

altered, the minor axis is least." Miss H. says: "This is equivalent to saying that the mean distance of the earth from the sun is a function of the eccentricity of the earth's orbit, . . ." and then she proceeds to find an expression for this mean distance, first from the standpoint of geology, and, second, from a consideration of the kinematical element of velocity. The result in the first case is that

$$r' \text{ (mean distance)} = a \sqrt{1 - e^2},$$

and in the second case that

$$r' = a \sqrt[4]{1 - e^2}.$$

From what is said in introducing the second case it appears as if the kinematical result were only an "as it were" mean distance, and not the actual average of all the different distances. If this were so, this part of the article would scarcely supply an interlinear reading for the passage from Ball, for it seems evident that he means the real average distance and not a virtual average. The geometrical result should give the real average, but does it—I mean does Miss H.'s geometrical result give it? This makes it equal to the semi-axis minor, but that surely cannot be true. Of course, it is quite true, and, as Miss H. says, it is easily shown that

$$\frac{1}{\pi} \int_0^\pi r d\theta = a \sqrt{1 - e^2},$$

but she does not show how it is shown that the mean distance

$$= \frac{1}{\pi} \int_0^\pi r d\theta.$$

As an assumption it does not seem to be convincingly reasonable.

The assumption made in the kinematical discussion seems much more reasonable. It is that the mean distance is the radius of a circle, in the circumference of which a point travels with the same areal velocity as that of the earth in its orbit. If the idea of velocity be dropped, we shall get back from kinematics into geometry, and the same assumption will give us for mean distance the radius of a circle whose area is equal to that of the given ellipse.

$$\text{Thus} \quad \pi r_0^2 = \pi a b$$

$$\text{and} \quad \therefore r_0 = \sqrt{a b} = a \sqrt[4]{1 - e^2}.$$

This is the same as Miss H.'s kinematical result, and, like it, agrees with the dynamical result in her equation (4).

ALICE PORTER.

Yarmouth, N.S., May 15.

### A Beautiful Spectacle.

I GIVE below a description of a phenomenon seen here on the evening of May 9 and wish you or some of your readers could tell me if it is rare or common, and what is the cause or its relation to other phenomena

On Tuesday evening, May 9, between 9.15 and 9.45 (north latitude 44°, west longitude 66°, but time is 60°), we were treated to a curious and beautiful spectacle. Right across the sky from west to east stretched a magnificent arch of luminous radiance. On the west it seemed to spring from a solid mass of black cloud which extended along the whole northwest horizon. Its width was nearly uniform from the western base up beyond the summit, and measured about two degrees. The summit was among the stars of Berenice's Hair, and was 15 to 20 degrees south of the zenith. The eastern branch narrowed as it neared the horizon, and tapered off to a point before quite completing the semi-circle. The color was fairly uniform throughout, and of a grayish or pale-bluish white, some say "yellowish." Except for the cloud-mass in the northwest the sky was beautifully clear, and the brighter stars along each side of the arch seemed to shine out with unusual brilliancy and sparkle. Those covered by the arch were not obscured, but twinkled through it as through a transparent veil. To some observers the summit seemed for a time to move very slowly a little farther south, and near the time of breaking up there were narrow, dark rifts crossing it obliquely; but, on the whole, the entire structure stood remarkably steady,

without any of the swaying, or shooting, or shimmering, or wavering motion generally seen in auroras. There had been some auroral outbursts about half an hour earlier, and this phenomenon was probably connected with them. Whatever it was due to, it was a splendid sight—such a sight as the rings of Saturn must be as seen from the surface of that planet—and it was much admired by all who saw it. It broke up and melted away before 10, and in another quarter of an hour the sky was clouded all over.

ALICE PORTER.

Yarmouth, N.S., May 12.

### A Fall of Colored Snow.

ON Jan. 8 1892, between one and five o'clock P.M., there fell about one inch of colored snow throughout the northern half of La Porte County, Ind.

Mixed with the snow was a large percentage of mineral and vegetable matter giving the snow a reddish-brown hue. Every flake of snow had a particle of this matter, that served as its nucleus, from which the mass became granular. The mass was moist enough to form a crust within twelve hours.

At the time it fell there were six inches of clean snow very evenly distributed over the surface, probably not any surface bare within fifty miles of the above-named area. This old snow was quite compact.

During the next twenty-four hours following the fall of colored snow about four inches of clean snow fell on top of it, and became a crust within a few days, thus embedding the colored snow between two compact strata of ordinary snow, by which it was kept free from contamination for about a month. During that time several persons procured samples of it for examination.

The meteorological conditions at the time of its fall were: Wind from west-southwest; all clouds moved in same direction. Temperature about zero at 8 A.M. Jan. 8, 12 to 3 P.M., rising; 8 P.M., zero. Thermometer stood at zero Jan. 9. At Chicago from 4 until 4 30 there was light snow, too light to measure. At Grand Haven, Mich., it snowed almost continuously from Jan. 5 to 10; and on Jan. 8, thermometer fell from 18 to 8 above zero (the coldest of the season); while at Chicago it went down as low as 5 below from 12 above zero. That station reports a high-pressure area for the whole northwest country, weather cold and clear. This area closely followed an area of low-pressure, which was central over Upper Lake Michigan during the morning of Jan. 8, moving rapidly northwestward during the succeeding twenty-four hours, general snow marking its passage. The Chicago observing station records wind from west to northwest Jan. 8-9.

Having had my attention called to some of these facts by an article in a local newspaper by Honorable G. H. Teeter of Rolling Prairie, Ind., I began to collect samples, and procured one from that gentleman. I sought to make a survey of the area covered by its fall, but was unable to locate bounds in any direction, although I traced it over an area 25 by 45 miles.

To avoid uncertainty in an analysis of the matter, I drove several miles into the country with Professor F. M. Watters, then science teacher in La Porte High School, to procure samples of it that should not be affected by dust from chimneys and railroads.

I made three analyses of it, besides carefully examining it under the microscope, using both low and high powers. Meanwhile, Mr. Teeter procured an analysis by Professor H. A. Huston, chemist of Indiana Agricultural Experiment Station, at Purdue University, Lafayette, Ind., as follows:—

"Loss on ignition (water and other volatile matter)	15.04
Silica.....	65.64
Alumina and oxide of iron.....	15.50
Lime.....	2.19
Magnesia.....	1.38
Phosphoric anhydride.....	.10
Oxide of titanium and undetermined.....	.15
Total.....	100.00 "

Professor Huston adds: "The composition of the material is such that one is led to believe it to be of volcanic origin, as it approximates very closely to some of the analyses of lava from the

Pacific islands, and from Iceland. I am, therefore, inclined to believe it is a volcanic product."

My analyses approximated Professor Huston's very closely, though I found mica scales, a trace of sulphur, in one analysis, with nearly one per cent vegetable matter in my first sample analyzed (this one procured from Mr. Teeter).

In precipitating the matter by melting the snow, the heavier portions fell to the bottom, and unless care was used the larger portion of the vegetable matter would be lost through being poured off. I found the coarser grains of silica (white sand) to be water-worn and scratched. Lime particles adhered to the sand-grains, just as one finds on the shores of lakes or rivers. Of the vegetable matter, I found the seed of a wild pea (*Lathyrus ochroleucus*), growing abundantly all over the Northwest. This seed, to make sure of no mistake I planted and grew the vine to maturity. Among wood-fibres identified were poplar (*Populus tremuloides*), pine (*Pinus strobus*), and casex (*Casex tenella*).

Now as to the source of this matter. It is plainly terrestrial; and, as the whole area traversed by the winds that carried it were covered with snow at the time, it is evident that it could not have been raised east of Lake Michigan. Its constituent elements preclude all possibility of its being meteoric or volcanic matter.

The fact that the sample analyzed by Professor Huston closely approximates certain volcanic samples can easily be accounted for on the ground that the precipitated mass was not homogeneous, and what was sent him could only have represented a portion of the mass, as another portion of it, sent to me by Mr. Teeter, out of the same lot, contained one per cent of vegetable matter, mica scales, and three small copper pyrites (yielding sulphur on ignition).

Every element of this matter is met with in abundance throughout all portions of the Northwest, and nowhere else do we find all of them on the surface. I conclude that this volume of matter must have been raised somewhere northwest, being carried south-eastward until it encountered the area of high-pressure that extended north of Chicago, and deflected in its course and fell within the area mentioned above.

Can anyone throw more light on the subject?

A. N. SOMERS.

La Porte, Ind., March 21.

### The Aurora.

I HAD thought that no matter what Professor Ashe might say in regard to my note printed in *Science* for April 28, I would refrain from further comment. Inasmuch, however, as he in effect demands that something further be said, as appears in the closing paragraph of his note printed in *Science* for May 19, p. 277, I presume that I have no option but to comply. The point to which he asks special attention is as to the element of "chance" affecting the conclusions at which I have arrived respecting the location upon the sun of the seat of the activities originating the aurora in any given instance. The manner in which he puts this inquiry, as well as the general drift of his criticism, shows that I have failed to make myself understood in spite of very persistent efforts in the various notes and papers which he mentions, and which certainly, therefore, must have been taken into consideration in the comments made in the letter above mentioned. This being the case it will be necessary to begin at the beginning and state the heads of the argument by which my conclusions have been reached, so that if there is any flaw in the reasoning its precise location may appear and so that it may be explained also once again what are the precise conclusions for which I have been contending. The substance of the argument, stated in a few propositions as briefly as possible, is as follows: The agreement between the curves, representing the frequency of auroras, magnetic storms and sunspots is exact, and the nature of these phenomena is such that there can be no doubt whatever that the aurora owes its origin to a special form of solar activity. This proposition can be controverted successfully only by denying that there is such agreement as is claimed of the curves mentioned, or by advancing some alternative explanation of their connection with each other which will leave solar activities out of the question. Until this is done, this proposition

must stand, the evidence in its favor being adequate and there being no evidence pointing in a different direction. The solar origin of the aurora being thus established, its manifest periodicity at intervals of  $27\frac{1}{4}$  days must be explained in accordance with its solar origin. If this can be done, the proof of such origin will incidentally be greatly strengthened. Now this period is totally indistinguishable from that of a synodic revolution of the sun—giving every evidence of being absolutely the same. This being the case we are able to formulate proposition number two to the effect that there is a periodicity of the aurora corresponding to the time of the rotation of the sun as seen from the earth. Here again the evidence is adequate and there is no evidence pointing to any other possible explanation. These two propositions being established there follows another, from which there is in the very nature of the case no possibility whatever of escape, and which is to the effect that whatever it is upon the sun which is capable of producing the aurora, it has this power during a very limited portion only of each revolution, which portion always remains the same during succeeding revolutions relative to the position of the earth in its orbit, otherwise the periodicity described could not exist. It remains only to identify the point whence the auroral effect proceeds. The period of auroral recurrence and that required for the completion of a synodic revolution of the sun as determined from the average rate of motion of spots being identical, there is no other way than to study the appearance of the sun at times of auroral recurrence in order to learn whether such recurrence is attended by any characteristic features. Thus it is found that no matter what appears elsewhere on the sun at such times there are always at the eastern limb areas on which spots are frequent and persistent. Thus the evidence is adequate that there must be something in that location in such cases which is responsible both for the sunspots and the aurora, and there is no evidence pointing in any different direction. On the contrary, the manner in which magnetic storms begin and the exactness of the periodicity manifest in their times of beginning are such as are totally inconsistent with any other explanation than that the originating impulse is brought to bear by coming into range suddenly around the sun's limb. But be this as it may, such behavior corresponds precisely with what is known in regard to the operation of electro-magnetic induction in which very precise arrangements of lines of force and development of poles in certain directions in the case of rotating bodies, or otherwise, are the rule, and there is no correspondence whatever to the mode of action of any other force of which we have knowledge. Thus at no point throughout the research, as above outlined, has there appeared to be even the slightest "chance" for an alternative hypothesis. The evidence in favor of each proposition stated has been adequate and all in one direction, and moreover, taken together it is cumulative; each point strengthening the others and nowhere developing any inconsistencies. Professor Ashe is mistaken in stating that there has been "no attempted refutation." I have letters and articles by the score from persons who started in with vehemence, some of them many years ago, but who have gradually become very respectful, finally being brought to a realizing sense, that it is facts and not a personality against which they had been contending.

M. A. VEEDER.

### Worms in the Brain of a Bird.

APRIL 7th, 1890, two common Bitterns (*Boturus mugitans*) were brought to me to be mounted. One of them was still alive but did not seem to be just natural, seemed to lack what we might call bird intelligence, and was smaller than the other and poor in flesh. This bird was given to one of my pupils in taxidermy, Miss Bernice Pike, to mount. When the head had been skinned and was ready to sever from the neck, which was done by cutting through the skull, the brain-cavity was found to contain a mass of thread-worms, occupying about one-third of the brain cavity. These were seemingly like the ordinary Gordius or Hair Snake, about the size of a Gordius that is three inches long, and coiled in a mass in the upper posterior part of the brain, and extending some down into the spinal canal. As near as I could say without removing them, they occupied the subarachnoid space,