TABLE VI.—COMPARATIVE ABSTRACT for 1872, and previous Years.

STATIONS.	Barometer.		Temperature from Self-registering Instruments read in Morning for Twenty-four hours previously.					Computed from Observations.		Rain.		Wind.		Cloud.
	Mean Reading.	Extreme Range.	Mean Temp. in Shade.	Mean Daily Range of Temp.	Extreme Range of Temp.	Max. Temp. in Sun's Rays.	Min. Temp. on Grass.	Mean Elastic Force of Vapour.	Mean Deg. of Moisture. (Saturation=100)	Total Fall in Inches.	No. of Days on which Rain fell.	Average daily force in Miles for Year.	Maximum Velocity in Miles in any 24 hours, and date.	Mean Amount (0 to 10).
NORTH ISLAND.														
Mongonui*	30·024	1·407	62·1	14·9	56·0	150·0	—	—	—	45·330	170	169	580, Feb. 21	5·3
Previous years	29·928	—	57·9	—	—	—	—	—	—	54·520	152	—	—	—
Auckland	30·024	1·404	60·2	13·9	56·4	157·0	13·0	·428	80	42·096	186	291	876, Sept. 8	6·3
Previous years	29·843	—	60·1	—	—	—	—	·413	72	44·333	183	—	—	—
Taranaki	30·001	1·429	58·4	17·0	52·0	153·0	22·0	·305	62	63·640	158	§—	—	6·7
Previous years	29·909	—	57·5	—	—	—	—	·405	76	53·484	157	—	—	—
Napier	29·980	1·420	59·7	17·0	64·0	147·0	—	·389	74	23·940	108	‡234	700, Mr. 3, May 15, Oct. 7 & 8	2·0
Previous years	29·876	—	57·2	—	—	—	—	·381	72	36·297	74	—		—
Wanganui	30·087	1·520	56·7	16·6	58·0	148·0	10·0	·333	71	38·120	135	256	692, Mar. 2	4·8
Wellington	29·934	1·629	55·8	11·6	51·5	145·0	21·0	·351	79	50·945	165	204	800, Apl. 20	5·6
Previous years	29·872	—	55·6	—	—	—	—	·312	68	46·728	143	—	—	—
SOUTH ISLAND.														
Nelson	29·919	1·518	56·7	20·9	65·0	185·0	15·0	·369	79	78·610	102	§—	—	6·1
Previous years	29·992	—	55·3	—	—	—	—	·385	74	62·634	84	—	—	—
Christchurch	29·915	1·702	53·6	15·0	74·2	160·2	5·2	·333	79	19·741	114	§—	—	5·6
Previous years	29·843	—	52·8	—	—	—	—	·328	76	25·160	107	—	—	—
Bealey†	29·798	1·433	48·0	15 9	70·0	142·6	−16·0	·291	85	97·130	170	§—	—	5·4
Previous years	29·852	—	46·1	—	—	—	—	·262	82	126·018	214	—	—	—
Hokitika	29·944	1·543	54·1	12·0	55·0	103·0	17·5	·361	85	123·210	179	146	403, July 16	4·8
Previous years	29·925	—	51·7	—	—	—	—	·376	90	119·403	206	—	—	—
Dunedin	29·819	1·351	51·4	14·7	61·0	184·0	20·0	·293	76	27·393	132	168	590, July 7, & Sept. 28	5·8
Previous years	29·902	—	50·8	—	—	—	—	·274	71	34·506	181	—		—
Queenstown	—	—	51·4	16·7	61·7	156·2	—	·257	67	28·880	117	139	273, April 20	5·3
Southland*	29·840	1·631	49·6	18·7	68·0	162·2	14·0	·275	77	40·110	144	191	621, Nov. 30	5·7
Previous years	29·768	—	49·8	—	—	—	—	·262	73	49·686	168	—	—	—

* For 11 months; no returns for December received. † 2,104 feet above sea level. ‡ For 10 months only.

§ Instruments out of order; observations incomplete.

Notes on the Weather during 1872.

January.—The drought in all the eastern parts of the colony, which commenced in the North in November, and in other parts in December, continued throughout this month, accompanied by very still weather, and atmospheric pressure above the average. The heat has been very seriously felt, the temperature registered both in the shade and sun being greatly in excess of any previous records.

February.—Rainfall and temperature in excess; winds generally moderate, but a strong S.E. to N.E. gale was felt at all stations from Nelson northward. Destructive floods at Greymouth on 8th.

March.—Weather on the whole remarkably fine throughout the Colony. Rainfall considerably below the average, and no storms of any note. Very high barometer readings were recorded at nearly all the stations on 21st and 22nd. Auroras in extreme south on 2nd.

April.—This month was remarkable for the unusual amount of rain that fell throughout the colony. There were no very severe gales except at Wellington, where on the 18th and 19th a heavy S.W. storm with excessive rainfall occurred, doing much damage. The atmospheric pressure was low throughout, and temperature higher than usual.

May.—Wet and stormy generally during this month, with prevailing westerly winds. Severe thunder-storm on 1st in North with rain; thunder also in the South towards end of month; the temperature higher than usual; snow and hail in South. At times weather pleasant.

June.—Very cold, severe weather throughout, with much snow, hail, and rain; unusually severe frosts and snow-storms in the South. In Southland snow continued from 10th for five days, fifteen inches deep, wind W., and low barometer.

July.—Stormy, wet, and severe weather generally throughout, with frequent thunder, hail, and snow, and heavy falls of rain. At Nelson and Hokitika, rain considerably above the average.

August.—Temperature below the average throughout, generally fine in North, and rain moderate; in the South very severe cold weather, especially in early part of month, when a heavy snow-storm occurred, also heavy rain and frequent thunder-storms. Aurora observed in North on 9th.

September.—Very fine weather throughout for time of year; no severe storms recorded, and unusually small rainfall; very high atmospheric pressure occurred about the middle of month with fine clear weather. Meteor reported at Christchurch on 14th.

October. — Weather generally fine and seasonable; rainfall about the average. Frequent auroras observed in the South. Meteor seen at Auckland on 3rd, very brilliant.

November.—Exceedingly dry hot weather throughout the colony, and in many places the drought was severely felt. No gales of any note occurred. At times there were cool, pleasant breezes. Very high atmospheric pressure at nearly all the stations on 20th, which continued for a few days and gradually fell; very fine weather at this period. Aurora in South on 24th; meteor seen at Wellington and Nelson on 28th, and at Queenstown on 22nd.

December.—Temperature about 5° above the usual average, accompanied by excessive drought at all stations except Hokitika and Bealey, where rain was in excess. Atmospheric pressure above the average, and especially high from 6th to 8th all over the colony. Maximum temperature in shade 92·3° at Christchurch, on the 25th. The range of temperature between night and day unusually great.

James Hector,
Inspector of Meteorological Stations.

Lecture on the Formation of Mountains. By Captain HUTTON, F.G.S., C.M.Z.S.

[*Substance of a lecture delivered in the Colonial Museum, Wellington, 13th November,* 1872.]

"We must never forget that it is principles, and not phenomena—the interpretation, not the mere knowledge of facts—which are the objects of inquiry to the natural philosopher."—SIR J. HERSCHEL.

THE formation of mountains does not very well describe the subject on which I propose to lecture to night, for, strictly speaking, mountains are formed by rain and snow sculpturing and grooving what would otherwise have been table lands, or the highest portions of the undulations of the earth's surface; but on this subject I do not mean to touch. I propose to deal with the undulations themselves, out of which mountains are carved by the rain.

It is well known that the solid surface of the globe is uneven and undulating, that the lower portions are covered by the ocean, while the higher are called the land, and it has also been proved, by observations extending over nearly a century, that these undulations have changed in form and position over and over again, and that changes are still going on. That the solid surface of the earth should heave and quiver, and sway up and down, is one of the most extraordinary phenomena of nature with which science has made us acquainted, and it is one which has never yet received a satisfactory explanation. I hope, however, to be able to show you that it is but the necessary effect of causes which we know from observation to be constantly going on on the surface, combined with the conduction outwards of the interior heat of the earth.

In order to make what I have to say quite clear to you, I must first briefly refer to some general considerations on the interior of the earth. Fortunately, it will not be necessary for me to enter into the hotly disputed question as to whether it is fluid or solid, for this is immaterial to the views that I have to advance; all that is necessary being that the interior is very hot. This is allowed, I believe, by all scientific men, the proof resting principally on the facts that we know from observations, wherever they have been made, that the temperature actually does rise as we descend, at an average rate of about 1° Fahr. for every fifty feet, and that the density of the earth is so small, not much more than twice that of the ordinary rocks of the surface, that there must be some expansive force in the interior sufficiently powerful to balance in a great measure the enormous pressure to which the interior of the earth would be subjected. Assuming then that the interior of the earth is intensely heated, and that the temperature, for a depth say of fifty miles from the surface, increases at the rate of 1° Fahr. for each fifty feet, it necessarily follows that the outer shell, or "crust" as it is commonly called, to a depth of somewhere about thirty-five miles has a temperature below the melting point of ordinary rocks at the surface, while all below this depth has a temperature above its melting point at the surface. Consequently, we have an outer crust in which the attraction of cohesion among the molecules is greater than the repulsion caused by heat, surrounding a nucleus in which the repulsion caused by heat among the molecules is greater than the attraction of cohesion.

The outer crust must therefore be more or less rigid, while the superheated interior must be in such a state that if the pressure that keeps it in its place is decreased at one point it will expand, and this expansion will permeate through the whole mass until the pressure is again equally distributed throughout. Conversely, if the pressure is increased on any point, this pressure will affect the whole mass and distribute itself evenly through it. Of course I need hardly say that the rigid state of the crust is not separated from the superheated state of the interior by a marked division, but the one passes imperceptibly into the other. Now each portion of this rigid crust must be maintained in its place by three forces, viz.—its weight, the lateral thrust of the arch, and the outward pressure of the superheated interior. While these three forces remain constant equilibrium will be maintained, and no movements will occur on the surface. But if one or more of these forces change in amount, the equilibrium will be subverted and movements of the surface will take place. If also the equilibrium be disturbed at one place, it follows, from what I have said about the distribution of pressure in the superheated interior, that the equilibrium will also be disturbed in all surrounding areas. If, for instance, an upheaval of the crust should take place at any point, the underlying superheated rocks, being thus relieved from the pressure above them, would expand and rise up, and fill the hollow; but this expansion would spread through the mass, and would therefore lessen the outward pressure of the interior in all the surrounding areas, which would consequently subside, and equilibrium would only be once more restored when the mass of the subsided areas equalled the mass of the elevated area. Consequently elevation implies subsidence, and *vice versâ*. Where now must we look for the causes that are in operation to disturb this equilibrium? The most obvious is the radiation of heat into space by the earth, and the consequent cooling and contraction of the superheated interior. This is at present almost universally accepted by geologists as the cause of the movements of the surface and the upheaval of mountain chains, but many arguments have been urged against it, and although I am willing to allow that it must have some effect in producing movements, these effects are, I think, completely absorbed by the much larger ones that flow from causes that I shall presently describe; and it is quite impossible that it can be the only cause of movement, partly because some effect must be produced by the other causes that I have yet to describe, and partly because since the glacial epoch the earth has been warming instead of cooling, and consequently no contraction can have taken place since then, while we know not only that extensive movements have taken place, but that they are still taking place on the surface of the globe.

The other cause of disturbance of the equilibrium, to which I have alluded, is the removal of matter from one portion of the earth by running water and its deposition on another portion. It is now nearly forty years ago since Mr. C. Babbage, in his celebrated paper, read before the Geological Society of London,* on the temple of Jupiter Serapis, proposed a theory to account for oscillations of the surface of the earth, which he called the theory of "the change of isothermal surfaces." At about the same time, Sir J. Herschel, in a letter to Sir C. Lyell,† proposed to account for the same phenomena by a theory which he called "the alteration of the incidence of pressure." Both these theories are founded on the same fact, viz., the removal of matter from one portion of the earth's surface and its deposition on another; but while Mr. Babbage laid the most stress on the changes of internal temperature that would be thus brought about, Sir J. Herschel laid the most stress on the change of direct pressure, or weight. These theories have never been taken up by geologists, but I hope to be able to show to you that, when combined, they are capable of explaining all, or nearly all, of the observed phenomena. I have already told you that, owing to its internal heat, the mean temperature of the earth increases as we descend into it at the rate of about 1° Fahr. for every fifty feet. If, therefore, the mean temperature of the surface at any place was 50° Fahr. the mean temperature 100 feet below would be 52° Fahr. If now the surface was covered up by a deposit of clay or sand 100 feet thick, and if its surface retained the same mean temperature as the old one, viz., 50° Fahr., the mean temperature of the old surface would be raised 2°, or to 52°, while at 100 feet below it would be 54°, and so on, so that the covering of the surface by a deposit 100 feet thick would raise the temperature of the whole underlying rocks 2°. If the deposit was thicker, the temperature would of course be more raised in proportion. Now we know that rocks expand on being heated‡ and contract on being cooled, and Colonel Totten and Mr. Adie have shown that this expansion for each degree of temperature is from $\frac{1}{25,000}$ to $\frac{1}{10,000}$ of the whole, according to the nature of the rock. If, however, the deposit was unconsolidated, like clay or sand, and the particles were free to move among themselves, this expansion would have very little effect in raising the surface; but if the deposit was rigid, like limestone, the effect would be totally different, and the irresistable pressure, caused by the expansion of the

* Q. J. G. S., III., 186. † Pro. G. S., II., 548., 596.

‡ Clay contracts on being heated, but this does not affect the theory.

rock, could only be relieved by the whole stratum bulging upwards and forming an arch, or more properly a dome; and as we know the rate of expansion, we can calculate what the elevation would have to be on a sphere the size of the earth, for various temperatures and for different areas, in order to relieve the pressure. This is exhibited in the following table, which is part of a larger table that I have calculated.* In it the upper line is the thickness in feet of the deposit, while the second line is the temperature due to that thickness. The left-hand column is the diameter, or breadth, in miles of the heated area, while the other columns show the elevation in feet that would take place :—

Thickness	500 feet	2,500 feet	10,000 feet	25,000 feet
Temperature	10°	50°	200°	500°
Breadth, 100 miles ...	1,140 feet	3,700 feet	8,700 feet	14,600 feet
„ 500 „ ...	1,550 „	7,220 „	24,200 „	49,300 „
„ 1,000 „ ...	1,570 „	7,700 „	28,600 „	65,400 „
„ 2,000 „ ...	1,900 „	7,800 „	30,700 „	74,400 „

From this table it will be seen that formations no thicker nor more extensive than those that we know to have been deposited, are quite capable of being elevated far above the highest known mountains.

It may have occurred to you that a bed of limestone would not be capable of supporting itself as an arch, and, therefore, that instead of being elevated it would break up into fragments; this is very true, if the arch was entirely unsupported, but as soon as the expansion overcame the rigidity of the crust, and movement commenced, the underlying superheated rocks, being relieved from pressure, would rise up and still press upwards on the rising arch, so that the pressure expended in elevation would be that capable of overcoming the rigidity only of the crust, and not its weight. You may also have noticed that unless the rate of deposition was greater than the rate of the conduction of heat outwards, no deposit would rise above the surface of the sea, for as soon as deposition ceased the increase of temperature would cease also; and conversely, the greater the difference between the rates the greater would be the rise, for the longer would be the time before the deposit attained its normal temperature.

The data to estimate these rates are not very exact, more especially the rate of deposition, but the following is the best information that I can collect :—

Monsieur Joseph Fourier has calculated that the earth decreases in tem-

* This table is calculated on the suppositions that the earth is a sphere, with a radius of 3,956 miles, and that rocks expand ·000005 for 1° Fahr.

perature by radiation 1° Fahr. in 3,000,000 years,* and from this we can deduce that the conduction outwards must be about one-tenth of an inch a year. Sir W. Thomson's calculations, founded on experiments made at Edinburgh, Greenwich, and Upsala, give an outward conduction two and three-quarter times as fast; but these experiments were made on dry rocks, and he allows that if the rocks were saturated with water, as all newly-formed deposits would be, his estimate would have to be reduced by one-half; which would then give an outward conduction of one-eighth of an inch per year. Peclet's experiments show also that the conductivity of limestone is only two-fifths of the average taken by Sir W. Thomson, or one-tenth of an inch per year. Consequently, we cannot be far wrong if we take the average conductivity outwards at one-ninth of an inch per year.

Professor Dana has estimated that limestone grows at the rate of one-eighth of an inch per year, and sandstone five to ten times as fast, or from five-eighths to one and a quarter inch per year; while the average increase in thickness of the clays of deltas appears to be about one-fifth of an inch per year; so that if we suppose a formation to be about one-third limestone, we get an average rate of deposition for the whole formation of one-third of an inch per year, or three times as fast as the conduction of heat outwards.

If at the present time the internal heat travels outwards at the rate of one-ninth of an inch per year, it would take 54,000 years to heat a deposit 500 feet in thickness; but a deposit 500 feet in thickness implies a rise of temperature in the underlying rocks of 10°, which implies an elevation of 1,140 feet if the heated area was 100 miles in diameter, or 1,900 feet if it was 2,000 miles in diameter, consequently in the first case the land would have risen 1,140 feet, and in the last 1,900 feet in 54,000 years, or at the rate of from two to three and a half feet per century, which is just the rate that Sir C. Lyell considers as most probable from observation.

But in former times when the internal temperature increased three times as fast as it does now, or 1° for seventeen feet, the conduction outward would be equal to the deposition, and consequently no land could rise above the water, and this may have been the cause of the "insular condition" which seems to have prevailed over the world during palæozoic times. According to the theory of the secular cooling of the earth advocated by Sir W. Thomson, these conditions must have occurred about eleven and a half millions of years after the formation of the crust, or about eighty-eight and a half millions of years ago. From that time to the present, elevation must have gone on in an increasing scale; but, although increasing in height, it must have also been decreasing in rapidity, and a time must inevitably arrive when elevation will be so slow that it will do no more than equal denudation, and when again,

* Theorie de la Chaleur, Paris, 1822.

therefore, no more land can rise much above the surface of the sea. Now, it has been estimated that a foot of soil is removed by denudation in from 500 to 12,000 years, according as the land is mountainous or level, and if we take the lowest estimate as that which will be nearest to the conditions at the time I am talking about, we find that the interior heat would have to increase at the rate only of 1° in 10,000 feet to bring about the result.* This, by Sir W. Thomson's theory, will not be for thirteen billions of years; so that the earth is but in its infancy between birth and the repose of old age, and we have plenty of time to look forward to for improvement and development.

But leaving these speculations, it is, I think, time that I gave you an illustration of the theory. I select the Wealden District in the South of England. This district extending through Kent, Sussex, and Hampshire, is formed by an anticlinal curve of the cretaceous and wealden strata. The thickness of the beds is 3,400 feet, and the highest part of the arch would have attained, if the upper portion had not been denuded off, a height of about 3,600 feet above the sea. The base of the arch below London is about 500 feet below the sea, so that the total rise of the arch must have been about 4,100 feet, while the breadth of the anticlinal from London to some point in the English Channel is about 100 miles; these, therefore, are our data. Now a thickness of 3,400 feet implies an elevation of temperature of 68°, and this over a breadth of 100 miles would give an elevation of 4,650 feet, that is to say 150 feet more than the actual rise. But as the land rose above the sea denudation would commence to work upon it, so that the temperature would not be able to rise the whole 68°, and this will account for the 150 feet which the anticlinal arch failed to attain.

I will give you another and more general illustration. During the Eocene period a large ocean, at least 5,000 miles long by 1,800 broad, extended over the south of Europe and the north of Africa, and was continued eastward through Asia Minor, Persia, and Northern India to China. In this ocean, what is known as the mummulitic limestone was formed to a thickness of 15,000 feet. Consequently if, as I have said, large limestone deposits produce elevation, it is here that we ought to find the evidence of it; and this we plainly do in the Atlas, Pyrenees, Alps, Apennines, Carpathians, Himalaya, and the mountain chains of Persia; we find, in fact, that the area of the mummulitic limestone embraces the most mountainous country in the world, and geology shows us that these mountains are all about the same age, and all have been elevated since the period of the mummulitic formation. A thickness of 15,000 feet of limestone over an area 1,800 miles in breadth is

* I need hardly say that these numbers are introduced as an illustration merely, and make no pretension to accuracy.

also more than sufficient to elevate into the air the most towering peaks of the Himalaya.

I might also adduce the Appalachian mountains in America as a beautiful illustration of the theory, every elevation that has taken place distinctly following the deposition of limestone, and occurring only where the limestone was deposited, except, perhaps, the last elevation after the carboniferous period, which at present I cannot account for—for, according to American geologists, the carboniferous limestone never overlaid these mountains. But, besides the deposition of matter, any other cause that changed the temperature of the surface, would also produce alterations of level—a rise in the temperature being followed by elevation, and a fall by subsidence owing to the cooling and contracting rocks forming part of the surface of a sphere. In this way, the Gulf Stream, by raising the temperature of Norway and Sweden, is causing them to rise; while the cold Arctic current, sweeping down through Baffins' Bay and striking against Greenland, is causing that country to sink.

Turning now to the second cause of the subversion of equilibrium, viz., change in the incidence of pressure, we find our data not so satisfactory, for we have no means of estimating the rigidity of the crust, and therefore of the weight it would bear before beginning to move, but nearly all geologists agree that most thick formations of sandstones and clays have been deposited upon a sinking area, and that in a large number of cases the subsidence has been approximately equal in rapidity to deposition. Now the chances are, of course, enormously against subsidence being equal to deposition, unless one is caused by the other, and when many cases of the kind are brought forward, our former suspicion becomes almost a certainty. This, of course, only applies to clays and sandstones, for I have already shown that limestones, as soon as they begin to expand by heat, rise up and relieve the pressure; but with unconsolidated beds, like clay or sand, the horizontal thrust could never get powerful enough to overcome the rigidity of the crust, and consequently they could never rise.

As large deposits of limestone, therefore, elevate areas, so large deposits of argillaceous strata depress them. But while argillaceous strata are being formed they will not only be compressed by the sinking of the spherical surface on which they rest, but they will also at the same time be expanded by heat, and these two, together, will throw the beds into folds or contortions.

If we suppose that the subsidence is equal to the thickness of the formation, which is the most reasonable supposition that we can make, we can calculate the amount of compression due to sinking, and to expansion from heat, due to different thicknesses. Some of these are given in the following table, the upper line of which represents the thickness of the formation in feet, and the lower the proportionate compression :—

Thickness	5,000 feet	10,000 feet	20,000 feet	25,000 feet
Compression	$\frac{1}{1000}$	$\frac{1}{500}$	$\frac{1}{300}$	$\frac{1}{200}$

A first inspection of this table will give the impression that these compressions are not nearly enough to account for the contortions we see in mountain districts, but I believe that our ideas of contortions are very incorrect, owing to the necessarily exaggerated sections that accompany geological descriptions. The only sufficiently accurate section that I have been able to see is Professor Ramsay's beautiful section through Snowdon, in North Wales, and after carefully measuring it, and allowing for the faults and intrusive rocks, I find that the compression in this mountainous district is one-sixteenth. We must also remember that the contortions that we now see are the sum of all the compressions that have taken place at various times, for the rocks after being bent do not straighten out again on being stretched, but elongate by faulting. A considerable amount of the contortions of the lower beds of a formation will also be a necessary consequence of elevation by expansion, for during elevation the lower beds will not be able to expand so much as the upper ones of the arch, although much more heated.

The subsidence of an area caused by the weight of newly-deposited matter will compress the underlying superheated rocks, and, as explained at the commencement of the lecture, this will cause an increase of upward pressure in the surrounding areas. This increase of upward pressure will cause elevation in the surrounding districts, the rocks will be subjected to tension, and fissures will be formed. Up these fissures the superheated rocks of the interior will rise, and if they reach the surface will form volcanoes and overflow as lava streams. In this way mountains of quite a different character to those we have lately considered will be formed.

I have now explained to you the theory of Messrs. Herschel and Babbage in its simplest form, but in nature we should rarely find this simplicity. These two great powers—expansion by heat, and increase of weight—would sometimes combine and sometimes interfere with each other. Complications would also arise from the different degrees of fusibility, conductivity, porosity, and expansion of rocks, while the changes in physical geography caused by the changes in the position of the land would constantly alter the mean temperature of the surface, so that very complex phenomena might result from these simple causes.

To sum up. Mountain chains are of two kinds. The first, of which the Alps may be taken as a type, are composed of folded and contorted strata, generally associated with metamorphic and granitic rocks. These have been formed by heavy argillaceous deposits, causing subsidence and contortion, which have been subsequently elevated by the superposition of calcareous beds. The second kind, of which the Andes may be taken as the type, are composed of nearly horizontal strata, generally associated with volcanic rocks.* These have been formed by the upward pressure of the underlying rocks caused by the subsidence of adjoining areas, and owe their height partly to this upward pressure, but often in great part to the overflowing of the superheated rocks on the surface.

There is, however, one other point that has still to be taken into account. If we calculate the mass of the ocean we shall find that it is sufficient, if the surface of the earth were level, to cover it entirely to a depth of at least two miles. Now, if it is true that the earth has been formed by the slow condensation of gaseous matter, we can see no possible reason why any of the gaseous materials should be confined in the interior solidifying portions, and by their attempts to escape cause eructations, or bubbles that could raise any part of the solid mass more than two miles high. In other words, I do not see how there could be any boiling or swelling up sufficient to form land above the surface of the ocean. If then there was no land in this primæval ocean for denudation to act upon, what was it that first disturbed the equilibrium of the crust and so led the way to those stupendous changes that we know have since taken place? But one answer can I think be given to this question, viz., *the origin of life.* Chemists are agreed that carbonate of lime was in solution in this primæval ocean, and when life, or rather life capable of secreting carbonate of lime, appeared it would abstract this substance out of the ocean and deposit it on particular areas, and thus, by disturbing the equilibrium, would prepare the world to be the habitation for those countless myriads of organised beings which now swarm over it.

I will hazard one more supposition. Over this primæval ocean the winds must have swept with great regularity, and currents must have followed in their wake. Now these currents would naturally take two directions, one N.E. and S.W., and the other at right angles to it. If, therefore, we suppose life to have originated at any one point, it would gradually spread in a N.E. and S.W., or N.W. and S.E. direction, and the first calcareous deposits, and consequently the first land, would take these directions also. This would give the direction of other deposits, and although much obliterated by the complications that have since taken place, we can possibly, even now, trace in the directions of our mountain chains some remnant of this primæval arrangement. But this is sheer speculation.

* See also Darwin "On Volcanic Phenomena in South America."—Trans. G. Soc., 2nd Series, V., 601.

Such is an outline of what I propose to call the Herschel–Babbage theory, after the two distinguished philosophers who originated it; it has the advantage over all other theories of the same nature of being capable of being proved or disproved by observations in the field. When firmly established, as I believe it will be, it will throw a new light on geology, for it gives significance to the thickness and composition of every rock, and to its geographical position; it gives significance to every bend and fold in the strata, to every fault and volcanic dyke; and it will also be found to furnish us with a key that will decypher many of the hitherto obscure passages in geological history.

On the Influence of Temperature on Infant Mortality. By Dr. DECK.

(With Illustrations.)

[*Read before the Otago Institute, 19th November*, 1872.]

ALTHOUGH I feel that the subject upon which I have to offer a few remarks is one especially suited to the meetings of a medical society; yet, in the absence of such in this place I have thought it might prove of some interest to the members of this Institute. I was led to collect the few data upon which my remarks are founded on hearing of the extreme heat of the weather during the past summer in England and in North America, and the consequent great increase in infantile mortality with which that hot weather was accompanied. This mortality is thus referred to in the "Lancet" for August, 1872 :—

"The effect of the great heat which we have had of late is manifest in the death returns by a large increase in the mortality from diarrhœa. In London the general mortality has risen from 17 to 26 per 1,000 in the last five weeks, the rate last week, 26 per 1,000, being higher than in any previous week this year, a result almost exclusively due to the fatality of diarrhœa, which caused last week 394 deaths. The mortality from this cause was nearly all among children under five years of age, of whom 324 died in their first year. In the eighteen large towns the deaths registered from diarrhœa in the week ending July 6th were 113, during the next fortnight they were successively 226 and 370, and during the last week 604. In Leicester and Leeds the fatality is greater than in London, while in Hull it is equal, but in all the other towns considerably less fatal than in London. The Registrar-General refers in this connection 'to the importance of pure water to children who drink freely in hot weather,' and no doubt that is a most important matter, but it must be remembered that the mortality is to a large extent among infants who are hardly likely to drink freely of water."

The mortality among children from the same cause in New York is thus referred to in the "Lancet" for 10th August, 1872 :—

"Heat as intolerable as that which beset the ancient mariner and his crew continues to afflict New York. Not only have cases of sunstroke reached a frightful average, but deaths from nearly every cause still swell abnormally the mortality returns; 1,056 deaths, about double the usual number, were recorded the week before last, while in Philadelphia they amounted to 885, about treble the average. But the infant mortality remains the most appalling feature. Cholera infantum, almost endemic in the Empire City, has assumed something of the proportions of an Egyptian scourge. The heat, with its insanitary sequelæ, operates disastrously on an infant population, which, owing to the premature marriages of its parents, is, as a rule, deficient in stamina and staying power. It is quite usual in New York for a beardless lad of 18 to wed a child of 16, with the inevitable result of begetting a progeny rickety, scrofulous, and (to use the indigenous phrase) hastily run up. Asiatic cholera has already numbered one victim, and New York trembles to think what ravages that pestilence will make when it fairly warms to its work."

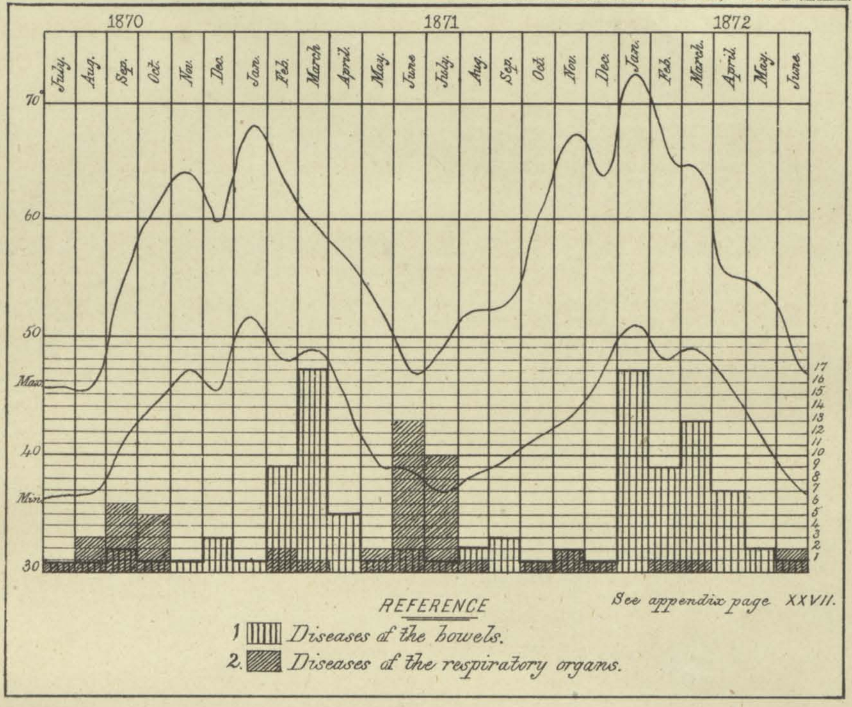
From another medical journal, published in Philadelphia, I extract the following paragraph :—

"The harvest of death. The protracted and unprecedented heat of the first and second weeks of July were accompanied by a mortality in this city of a most startling character. The whole number of deaths for the week ending 6th July was 764, an increase of 350 over the week previous. Of these 274 were from cholera infantum. High as these figures are, they were exceeded by those of the following week, when they reached 852; of this number 497 were children under two years of age, and 383 under one year. Of cholera infantum there were reported 310 cases, and of sunstroke 68."

Recollecting that we had passed through a summer hotter than usual, I determined to compare the mortality among children under two years of age during that season with the mortality during the previous summer, and during other periods of the year.

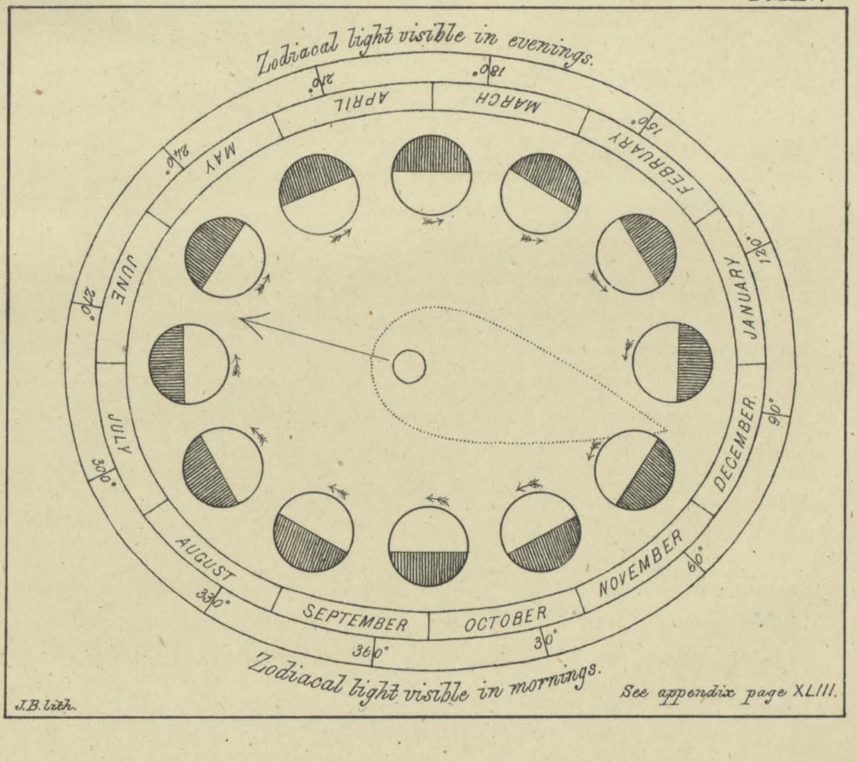
I examined in the Registration Office for the Dunedin district the records of all deaths among children under two years of age during the period of the two years, from 1st July, 1870, to 30th June, 1872. During that time 232 deaths took place. I divided the causes of death into three classes: deaths from affections of the brain, deaths from affections of the respiratory organs, and deaths from intestinal affections. Of the 232 deaths, eight occurred in children that had not reached the age of one week, and the circumstances of their birth had probably more to do with their death than any external cause acting upon them; thirty-three died from affections of the brain, fifty-eight from affections of the respiratory organs, 103 from intestinal disorders, twenty-six deaths were recorded as having occurred from debility or atrophy, two from tubercular disease, without specifying the locus of that disease, one from jaundice, and one from heart-disease. It is to be regretted that the causes of so many deaths are recorded in such an indefinite manner, it takes somewhat from the little value that statistics have, when twenty-six deaths are set down as having occurred from debility or weakness, without specifying the cause of that weakness, or what organs were especially affected.

On examining the periods of the year during which the deaths occurred from these three classes of disease. I found that there was no particular time during which deaths from brain affections were especially prevalent. The summer heat is not intense enough here to produce sunstroke, with ordinary precautions; at least that disease is not of frequent occurrence, and no deaths among infants are attributed to it. The thirty-three deaths that occurred



See appendix page XXVII.

Pl. XIV.



See appendix page XLIII.

J.B. lith.

from cerebral affections, such as convulsions, congestion of the brain, and tubercular meningitis, took place, as far as the records of this small number show, with about equal frequency at all periods of the year.

The fifty-eight deaths that took place from affections of the respiratory organs, such as croup, bronchitis, and broncho-pneumonia, occurred with decidedly greater frequency during the winter months. I have laid before you a diagram (Pl. XIII., 2.) which I have drawn up with the desire to show the variations in the mean, maximum, and minimum temperature for each month during the years above mentioned, and the shaded spaces below show the number of deaths that occurred from diseases of the respiratory organs during those several months. It will be noticed that they occurred principally during, or just after, the depression of the line of temperature in the winter months, during August, September, and October, in 1870, and during June and July in the year 1871.

The 103 deaths that took place from intestinal disorders, such as diarrhœa, dysentery, and cholera infantum, being nearly twice as many as the deaths from respiratory diseases, and three times as numerous as those from brain affections, show that this class of disorders is the one the most serious to infant life. They occurred, as may be seen from Pl. XIII., 1, during the hot summer weather, or they followed it very closely. They occurred principally during February and March in the year 1871, and during January, February, March, and April in the year 1872; the number of cases in each month respectively being indicated by the shaded spaces in the lower part of the diagram.

It will at once be noticed on looking at the curves of temperature in that diagram that the summer of 1871–72 was much hotter than the summer of 1870–71, and the greater rate of infant mortality is visible at once on comparing the shaded spaces referring to those summers. It is to the influence of high temperature in increasing the rate of infant mortality from these affections that I wish especially to draw your attention. The same rule holds good here which has been found to apply in London, New York, and other places, that the higher the rate of the summer temperature the greater the rate of infant mortality that is observed from intestinal affections.

And I would observe that the increased temperature itself, rather than the insanitary sequelæ to which it gives rise (although, no doubt, they are in some measure accountable for the result) seems to be the chief factor at work among the causes that combine together to produce this increased mortality. The case is different when we consider the increased rate of mortality from disease of the respiratory organs during the winter months. Cold weather is often most healthy weather. There may be severe cold, cold weather such as that we experienced during the past winter, when the snow lay upon the ground for days, and it may be that no time of year is more healthy. I am quite aware that intense cold does produce very dangerous attacks of capillary bronchitis, especially in old people, but I do not think that many bronchitic attacks among infants are thus originated. The epidemics of influenza and bronchitis, to which such attacks are generally due, that occur so frequently during the winter and spring months, seem to be produced by some other agency than the cold itself. Sudden changes of temperature in themselves are scarcely sufficient to bring about such results. There seems to be some other factor at work in these catarrhal disorders that are at times epidemic over such a large area of country. They may be due to some peculiar state of electrical tension in the atmosphere, that is produced at times in cold weather, or rather during sudden changes of temperature; or they may be allied to those zymotic disorders that seem to be dependant on the influence of some germs of which we know little, either as to their mode of production or their character, but I think we may safely come to the conclusion that cold itself is not by any means a chief factor in their production.

But a careful consideration of the circumstances under which intestinal disorders are most prevalent, leads to the conclusion that high temperature, or rather *continued* high temperature, is one of the most important factors at work. It seems to act, if I may use the expression, in a *cumulative* manner. The high mortality takes place only after a certain quantum of heat has been allowed to expend its influence. Suddenly occurring high temperature, which lasts only for a short time, is not followed by these pernicious effects. The high temperature seems to act in the first place as a predisposing, and in the second place as an exciting cause. After the system has been weakened by continued exposure to excessive heat, a further exposure is found to produce these intestinal affections.

It is on this account that in the summer of 1870–71, which was comparatively cool, the chief mortality was at the end of the summer, even with a declining average temperature for the month of March in which it occurred; but in the summer of 1871–72, after a hot November and December, the sudden high increase of temperature in January is accompanied by a sudden and great increase in mortality. The gradual decrease in the mortality after that date is in accordance with the laws that are found to obtain in all epidemics, a certain number of those exposed to any morbid influence, on account of some previous constitutional state are especially susceptible, and are first and chiefly affected, and the rest suffer in a minor degree. Thus the continuation of the high temperature in February and March, 1872, might produce a less result than a lower temperature in February and March, 1871, because those most susceptible to the morbid influence of heat had been already attacked in January of that year.

I have not made any distinction in this diagram between the different kinds of intestinal disorders to which children are susceptible. The manner in which the deaths were registered seemed not to be sufficiently accurate to enable me to do so with any degree of satisfaction. Until some uniform system of nomenclature of disease is adopted by medical men this will be the case. On this account I have preferred to group them all together, but if I could have made a distinction between them we should probably have found that dysenteric diarrhœa was especially prevalent towards the end of the summer, during the prevalence of hot days and cold nights, and that it was that form of intestinal disorder termed cholera infantum which was developed during very hot weather, and was the chief cause of fatality at that time.

These conclusions, which I think may be fairly deduced from the examination which I have made into the causes of the infant mortality during the past two years in the Dunedin district, are more clearly demonstrated by diagrams, showing in an accurate manner, day by day, the connection between high temperature and infant mortality, which have been compiled by Dr. Pemberton Dudley, of Philadelphia. These diagrams show for the summers of the years 1869 and 1870, both the daily maximum temperature from the 15th June to the end of August, and the daily death-rate among infants under two years of age from cholera infantum in the city of Philadelphia, and from them Dr. Dudley arrives at the following conclusions :—

1. That there are marked and sudden fluctuations in the number of deaths from cholera infantum from day to day.

2. That these fluctuations correspond very frequently with fluctuations of temperature, the increase of mortality occurring either on the same day as the increase of temperature, or on the day following.

3. That these fluctuations are more marked about the time that the epidemic is at its height, than at any other period before or afterwards.

4. That there is a gradual rise in the daily mortality from the beginning of the epidemic, and a gradual falling off towards its close, which are not attended with a gradual increase and diminution of temperature.

He adds, "The correspondence between the increase of mortality and the rise of temperature does not entirely disappear at any time during the continuance of the epidemic. It will be perceived, however, that slight changes of temperature are not always attended by any noteworthy increase in the death-rate, and there are times when the temperature on a given day rises to a very high point without being attended by any marked increase in the mortality ; but it will be also observed that such days have been preceded by a period of comparatively cool weather. This fact, taken in connection with what was advanced in the fourth conclusion, appears to indicate that a certain amount of hot weather is necessary to create a predisposition to the disease, and that when the predisposition is once developed, the high temperature of a single day acts as an exciting cause, or at least as an aggravating influence."

He also adds, "Reasoning from these facts alone, we must not conclude that the predisposition induced by hot weather is a mere debility, since if such were the case we should find the greatest mortality towards the end of the hot weather, when this debility is the greatest in degree, and most extensively prevalent; which is not the fact, the records showing a steady decrease in the daily list of deaths, even though the temperature should remain above ninety degrees. Looking at all the facts, is it illogical to infer that cholera infantum requires for its development generally a certain occult condition of the system, which, when acted upon by a certain atmospheric temperature continued for a longer or shorter period, induces a predisposition to the disease; and that children who are not previously in this occult state are not liable to the disease at all, no matter what the temperature and its resulting debility might be?"

These remarks coincide with what I have stated as obtaining in all epidemics. In order to obtain some idea as to what the occult state may be, we ask the following question: At what ages are infants most liable to these disorders? The general idea is that the process of dentition has much to do with these affections, and teething time is looked upon by the public at large with anxiety as the period of infant life most fraught with danger.

The following statement of the ages at which the 103 deaths from intestinal disorders took place, leads to a rather different conclusion:—Out of the 103 deaths, two occurred during the first month, five during the second month, ten during the third month, nine during the fourth month, thirteen during the fifth month, eleven during the sixth month, eleven during the seventh month, fifteen during the eighth month, two during the ninth month, three during the tenth month, eight during the eleventh month, and four during the twelfth month. Ten deaths took place during the second year, and one just over two years old. Seventy-five deaths out of the 103 occurred during the first eight months, and the eighth month was the most prominently fatal.

Dr. Dudley arrives at somewhat similar results from the records of a much greater number. Out of 4,013 deaths that took place in Philadelphia in children under two years of age, during a period of five years, from cholera infantum, he found that 2,073, or more than half, perished before the end of the eighth month, and three-fourths of the number perished before the end of the first year. He found the fifth and the seventh months to be the most prominently fatal of all.

The process of dentition, beginning at the seventh or eighth month, is not completed until the twenty-fourth or thirtieth. It therefore follows that this disease manifests its power upon infants in whom this process, commonly speaking, has not yet commenced. By the time that the first molar teeth usually make their appearance, the susceptibility to the disease is nearly past. Whatever the process of dentition may have to do in favouring this susceptibility, it must be the development of the teeth in the bony structure of the jaws rather than their eruption through the gums that acts unfavourably.

But there are other considerations that lead to the conclusion that there are agencies at work during these early months of life in producing this susceptibility other than the process of dentition. Cholera infantum seldom attacks in a severe manner children that are *properly nourished,* and at no period of life do causes of mal-assimilation of food, and consequently of mal-nutrition, exist so frequently as during these first eight or twelve months. Suppose a child for whom the maternal supply of food is poor in quality or insufficient in quantity, and that there is a want of suitability in the nourishment that has been given to make up the deficiency, mal-nutrition must be the result. How frequently do such cases occur! How much more likely are they to present themselves during these early months of life than afterwards when the digestive organs are more fitted to act upon a variety of food, Faulty dietetics are most likely to obtain just at those months that we have found to be most fatal to infant life.

Their intimate connection is still further apparent when we consider the organ that this mal-nutrition will act upon with the greatest intensity. Dr. West says, "There is no organ in the body, with the exception of the pregnant womb, which undergoes such rapid development as the brain in early childhood. It doubles its weight during the first two years of life." The brain, then, and the medulla oblongata, the head-centres of nervous life, will be the organs upon which this mal-nutrition will be most injurious. And we can readily understand how injurious the depressing influence of high temperature must be on a system in which these important organs are in a weak, badly nourished, state. And we find in all severe cases of cholera infantum that the brain is as much affected from the onset of the disease as the intestinal canal. That which might have been only a simple diarrhœic attack from some passing irritation, is changed into a severe, perhaps fatal, intestinal disorder through this weakened state of the brain and nervous system. The intimate connection that exists between the two I need not now enlarge upon; the effect upon the intestinal canal of any sudden shock or emotion, which must act through the brain, is well known to everybody. I will only add that the recent experiments of Ranvier throw some light on the nature of this connection. He has shown that œdema of the leg may be produced by section of the vasomotor nerves which supply its vessels, and does not follow ligature of the femoral vein. He has demonstrated that venous congestion alone is not sufficient to produce œdema, but that the increased exudation from the vessels is rather dependent on want of power in the vasomotor nerves. Supposing this want of nerve-power suddenly to obtain in the vasomotor nerves, regulating the tension and secretory powers of the vessels of the intestinal canal, how soon may this be followed by that which may be equivalent to œdema in the leg, the symptoms that obtain in cholera infantum. I only suggest a consideration of these experiments of Ranvier, as throwing some light on the essence of this disease. An account of these experiments will be found in the "British Medical Journal," 15th June, 1872.

On these accounts I look upon mal-nutrition from faulty dietetics, this mal-nutrition affecting principally the integrity of the brain and nervous system, as the occult predisposing state on which high temperature acts in such a prejudicial manner in producing these intestinal disorders. I will only add that these considerations show how important it is that great attention should be paid to the diet of young infants during hot weather, especially after any continuance of it of long duration. Weaning a child at such a time would be very unwise, and likely to render it susceptible to a severe attack of intestinal disorder, should such occur. All young infants should be protected as much as possible from the effects of high temperature, and an endeavour made during its continuance to invigorate the whole system, and the nervous system in particular, by tepid or cold bathing, and plenty of fresh air during the cool parts of the day.

I would desire also to call attention to the need that exists that some uniform system of nomenclature of disease should be used by all medical men in giving certificates of death. Some uniform system such as that adopted by Dr. William Farr, the Registrar-General of England, should be used by all. At the third conference of the Statistical Congress of the Great Powers of Europe, held in 1857, a nomenclature was agreed upon for adoption in all the States of Europe; it would be well if all the medical men in the colony were supplied with some such system of nosology, that the causes of all deaths might be registered in a methodical and uniform manner. I see by a foot-note in Dr. Aitken's "Science and Practice of Medicine," fourth edition, page 178, that a committee of the Royal College of Physicians of London was then (1864) at work upon a scheme of defining and classifying diseases, which might be an improvement upon that of Dr. Farr's. But I do not know what has been done in the matter. I call attention to this as a matter deserving the attention of all medical men, and I should be glad to learn that something was done in the matter by the authorities at Wellington.

Observations on the Zodiacal Light, tending to show its Connection with the Sun's Motion in Space. By H. SKEY.

(With Illustrations.)

[*Read before the Otago Institute, 12th March,* 1872.]

THE remarkable illumination in the heavens, known as the Zodiacal Light, is visible just after sunset, when the air is very clear, during the months of March and April, and again, just before sunrise, during the opposite months of September and October, and follows in a general direction the course of the ecliptic, or, according to Sir John Herschel, that of the sun's equator. Its apparent angular extent from the sun at its base to the vertex of the cone of illumination varies from 40° to 50°, and sometimes even to 90°, with a breadth varying from 10° to 30°. It has been conjectured that it derives its form (that of a lenticularly formed envelope) by its rapid revolution with the sun on its axis, only the upward half of which we see at one time, the other half being below the horizon.

An insuperable objection, however, to this explanation must at once present itself. If we see the upward half of this figure just after sunset, in March, what is there to prevent the other half from being seen during the same month in the mornings, just before sunrise? Why have we to wait till the opposite season?

It follows, therefore, that whatever may be the cause of this illuminated cone, it exists on one side only of the solar orb; and the next step is to account for its visibility at one time of the year only in the evenings, and at the opposite season only in the mornings. Let the accompanying figure (Pl. XIV.) represent the earth's annual motion along the ecliptic, the small arrows indicating the direction of its diurnal rotation; then, as the Zodiacal Light during September is visible in the mornings, it follows that the direction of the cone must point towards some portion of the earth's orbit lying between September and March. For reasons hereafter adduced, let us assume it as constantly extending towards the earth's position early in December (as far as longitude is concerned), and examine the appearance it would present in March, when the earth has arrived at a diametrically opposite part of its orbit.

It will be seen on reference to the diagram that the Zodiacal Light can then only be visible in the evenings, just after sunset, when its extremely delicate illumination ceases to be overpowered by the direct solar light.

In accounting physically for the existence of matter, or of a medium susceptible of illumination, on the one side only of the sun, let us consider the direction of the sun's proper motion in space in connection with some interstellar and resisting medium. From the investigations of astronomers and mathematicians, conducted in a variety of ways, there cannot remain a shadow of a doubt of the reality of solar motion, or as to its direction in space to a point near to Right Ascension, 261° 29′; and to North Polar Declination, 65° 16′, which are the results deduced by Mr. Airy. The point determined by M. Argelander is in R. A., 256° 25′, and N. P. D. 51° 23′, resulting from the examination of twenty-one stars having a proper motion exceeding one minute per annum in arc. The velocity of the sun's motion relatively among the stars, according to M. Otto Struve, is 422,000 miles, or nearly its own semi-diameter per diem.

With a velocity approaching to this, it is not difficult to conceive the effect it must have on the solar atmosphere, if the existence of a resisting medium can be demonstrated. Perhaps the best proof of such a medium is in the observation of comets. They are known to be bodies of extreme tenuity, and Encke's comet has a period of revolution round the sun which is continually diminishing, proving that it is gradually approaching that luminary. The solution proposed by Encke, and the one generally adopted, is that it is retarded by a very rare ethereal medium pervading the regions in which it moves.

In the diagram, the direction of the sun's motion, as projected on the plane of the ecliptic, is shown as Right Ascension 261° 29′, but the North Polar Declination of its motion being 65° 16′, its course will be obliquely upward on the north side of this plane. Here we must consider the difficulty of determining with exactness the direction of the solar motion. Sir John Herschel remarks, "The whole of the reasoning upon which the determination of the solar motion in space rests, is based upon the entire exclusion of any law either derived from observation or assumed in theory, affecting the amount and direction of real motions both of the sun and stars. It supposes the non-recognition in those motions of any general directive cause, such as, for example, a common circulation of all about a common centre."

I might thus illustrate the case. During a calm at sea the smoke from an ocean steamer would give the exact direction of its motion, both when the water was motionless, and also if it was influenced by an ocean current. A ship might be steaming in a northerly direction, and a current might be moving westerly; if both velocities were the same, then the true motion of the vessel would be north-west, as also manifested from the line of smoke; and a person in the ship taking observations on other ships also moving in the same current, but otherwise stationary, would conclude that his ship was moving due north, but in reality the line of smoke would give the resultant of all the compounded motions affecting the vessel. Similarly we may be unable to determine the sun's true direction in space by the apparent proper motions of the stars, for we may suppose a general movement of the stars in the sun's neighbourhood as drifting in a line parallel to the sun's equator (the most reasonable direction by analogy), then the direction of the Zodiacal Light would be brought nearer still to its observed direction.

From modern researches in solar chemistry we are certain of the existence of the vapours of many metals, and also hydrogen, in the sun's atmosphere. Substances, therefore, of extreme tenuity exist in the vast laboratory of the solar orb. Portions of these substances, under the influence of heat repulsion, must exist at a considerable elevation above the surface, and when subjected to such commotions as have been actually observed (120 miles per second) would be transported to such a distance from the sun as to preclude their revolving around the sun in the same time; moreover, on account of their sudden translation from near the sun's surface to such an increased distance from the centre of diurnal rotation of the sun, some time must elapse before they acquire the additional velocity required. Such masses therefore lag somewhat behind in their daily rotation, and in consequence of the sun's proper motion accumulate in rear thereof. Other portions doubtless might become detached from time to time, forming comets with greatly elongated orbits, having their perihelion passages very close to, and in advance of, the sun's motion until perturbed by the planets.

It becomes interesting to enquire whether the earth ever comes in contact with any portion of this matter, and if so in what part of its orbit?

The illuminated medium known as Zodiacal Light has sometimes been observed reaching our zenith, proving that it extends at times to a distance from the sun fully equal to that of the earth; therefore, if its direction from the sun were truly on the plane of the ecliptic, then the earth must pass very near, if not actually through, its cone, and this at a certain fixed time aunually.

In the diagram the cone is drawn on the ecliptic in Right Ascension 261° 29′. If the general direction of this cone extending from the sun were stationary, then the earth would pass very near, if not actually through, it early in December; but it must be borne in mind that the constant attraction of the earth for months too before it reaches this part of its orbit must hasten the time of contact. The November meteors appear to furnish convincing proof of such collision. They were observed in the year 472 (the sky appeared to be on fire over the city of Constantinople, with coruscations of flying meteors); next by the Moravian missionaries in Greenland, and by Humboldt in South America, in which the whole sky was filled with fiery particles, thick as hail, for four hours. Mr. Ellicot also observed these near the West India islands, when the whole heavens appeared as if illuminated with sky rockets, moving in all directions, excepting from the earth, to which they all seemed inclined more or less, some of them descending perpendicularly over the vessel he was in. They were again seen in the autumn of 1818, when in the language of one of the observers, the surrounding atmosphere seemed enveloped in one expansive ocean of flame. The next exhibition on the grand scale was in November, 1831. This was followed by another in 1832, at the same time. The most splendid display was in November, 1833, when the whole sky is said to have been lit up with these meteors and immense fire-balls. One was observed nearly stationary in the zenith for some time, emitting streams of light. Luminous trains marked the path of these meteors, which remained in view for some minutes. This remarkable fact was established, that they all moved in lines which, when traced backwards, converged to the same point in the heavens. The position of this radiant point among the stars was near Leo, which point remained stationary among the stars during the whole exhibition. They were again observed, but on a smaller scale, in Europe and America, in November, 1834, tending, moreover, from the same radiant point. No less than twelve displays have been noticed. They are also found to be more frequent every thirty-three and a quarter years. Accordingly they were anxiously looked for in 1866, at which time they also made their appearance, and their radiant point fixed in reference to the ecliptic in long. 142° 35′, and lat. 10° 27′ N. Now it must be regarded as a very significant fact, that if this point is projected on the plane of the ecliptic it would be very nearly in a line with a tangent to the earth's orbit on the 13th of November. It follows, therefore, that the earth is moving very nearly towards their radiant point. Taking the velocity of the earth in its orbit at twenty miles per second, and the mass and source of the meteors as stationary (excepting, of course, the retrograde velocity imparted to the mass by the earth's attraction, and which would increase the collision), then the compression suddenly exerted on the meteoric matter, and on a portion of the earth's atmosphere, must be enormous, and far quicker than the rate of diffusion which gases are known to possess. Portions of the earth's atmosphere must be arrested, as it were, and its motion partly communicated to unmixed and contracting portions of the meteoric matter, which manifests itself by intense heat. The common experiment of compressing air in a glass syringe, thereby igniting various substances, will give some idea of the heat actually developed. From the suddenness of compression, there would not be time to allow at first the radiation of this heat; consequently ignition must occur, attended, probably, with new chemical combinations, and when that commences, a few seconds suffice to dissipate the smaller meteors; and the larger ones, when they reach the denser atmosphere near the earth, remain for a time suspended (as proved by actual observation);

further contraction then ceases, their heat is radiated, and their gases become diffused in the atmosphere.

That these November meteors differ from the aerolites which have been known to have reached the earth at various times, is clearly proved by none having reached us in a solid state, notwithstanding their extraordinary numbers. The presence of aerolites is also accompanied with loud reports, which are absent in the case of these meteors; surely if they were solid bodies some would have reached the earth and exploded.

Their retrograde motion might be cited as another proof.

It is worthy of note that during the month of December the earth is situated on the sun's equatorial plane, and it appears that it is near its equatorial regions that all the forces emanating from the sun (motion included) are principally exercised.

Notes on the Zodiacal Light. By J. S. Webb.

[*Read before the Otago Institute, 19th November,* 1872.]

Having recently met with an account by Signor Respighi of some spectroscopic observations of the Zodiacal Light, I felt interested to ascertain how far the facts indicated are compatible with the theory broached by Mr. Skey, in the paper he read at our meeting in March last (see preceding article). Looking for other information on the subject to assist the inquiry, I was surprised to find how little was to be obtained. This being so, I have thought that I should render what I have to say more interesting by prefacing it with a general account of this interesting and ill-understood phenomenon.

The account of the Zodiacal Light given by Sir John Herschel is substantially the same as that to be found in Mr. Skey's paper. It has remained unchanged throughout the successive editions of his "Outlines of Astronomy," although some interesting additions to our knowledge of the subject have been made in the meantime. I think Mr. Skey has been somewhat misled by this, as he lays stress on the fact that the Zodiacal Light is, as stated by Herschel, only visible about the vernal and autumnal equinoxes, and for a few weeks before and after those dates, whilst in point of fact it is visible all the year round, or nearly so. This error does not, as it appears to me, invalidate Mr. Skey's theory, but a knowledge of it would probably have led him to alter his diagram (Pl. XIV.) somewhat, and to avoid some of the remarks he has been led to make.

After a search through all the books accessible here which were likely to afford any information, I found the best account of the Zodiacal Light where I least expected it, namely in the introductory notes to Keith Johnston's "School Atlas of Astronomy." These notes are by Mr. J. R. Hind, and I cannot do better than quote what he says on this subject [Extract read; Keith Johnston's "Atlas of Astronomy," page 5.] I have brought with me the illustration referred to by Mr. Hind which, judging from my own observation and from the accounts of others, conveys a fair representation of the phenomenon.

With regard to the time of the year during which the Zodiacal Light is visible in England, the record of the Rev. T. W. Webb in "Nature," 8th February, 1872 (vol. v., p. 285), of the latest observations there, of which I have seen an acount, corroborates what Mr. Hind says on the subject.

Of the only observation of the Zodiacal Light which I have had the opportunity of making in this hemisphere, I can only speak from memory. It was during the winter months, and the apex of the cone of light, which was on that occasion defined with more than usual clearness, was near one of the brighter stars in the constellation Leo. The sun was at the time far below the horizon, and the distance of the apex from the horizon was fully 40°. A reference to Mr. Skey's diagram will show that if the figure he has given as an approximation to that of the Zodiacal Light is to be taken as an essential detail of his hypothesis, this observation, and indeed all observations during our winter months, decidedly invalidate it. As I have already said, this particular detail does not appear to be essential to the theory, although it renders it desirable that the manner in which it has been expressed should be revised. The general form of the envelope from which we derive the Zodiacal Light may be somewhat as Mr. Skey has supposed, but it is necessary to admit of a very considerable extension in all directions from the sun in and near the plane of his equator, in order to account for its visibility throughout the year.

Various hypotheses as to the constitution of this solar envelope have been put forward. Sir John Herschel speaks of it in his "Outlines of Astronomy," paragraphs 897 and 898. Becquerel, in a recent work, gives the prevalent opinion among French physicists as follows:—"Many explanations of this phenomenon have been offered, the most probable being that which considers it due to a group of bodies which form, as it were, a zone around the sun of solid asteroids, widely separated from one another, but occupying an enormous space, in the midst of which the earth is plunged; aerolites and shooting stars will then be but isolated bodies belonging to this group, which, drawn within the sphere of the earth's activity, fall upon its surface. According to this hypothesis, the Zodiacal Light will be due to reflected solar light, and the absence of polarization which has been observed in it is a result of the light being reflected in all possible planes from the variously presented surfaces of this multitude of bodies."—(Becquerel, *La Lumière ses Causes et ses Effets*, Tome I., p. 7, 1867.) Mr. Skey, as you are aware, has suggested that the bodies which yield us this mysterious light are wholly gaseous, and form the source of our November meteors.

M. Liais, to whose previous communication Signor Respighi refers in his letter to the *Comptes Rendus de L'Académie des Sciences*, Tome 74, p. 514, 19th Feb., 1872, has long held the opinion that the Solar Corona and the Zodiacal Light are phenomena intimately connected with one another. He endeavours to reconcile the different results they give when examined by the polariscope by suggesting that the bodies which, when seen as the Zodiacal Light, reflect the solar rays from their surfaces, when they approach the sun so closely as to form part of the corona are rendered gaseous and incandescent by the excessive temperature to which they are subjected. (*Comptes Rendus*, Tome 74, p. 263, 22nd Jan., 1872.) His observations serve to connect the Zodiacal Light with that of the Corona; those of Signor Respighi and others demonstrate an intimate likeness between the former and the light of the Aurora, whilst others, still more numerous, have shown independently that at least a part of the Coronal Light is identical in its character with that of the Aurora. I shall not attempt a technical account of these. Plate IX., in Schellen's "Spectrum Analysis," English Translation, 1872, exhibits very clearly the coincidence between certain lines observed in the spectrum of the Corona with those which are peculiar to the Aurora. One of these is the "line in the green," observed by Signor Respighi in the spectrum of the Zodiacal Light. Recent observations of the Aurora have shown that it also yields a faint, almost continuous, spectrum situated similarly to that which Respighi and Liais describe as belonging to the Zodiacal Light. Further, the eclipse observations of December last have shown definitely that besides the lines shown in the plate just referred to, the Corona also yields a continuous spectrum, a part only of which is due to reflected solar light. We must conclude from all this that, though these three phenomena are far from being shown to be identical in character, and although it is not probable that they are so, they have at least one character in common. Though suspected before it is only now that this has been positively demonstrated.

It appears then that the revelations of the spectroscope as to the nature of the Zodiacal Light do not invalidate Mr. Skey's hypothesis that it is yielded by gaseous bodies driven off from the sun during solar cyclones. They nevertheless show that this is not a complete explanation of the phenomenon. The argument for the existence of solid or liquid reflecting bodies in the envelope which the non-polarization of the light affords is in the meantime unanswerable, and is supported by the results of spectroscopic observation. But, so far as our knowledge will carry us, we must assume that attenuated gaseous matter also exists in the region of the Zodiacal Light, and that at least a part of the light we see is caused by electric action upon such matter.

On the Work of the Past Year in Astronomy and Celestial Physics.

By J. S. Webb.

[*Read before the Otago Institute, 17th September,* 1872.]

What I am about to say this evening will not fulfil the promise of the title under which it has been announced, but I hope that it will be of none the less interest on that account. Indeed, any attempt properly to sketch the work of a twelvemonth in such a wide range of research, within the limits of a single address, must of necessity end in a barren and uninstructive catalogue of details. I have only selected for remark a few matters of the greatest interest to all, and in regard to those have confined myself almost wholly to the part of a narrator.

Before entering on the proper subject of this address, I cannot refrain from expressing a regret, which I have no doubt you all share with me, at the fact recently made public that the Astronomer Royal has replied in discouraging terms to the communications addressed to him by the Astronomical Society of Christchurch on the subject of the formation of an efficient Observatory at that city. On the last anniversary of the foundation of the sister Province—on the day of its attaining the mature age of twenty-one years—in one of those bursts of genial enthusiasm so often inspired by the celebration of anniversaries, especially where those celebrations take the form of public dinners, and which do occasionally lead to very useful results, it was determined by some of our public-spirited fellow-colonists in Christchurch that a lasting memorial of the day they were celebrating should be enterprised, and their aspirations (determined by what influences I cannot say) took definite shape in the formation of the society I have alluded to, under whose auspices it is proposed to establish such a memorial in the form of an Astronomical Observatory. This society set about its work in right good earnest, and I think we ought most cordially to wish it success. Within a few years from the present time it is almost certain that an Observatory will be founded in New Zealand. Those of you who take a deep interest in those sciences to which we owe our knowledge of what is beyond the little globe on which we live, will join with me in desiring that this Observatory shall be as near to us as may be. It seems, however, to be very probable that if our friends in Christchurch fail in this creditable enterprise, Auckland will be the chosen spot. In every list that I have seen of the places from which it is intended that British astronomers shall observe the transit of Venus over the sun's disc, in 1874, I notice that Auckland is mentioned. How far this is authoritative I do not know, but it is now nearly two years since it was mooted at a meeting of the Auckland Institute that some steps should be taken to secure the instruments which would be sent thither for the observation of the transit and so to form the beginnings of an observatory. Now, quite independent of all selfish feelings, I think we shall have cause for regret—a regret which will be shared with us by a great many persons in all parts of the world—if the future observatory of New Zealand is fixed at Auckland rather than in the South Island. In latitudes similar to that of Auckland the world is now girt with a chain of observatories. There used to be in former days an observatory—of what pretensions I do not know—at Hobart Town, but it has, I believe, been dismantled for some years. It is to be desired, therefore, that our New Zealand Observatory should be placed in as high a latitude as possible, and the more so now that the work of an observatory includes so much more than the watching of stars and planets. Only here and in South America can any observatory be planted nearer by any considerable approximation to the South Pole of the earth than that chain of southern observatories to which I have just alluded—a chain which may be considered to be now fairly complete, including as it does those of Paramatta, Melbourne, the Cape of Good Hope, Cordoba, and Santiago. The Christchurch Society has not been wholly discouraged by the unfavourable advices lately received. The nature of these has not, so far as I am aware, been made public, but whatever the objections I trust they will be eventually overcome.

The allusion just made to the chain of observatories which encircles the globe in a line nowhere far distant from the thirty-fifth parallel of southern latitude, reminds me that the establishment of one of these forms part of the work of the past year. It was in October, 1871, that the Argentine Observatory at Cordoba was inaugurated. The staff of observers had been on the spot for some time beforehand, but there had been considerable delay in bringing the instruments into position and working order. In the meantime Professor Gould, the director of the observatory, and his assistants, did not forget that there were astronomers before instruments of modern type were thought of. What they continued to do whilst waiting for their instruments offers an example to all lovers of the science to which they have devoted their lives. After computing the tables necessary for their future work, they set themselves to form a catalogue of all the stars of the southern heavens which are visible to the naked eye. Probably when they began this work they were only endeavouring to familiarize themselves with the field of their future researches, but the patient toil has been rewarded by many interesting discoveries. The variability of a great number of our southern stars has been determined by a comparison of these new observations with those of previous observers. Two of the stars, whose variable character has been established by these patient workers, are remarkable for the short periods during which they pass from maximum to minimum of brightness. One of these stars is in the constellation Musca. It passes through all its changes in the extraordinarily short period of twenty-one hours and fifteen minutes, and during one-fifth of that time is invisible to the unassisted eye. The other is in the Southern Triangle, and has a period of three and a half days, during part of which time it also is invisible. Professor Gould, in his inaugural address, calls attention to the great field that lies before the southern observer of the fixed stars. The number of stars belonging to the Northern Hemisphere whose positions and magnitude have been catalogued is 330,000, whilst less than a sixth of that number have been defined in the regions south of the equator. Of the latter by far the greater number are stars which are visible in Europe. A circle drawn round the South Pole, with a radius of 60° of latitude, will only include 13,000 known stars, whilst a similar tract of the northern sky includes 164,000. Great as is the difference between the two regions in brilliancy, it is certain that much work has to be done before the catalogues of southern stars reach anything like the perfection of those of the north.

The Cordoba observers have been watching the variabilities of stars. Far vaster changes in celestial objects have been subjects of investigation to other astronomers in this hemisphere. It is now some years since Mr. Abbott, of Hobart Town, pointed out the fact that the star *eta* Argus is no longer actually in the nebula where it was seen by Sir John Herschel. The careful observations of the nebula and neighbouring stars which have been incited by this discovery, have led to the knowledge of extraordinary changes now in progress in this distant object. The Melbourne observers have paid great attention to the subject, and Mr. M'George, in a paper read a few months ago before the Royal Society of Victoria, gave a sketch of the results, illustrated by five drawings of the nebula, as observed at different times. The changes which have occurred since the great Melbourne reflector was first turned towards it have been rapid and most extraordinary. It is much to be regretted that the Royal Society of Victoria is not in a position to publish the more important of the papers read at its meetings, some of which are of world-wide interest. Now that the Melbourne Observatory possesses one of the finest telescopes in the world, we may expect that from year to year the indefatigable and able men who have the charge of it will be in a position to add greatly to our knowledge of the phenomena of the southern heavens. The attention they are paying to this and other nebulæ will no doubt lead to an increase of our knowledge of the physical constitution of these wonderful objects. For instance, when *eta* Argus was first observed to have broken loose from the dense nebula in which it was seen by Herschel, the lines of burning hydrogen were distinctly seen by Mr. Le Sueur in its spectrum. He then offered the conjecture that the star had consumed the nebula. In the latest observations of which I have seen any account no trace of the bright hydrogen lines was found, but the star was found to be nebulous, the nebulosity being most condensed near it. If, as Mr. Le Sueur conjectured, this star had consumed the nebulous matter which formerly surrounded it, it would appear to have found a fresh envelope.

Mankind for ages believed that the celestial orbs ruled the destinies of men in some occult but very direct manner. Science is gradually restoring to us some phases of this faith. The influence which the physical circumstances that surround him have upon the character and actions of the individual man, has been made clear by the comparison and classification of innumerable observations. The statistics of human life, of human action, and human manners, have been brought into conjunction with those of the physical conditions to which our race is subjected, and wonderful and most convincing coincidences have been revealed; and at the same time we have been learning how intimate is the tie which binds together all things that exist in the universe. It is no new thing to acknowledge the rule which the sun has over the physical conditions which prevail upon our planet; but it is only of late years that we have been taught to appreciate at their full intent the influences which are brought to bear upon the sun from without—influences which dictate the character of his dealings with the subordinate members of this system, of which he is the ruling centre. The time has gone by when the sun was accepted as a self-sufficient source of light, and heat, and power. Irreverent investigators inquire into his pedigree, speculate upon the sources of his annual income of force, calculate the probable length of his present existence, and dogmatize on the nature of the "future state" that is provided for him. We have long since satisfied ourselves that there is no certainty about the sun; we suspect him of being influenced by the fair face of any planet that happens to be in aphelion, accuse him of consuming comets behind the scenes, and of devouring myriads of asteroids to keep himself and his subject planets warm. And so we have come to recognize the fact that, as the moral condition of a nation depends upon its harvests, so do these harvests depend upon the physical condition of the sun's surface, whilst this, in its turn, depends upon other things of which we have as yet but little knowledge, but of which we know enough to certify us that they again are not independent phenomena, but are moulded and made what we find them by the flux and reflux of cosmical forces whose origin is far beyond our ken, and of whose mode of action we have but a faint glimmer of knowledge.

Such reflections as these are inevitably excited in the minds of those who address themselves to the study of the current labours and speculations of their fellow men in the departments of science with which we are occupied to-night. The past winter in the Northern Hemisphere, as with ourselves, was remarkable for the occasional intensity of its cold, and general severity of its weather. In November and December the cold was very severe, then followed a period of eight or ten weeks during which the temperature was above the mean, followed by another period of unusual cold. A careful examination of such records as are available convinces us at once that such a circumstance is no fortuitous accident. There is a weather cycle, not yet perhaps so clearly defined, but certainly as well ascertained as any of those cycles of celestial movements which depend on the unvarying law of gravitation. The temperature of the earth's surface varies from year to year, and shows a maximum every eleven years, or rather in periods of a little more than eleven years. Just before the maximum, and just after it, come the periods of lowest temperature. Very lately, Professor Smyth in Edinburgh, Mr. Stone at the Cape of Good Hope, and Mr. Abbe at Cincinnati, each working upon different materials, have pointed out the close coincidence between the curve of varying terrestrial temperature and that of the sun-spot periods; this is the first generalization. Observations of the sun's surface have not yet extended over a period sufficiently long to admit of a comparison of the phenomena presented with that more extended cycle of about forty-one years which M. Renou long ago deduced from his investigation of the records of great winters. The connection between terrestrial variations of climate and the sun-spot period being established, we at once desire to push our investigation a step further. If the character of our winters depends on the condition of the sun's surface what is it that rules the latter phenomenon? Is the cause within the sun itself, or may we look for it without? Analogy and the cyclic character of the variations lead us to prefer the latter solution. The extraordinary character of the weather of the last and preceding years, the recent extension of our means of examining the surface of the sun, the unusual magnificence of certain auroral displays which have occurred during the period I have under review to-night, have combined to direct the attention of physicists to the inter-connection of various cosmical and terrestrial phenomena. The result has been that every research leads to surer convictions of the interdependence of natural phenomena; whilst the further we push our investigations the more we feel that the ultimate cause of those phenomena eludes our grasp.

No natural phenomenon of modern times has evoked at the moment of its occurrence a greater mass of scientific record and speculation than the aurora which, on the night between the 4th and 5th of February last, astonished Europe, and fired the skies over one-half the globe. This aurora was visible in North America and the West Indies, over the whole breadth of Europe, in Western Asia, at the Mauritius, and in Western Australia. There can be very little doubt that, had daylight not interfered to prevent it, the magnificent spectacle which it presented would have been seen from every point on the surface of the globe.

This splendid aurora was coincident with a period of equally notable agitation of the surface of the sun. Signor Tacchini, the Director of the Observatory at Palermo, who devotes himself with great ardour to the spectroscopic observation of the sun, thus describes the condition of things which he found prevailing when the sun rose on the morning of the 5th :—" All the surface of the sun was in abnormal circumstances; the entire rim was covered with splendid flames; towards the North Pole these rose to the height of 20″ (equal to about 9,000 miles), over an arc of 36° to the right and to the left, corresponding to a region of (incandescent) magnesium which on the western border extended to the Equator. In this region, at 50° from the pole, a magnificent protuberance was observed which rose to a height of 2′ 40″ (more than 70,000 miles), and from this point through an arc of 40° the rim presented numerous brilliant flames, and the atmosphere was completely encumbered with luminous threads and shining points up to a height of 2′ (55,000 miles). The chromosphere was throughout more elevated than usual." Along with this agitation of the surface of the sun was to be noted the striking brilliance of the zodiacal light, which some physicists are now maintaining to be in fact a solar aurora. Intimately connected with the auroral display was the appearance of a group of meteors, the radial point of which was in an unusual position. As usual on such occasions a magnetic storm prevailed, and the various telegraph lines including the Atlantic cable were taken possession of by induced currents, which for a considerable period rendered it impossible to work them.

The unusual climatic conditions, and the exceptional prevalence and intensity of auroras (that of 4th February was only the most conspicuous of an extensive series), have filled the transactions of scientific societies and the pages of periodicals devoted to science with statistics, arguments, and theories, all having for their object the elucidation of the cosmical origin of those terrestrial phenomena. I purposed to have given a general account of these to-night, but time will not permit. The intimate connection between both these classes of phenomena and the condition of the surface of the sun is, of course, a fundamental feature with all of them. Signor Tacchini gives it as his opinion that "our polar auroras are nothing else, at least in the majority of cases, than phenomena of electric induction due to the immense auroras produced on the sun." In one form or another this is admitted by almost all theorists on the subject. One theorist has with much ingenuity attempted to connect these phenomena with one another, not as cause and effect, but as both resulting from a common cause. M. Silbermann, after a life-long study of atmospheric currents, forms of clouds, shooting-stars, auroras, and solar phenomena, has reached the conclusion that the innumerable streams of meteors which the earth is continually passing through, are the efficient causes of all these phenomena. In regard to the auroras of last February he has pointed out that a meteor-stream made its presence known by shooting-stars at the time of the auroral display of the 4th. In the case of auroras in January he traces a stream of meteors from the neighbourhood of Jupiter, where but a short time before phenomena of a very singular character were observed, which he claims to have been the effect of similar auroral displays in the Jovian atmosphere. Jupiter's third satellite passed between us and the disc of the planet in December last. Those who were observing it saw with surprise that instead of appearing bright on the grey background of the planet's atmosphere, it appeared black in contrast with a light of unusual brilliance and of a rosy tint, which seemed to be produced in the atmosphere of the planet, and which some observers conjectured to be a Jovian aurora. A few days afterwards, early in January, 1872, some fine auroras brightened the atmosphere of our own planet, and very shortly afterwards an extraordinary number of protuberances and hydrogen jets made their appearance on the sun. The stream of meteors—to the action of which M. Silbermann attributes the occurrence of all these phenomena—continued to pass the earth for some weeks afterwards, making its presence known by shooting stars radiating from a particular point in the heavens, near the place of Jupiter, and by the auroras of 4th February and 22nd and 23rd of the same month, which accompanied these apparitions.

The central point in the astronomical work of the past year is undoubtedly the observation of the eclipse of December last. The secrets of the chromosphere having been so successfully unravelled, the attention of astronomers was, during the last eclipse, devoted in a great measure to the solution of another grand solar problem—the constitution of the Corona. This question may be said to have been definitely set at rest by the observations then taken. The most successful observations were those of M. Janssen, and I very much regret that his detailed account of them has not yet reached this distant corner of the world, not having been presented to his associates of the French Academy of Sciences up to the end of June. On this account, and because this address is otherwise too long, I propose to remit my remarks on this eclipse to some future occasion. Here I will merely say that the observations of December last definitely prove that the coronal light, which is seen during a total eclipse, is not a simple phenomenon. It is partly derived from reflection of solar light by the particles of a true solar atmosphere, and partly from hydrogen, and probably some other substances, which are at a sufficiently high temperature to be self-luminous.

Setting aside the eclipse observations for the present, the most interesting of the work that has been performed by astronomers during the past year is that which relates to the chromosphere of the sun. Since the method of spectroscopic observation, by which it is possible to examine this curious region of the sun in broad daylight, was made known, it has been the subject of most ardent investigation in all parts of the world. The Directors of some of the Italian Observatories have, however, taken a decided lead in this interesting field of research. Signor Tacchini, to whom I have already alluded, and Father Secchi, the Director of the Roman Observatory, have been making simultaneous daily observations and drawings of the borders of the sun. Their labours, coupled with those of Lockyer, Huggins, and others in England, have already secured for us a knowledge of what is going on in this particular region of the sun, which may almost be looked upon as complete so far as the phenomena themselves are concerned, although we are yet very far from having anything which we can fairly dignify with the name of knowledge of the proximate causes of what we observe. Father Secchi has contributed to the Proceedings of the French Academy of Sciences what I may call a descriptive catalogue of the phenomena which are to be observed in the chromosphere and protuberances. . . . [Mr. Webb proceeded to give a description of the various appearances presented from time to time by the chromosphere and the "red prominences" which arise from it, which would scarcely be intelligible without the drawings by which it was illustrated. These drawings were coloured copies on a large scale of those which illustrate Father Secchi's paper in the "Comptes Rendus," T. LXXIII., pp. 826, *et seq.*, 2nd October, 1871. After an allusion to the forms of some of these detached masses of flame which have the character of clouds, as "evidently due to the action of fierce atmospheric currents," he proceeded as follows :—] Father Secchi considers that he has established, by a twelvemonth of patient observation, the existence of such currents on the sun having a general set from the equator to the poles, varied by local circumstances in the neighbourhood of important sun-spots. This result has been contested with some spirit, especially by M. Faye, the President (last year) of the French Academy. Those astronomers who are most familiar with the chromosphere appear, however, to accept Father Secchi's theory, satisfied that the observed phenomena coincide with it, and not disposed to make too much of theoretical difficulties. The latter are, indeed, found to be very great when we attempt to explain to ourselves how a circulation can exist on the surface of the sun having any analogy to the trade winds which prevail in certain regions of the earth—or rather to those upper reverse currents which accompany these phenomena. We can account for our own winds by the action of the sun upon our atmosphere, but we are entirely at a loss when we come to inquire how an atmospheric circulation similar to that which the earth enjoys should be engendered on the sun itself. That such currents do exist appears to be established, and when we find a satisfactory theory by which to account for the extraordinary peculiarities of the solar rotation, which is far more rapid at the equator than near the poles, we shall probably also find the clue to the problem of solar trade winds.

I fear I have exhausted your patience. I have certainly exhausted the time at my disposal either for the preparation or delivery of this address. My subject, however, remains quite inexhaustible; and, with the eclipse of December last, at least one-half of my notes must be remitted as material for a future address.