good at distinguishing species, yet habits, when well marked, are usually much more familiar to them as a class than to naturalists.

My own observations on prepared skins also point very strongly toward the same conclusion. In no manner did these indicate an abnormal physical state. Those observed in the United States in captivity evidently were healthy; and so good an observer as Mr. Bartlett would hardly have failed to have stated the fact if the specimen under his care was in poor health. That bucks killed in December and January all have short horns, grading insensibly into skin, and with base covered with hair, while those killed in the spring and summer months almost all have large horns, definitely marked off from the skin, can, I think, be explained in no other way than by an annual moult. That such is the almost universal belief of naturalists, my citations tend to show.

As the point is one of considerable interest, I have ventured to take up so much of your space, hoping thereby to call out some original observations from your readers. HENRY L. WARD.

Tacubaya, D.F., Mex., Jan. 10.

Felspar, or Feldspar?

I HAVE read with interest the recent notes in *Science*, Nos. 305, 306, and 309, on the orthography of "felspar" or "feldspar."

Whether the error consists in the omission or in the insertion of the *d*, seems doubtful. But apart from "national prejudice" in the matter, — which, if it exists elsewhere than in the imagination of your correspondent, "J. D. D.," is certainly to be deprecated, — there are, it seems to me, good reasons for defending and adopting the British custom of spelling the word. These are based on probability, common sense, and, last, though not least, appropriateness.

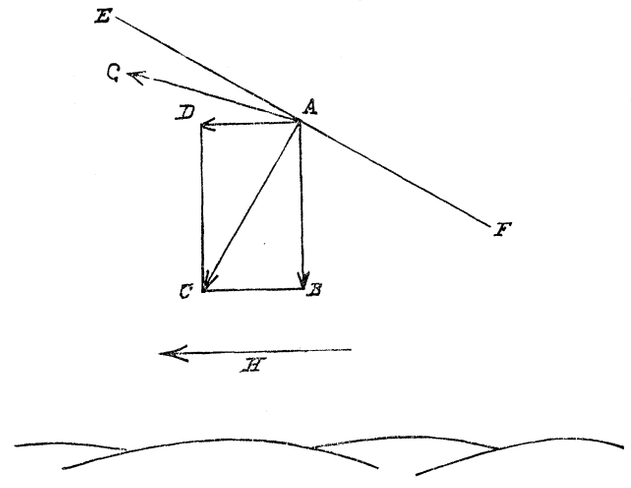
It may, I think, if there is no proof to the contrary, be admitted that the name was originally given by a miner, or a mineralogist, and not by an agriculturist; and, if so, then it is in the highest degree improbable that either the miner or the mineralogist would associate this particular substance with the fields, with which it has no obvious connection, and it is in an equal degree probable that he would associate it with the rocks of which it is one of the chief constituents. In any case, the British custom of referring it to *fels*, or *felsen* ("a rock," "rockspar") is both sensible and appropriate, neither of which can be said of its reference to *feld* or *felt* ("a field," "fieldspar").

On these considerations alone, and not because of "national prejudice," or even custom, I consider it advisable to adhere to the spelling adopted by nearly all English geologists until some better reason than mere assertion, or the custom elsewhere, is advanced for not doing so. ALFRED R. C. SELWYN.

Ottawa, Can., Jan. 14.

The Soaring of Birds.

PROFESSOR PICKERING may possibly have the correct explanation of the soaring of birds; and, if so, will he be kind enough to explain it more strongly, so that the explanation may have the force of a demonstration in geometry? As it now stands, there appears to be a fallacy somewhere.



If the bird is acted on by two forces, *AB* and *AD*, the resultant force would carry him to *C*; and he could by no means get to *G* except by the action of a third force, which might be represented by a line drawn from *C* to *G*. Professor Pickering makes no mention of any such third force, but without it how could the bird get to *G*? WM. KENT.

Passaic, N.J., Jan. 12.

The Color of Katydid.

I OBSERVE in *Science* of Jan. 11 mention of a pink katydid found by L. N. Johnson, Evanston, Ill. A large female specimen was found on my place at Wood's Holl, Mass., as early as 1874, and sent to Professor Packard. Two others have been found at the same place, so that it would seem to be a defined species.

JOS. STORY FAY.

Boston, Mass., Jan. 21.