annual appropriations of about \$75,000. I know him to be an indomitable worker in the field, and well remember the day when his annual arrival in our department was hailed with the greatest interest, and was the signal for every possible act of kindness and assistance from one end of the command to the other.

Prof. Geikie's quotation, that the presence of an armed escort needlessly irritated the hostile Indians, is out of the pale of decent characterisation. In those days army men were of the opinion that no party having less than 100 long-range breech-loading rifles could safely pass into certain portions of the Sioux country, and that minimum-sized parties could just about take care of themselves on the defensive. That was the basis upon which my party was organised. That there was some error in this judgment was shown by the Custer massacre not long after. To the other portion of the quotation, that the geologists of the Interior Department were never molested by Indians, I will state, from personal knowledge, that they have always taken the most precious care not to operate in the neighbourhood of dangerous Indians, a very sensible proceeding for parties without armed escort. Through carelessness and lack of knowledge of the system of guarding camps, they have been stampeded once or twice by thieving Indians who were after plunder, but did not dare to kill anybody. This misfortune has recently befallen them in Yellowstone Park, where the commonest precautions would have made it impossible. Their presence alone, without armed escort, seems to have been "irritating to the Indians."

W. A. JONES

Geological Climate and Geological Time

I HAVE been much interested in Prof. Haughton's communication to NATURE, vol. xviii. p. 266, on the subject of geological climate and geological time. I fully agree with him that geological climates cannot be explained by any change in the position of the poles, even supposing such change possible, and for the reason assigned by him, viz., that we have no palæontological evidence of an arctic climate in any portion of the earth in any geological period previous to the glacial. But I have some objections to make to the data on which he bases his estimates of time, and therefore to his views as to the cause of geological climates.

I. He supposes aqueous agencies to commence operation, and therefore the archæan (azoic) era to commence when the earth surface had cooled to 212° F., evidently because, as he thinks, water could not exist as a liquid on the earth's surface at a higher temperature. But the writer forgets that with all the water of the ocean in the air as vapour, and the large quantity of carbonic acid now existing in the form of carbon and the carbonates also in the air, the pressure of the primeval atmosphere must have been many times—perhaps several hundred times—greater than now, and the boiling, or rather precipitating, point of water very much higher than 212°.

II. Again: Even with a surface temperature from internal causes of 212°, the crust of the earth must have been *very thin*, not more than 40–50 ft. (for increased atmospheric pressure, though greatly affecting the boiling-point of water, would not sensibly affect the fusing-point of rocks). Under these conditions—by the law of equilibrium—the inequalities constituting land-surfaces and ocean-bottoms could not possibly exist; the ocean would be universal, and therefore there would be no erosion and sedimentation. Therefore when dry land first appeared and erosive agencies commenced to act at the beginning of the azoic era, the surface temperature from internal causes must have been much less than 212°. For my part I believe that this temperature had already become very small, the surface had become substantially cool, and the crust very thick before land could exist, and [the history recorded in stratified rocks could commence.

III. Therefore, though I agree with Prof. Haughton that all the evidence we have indicates uniform climates in early geological times, I would not, like him, attribute this to warm decrease of surface temperature from internal causes alone. I would attribute it almost wholly to external causes. Among these are:—1. The constitution of the atmosphere. The greater amount of carbonic acid and water in the atmosphere would shut in and accumulate the sun's heat on the earth surface according to the principle discovered by Tyndall, and applied to the explanation of geological climates by Sterry Hunt. 2. It is probable that the heat received from the sun was much greater

then than now; for the sun is now cooling, and has been cooling throughout all geological times much faster than the earth. 3. The idea of Poisson, that in the journey of our system through the stellar universe it may be now in a region in which the heat received from space is exceptionally small, has been, perhaps, too much neglected in these speculations concerning geological climates. 4. The more uniform distribution of this greater surface-temperature from any or all these causes, would of course still farther increase the temperature of high latitudes. This more uniform distribution might be due to the position and shape of land masses, or to the less area and the less height of the then lands.

IV. Lastly: I think that a little reflection will show that while it may be allowable to roughly estimate the relative lengths of different eras by the relative extreme thickness of their strata; it will not at all do to estimate the absolute length of geological times by the extreme thickness of all the strata. For as the measuring rod is not the rate at which sediments are now accumulating at any one place, but the average rate over the whole bottom of the sea, so the thing to be measured is not the extreme thickness of the strata at any one place, much less the extreme thicknesses of different formations in different places piled one atop the other, but the average thickness of the strata over the whole earth surface.

Most of the points brought out here have already been discussed by me in my recently published "Elements of Geology."

JOSEPH LE CONTE

University of California, October 2

The Magnetic Storm of May 14, 1878

I WAS much interested in Mr. Perry's note inserted in NATURE, vol. xviii. p. 617, showing the simultaneity of the magnetic storm of May 14, 1878, in different parts of the earth's surface. As these magnetic disturbances are always accompanied by electric disturbances (earth currents) in telegraph wires, I was anxious to find out what effects were observed upon our wires in England on the same day, viz., May 14. I append an extract from the diary of the Relay Station at Haverfordwest, a very important station on the wires running from London to Valentia, and where very careful observations are made of all interferences with the regularity of the working of the wires:—

May 14.

P.M.

6.40.—First appearance of relays not closing automatic switch well on down side of 202.

7.10.—London finds some difficulty in reading on 199; no cause visible here.

7.40.—Up side of automatic switch on 202 rather unsteady.

9.0.—Variations in London's current on 200.

9.10.—Strong positive deflections (earth currents) on Cork wires.

9.15.—Cork complains of marks missing and running; no doubt the result of deflections.

9.30.—Great difficulty with automatic switches on Cork wires owing to continuous strong positive deflections which tend to opening of switches when battery currents are in opposite directions, and to close them when in same direction. Probably earth-currents influenced them earlier in evening.

9.40.—Deflections diminishing; wires going better.

9.45.—Deflections ceased.

10.0.—All wires going well; weather wet.

10.10.—Aurora Borealis visible about this time.

11.25.—Earth currents very strong again on all wires, causing much trouble with automatic switches. Almost full deflection on up side of 201 and 202.

10.40.—Deflection on 202 reversed, and not so strong as before.

10.45.—Full deflection on up side of 202.

10.48.—Deflection on 199 causes up automatic switch to remain depressed.

MIDN.—Earth currents disappearing.

May 15.

A.M.

12.40.—Electric storm seems to have spent itself. Weather fine; clear moonlit sky.

May 15.

A.M.

1.15.—Weather wet. High winds coming on.

7.0. —Nearly six hours' rain and wind.

The numbers 199, 200, 201, and 202 are those of the wires.

Mr. Ellis has also told us (p. 641) of what was observed at Greenwich. Although the magnetic disturbance commenced about 6 P.M. at Greenwich, it only reached a maximum at 9 P.M., when the electric disturbances were observed at Haverfordwest, though indifferent working, most probably due to them, commenced at 6.40. At 11.45 P.M. the north end of the needle at Greenwich moved sharply westward, and about the same time the currents were reversed at Haverfordwest. From midnight to 12.40 A.M. the currents gradually disappeared. Earth currents were also active at Greenwich, as indeed they were everywhere. Unfortunately the wires at the post-office were so fully occupied with press work during these hours, that no observations could be taken. It will be noticed that the aurora borealis was observed.

W. H. PREECE

October 21

Giddiness

A TRUE theory of the cause of giddiness ought to explain the following extreme experience, which deserves record in print. The method was first told to me by a friend; I once tried it myself successfully in a mitigated form, and will assuredly not repeat the experiment, and I persuaded a philosophical friend to try it also, with much the same result. Stand in the middle of a soft field where you can't hurt yourself by tumbling on the ground or against anything; avoid having your best clothes on, and secure appreciative spectators. Then put both hands one above the other on the top of your umbrella or walking-stick, and bend down until your forehead rests on the back of your hands. Thus your legs will be vertical, your body will be more or less horizontal, and the umbrella will be vertical. Shut your eyes. Then get a friend, by touching your hips, so to guide you that you shall circle three times, with a sidelong gait, round the vertical umbrella as an axis. Finally raise your head quickly, and try to walk straight as though nothing particular had happened. What will occur is a frightful giddiness and feeling of sickness, a sense of the ground rising up tumultuously on all sides, a wild rush to save yourself, and a headlong tumble.

F. G.

OUR ASTRONOMICAL COLUMN

THE SATURNIAN SATELLITE HYPERION.—In a letter from Prof. Asaph Hall it is remarked that the ephemeris for September last, given in NATURE, requires correction by nearly two days, although it was deduced from the elements which he showed to closely represent the Washington observations in 1875 and 1876 (*Astron. Nach.*, No. 2,137). There appear to be great difficulties attending the satisfactory determination of the orbit of this satellite, doubtless arising in the main from the magnitude of the perturbations with which its motion is affected, but for this reason it is the more necessary that it should be regularly observed, and a rough indication of the position of so extremely faint an object is better than none.

If we take for a peri-saturnium passage September 24.8393 G.M.T. with Prof. Hall's other elements, the calculated and observed distances on September 27 agree, and the computed angle is one degree in excess of observation. On the same system the following angles and distances are found for 10h. G.M.T.:—

	Angle.	Distance.		Angle.	Distance.
Oct. 25 ...	271 ...	137	Nov. 1 ...	95 ...	217
" 26 ...	268 ...	82	" 2 ...	94 ...	225
" 27 ...	250 ...	23	" 3 ...	94 ...	211
" 28 ...	107 ...	41	" 4 ...	93 ...	175
" 29 ...	99 ...	99	" 5 ...	92 ...	119
" 30 ...	97 ...	150	" 6 ...	87 ...	49
" 31 ...	96 ...	191	" 7 ...	287 ...	29

THE MEAN PARALLAX OF A STAR OF FIRST MAGNITUDE.—Prof. Gylden, director of the Observatory at Stockholm, has reported the result of a preliminary calculation bearing upon the mean distance of a star of the first magnitude. Remarking that in the actual state of our knowledge, when a general result is to be deduced from the parallaxes so far measured, we must not only take into account the apparent brightness of the stars concerned, but also their apparent proper motions, since the magnitude of the proper motion is to be viewed as at least as sure a criterion of a measurable parallax as the apparent brightness; as a hypothesis, it is then assumed that the actual parallax p of a star of n th magnitude with the apparent motion s , will be given by the formula

$$p = P \frac{s}{\sigma_n M_n}$$

where σ_n signifies the mean apparent motion of a star of n th magnitude and M_n the distance estimated according to its brightness. P is a constant, which for $M = 1$ indicates the mean parallax of a star of the first magnitude.

Prof. Gylden takes the following data, depending upon observation, for sixteen stars, of which the parallaxes are supposed to be known with the greatest degree of approximation:—

	mag.	p	s
α Centauri ...	1 ...	0".900 ...	3".674
δ Cygni ...	5 ...	0".511 ...	5".221
L. 21185 ...	7 ...	0".501 ...	4".734
34 Groombr. ...	8 ...	0".307 ...	2".801
L. 21258 ...	8.5 ...	0".260 ...	4".403
Oelt. 17415 ...	9.5 ...	0".247 ...	1".200
σ Draconis ...	5 ...	0".222 ...	1".925
Sirius ...	1 ...	0".193 ...	1".252
γ Ophiuchi ...	4 ...	0".162 ...	1".108
α Lyrae ...	1 ...	0".153 ...	0".349
1830 Gr. ...	7 ...	0".147 ...	7".053
ϵ Urs. maj. ...	3 ...	0".133 ...	0".525
α Bootis ...	1 ...	0".127 ...	2".258
γ Draconis ...	2 ...	0".092 ...	0".063
α Aurigae ...	1 ...	0".046 ...	0".438
α Urs. min. ...	2 ...	0".046 ...	0".045

From the adopted values of p , s , together with the products $\sigma_n M_n$, 16 equations of condition can be formed, the solution of which by the method of least squares will furnish the value of P . But Prof. Gylden points out that this mode of treatment will not be found to answer the object in view, since the determination of the weights of the different equations, which can in no wise be considered equal, is attended with great difficulties. On the assumption of equal weight, the value of P comes out, 0".048. As another mode of treatment, normal equations may be formed in various ways, each containing the unknown quantity P , and consequently each serving for a determination of the quantity sought. The sum of all the equations thus obtained gave $P = 0".062$.

This value, however, is greatly influenced by several stars of the first magnitude with large proper motion. Omitting α Centauri, α Bootis, and Sirius, the remainder give $P = 0".086$, or if all stars of the first magnitude are omitted, $P = 0".083$. Again, if all stars with extreme motions are neglected, and a value of P derived from the nine stars which remain, with proper motions less than 2" annually, it is found to be 0".084. Prof. Gylden considers that the near agreement of values obtained from these two calculations, in which the extreme case of brightness and motion enter, affords some support to the inference that the relation indicated by the above formula between parallax, apparent motion, and apparent brightness may be taken as an approximation to the truth. It may be remarked that Prof. Peters found for the mean parallax of a first-magnitude star, 0".102 $\pm$ 0".026; the new value is within the limit of his probable error.