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REMARKS ON THE NEGATIVE INDEX OF A FUNCTION.

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In the consideration of indices, whether used to denote powers of numerical or algebraical expressions, or the successive performance of some operation or function on a quantity, it is usual in examining the meaning of negative or fractional indices to state that *it is convenient* to assign certain interpretations, because of a certain generality which then obtains in the results. In the words of a recent author,* "Experience will prove that the notation here given is often convenient, and we may shew that it is not altogether an *arbitrary* notation but one that naturally presents itself." It appears to me that this, at any rate in the case of negative indices, is an inadequate mode of expressing the ground on which these indices are interpreted, and that the meaning to be assigned to the index is not only one that *naturally presents itself*, not only *not altogether arbitrary*, but *the* meaning which *must* be assigned, exclusive of any other meaning, and no more arbitrary than the use of the notation for positive integral indices. With respect to fractional indices even, I am of opinion that the above would be an insufficient account of the reasons by which we are led to accept an interpretation of the index since it would leave an impression that we are guided rather

* See Todhunter's "Plane Trigonometry."

by what is *convenient* than by what is true, and that perhaps some other more convenient explanation might some day replace the one now adopted. But in the case of the negative index such a mode of expression is still less admissible, because the steps by which the meaning is established are so easy and straightforward.

If any operation performed on a quantity x be denoted by $f^1(x)$, we should denote the same operation performed upon $f^1(x)$ by $f^1(f^1(x))$ or conveniently by $f^2(x)$. $f^2(x)$, therefore, denotes the operation f^1 performed *once* upon $f^1(x)$, or *twice* successively on x. Similarly $f^3(x)$ may be used to denote the function f^1 performed *once* on $f^2(x)$, *twice* successively on $f^1(x)$, or *three times* successively on x, and so on. Adopting this notation we shall have $f^m(x)$ to represent the operation f^1 performed m times on x successively, and $f^{m+n}(x)$ or $f^{n+m}(x)$ to represent either the performance of the operation f^1 m times on $f^n(x)$, *i.e.*, $= f^m(f^n(x))$ or n times on $f^m(x) = f^n(f^m(x))$ or $m+n$ times on x, the result being in each case the same, *i.e.*,

$$f^{m+n}(x) = f^n(f^m(x)) \qquad (\alpha)$$

$$= f^m(f^n(x)) \qquad (\beta)$$

Hence $f^m(x)$ is derivable from $f^{n+m}(x)$ by *undoing* the n operations denoted by f^n in (α) and $f^m(x) = f^{\overline{m+n}-n}(x)$.

Hence $-n$ in the index must be regarded as undoing the operation f^1 *n times* supposing it had been performed *more than n times* on x.

But what does $f^0(x)$ or $f^{-n}(x)$ represent of itself, when there is no operation to undo?

Now we observe that f^1 denotes an operation performed *once*, f^2 *twice*; f^m *m times*.

$\therefore f^0$ represents the operation performed *no* times, that is, *not performed at all*, or $f^0(x)$ is the same as x, for just as truly as f^m represents m operations, so truly does f^0 represent no operations:

one is as general as the other. Hence we can assign the meaning of the negative index, for f^{-1} means the reverse operation to f^{1}; if both f^{1} and f^{-1} be performed on x, one undoes what the other does, and the result is x. So that $f^{-1}(x)$ represents that quantity or quantities, for there may be more than one, on which if you perform the function denoted by f^{1} the result is x. And so f^{-2} denotes that quantity or quantities on which if you perform the function f^{1} *twice* the result is x. It will not be possible in every case to assign a numerical or even symbolical expression of every inverse function that may occur, but it appears to me that the *meaning* of the notation is perfectly definite, and that it ought to be treated as such. The theory of indices stands on very different grounds from any arbitrary convenient explanation of, for instance, the symbol $\sqrt{-1}$, derived from the truth of results obtained by treating it as a real quantity. It may, however, be as well in conclusion to notice one or two obvious cases to which the above remarks are applicable:

(1). Theory of Indices in multiplication or division of like quantities in arithmetical algebra,—

Here $a^{m} = a \times a \times a$ to m factors.

Now a denotes an operation performed on unity, namely, multiplying it by a. Hence a replaces f^{1} and 1 replaces x, 1 being usually for simplicity omitted. Thus $a^{0} = a^{0}(1) = 1$.

a^{-1} = a quantity which, multiplied by a, will $= 1$, *i.e.*, $= \frac{1}{a}$.

a^{-2} = a quantity which, multiplied twice by a, will give 1, *i.e.*, $= \frac{1}{a^{2}}$, and so on.

Unity is here abstract or concrete, and the result abstract or concrete accordingly. In the few cases in which an interpretation may with more or less strictness be applied to the multiplication or division by one another of concrete magnitudes, the unit will of course be of that denomination which is denoted by the index after such multiplication or division.

(2). Indices denoting Trigonometrical Functions, for example,—

Sin $^{0}(x)$ means x.
Sin (x) " the sine of x.
Sin$^{-1}(x)$ " that angle of which the sine is x.
Sin$^{2}(x)$ " the sine of the sine of x, and so on.

N.B.—These must carefully be distinguished from $(\sin x)^2$, $(\sin x)^{-1}$, $(\sin x)^0$, which come under the former or following head, and are frequently, though inaccurately, written as above.

(3). Indices denoting any function whatever,—

Example (1): Let $f^1(x)$ be the differential of $x = dx$, $f^0(x)$ is x, $f^{-1}(x)$ is $d^{-1}(x)$ meaning that which, if differentiated, will give x—in other words the integral of x. $f^{-2}(x)$ is $d^{-2}x$ that which, if differentiated *twice*, will give x, or the *second* integral of x, and so on.

It will be observed that this illustration shews clearly that a definite meaning is attached to the inverse symbol, for although our analysis may not be sufficient to enable us, in any special case, to integrate the required number of times, yet the operation is not only *conceivable* but never beyond the bounds of *possibility*, and may be *practicable*, and, what is more, may in every case be performed independently of our knowledge of the results of differentiation.

Example (2): Let $f(x) = x + \frac{1}{x}$

$$f^0 x = x$$

$$f^{-1}(x) = \frac{x}{2} \pm \frac{\sqrt{x^2 - 4}}{2}$$

For performing the function f on this we get,—

$$\frac{x \pm \sqrt{x^2 - 4}}{2} + \frac{2}{x \pm \sqrt{x^2 - 4}} = \frac{2x^2 - 4 \pm 2x\sqrt{x^2 - 4}}{2(x \pm \sqrt{x^2 - 4})} + \frac{4}{2(x \pm \sqrt{x^2 - 4})}$$

$$= \frac{2x}{2} = x$$

And similarly,

$$f^{-2}(x) = \frac{x \pm \sqrt{x^2 - 4} \pm \sqrt{2(x^2 - 10 \pm \sqrt{x^2 - 4})}}{4}$$

which may be verified. Beyond this point, the analysis fails to give the inverse function, though equations may be found to determine them. To take one more example,—

$$f(x) = \sqrt{a + x}$$
$$f^0(x) = x$$
$$f^2(x) = \sqrt{a + \sqrt{a + x}}$$
$$f^{-1}(x) = x^2 - a$$
$$\text{for } f^1 f^{-1}(x) = \sqrt{a + x^2 - a} = \sqrt{x^2} = x$$
$$f^{-2}(x) = (x^2 - a)^2 - a$$
$$\ldots = \ldots\ldots$$
$$f^{-n}(x) = \Big\{ \ldots \Big\{ \Big\{ (x^2 - a)^2 - a \Big\}^2 - a \Big\}^2 - a \Big\} \ldots\ldots \Big\}$$

to n brackets.

Note:—Since writing the above, the invaluable treatise of Professor Boole on Differential Equations has been published. In his XVIth chapter there are a few remarks on *inverse forms*, which seem to bear out what has been said on their proper interpretation. He writes, commenting on the index laws as applied to functions: "All that is said above relates to the performance of operations definite in character upon subjects proposed to be given. But an inverse problem is suggested in which it is required to determine, not what will be the result of performing a certain operation upon a given subject, but *upon what subject a certain operation must be performed in order to lead to a given result.*" So below he adds: "If π represent any operation or series of operations possible when their subject is given, and then termed *direct*, and if in the equation $\pi u = v$ the subject u be not given but only the result $= v$ then we may write $u = \pi^{-1} v$. And the problem or enquiry contained in the inverse notation will be answered when we have, by whatever process, so determined the function u as to sstisfy $\pi u = v$ or $\pi \pi^{-1} v = v$. By the latter equation the inverse symbol π^{-1} is defined. Thus it is the *office* of the inverse symbol to propose a *question*, not to describe an operation."

If the inverse symbol has an *office*, it is obviously more than a mere convenient notation. The *form* of the above statement may perhaps be open to objection, since when two precisely reverse operations are performed it seems as fair to denote one of them a question as the other. But the view taken of the inverse symbol is the same, whatever be thought of the propriety of this statement.

REMARKS ON SOME GENERAL PROPERTIES OF CURVES.

BY J. W. MARTIN, LL.D.

THE geometric method of investigation, so highly esteemed by Newton and his followers, has experienced considerable vicissitude as regards the amount of attention bestowed upon it by mathematicians at different periods. Having for more than a century held undisputed sway in the universities of Great Britain, it was at length obliged to yield to those more powerful methods of investigation, which, prosecuted with untiring zeal and ingenuity by men possessing unrivalled powers of analysis, had placed the continental mathematicians so far in advance of those in England. Though for a time decried as much as it was before injudiciously extolled, the geometric method has never been utterly neglected. It possesses merits of its own that must ever claim the attention of men of science. It affords solutions of many questions far more concise than can be furnished by the analyst, and occasionally presents us with theorems which, as beautiful as unexpected, shew that its powers have not even yet been developed to the utmost.

1. If two curves lie, the one inside the other, and a right line be drawn cutting the curves so that the sum of the areas of the segments cut off shall be constant, the envelop of the right line is the locus of the centre of gravity of the sum of the chords.

2. Similarly, if the difference of areas is constant the envelop of line is locus of centre of gravity of difference of chords, that is of the portions of the right line enclosed between the two curves.

These theorems have been slightly altered in form so as to exhibit more strongly an analogy to a theorem given by Professor Cherriman, in the *Canadian Journal*, February, 1863.

3. The envelop of chords cutting a curve at equal angles is locus of a point dividing these chords, so that rectangle under segments is constant.

4. The envelop of chords joining points of taction of parallel tangents is locus of a point dividing those chords in a given ratio.

If the curve is a central conic the envelop is a point, the centre of conic.

5. If the curves S and S' are so related that tangent at any point

P in S cuts the curve S' at a constant angle in P', tangent to evolute of S makes with evolute of S' a constant angle.

6. If S and S' intersect in the point O, the arc OP' bears a constant ratio to the difference between the arc OP and the tangent PP'.

The logarithmic spiral will serve to illustrate the two last theorems.

7. If right lines drawn from any point R in the curve S to touch the curves S' and S'' in the points P and Q are equal, the product of the tangents of the halves of the angles which the lines RP, RQ make with the tangent to S at the point R is constant.

As particular examples of this theorem we may take, firstly, the case of tangents drawn to a circle from any point in a line given in position.

Secondly, tangents drawn to two given circles from any point in their radical axis.

8. In the same figure as the last, if instead of having the tangents equal we have the angle PRQ constant, the circle passing through the three points P, R, Q, touches the curve S at the point R, and the normals to the three curves at the points P, R, Q, meet in a point.

9. If right lines drawn from any point R in the curve S touching the curve S' in the points P and Q contain with the arc PQ a constant area, tangent at R is parallel to the right line joining P and Q.

10. If the vertex of a constant angle is at the point O, and the sides of the angle cut the curve S in the points P and Q, and the curve S' in P' and Q', area of the figure $PQ\,P'Q'$ is a maximum when difference of squares of OP and OP' is equal the difference of squares of OQ and OQ'.

Hence if from a point O outside a circle it is required to draw two secants containing a given angle, so that the area of the figure contained by the secants and the circumference of the circle may be a maximum, it is wl the secants make equal angles with the diameter passing through the point O.

11. If the vertex of a constant angle is at the point O, and the sides of the angle cut the curve S in P and Q, the sum of OP and OQ is a minimum when the ratio of OP to OQ is equal to the ratio of the tangents of the angles which the sides of the given angle make with the curve.

REMARKS ON THE TEMPERATURE COEFFICIENTS OF MAGNETS.

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It has been long a matter of notoriety, that the correction to the scale-reading of a force magnetometer which is due to a change of one degree in its temperature, and which is derived from a comparison of the changes in its scale-readings with those of the attached thermometer, is often greatly at variance with $\left(\frac{q}{k}\right)$, the value of the correction in which q is the temperature coefficient of the magnet found by the ordinary hot and cold water experiments, and k the scale coefficient. The discrepancy has been attributed to the action of changes of temperature on the supports and appendages of the magnet when in adjustment, independent of, and in addition to, the alteration that such changes effect in the magnetic moment of the magnet; and as these effects cannot be determined *a priori*, it is safer, instead of relying on the experiments, to adopt the practice now almost general, of deriving the temperature corrections from the recorded observations with the instrument and its thermometer, either by grouping them in seasons, as explained in page ii. of the Introduction to the third volume of the *Toronto Observations*, or by comparing groups of scale-readings at intervals of a few days, as in pp. xxiii. and xxiv. of the same volume.

Since, however, the first of these methods requires an unbroken series of two or more years, and the second a series of a year in length at least, it may be worthy of consideration whether the method of obtaining the correction from the temperature experiments cannot be made to yield results in sufficient accordance with the truth to serve the purpose of provisional reduction, or to meet the case in which the bifilar is needed as an auxiliary instrument to aid in the reduction of the absolute determination of the horizontal force, where since the range of temperature during the observations is small, the effect of a small error in the correction to the bifilar readings will be of less moment.

Let (m) be the magnetic moment of the magnet whose temperature coefficient (q) is sought, and which is placed as a deflector with its axis at right angles to that of the suspended magnet in its deflected position, the axes of the two magnets being in the same horizontal plane, and the centre of the unifilar in the prolongation of the axis of the deflector.

Also let r be the distance between the magnetic centres,
u the angle of deflection,
X the horizontal component of the force.

The relation between m, r, u, and X is given by the formula

$$m = f(r) \text{ X} \sin u, \text{ (where } f(r) \text{ is some function of } r)$$

and that of their simultaneous small changes by

$$\frac{\Delta m}{m} = \frac{f'(r)}{f(r)} \Delta r + \cot u \, \Delta u + \frac{\Delta \text{X}}{\text{X}}$$

Now, if $\frac{\Delta m}{m}$ be the increase in the magnetic moment due to a decrease of $(t—t_0)$ in the temperature, and q that due to a decrease of 1°, so that $\frac{\Delta m}{m} = q\,(t—t_0)$, the preceding equation will become

$$q = \frac{1}{t-t_0} \left\{ \frac{f'(r)}{fr} \Delta r + \cot u \, \Delta u + \frac{\Delta \text{X}}{\text{X}} \right\}.$$

It is customary to assume that $\Delta r = 0$, or that the magnetic centre occupies a fixed position in the magnet during the changes of temperature. Such will probably be the case if the magnet be strictly homogeneous throughout ; but if its molecular condition be not uniform, it is at least conceivable that a change of temperature will affect differently the different parts of the magnet, as it is already known to affect the general magnetism of two different magnets.

Suppose, then, the north end of the deflector to be directed towards the suspended magnet, and that a decrease of 1° in temperature causes the magnetic centre to recede from the north end by the small quantity (a), so that $\Delta r = (t—t_0)a$. Also, suppose q_1 to be the value of q determined in this case on the supposition that r is constant or that $\Delta r = 0$.

We shall then have

$$q = \frac{f'(r)}{f(r)} a + q_1$$

Similarly, if q_2 be the value determined on the same hypothesis when the south end of the deflector is presented,

$$q = -\frac{f'(r)}{f(r)}a + q_2,$$

Whence $$q = \tfrac{1}{2}\left\{q_1 + q_2\right\},$$

or the temperature coefficient q will be the arithmetic mean between q_1 and q_2, the values derived from the experiments in which the North and South Poles respectively are presented.

The probability that an alteration in the distribution of magnetism does sometimes accompany a change in the temperature of a magnet, was suggested by the results of temperature experiments made by me in March, 1861, on two magnets in use at the Toronto Observatory. With one of these—the magnet of our small bifilar—the results were as follows :

North pole presented	q_1 =0000603 ;
South "	q_2 =0001105 ;
Giving	q =0000854 ;
But the scale coefficient	k =000115 ;
Whence	$\frac{q}{k}$ =0·74 nearly.

But from the observations in the period to which the foregoing value of k belongs, and by the method on pp. xxiii. and xxiv. of the Introduction to the third volume of the *Toronto Observations*, the equivalent in scale divisions for a change of one degree of temperature, was 0·66 nearly, a result with which the above value of $\frac{q}{k}$ shews a very tolerable accordance.

The value of q_2 = 0001105, which is given above, agrees very fairly with the results of a series of experiments in 1843 and another in 1845, which gave respectively q=0001032 and q=0001138.

Again, in page xxvii. of the third volnme of the *Toronto Observations,* we find that by experiments in 1843–44, on the magnet of Lloyd's Vertical Force Magnetometer,

$$q = 000112;$$

and by experiments in 1846,

$$q = 00007.$$

But in March, 1861, when the North Pole of this magnet was presented, I found the partial value of the temperature coefficient to be

$$q_1 = 000106;$$

and when the South Pole was presented,

$$q_2 = \cdot 000067;$$

giving $q = \cdot 000086$ nearly as the true temperature coefficient.

The remarkable accordance of q_1, q_2, with the results of the two earlier experiments, makes it very probable that the North Pole was presented in the experiments of 1843–44, and the South Pole in those of 1846. Should such be the case, the true value of q during that period would have been ·00009 nearly. But it is shewn on the same page, that by the multiplication of the equivalent to a degree of temperature by k the scale coefficient, there is obtained

$$q = \cdot 0001105,$$

which agrees much better with $q = \cdot 000112$, the value derived from the experiments of 1843–44, than it does with $q = \cdot 00009$; from which it would appear that the error that would be committed by taking $q = \cdot 000112$, and which is caused by a change in the distribution of the magnetism, would be almost completely compensated by the superposed effects of temperature on the instrument.

The discordance above referred to between the results of temperature experiments in which the two poles are successively presented, may be an exceptional property. Of eight magnets tested at my suggestion, by Mr. Stewart, of the Observatory at Kew, through the kind intervention of General Sabine, one only showed any material difference in the results derived from presenting both poles; and for other magnets that I have tried, results materially the same have been obtained, whichever pole was presented; nevertheless, the fact that it has been occasionally otherwise is a sufficient motive, I think, in conducting temperature experiments to present each pole of the deflector instead of one only.

NOTE ON POINSOT'S MEMOIR ON ROTATION.

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This celebrated memoir of Poinsot's, which, in connection with his invention of couples, has revolutionised our whole system of mechanics, treats the subject partly in an analytical, partly in a geometrical manner. In our modern text books the analytical method is mainly adopted, and it has seemed to me that the beauty and simplicity of the system have thereby been much overlooked. In following, and possibly simplifying by a more elementary geometry, Poinsot's course, we commence with the general reduction of a set of statical forces to a single resultant force and a single resultant couple.

1. Let P be one of a set of forces acting at assigned points of a rigid system, and let A be a point arbitrarily assumed as an origin. At A apply two opposite forces, each equal and parallel to P. Then the original force P is replaced by an equal and parallel force acting at A, and a couple. Each of the forces of the system may be treated in the same way, and the whole set will be replaced by a set of forces acting at A, (which may be combined into a single Resultant R), and a set of couples which may be combined into a single couple G.

2. Since R is compounded of a set of forces which are severally equal and parallel to those of the original set, R evidently remains the same in direction and magnitude, whatever origin be assumed; G in general varies for different origins in both respects, but evidently remains the same for all origins which lie in the direction of R.

3. To examine the changes which G undergoes in passing from one origin to another, let B be any other origin, and at B apply two opposite forces, each equal and parallel to R. We have then, R at B, the couple G, and the newly introduced couple Ra (a being the distance between the directions of R at A and R at B). Now suppose G to be resolved into two couples, whose axes are severally parallel and perpendicular to R; these will be, $G \cos\theta$, and $G \sin\theta$, where θ is the angle between R and the axis of G. Then the axis of the couple Ra being perpendicular to R, this couple will combine with $G \sin\theta$, but will not affect the other resolved part $G \cos\theta$. Hence, whatever origin be adopted, the resolved part $G \cos\theta$, whose axis is in direction of the resultant force, always remains the same. The other component of the couple admits of all values according to the origin adopted. We may therefore adopt an origin (or in fact a line of origins parallel to R) such that this other component shall be zero, and we have then remaining a couple whose axis is in the direction of the resultant force. In this case, the resultant couple evidently has its least possible value.

4. Calling G' this value of it, on transferring to another origin as in (3), the new couple will be compounded of G' and Ra, the axes of which are at right angles to each other; and the new couple

will therefore have the same value so long as a remains the same—that is:—for all origins lying on a circular right cylinder about the line of origins spoken of, and for this reason this line of origins is called by Poinsot the *central axis*.

5. Since R is the same for all origins, the set of forces is not reducible to a single couple, unless it should happen that $R = 0$.

In this case, the forces must be capable of being represented in magnitude and direction by the sides of a polygon (or of several polygons) taken in order. If the forces were represented in position also by the sides of the polygon, and the polygon moreover were a plane one, then the magnitude of the resultant couple would be independent of the position of the forces with regard to the system, being in fact represented geometrically by the area of the polygon.

6. Since $G \cos\theta$ is the same for all origins, the set of forces is not reducible to a single force, unless it should happen that $G \cos \theta = 0$.

That this may be the case, we must either have $G = 0$, or $\theta = \frac{\pi}{2}$; that is:—we must find at our assumed origin either the resultant couple vanishing, or else its axis at right angles to the direction of the resultant force. If the latter be the case at any one origin, it must plainly be so at all origins, and it is easy to see in what way the reduction to a single force is effected. For the plane of the couple can be moved so as to contain R, the couple can be turned till one of its forces is opposite to R, and the arm can be altered till this force is equal to R; these two forces being then removed, there remains the other force (R) of the couple for the single resultant, acting in a line whose distance from the direction of R through our assumed origin is equal to $\frac{G}{R}$. (Of course if R should happen to be 0 this transformation is illusory.) This condition is evidently satisfied when the forces of the system are all parallel, and the single resultant in this case is equal to the algebraic sum of the forces, provided that this sum be finite.

7. Any set of forces can also in the general case be reduced, in an infinite variety of ways, to two, acting along lines which neither meet nor are parallel. For, let the couple G be transferred till the direction of one of its forces intersects that of R; then these two can be compounded into a single force, and this and the remaining force of the couple constitute the two forces acting as stated. The elements of these two forces are of course not

entirely arbitrary, but may be shown to be subject to the condition that $TT'a\sin\phi$ is constant, where T, T', are the two forces, a is the shortest distance between their lines of action, ϕ the angle between these lines. (Cambridge S. H. 1833.) For let the couple be changed so that its forces are T, T, and a is its arm, and let it be placed so that T acts at the same point as R, and the arm is at right angles to R. Then T and R being compounded into T', the angle between T' and T will be ϕ, and we have $T'\sin\phi = R\cos\theta$. Also $G\cos\theta$ being constant, and Ta being equal to G, $Ta\cos\theta$ is constant, and therefore, since R is constant, we have $TT'a\sin\phi$ also constant. This can also be expressed geometrically by saying that if the two forces be represented in position and magnitude by two straight lines, and the extremities of these lines be made the angular points of a pyramid, the volume of this pyramid will remain the same, whatever way of reduction be chosen. This elegant proposition was first given (so far as I am aware) in the *Ladies' Diary*, 1836.

In a subsequent note the analogous propositions in the motion of a rigid system will be discussed.

FORMULÆ FOR THE COSINES AND SINES OF MULTIPLE ARCS.

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§1. Take the expressions,

$$T_0=2,\ T_1=1,\ T_2,\ T_3,\ \&c., \qquad (1)$$

So that, t being any quantity, and c a number greater than zero, the relation

$$T_{c+1} = T_c - t^2 T_{c-1} \qquad (2)$$

always subsists. Hence $T_2 = 1 - 2t^2$, &c. In like manner, take the expressions,

$$t_0=0,\ t_1=1,\ t_2,\ t_3,\ \&c., \qquad (3)$$

So that, t being any quantity, and c a number greater than zero, the the relation

$$t_{c+1} = t_c - t^2 t_{c-1} \qquad (4)$$

always subsists. Hence $t_2=1$, $t_3=1-t^2$, &c. The law of the formation of series (3), expressed in equation (4), being the same with that of series (1), expressed in equation (2), the difference between the series (3) and the series (1) arises solely from the difference in their first terms. The general terms T_m and t_m are easily found. In fact, m being any number greater than zero,

$$T_m=1-mt^2+\frac{mt^4}{\lfloor 2}(m-3)-\frac{mt^6}{\lfloor 3}(m-5)(m-4)+\frac{mt^8}{\lfloor 4}(m-7)(m-6)(m-5)-\&c., \quad\ldots\ldots\ldots\ldots \quad (5)$$

and $$t_m=1-(m-2)t^2+\frac{(m-3)(m-4)}{\lfloor 2}t^4-\frac{(m-4)(m-5)(m-6)t^6}{\lfloor 3}+\&c., \quad\ldots\ldots\ldots\ldots \quad (6)$$

When m is even, the number of terms in the value of T_m is $\frac{m+2}{2}$, and $\frac{m}{2}$ in the value of t_m. When m is odd, the number of terms in each of the expressions T_m and t_m is $\frac{m+1}{2}$. To prove (5), we observe that $T_1=1$, and $T_2=1-2t^2$. Hence the law is true for the first two steps. Assume it to hold for $m-1$ steps. Then

$$T_{m-1}=1-(m-1)t^2+\frac{(m-1)t^4}{\lfloor 2}(m-3)-\&c.$$

and $t^2\,T_{m-2}=\qquad t^2-(m-2)t^4+\&c.$ Therefore, by (2),

$$T_m\quad=1-mt^2+\frac{mt^4}{\lfloor 2}(m-3)-\&c.:$$

which proves the Law universally. In the very same manner equation (6) can be shewn to hold.

§2. The following formulæ may now be established:

If $2t\cos\theta=1$, and $2t\sin\theta=k$,

then $2t^m\cos m\theta=T_m$, $\quad\ldots\ldots\ldots\ldots\ldots\ldots$ (7)

and $2t^m\sin m\theta=kt_m$. $\quad\ldots\ldots\ldots\ldots\ldots\ldots$ (8)

§3. To prove (7), we remark, that, by hypothesis, the Law holds for the first step, that is, when $m=1$. Assume it to hold for $m-1$ steps. We have only to shew then that it holds for the succeeding step. Now, since the Law holds for $m-1$ steps,

$$T_{m-1}=2t^{m-1}\cos(m-1)\theta=\frac{4t^m\cos(m-1)\theta}{2t}=4t^m\cos\theta\cos(m-1)\theta,$$

and $t^2\ T_{m-2}=2t^m\cos(m-2)\theta$. Therefore, by (2),

$$T_m=2t_m\{2\cos\theta\cos(m-1)\theta-\cos(m-2)\theta\}=2t^m\cos m\theta.$$

§4. To prove (8), we observe, that, by hypothesis, the Law holds for the first step, that is, when $m=1$. Assume that it holds for m—1 steps. Then

$$kt_{m-1}=2t^{m-1}\sin(m-1\theta=\frac{4t^m\sin(m-1)\theta}{2t}=4t^m\cos\theta\sin(m-1)\theta,$$

and $kt^2t_{m-1}=2t^m\sin(m-2)\theta$. Therefore, by (4),

$$kt_m=2t_m\{2\cos\theta\sin(m-1)\theta-\sin(m-2)\theta\}=2t^m\sin m\theta:$$

which proves the Law universally.

§5. In equations (7) and (8), m may be negative as well as positive. The series (1), starting from the terms T_0 and T_1, may be carried not only forwards in the direction of the terms T_2, T_3, &c., but also backwards through the terms T_{-1}, T_{-2}, &c.; the relation expressed in (2) always subsisting. In fact, by (2),

$$T_1=T_0-t^2T_{-1}\therefore T_{-1}=t^{-2}=t^{-2}T_1.$$

In general, it is easily seen that

$$T_{-m}=t^{-2m}T_m. \quad\ldots\ldots\ldots\ldots\quad (9)$$

Similarly, $t_{-m}=-t^{2m}t_m.$ .... (10)

By equating the values of T_m in (9) and (7), we have

$$T_{-m}\,t^{2m}=2t^m\cos m\theta=2t^m\cos(-m\theta)$$

$$\therefore 2t^{-m}\cos(-m\theta)=T_{-m}.$$

In like manner, by equating the values of t_m in (10) and (8), we have

$$2\,t^{-m}\sin(-m\theta)=kt_{-m}.$$

§6. As an instance of the application of the formulæ which have been obtained, we shall now find cos $m\,\theta$ in terms of cos θ, m being a positive integer. In (5) substitute for T_m its value in (7), and replace t (see §2) by $(2\cos\theta)^{-1}$. Then

$$2t^m\cos m\theta=1-mt^2+\frac{mt^4}{\lfloor 2}(m-3)-\frac{mt^6}{\lfloor 3}(m-5)(m-4)+\&c.$$

$$\therefore 2\cos m\theta=\frac{1}{t^m}-m\frac{1}{t^{m-2}}+\&c.$$

$$=(2\cos\theta)^m-m(2\cos\theta)^{m-2}+\frac{m(m-3)}{\lfloor 2}(2\cos\theta)^{m-4}-\&c.$$

§7. In like manner, we may find $\sin m\,\theta$, m being a positive integer. In (6) substitute for t^m its value in (8). Then

$$2k^{-1}t^m \sin m\,\theta = 1-(m-2)t^2+\frac{(m-3)(m-4)}{\lfloor 2}t^4 - \&c.$$

$$\therefore 2k^{-1}\sin m\,\theta = t^{-m}-(m-2)t^{-(m-2)}+\frac{(m-3)(m-4)}{\lfloor 2}t^{-(m-4)} - \&c.$$

But because $2t\cos\theta = 1$, and $2t\sin\theta = k$, $k=\dfrac{\sin\theta}{\cos\theta}$. Therefore

$$\frac{2\cos\theta}{\sin\theta}\sin m\,\theta = (2\cos\theta)^m-(m-2)(2\cos\theta)^{m-2}+\&c.$$

$$\therefore \sin m\,\theta = \sin\theta\left\{(2\cos\theta)^{m-1}-(m-2)(2\cos\theta)^{m-3}+\&c.\right\}.$$

§8. Another very simple instance of the application of the formulæ which we have obtained is the following. By (2),

$$T_{n+1}=T_n-t^2T_{n-1}=T_n-t^2(T_{n-2}-t^2T_{n-3})=T_n-t^2T_{n-2}+t^4T_{n-3} = \ldots\ldots$$

$$=T_n-t^2T_{n-2}+t^4T_{n-4}-t^6T_{n-6}+\ldots+(-)^c t^{2c}T_{n-2c} + (-1)^{c+1}t^{2(c+1)}T_{n-2c-1}.$$

Substitute for T_{n+1}, T_n, &c., their values in (7), and divide by $2t^n$. Then

$$\cos n\,\theta-\cos(n-2)\,\theta+\cos(n-4)\,\theta-\ldots\ldots+(-1)^c\cos(n-2c)\,\theta$$

$$=t\left\{\cos(n+1)\theta-(-1)^{c+1}\cos(n-2c-1)\theta\right\}$$

$$=\frac{\cos(n+1)\theta-(-)^{c+1}\cos(n-2c-1)\theta.}{2\cos\theta}$$

In like manner,

$$\sin n\,\theta-\sin(n-2)\theta+\sin(n-4)\theta-\ldots\ldots+(-1)^c\sin(n-2c)\,\theta$$

$$=\frac{\sin(n+1)\theta-(-1)^{c+1}\sin(n-2c-1)\theta}{2\cos\theta}$$

MATHEMATICAL NOTES.

1. *On Linear Asymptotes in Algebraic Curves:*

A method of finding asymptotes, given by D. F. Gregory in Vol. IV., p. 42, of the *Cambridge Mathematical Journal* (to which my attention was called by Prof. Irving), is so elegant and simple that it is surprising it has not yet found its way into the text-books.

Let the equation to the curve, expressed in rational and integral form, be of n dimensions, and be arranged in homogeneous functions of x and y in descending order, as follows:

$$f_n(x, y) + f_{n-1}(x, y) + \ldots\ldots\ldots = 0$$

Then the equations to the asymptotes, (x', y' being current coordinates), are given by

$$\left\{ \begin{array}{l} f_n(x, y) = 0 \\ x' \frac{d}{dx} f_n(x, y) + y' \frac{d}{dy} f_n(x, y) + f_{n-1}(x, y) = 0 \end{array} \right\}$$

The expression is left by Gregory in this form, but a little further reduction will give it us in a shape in which the equation to an asymptote can at once be written down '·· inspection merely. Thus let $\frac{x}{l} - \frac{y}{m}$ be a factor of $f_n(x, y)$, and let $\phi(x, y)$ be the quantity containing the remaining factors, so that the equation to the curve may be written

$$\left(\frac{x}{l} - \frac{y}{m}\right) \phi(x, y) + f_{n-1}(x, y) + \ldots\ldots\ldots = 0$$

then the equation to an asymptote is

$$\left(\frac{x}{l} - \frac{y}{m}\right) \phi(l, m) + f_{n-1}(l, m) = 0.$$

The case of an asymptote parallel to one of the axes (*e.g.*, that of y) is included in this by making $l = 1$, $m = \infty$ and evaluating $(\phi : f_{n-1})$ in the usual way.

The method fails when the above equation becomes indeterminate by the simultaneous vanishing of ϕ and f_{n-1}, which can only happen when $\phi(x, y)$ contains the same factor $\left(\frac{x}{l} - \frac{y}{m}\right)$; that is, when there are parallel asymptotes. Perhaps the easiest way of treating this case is to substitute in the equation to the curve $f(x, y) = 0$, for x and y the quantities $lr + x$, $mr + y$, and to arrange in descending powers of r. Then, as before, $f_n(l, m) = 0$ will give the directions of the asymptotes, and the coefficient of the next lower power of r which does not identically vanish for these values of $l : m$, will, on being equated to zero, give the asymptotes.

This also shews clearly the reason of the occasional failure of the common rule, when terms of the second highest dimension are wanting, viz.: equate to zero the terms of the highest dimension. The rule succeeds when the expression of the highest dimensions consists of factors occurring singly, but may fail when the same factor occurs in it more than once.

2. *On a Reduction of Curves of the Second Order:*

In the modern system of analytical geometry, as pursued by Salmon, Puckle, and others, the curves of the second order, as represented by the general equation in Cartesian rectangular coordinates, are first separated into central and non-central, and the further reduction of the equation is then effected by transformation of coordinates, which is a rather long and troublesome process. It has occurred to me that this reduction might be simplified by following the course taken by Euclid with regard to the circle, namely, by seeking whether there exists a line (or lines) with regard to which the curve is symmetrical. For this purpose let us take the curves separately.

I. Central curves, $C^2 - AB$ is not zero, and the equation referred to the centre takes the form

$$Ax^2 + By^2 + 2\,Cxy = F.$$

Let the curve be cut by the line

$$\frac{x-\alpha}{l} = \frac{y-\beta}{m} = r,\ (l^2 + m^2 = 1) \ldots\ldots\ldots\ldots (1)$$

then we obtain a quadra[illegible] for the values of r at the points of section, by substituting for x, y, in the equation to the curve, and the coefficient of the simple power of r in this, is

$$Al\alpha + Bm\beta + C\,(l\beta + m\alpha),$$

and if this vanish, the values of r are equal and opposite, and (α, β) will be the middle point of the chord of section. Now this condition is

$$(Al + Cm)\,\alpha + (Bm + Cl)\,\beta = 0 \ldots\ldots\ \ldots\ldots\ldots (2)$$

and if $l : m$ be given, the locus of this equation is a straight line through the origin.

Now we can always assign such a value to $l : m$, that (2) shall be at right angles to (1). For the condition of perpendicularity is

$$\frac{Al + Cm}{l} - \frac{Bm + Cl}{m} = 0,$$

or,
$$l^2 - \frac{A - B}{C} lm - m^2 = 0$$

which, being a quadratic in $l : m$ with its last term negative, has necessarily real roots. (Indeed it shews that there are *two* directions, at right angles to each other, in which the chords may be drawn, and in fact gives the directions of the axes of the curve).

Hence there exists a straight line such that it bisects all chords of the curve drawn at right-angles to it; that is, such that the curve is symmetrical with regard to it.

Now let us take this line for the axis of x; then for any given value of x, the equation to the curve must be satisfied by $-y$ as well as $+y$, and this requires $C = 0$. The equation thus reduces to

$$Ax^2 + By^2 = F,$$

the form of it proving again that the axis of y is also a line of symmetry.

The equation is now reducible to the three known varieties, according to the nature of the intercepts of the axes, namely:

(1) .... the ellipse, $\frac{x^2}{a^2} + \frac{y^2}{b^2} = 1$;

(2) .... the hyperbola $\frac{x^2}{a^2} - \frac{y^2}{b^2} = \pm 1$, including two intersecting lines $\frac{x^2}{a^2} - \frac{y^2}{b^2} = 0$.

(3) ......wholly imaginary, $\frac{x^2}{a^2} + \frac{y^2}{b^2} = -1$.

II. Curves in which the centre is at an infinite distance, and $C^2 = AB$, the equation being

$$Ax^2 + By^2 + 2\,Cxy + 2\,Dx + 2\,Ey = F.$$

The same process as before demonstrates the existence of a line with regard to which the curve is symmetrical. Taking this for axis of x, we must have

$$C = 0, E = 0$$

But $C = 0$ requires either $A = 0$, or, $B = 0$

The latter reduces the equation to

$$Ax^2 + 2\,Dx = F$$

representing two parallel (or, it may be, coincident) straight lines; the former reduces it to

$$By^2 + 2\,Dx = F,$$

and by taking the origin on the curve, still further to

$$y^2 = Lx,$$

representing the parabola.

3. *On a method of Approximating to the Square Root of a Number:*

The following singular proposition is given by Murphy in his *Theory of Equations*, Art. 77, and is very characteristic of a mathematician, perhaps, the most original of modern times. The demonstration that follows is his own, somewhat simplified. Let N be the number, and let $\sqrt{N}$ be between n and $n + 1$. Put $N - n^2 = a$, $(n + 1)^2 - n^2$, or, $(2\,n + 1) = b$. Take any proper fraction $\frac{u_0}{v_0}$, and let a series of fractions be successively formed by the law

$$u_{x+1} = av_x + u_x,\ v_{x+1} = bv_x + u_x,$$

then $\frac{u_x}{v_x}$ converges to the decimal part of $\sqrt{N}$.

For, $\frac{u_{x+1}}{v_{x+1}} = \frac{av_x + u_x}{bv_x + u_x}$, and is a proper fraction since $a < b$,

$$= \frac{a + \frac{u_x}{v_x}}{b + \frac{u_x}{v_x}}$$

Let then $y = \text{Limit}\ \frac{u_x}{v_x} = \text{Limit}\ \frac{u_{x+1}}{v_{x+1}}$;

then ultimately $y = \frac{a + y}{b + y}$

$$\text{or, } y^2 + (b - 1)\,y = a$$

$$\text{and } y^2 + 2ny + n^2 = N.$$

$$\text{whence } y = -\,n + \sqrt{N},$$

since the positive sign must be taken.

$$\text{Hence, Limit}\ \frac{u_x}{v_x} = \sqrt{N} - n,$$

or $\frac{u_x}{v_x}$ converges to the decimal part of $\sqrt{N}$.

Murphy gives as an example $\sqrt{10}$. Assume the fraction $\frac{1}{6}$; then $a = 1$, $b = 7$, and the successive convergents are

$$\frac{1}{6}, \frac{7}{43}, \frac{25}{154}, \frac{179}{1103}, \frac{1282}{7900}, \ldots\ldots\ldots\ldots$$

of which the last written = 0·162278, which is correct for $\sqrt{10}$ except the final figure which should be 7.

He does not give any method of determining the limits of the error of any convergent, without which the process is of little practical use.

J. B. C.

ABSTRACT OF METEOROLOGICAL OBSERVATIONS, FOR THE YEARS 1861 & 1862, TAKEN AT STRATFORD, CANADA WEST.

BY CHARLES JOHN MACGREGOR, M.A.

HAVING been engaged, in my capacity of head master of the Grammar School, Stratford, in taking the observations required by law to be made at each county town in Upper Canada, I have thought that it would not be uninteresting to the members of the Institute, if I should lay before them the results of these observations for the years 1861 and 1862. I am induced to do so from the fact of having observed, in various numbers of the valuable Journal issued by the Canadian Institute, a notice calling on the members generally to furnish reports of any phenomena that may fall under their observation.

The instruments used were supplied by the Chief Superintendent of Education, and were, I believe, tested at the Provincial Observatory prior to their distribution to the schools. They consist of a barometer, dry and wet bulb hygrometer, maximum and minimum self-registering thermometers, rain gauge, and wind vane. The means are reduced from tri-daily observations taken at 7 a.m., 1 p.m., and 9 p.m. The self-registering thermometers are read at 9 p.m. each day. No observations are taken on Sunday. The thermometers are fixed in position in a shed attached to the Grammar School building, which protects them from being unduly influenced by radiation and the direct force of the wind.

An approximation made by means of the levels taken on the line of the Grand Trunk Railway, kindly furnished me by an engineer of the company, gives the height of Stratford above Lake Ontario at Toronto, as 948 feet, which will consequently make it 1182 feet above the sea level.

GENERAL METEOROLOGICAL REGISTER FOR THE YEARS 1861 AND 1862, TAKEN AT STRATFORD, CANADA WEST.

Elevation above the Sea, 1182 feet.

		January.	February.	March.	April.	May.	June.	July.	August.	Septemb'r.	October.	November.	December.	Winter. Dec. 1860 inclusive.	Spring.	Summer.	Autumn.	Year.
1861	Mean temperature	18°.57	24°.55	26°.77	42°.04	46°.59	62°.33	65°.94	64°.02	56°.47	49°.06	34°.37	31°.66	21°.43	38°.47	64°.10	46°.63	43°.53
	Highest temperature	37.8	50.1	53.6	72.4	70.1	83.9	84.2	90.4	76.1	67.8	50.6	57.5	...	...	...	...	90.4
	Lowest temperature	—20.4	—17.1	— 7.9	23.7	26.2	37.4	36.9	39.9	33.4	24.0	19.0	1.9	...	...	...	...	—20.4
	Monthly & annual ranges	58.2	67.2	61.5	48.7	43.9	46.5	47.3	50.5	42.7	43.8	31.6	55.6	...	...	...	...	110.8
	Mean max. temperature .	24.92	31.08	34.59	49.86	56.15	71.18	74.27	73.89	64.26	57.89	39.34	38.09	...	...	...	...	51.29
	Mean min. temperature...	9.27	15.14	17.44	32.42	36.76	48.58	52.38	53.71	47.06	39.20	28.40	23.56	...	...	...	...	33.66
1862	Mean temperature	19.99	19.87	27.15	40.03	53.18	61.25	65.57	65.73	58.06	47.73	34.22	27.64	23.84	40.12	64.18	46.67	43.37
	Highest temperature	40.1	38.1	40.8	69.2	76.9	79.6	86.0	86.0	79.2	74.6	59.7	52.5	...	...	...	...	86.0
	Lowest temperature	—17.7	—10.4	0.4	15.8	23.9	33.1	40.6	38.9	35.2	23.7	21.8	— 3.1	...	...	.	...	—17.7
	Monthly & annual ranges	57.8	48.5	40.4	53.4	53.0	46.5	45.4	47.1	44.0	50.9	37.9	55.6	...	...	..	...	103.7
	Mean max. temperature ..	26.11	26.76	33.29	47.82	63.98	71.16	74.25	74.80	67.91	53.93	39.18	33.38	...	...	...	...	51.10
	Mean min. temperature...	10.95	9.73	13.56	31.66	39.29	46.41	54.46	54.85	47.31	40.05	28.92	20.84	.	.			33.58
1861	Mean height of barometer	28.6947	28.6033	28.6745	28.6551	28.6717	28.7052	28.6788	28.7850	28.7351	28.6872	28.6235	28.8322	28.6749	28.6671	28.7250	28.6819	28.6955
	Monthly range	1.211	1.015	0.972	0.965	1.118	0.484	0.458	0.420	0.886	1.012	0.886	0.942	...	...	...	...	0.862
	Greatest daily range	0.649	0.951	0.888	0.498	0.638	0.453	0.226	0.298	0.521	0.595	0.627	0.761	.	..	...	..	..
1862	Mean height of barometer	28.7727	28.6710	28.5807	23.7934	28.7118	28.7003	28.6941	28.7678	28.8075	28.7292	28.7141	28.7532	28.7586	28.6953	28.7207	28.7503	28.7246
	Monthly range	1.150	1.018	0.878	1.045	0.713	0.860	0.680	0.526	0.801	0.896	1.227	1.146	...	..	...	...	0.912
	Greatest daily range	0.913	0.864	0.497	0.695	0.363	0.476	0.403	0.201	0.458	0.581	0.565	0.707	...	...	...	...	
1861	Mean humidity of the air	87	85	79	70	67	76	82	86	86	88	82	81	...	...	...	...	81
	Mean elas. of aqueous vap.	.096	0.124	0.129	0.200	0.219	0.430	0.536	0.520	0.402	0.31	0.166	0.159	...	...	...	...	0.275
1862	Mean humidity of the air	86	85	81	70	61	71	78	82	82	84	81	86	...	...	.	...	79
	Mean elas. of aqueous vap.	0.102	0.097	0.123	0.196	0.254	0.385	0.497	0.531	0.409	0.302	0.164	0.139	..	...	...	...	0.268
1861	Total amount of rain	0.4138	1.0439	4.4239	2.6122	2.4415	2.3068	4.4772	3.4602	2.9688	4.1070	2.3425	1.2157	...	...	...	..	31.8135
	Number of days of rain...	4	6	10	10	12	11	10	12	15	14	11	4	...	...	...	...	119
1862	Total amount of rain	0.4520	0.1528	2.8133	1.2464	0.9191	3.8408	4.7205	4.1436	4.0103	4.0904	2.9025	2.6885	...	...	...	...	31.9802
	Number of days of rain ...	4	2	6	7	4	8	10	9	11	15	10	5	...	...	..	..	91
1861	Total amount of snow......	25.8	26.7	9.6	5.5	0.6	...	...	...	...	3.0	2.3	6.9	...	...	...	...	80.4
	Number of days of snow...	15	13	13	3	1	...	...	...	...	1	6	7	...	...	.	...	59
1862	Total amount of snow......	24.8	17.0	22.9	Inapp.	...	...	...		...	0.9	4.1	11.1	...	...	...	...	80.8
	Number of days of snow. .	16	14	6	2	...	...	...	...	...	3	11	11	...	...	...	...	63

The following table gives the mean temperature and the mean height of the barometer in the different quarters, the winter quarter in each case being taken so as to include December of the preceding year.

	Winter.	Spring.	Summer	Autumn.
	°	°	°	°
1861—Mean temperature.............	21 43	38.47	64.10	46 63
1862—Mean temperature..............	23 84	40 12	64.18	46.67
1861—Mean height of barometer	28.6749	28 6671	28.7230	28.6819
1862—Mean height of barometer	28.7586	28.6953	28.7207	28.7503

Comparative view, in the years 1861 and 1862, of certain meteorological results :

	1861.	1862.
TEMPERATURE.		
Mean temperature of the year.	43 53	43.37
Warmest month	July	August
When the mean temperature of the month was..	65.94	65.73
Coldest month	January	January
When the mean temperature of the month was..	24 92	26.11
Difference between the warmest and coldest months	41.02	39 62
Warmest day	August 2nd	July 5th
When the mean of the day was	78 5	76.9
Coldest day	Feb. 8th	Jan. 14th
When the mean of the day was	—6.2	1 43
Highest temperature	90.4	86 0
Which occurred on	August 2nd	July 6th Aug. 8th
Lowest temperature	—20 4	—17.7
Which occurred on	Jan. 13th	Dec. 14th
Range of the year..........................	110.8	103.7
BAROMETER.		
Mean pressure of the year	28.6955	28.7246
Month of highest pressure	December	September
When the mean pressure of the month was......	28.8322	28 8075
Month of lowest pressure	February	March
When the mean pressure of the month was.....	28.6033	28.5807
Maximum pressure of the year	29.317	29.436
Which occurred...........................	Jan. 22, 9 p.m.	Nov. 5, 1 p.m.
Minimum pressure of the year	27.943	28 021
Which occurred...........................	May 7, 7 a.m.	March 3, 9 p.m.
Range of the year..........................	1.374	1.415

Comparative view of meteorological results—*Continued.*

	1861.	1862.
HUMIDITY.		
Mean humidity of the year	81	79
Month of greatest humidity	October	December
When the mean humidity of the month was	88	86
Month of least humidity	May	May
When the mean humidity of the month was	67	61
RAIN.		
Total depth of rain in inches	31.8135	31.9802
Number of days on which rain fell	119	91
Greatest depth in one month fell in	July	July
When it amounted to	4.4772	4.7205
Rainy days were most frequent in	September	October
When their number was	15	15
SNOW.		
Total depth in the year	80.4	80.8
Number of days on which snow fell	59	63
Greatest depth in one month fell in	February	January
When it amounted to	26.7	24.8
Days of snow were most frequent in	January	January
When their number was	15	16
WIND.		
Most windy month	February	April
Least windy month	August	July
AURORA BOREALIS.		
Aurora visible on	16	16
No. of nights on which it was possible to see aurora	166	162
do. do. impossible to see aurora	199	203
PERIODICAL PHENOMENA.		
Spring birds firs seen	March 5	March 17
Crows first seen	——	March 22
Thunder first heard	March 29	April 2
Wild pigeons seen	April 13	——
Mill pond free from ice	April 14	April 15
Swallows seen	——	April 15
Frogs heard	April 20	April 16
Latest snow of the season	May 1	April 23
Currant and lilac bushes in leaf	April 29	May 8
Plum trees in blossom	May 21	May 15
Forest trees in leaf	May 25	May 18
First hoar frost of autumn	——	September 3
First ice of the season	October 21	October 20
First snow of the season	October 23	October 22
Indian summer	——	October 30
Mill pond frozen	November 17	November 9

HALOES.—There were nine lunar haloes observed in 1861, and ten in 1862. Eight solar haloes were observed in 1861, and one in 1862.

Of these, the only one that deserves particular mention was a lunar halo observed on the 16th of June, 1861. It consisted of arcs of three circles. The arc of the circle round the moon had a paraselene near one of its extremities; at the apex of this circle was a tangent circle, apparently of the same radius as the first one; and through the centre of the tangent circle was a third, parallel to the last mentioned, but of a greater radius. The sky at the time was clear, with the exception of light *stratus* clouds, extending from the S.W. to the N. horizon, by which the lower portion of the first mentioned circle was obscured.

NOTICES SCIENTIFIQUES.

PAR. M. ARAGO.

Notice sur les observations qui ont fait connaître la constitution physique du Soleil et celles de diverses étoiles. Esamen des conjectures des anciens philosophes et des données positives des astronomes modernes, sur la place que doit prendre le Soleil parmi le nombre prodigieux d' étoiles dont le firmament est parsemé.

(*Lu dans la séance publique des cinq Académies, le* 25 *Octobre,* 1851.)

[This memoir is an excellent example of the popular and yet strictly scientific *résumes* which we owe to the pen of the illustrious Arago, and is also a good illustration of the charm of his peculiar style. Although recent researches have reversed some of his conclusions, as we have indicated in a few notes, the memoir itself will always be a classic in the history of science, as a thorough reaction of the state of knowledge at the time it was written. Ed.]

Towards the middle of the month of July last, astronomers belonging to the principal observatories in Europe, betook themselves to Norway, Sweden, Germany, and Russia, and fixed their stations in places where the solar eclipse of the 28th of that month would be total. They hoped that this phenomenon, studied with powerful instruments, would lead to plausible explanations of sundry appearances noticed in previous eclipses, on which nobody had dared to pronounce in a decisive manner. "What!" cried some ill-tempered spirits (little acquainted, I must suppose, with the history of astronomy); "What! can the science, which is called the most perfect of all, find still some problems to solve, even with respect to the body around which all the planetary movements are performed? Is it true, that in many respects we are not more advanced than the philosophers of ancient Greece?"

It has been thought that these questions should meet a serious reply, and I have undertaken the task of supplying it, not concealing from myself the dryness which must needs pervade it, nor forgetting that details, which have become at the present day elementary truths, will force themselves prominently under my pen; yet I have thought that your indulgence will not fail to one who is in the performance of a duty.

A general glance at the labors of ancient philosophers and modern observers, will readily prove that, if the sun has been studied for two thousand years, the point of view has often changed, and that, during this interval, science has made immense steps in advance.

Anaxagoras asserted that the sun was scarcely larger than the Peloponnesus. Eudoxus, who enjoyed a great reputation in antiquity, assigned to this star a diameter nine times greater than that of the moon. This was a great step, if we compare this value with that of Anaxagoras, but the number given by the philosopher of Cnidus was still enormously wide of the reality. Cleomedes, who wrote in the reign of Augustus, tells us that the Epicureans, his contemporaries, regarding only appearance, maintained that the real diameter of the sun did not exceed one foot.

Let us now compare with these arbitrary guesses the value which is deduced from the labors of modern astronomers, executed with the most minute care, and by the aid of instruments of extreme delicacy. The sun has a diameter of 357,000 leagues (of 4 kilometres.) There is some difference, we see, between this number and that adopted by the Epicureans.

Supposing the sun to be spherical, his volume is fourteen hundred thousand times that of the earth.

Numbers so enormous not being frequently employed in common life, and failing to convey a precise conception of the magnitudes which they imply, I shall here recall a remark which will enable us better to grasp the immensity of this solar volume. Imagine the centre of the sun to coincide with that of the earth; his surface would then not merely extend to the orbit in which the moon revolves, but would reach nearly as far again beyond.

These results, so remarkable for their immensity, possess all the certainty of the principles of elementary geometry which have served as their base.

The course which I have to pass over being sufficiently long, I will not enter upon any detailed comparison between the results (which are really absurdly small) at which the ancients stopped in estimating the distance of the sun from the earth, and those which have been deduced from modern observations. I shall limit myself to saying here, that it has been *demonstrated*—(and it is not without reason that I make use of so positive a word)—that it has been demonstrated, since the observation of the transit of Venus in 1769, that the mean distance of the sun from the earth is thirty-eight millions of leagues, and that, during summer and winter, his distance from us varies by more than a million leagues. Such is the distance of this immense globe, whose physical constitution modern astronomers have made some progress towards determining. In the ancient philosophers we find nothing on this subject which is worthy of occupying us for a moment. Their disputes on the question as to whether the sun were a fire pure or gross, eternal or capable of extinction, not being founded on observation, left in the deepest darkness the problem which the moderns have tried to solve.

The progress which has been made on this track dates from 1611. At that period, which is not far from the invention of the telescope, a Dutch astronomer (Fabricius) observed distinctly the apparition of some dark spots on the eastern limit of the sun, which, after advancing gradually to the centre, crossed it, and moved to the western edge, disappearing finally after a certain number of days. From these observations, frequently repeated since then, we can infer that the sun is a spherical globe, possessing a motion of rotation about an axis through its centre, the duration of which is twenty-five days and a half.

These dark spots, variable and irregular, but well defined in outline, have sometimes considerable dimensions; some of them have been observed of a magnitude more than five times that of the earth. They are generally surrounded by an *aureola* less luminous than the rest of the surface of the star, to which has been given the name of *penumbra*. This penumbra, first remarked by Galileo, and carefully observed, with reference to the changes it undergoes, by astronomers since his day, has led to a supposition with respect to the physical constitution of the sun, which at first sight looks singular enough.

The sun might be a dark body, surrounded by an atmosphere at some distance, which is comparable with the atmosphere of the earth, as being the seat of a continuous layer of opake and reflective clouds. To this first atmosphere might succeed a second, self-luminous, which has been called the *photosphere*, and this, distant more or less from the interior cloudy atmosphere, would determine by its outline the visible limits of the star. Pursuing this hypothesis, spots would be formed on the sun, as often as there occurred, in these two concentric atmospheres, corresponding openings *(eclaircies)* which permitted a view of the dark central body. Persons who have studied the phenomena with powerful telescopes—astronomers by profession, and competent judges—recognize in the hypothesis of the sun's physical constitution, which I am going to speak of, a satisfactory account of the observed facts, yet it is not generally adopted.* Some writers of authority would represent the spots to be merely *scoriæ* floating on the liquid surface of the star, and given out by the solar volcanoes, of which we have only a feeble image in those of our earth. It was desirable, therefore, that we should proceed, by direct observations, to determine the nature of the sun's incandescent matter. But when we reflect that we are distant from this star by an interval of thirty-eight millions of leagues, and that we can only communicate with his visible surface by means of the luminous rays which proceed from it, to propose this problem to ourselves seemed to be unjustifiable rashness. The recent progress in the science of Optics has, however, furnished the means of completely solving it, and certain details, which you will pardon my laying before you, will render this solution evident. Everybody at the present day is aware that physicists have been led to distinguish two kinds of light—natural and polarized. A ray of the first species possesses properties which are the same for each point of its contour; but it is not so for polarized light, where different sides of the rays have not the same properties. These differences are shewn in numerous phenomena which I need not here mention. Before going further, let us remark that there is something strange in the results which have logically led physicists to speak of different sides of a ray of light, thus drawing a distinction between one side and

* At the present day this hypothesis finds even still less favor. We shall see presently that the argument for the existence of this dark central body is inconclusive, and is opposed to more recent experiments. All the phenomena connected with the spots can be satisfactorily explained on the supposition that they are cloudy masses floating in the sun's atmosphere; of the reality of which clouds the subsequent remarks of the author leave no doubt. —(*Trans.*)

another; and the word *strange*, which I have used advisedly, will certainly appear natural to those who reflect that millions upon millions of these rays can pass together through the eye of a needle without interfering with each other.

The polarization of light has enabled astronomers to enrich their means of investigation, by the addition of some curious instruments which have already done good service, and among these is the one named the *polariscope*.

If you look directly at the sun through one of these polariscopes, you will see two images of the same intensity and tint—both white. Suppose, now, that you look at the sun's image reflected at the surface of water, or of a glass mirror. In the act of reflexion, the rays become polarized; the polariscope no longer gives two white and similar images, but on the contrary, they are tinted with most vivid colors, although their form does not undergo alteration. If the one is red, the other will be green; if the first is yellow, the second will have the violet tint, and so on—the two tints being always *complementary*, as it is called, that is, capable of forming white light by their mixture. Whatever be the process by which natural light becomes polarized, the colors are exhibited in the two images of the polariscope, just as if we had been looking at light reflected from water or glass. The polariscope, then, furnishes a very simple mode of distinguishing polarized from natural light.

It was for a long time thought that the light proceeding from any incandescent body reaches the eye in the condition of natural light, provided that in the passage it had not been partially reflected, or much refracted, but this proposition fails in certain cases. A member of the Academy has discovered that the light which proceeds, under a sufficiently small angle, from the surface of an incandescent body, whether liquid or solid, and even when it is not polished, offers evident traces of polarization, so that by passing into the polariscope it becomes decomposed into two colored portions (*faisceaux colorès*). The light which proceeds from a gaseous substance in the act of burning (as the gas which to-day illuminates our streets and shops) is, on the contrary, always in its natural state, whatever may have been the angle of emission.*

* The incandescent bodies of which the light emitted under different angles has been examined with the polariscope, are the following: *solids*, forged iron and platinum; *liquids*, melted iron and fused glass. According to these experiments, some one may say, you have a right to affirm that the sun is neither melted iron nor fused glass, but what authorises you to

The process in order to decide whether the substance which renders the sun visible is liquid, solid, or gaseous, will be nothing more than a very simple application of the preceding remarks, notwithstanding the difficulties which appeared to arise from the enormous distance of that star.

The rays which render visible to us the borders of the disk have evidently issued from the incandescent surface at a very small angle. If, then, the borders of the two images seen directly through the polariscope, appear colored, the light of these borders must proceed from a liquid body, for every supposition which would make the exterior of the sun a *solid* body is definitely excluded by the observation of the rapid change of form in the spots. And if the borders retain in the polariscope their natural whiteness, they are of necessity gaseous in character. Now, observations made by viewing the sun directly any day of the year through large polariscopes, fail to discover the least trace of polarization. Therefore the substance in combustion which defines the sun's outline is gaseous, and we can generalize this conclusion, because the different points of the sun's disk, by reason of the movement of rotation, come, each in its turn, on the border.

This experiment removes from the region of mere hypothesis the theory we have above indicated of the physical constitution of the solar photosphere.

We do not find any thing, properly understood, either in the arbi-

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generalise? This is my answer: according to the two only explanations that have been given of the abnormal polarization presented by the rays emitted under small angles, the results ought to be the same in all respects, except that of magnitude, whatever be the liquid examined, provided that the surface of emergence has a sensible reflecting power. The only case of exception might be that of an incandescent body which should be, as regards density, analogous to a gas, as, for example, the fluid of an almost ideal rarity, which many geometers have been led to place hypothetically at the extreme limit of our atmosphere, where the phenomena of polarization and color might possibly disappear. I am not ignorant that I should add weight to the results mentioned in the text, by discussing them in a photometric point of view. I possess all the materials for such an examination, but this is not the place to develope them. I will, however, here anticipate a difficulty. It ought to be remarked that the lights proceeding from two liquid substances may, according to the special nature of these substances, not be identical as regards the number and position of the dark lines of Fraunhofer, which occur in their prismatic spectra. These differences are of a kind to be considerably augmented by the differently constituted atmospheres which the rays have traversed before reaching the observer.—(*Author's note.*)

The experiments spoken of in the text and note have been objected to as inconclusive, by M. Kirchoff, on the ground that the liquids there examined were in a state of rest. If their surfaces were in much agitation, as that of the sun must doubtless be, the rays would be emitted at all angles, and every trace of polarization would probably disappear. In addition to this, it may be remarked that Arago takes no account of the possible effect which might be produced on the rays by passing through the sun's atmosphere.—(*Trans.*)

trary conceptions springing from the brilliant imagination of the ancient Greek philosophers, or in the relics of the labors of the most famous astronomers of the Alexandrian school, which can, even by a forced comparison, be likened to the results I am announcing. These results, let us loudly proclaim, are due entirely to the united efforts of observers of the 17th and 18th centuries, as well as, in part, to those of our contemporary astronomers.

Let us here notice a remark which we shall presently have occasion to apply when we endeavour to determine the physical constitution of the stars.

If the matter of the solar photosphere be liquid, and so the rays issuing from its border be polarized, we shall not merely see colors in each of the two images given by the polariscope, but they will be different at different points of the contour. If the highest point in one of the images is red, the point diametrically opposite in this image will also be red. But the two extremities of the horizontal diameter will both be green, and so on. If, then, we proceed to reunite, in a single point, the rays proceeding from all parts of the sun's limb, even after their decomposition in the polariscope, the mixture will be white. Such a constitution of the sun as I am here establishing will equally serve to explain the existence on its surface of spots not dark but luminous. The former, which are designated *faculæ* (*facules*), were first observed by Galileo; the others, of much smaller extent, and for the most part circular in form, were seen by Scheiner* and by him denominated *maculæ* (*lucules*), and give to the sun's surface a sparkling appearance. I may refer (a somewhat singular circumstance) the discovery of one of the principal causes of these faculæ and maculæ to an administrative visit I paid to a fashionable shop on our Boulevards.

"I have reason to complain of the gas company," said the proprietor of the establishment; "they ought to turn on to my goods the broadest part of this bat-wing jet, and yet often, through the negligence of their agents, they place it so as to throw the light edgeways." "Are you quite sure," replied one of the assistants, "that in this position the flame throws less illumination than in the other?" The doubt appearing ill-founded, and, I may say, even absurd, exact experiments were resorted to, and it turned out that a flame throws

* Scheiner's claim to the discovery is doubtful. John Fabricius and Galileo were the first observers of them, nearly contemporaneously, and Harriott also, a little later, made the same observations independently.—(*Trans.*)

the same quantity of light on an object whether the broad part or the edge of the flame is turned to it.* It follows from this that an incandescent surface of gas of a definite extent appears more luminous when we view it obliquely than under a perpendicular incidence; and consequently if the sun's surface presents inequalities, like our atmosphere when it is covered with dappled clouds, it ought to appear feebly illuminated in comparison in those portions of the inequalities which are presented to the observer perpendicularly, and more brilliantly in the portions oblique to him. Every conical cavity ought then to appear to us as a *lucule.* It is not therefore necessary for the explanation of the appearances to suppose the existence of millions of points more incandescent than the rest of the disk, or of millions of spots distinguished from the neighbouring regions by a greater accumulation of luminous matter.†

After having proved that the sun consists of a dark central body, of a cloudy reflective atmosphere, and of a photosphere,‡ we ought naturally to ask if there is nothing beyond, and whether the photosphere ends abruptly without being surrounded by a gaseous atmosphere, less luminous than itself and of feeble reflective power. This

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* If $2b$ be the length of the jet, considered as a luminous line, and h the distance of an illuminated small area from the centre, the ratio of the intensities of the illumination in the two cases will be as $\sqrt{\left\{1+\frac{b^2}{h^2}\right\}}$ to $1-\frac{b^2}{h^2}$ which if b be small compared with h is sensibly 1. (*Trans.*)

† We may add here the curious discovery of Mr. Nasmith, that the surface of the sun is mottled with an enormous number of lens-shaped or willow-leaved figures, disposed without the least attempt at symmetry. Also the fact of the decennial period of a maximum occurrence of the solar spots, and its coincidence with a corresponding maximum in the disturbance of the terrestrial magnetism due to the sun.—(*Trans.*)

‡ The recent researches of MM. Kirchoff and Bunsen, on the prismatic spectrum, which have led to the most beautiful discovery of modern times, have thrown an unexpected light on the question here discussed by Arago. The following brief resumé may be excused. The light proceeding from incandescent bodies, whether solid or liquid, gives a continuous spectrum when refracted through a prism, but when a flame in which such substances are volatilised is examined, the spectrum is found to be crossed by a number of bright lines of different colors, the number and position of such lines for each distinct substance being always the same. When a pure light is transmitted through such a flame, so as to overpower it, the bright lines become replaced by dark ones in the same positions. Now, when the solar beam is examined, it is found to be crossed by dark lines, which occupy the known places of the bright lines of various substances. It is thence inferred that the light of the sun proceeds from an incandescent solid or liquid body, and has passed through a vapor in which these substances are volatilised. Among the substances thus detected are sodium, lithium, iron, calcium, magnesium, chrome, nickel, cobalt, barium, copper, zinc, besides very many yet undetermined. Hence we are led to reject the hypothesis of Arago, (or rather of W. Herschel) and to adopt the more obvious supposition, that we really see the incandescent body of the sun through a transparent atmosphere, of considerable extent and feeble illumination, in which many known terrestrial substances exist in a state of vapor.—(*Trans.*)

third atmosphere would commonly disappear in the ocean of light by which the sun appears always surrounded, and which arises from the reflection of his rays by the particles composing the terrestrial atmosphere.

A mode of resolving this doubt presented itself, by choosing the moment in a solar eclipse when the moon completely covers the sun. Just at the instant when the last rays issuing from the borders of the luminary disappear behind the opaque screen formed by the moon, our atmosphere, in the region where the two bodies are projected, and the surrounding parts, cease to be illuminated.

Now we see what was the principal object aimed at by the astronomers who in 1842 betook themselves to the south of France, to Italy, Germany and Russia, where the solar eclipse of July 8 would be total.

In researches of every kind, the part played by the unforeseen is always immense. Thus the observers were strangely surprised, when, after the disappearance of the last direct rays of the sun behind the rim of the moon, and of the light reflected by the surrounding terrestial atmosphere, they saw some rose-colored protuberances, of from two to three minutes in height, shoot forth, so to speak, from the contour of our satellite. Each astronomer, following the ordinary bent of his ideas, arrived at a particular conclusion as to the cause of these appearances. Some attributed them to mountains of the moon, but this hypothesis will not bear a moment's examination; others would see in them only the effects of diffraction or refraction. But calculation is the touch-stone of all theories, and the most indefinite vagueness was found to accompany those of which I am speaking in their application to the phenomena under notice. Explanations which give us no precise account either of the height, the form, the color, or the permanence of a phenomenon, ought not to find place in Science.

Let us take up the idea, strongly recommended for a time, that the protuberances of 1842 were solar mountains whose summits passed beyond the photosphere covered by the moon at the moment of observation.

According to the most moderate computation, the height of one of these summits above the sun's disk must have been 19000 leagues. I am well aware that no argument based on the enormous amount of this height ought to lead to a rejection of the hypothesis. But we can forcibly upset it by remarking that these pretended mountains had large portions out of the perpendicular, which consequently in virtue of the sun's attraction ought to have overturned.

Let us cast a rapid glance at a fourth hypothesis, according to which these protuberances resembled solar clouds swimming in a gaseous atmosphere. We shall not find any physical principle which will prevent our admitting the existence of cloudy masses of from 25 to 30,000 leagues in length, with abrupt and irregular contorted outlines. Only, in following the hypothesis further, we shall claim the right to be astonished that no such solar cloud had ever been seen entirely separated from the limb of the moon, and it was to this point, the crucial test, that the researches of astronomers had to be directed. A mountain not being able to sustain itself without a base, there was only wanting a chance observation of a protuberance visibly separated from the moon's limb (and, by consequence, from the real border of the solar photosphere) to overthrow the hypothesis of solar mountains from top to bottom. But, let us here mark well, it is not in astronomical researches as in those of chemists and physicists. These latter have the power of varying at will the conditions under which they work, and of changing the nature of their results; but astronomers can exercise no influence on the phenomena they are studying, and are obliged to wait sometimes for centuries in order that the stars may present themselves in positions favorable for the solution of a difficulty.

In the present case, the doubtful points raised by the observations of 1842 have already been able to be submitted to a new experimental examination, during the last year. An eclipse of the sun was announced for August 8, 1850, which would be total in the Sandwich Islands. The naval captain, Bonnard, in command of our station at Otaheite, was struck by the happy idea of dispatching the engineer of bridges and roads, M. Kutscyki, from the island of Tahiti to Honolulu, the capital of the Sandwich archipelago. The account which we have received from this able observer contains the following passage:—"The part, detached and reddish in color, which was near the northern protuberance, has appeared completely separated from the limb of the moon." Later, in the eclipse of July 28, 1851, MM. Mauvais and Goujon, at Dantzic, and the foreign astronomers of great celebrity who had gone to divers points of Norway, Sweden, and North Germany, saw, all of them, at every station, a spot, likewise of reddish hue, which was separated from the moon's limb.

The observation of M. Kutscyki, and the concordant observations of 1851, put a stop without possibility of recurrence to those explanations of the protuberances which are founded on the supposition that there existed in the sun mountains whose summits extended considerably beyond the photosphere.

When it shall be rigorously proved that these luminous phenomena cannot be the effect of the inflexions which the sun's rays undergo in passing near the inequalities which border the moon's contour; when it shall be proved that these rosy tints cannot be assimilated with mere optical appearances—that they have a real existence, and are veritable solar clouds :—then we shall have a new atmosphere to add to the two of which we have already spoken, for clouds could not sustain themselves in a vacuum.*

Everyone now knows what the uncertainty is which remains as to a very special point in the sun's physical constitution. When we reflect that the phenomena which might serve to resolve all our doubts are habitually invisible and that they can only be seen during total eclipses of the sun—that such total eclipses are few in number—that, since the invention of the telescope, the astronomers of Europe and America have hardly had the opportunity of making proper observations on more than six occasions—no one will have a right to be astonished that, in the middle of the 19th century, the question raised by these mysterious red flames, of which we have spoken so much, is yet a subject of study.

After these examinations, of which you will pardon the length, let

* In order that these clouds might sustain themselves in a vacuum, it would be necessary that the centrifugal force arising from their circular motion should be at each instant equal to the gravitation which would tend to make them fall to the sun. It would be necessary to transform them into actual planets revolving about this body with an extreme rapidity. Such is, in substance, the explanation which M. Babinet has given of the protuberances of 1842, at the meeting of the Academy of Sciences, on 16 February, 1846. The reader will see, in the memoir of the learned academician, the ingenious considerations on which this theory reposes, and how it may be connected with the cosmogonic system of Laplace. I believe, now that the phenomenon has been minutely observed, that M. Babinet will find more than one difficulty in reconciling the immense velocity which he is forced to assign to the matter of those protuberances, with the relative immobility of those which were observed in 1851, and the change of height which they presented. These difficulties disappear when the spots are assimilated to clouds, floating in a solar atmosphere which has a rotatory motion of small rapidity. I would besides remark that the existence of this third atmosphere is established by phenomena of an altogether different kind, namely, by the comparative intensities of the rim and centre of the sun, and also, in some respects, by the zodiacal light which is so visible in our climates at the time of the equinoxes. But the question considered in this point of view would require details that I am forced to omit.—(*Author's note.*)

The existence of an atmosphere extending beyond the visible photosphere is certainly proved by its actual appearance in the shape of a corona or ring of light, which is seen to surround the sun during a total eclipse.—(*Trans.*)

us indicate in few words the series of measurements and deductions by which Science has been able to fix the sun's real place in the totality of the universe.

Archelaus, who lived 448 B.C., and was the last philosopher of the Ionian sect, said of the sun—"He is a star; only this star exceeds all the rest in magnitude." This conjecture (for that which is founded neither on measurement nor experiment deserves no other name) was certainly very bold and beautiful. Let us pass across an interval of more than two thousand years, and we shall find the relations between the sun and the stars established by the labors of the moderns on bases which defy all criticism. About a century and a half ago, astronomers sought to determine the distance of the stars from the earth. Repeated unsuccessful attempts seemed to prove that the problem was insoluble. But what are the obstacles over which genius united to perseverance cannot ultimately prevail? We have learned within the last few years the distance which separates us from the nearest stars. This distance is about 206,000 times the sun's distance from the earth, that is more than 206,000 times 38 millions of leagues. The product of 206,000 times 38,000,000 would too far exceed numbers we are in the habit of considering, to render it of any use to state. The imagination will be more struck by the immensity of this number if I connect it with the velocity of light. The star Alpha of the constellation Centaur is the earth's nearest neighbour, if indeed we may speak at all of neighbourhood when we are dealing with such distances as in this case. The light of Alpha Centauri takes more than three years to reach us, so that if the star were annihilated, we should still see it for three years after its extinction. When we remember that light traverses 77,000 leagues (308,000 kilometres) in a second of time, that the day is composed of 86,400 seconds, and the year of 365 days, we may well stand, as it were, aghast at the immensity of these numbers. Furnished with these data, let us transport the sun to the distance of the star which is nearest to us of all, then this circular disk so vast, which in the morning lifts itself so gradually and majestically above the horizon, and in the evening takes a considerable time to descend completely below that plane, will no longer possess sensible dimension even in the strongest telescopes, and its brightness will range it among stars of the third magnitude. You see, gentlemen, what has become of the conjecture of Archelaus! We may possibly feel a little humiliated at the result which reduces to so small a matter our place in the material world. But let us reflect that man has arrived at this result by drawing all from his own peculiar fund, and we shall recognise in this his elevation to the most eminent rank in the domain of ideas. Astronomical investigations may therefore well excuse a little vanity on our part.

Would that it were permitted to me to follow modern astronomers in their immortal career across the multitude of suns that glitter in the firmament!

We should observe them, in the first place, determining with the aid of their instruments the relative positions of these stars by cataloguing some hundred thousand of them. We know that the Elder Pliny was astonished that Hipparchus had endeavoured to observe 1022 of them, and that he compared this work to that of a deity! We should remark in some recent works complete enumerations which would show us that the number of stars visible to the naked eye in a single hemisphere—the Northern—is less than 3000—a result which is certain, but which, from its smallness, will strike with astonishment those who have vaguely examined the heavens in the fine winter nights. This astonishment would change its nature if we pass to the telescopic stai In this case, carrying the enumeration as far as stars of the fourteenth magnitude, the last we can perceive in our most powerful telescopes, we should find, by a calculation which furnishes only an inferior limit, a number greater than forty millions (forty millions of suns!!), and the distance of the furthest of them would be such that light would require from three to four thousand years to traverse it. We should then be amply authorised to say that the rays of light, these messengers so rapid, bring to us, if we may so speak, the very ancient history of these distant worlds.

A photometric investigation, of which the first hint is to be found in the Cosmotheoros of Huyghens, undertaken by Wollaston a short time before his death, would teach us that it would be necessary to unite twenty thousand stars like Sirius, the most brilliant of the firmament, in order to throw upon our globe a light equal to that of the sun.

Guided by the genius of William Herschel, we should examine the stars which are apparently in contact, and this great astronomer would prove to us that these stars, coupled together in some manner, do not merely appear to us near to each other by an effect of perspective, but are really in mutual dependence, and revolve about their common centre of gravity in periods of sufficiently short duration, which have already in certain cases admitted of determination. Observing that these double stars are of colors very unlike, our thoughts would naturally be carried to the inhabitants of the planetary bodies, non-luminous and turning about their own axes, which to all appearance revolve about these suns, and we should remark, not without a real anxiety for the works of the painters in these distant worlds, that to a day illuminated by a red light there succeeds, not indeed a night, but another day, of equal brilliancy, only illuminated by a green light. The comparisons of the positions of the stars determined at different epochs would prove to us that they are very improperly denominated *fixed*: that in fact they are in motion in space in different directions, so that in the course of time, the form of the actual constellations will be completely changed; that the absolute velocities of these stars are unequal, but that the velocity in one of the cases which have been determined with entire certainty is at the rate of twenty leagues a second; lastly that the sun, like all the other stars in this respect, is not stationary, and carries in his train the family of planets with which he is surrounded. We should be struck by the unequal distribution of the stars in the celestial sphere. In one place, we should see more than twenty thousand in an area equal to the tenth part of the moon's apparent surface. In another, in an area of the same extent, not a single luminous point would be visible, even with the best telescopes.

After having cast an attentive glance at the luminous matter scattered over immense spaces, which, by its agglomeration continued through centuries, seems capable of giving birth to new stars, we should discuss the noble conceptions of Wright, Kant, Lambert, and W. Herschel, on the constitution and dimensions of the milky way. Finally, some steps further in conjectural astronomy—that is to say, in that branch of the science which rests only on imposing probabilities and natural generalisations, there would be unveiled to us phenomena, which by their nature, or the enormity of the numbers which measure them, would cast the strongest minds into a sort of vertigo.

But let us leave these speculations, however worthy of admiration they be, to return to the main question which I proposed to treat in this note, and to try, if it is possible, to establish some connexion between the physical nature of the stars and that of our sun.

We have succeeded, by aid of the polariscope, in determining the nature of the substance which composes the solar photosphere, because, by reason of the large apparent diameter of this body, it has been possible to observe separately different points of his contour. If the sun were removed from us to the distance where his apparent diameter would be inappreciable, as that of the stars is, the method would become inapplicable. The colored rays, proceeding from different points of his contour, would then be found closely mingled, and we have already [illegible] that their mixture would produce white. It appears then that we must give up the application to stars not possessing sensible dimensions of the process which has led us so well to our goal in the case of the sun. There are however certain of these stars which lend themselves to this method of investigation. I allude to variable stars.

Astronomers have remarked stars whose brightness changes considerably. There are some of them which pass in a very small number of hours from the second to the fourth magnitude. There are others in which the change of brilliancy is much more decided. Such stars, very visible at certain epochs, disappear afterwards totally, to appear anew after intervals, longer or shorter, and subject to some slight irregularities. Two explanations of these curious phenomena present themselves to the mind. One of which consists in supposing that the star is not equally luminous at all points of its surface, and that it has a motion of rotation on its own axis. Consequently, it appears brilliant when its luminous face is turned towards the earth, and sombre when its dark face comes into that position. On the second hypothesis, a satellite, opaque and not self-luminous, revolving about the star, would periodically eclipse it.

In reasoning on one or other of these two suppositions, the light which is sent to us some time before the disappearance or the reappearance of the star, has not issued from all the points of its contour, and there can no longer be occasion for the complete neutralisation of the tints we just now spoke of. If a variable star, examined with the polariscope, remains perfectly white in all its phases, we may be sure that its light proceeds from a substance like our clouds or burning gases. Now, such is the result of the small number of observations that we have yet been able to make, and which it will be of much utility to complete.* This same mode of investigation requires more care, but succeeds equally well when it is applied to stars which

* I am not aware that these experiments have been successfully prosecuted, but the method of prismatic examination of Kirchoff and Bunsen, alluded to in a previous note, has been applied with success to various stars, and has resulted in similar conclusions to those drawn in the case of the sun.—(*Trans.*)

undergo only a partial variation of brightness. The result to which these observations lead us, and which we can, I believe, generalise without scruple, can be announced as follows :—our sun is a star, and its physical constitution is identical with that of the millions of suns with which our firmament is everywhere strewn. I have been compelled, in the duty which was committed to me in the commencement, to give a sketch of all our present knowledge relative to the volume, distance, and physical constitution of the immense globe which illuminates us. This sketch, within the prescribed limits, will be sufficient to undeceive those persons who had thought it necessary to call in question the importance and certainty of the results obtained by modern astronomers. They will acknowledge, if they are candid, that in the history of the progress of knowledge, a progress which will without doubt be unlimited, the labors of the astronomers of the nineteenth century will not pass unperceived. As to criticisms not inspired by the love of truth, they would not deserve to fix for a moment the attention of this assembly, and I think that I may, for my own part, pass them by with contempt.—*Translated from the "Annuaire du Bureau des Longitudes pour l'an* 1852."

J. B. C.

SCIENTIFIC AND LITERARY NOTES.

THE ENTOMOLOGICAL SOCIETY OF CANADA.

A meeting of Canadian Entomologists was held at Toronto, in the rooms of the Canadian Institute, on Thursday, the 16th of April, for the purpose of taking into consideration the propriety of forming a society for the advancement of Entomological pursuits.

The following gentlemen were present:—Rev. Prof. W. Hincks, F.L.S.; Prof. H. Croft, D.C.L.; Beverly R. Morris, Esq., M.D.; J. H. Sangster, Esq., A.M.; and J. Hubbart, Esq., of Toronto. Thomas Cowdry, Esq., M.D.; and H. Cowdry, Esq., York Mills. Rev. C. J. S. Bethune, M.A. Cobourg; and W. Saunders, Esq., London.

Prof. Hincks was appointed chairman, and Mr. Bethune Secretary *pro tem.*

Letters of apology for non attendance were read from E. Billings, Esq. F.G.S., Montreal; R. V. Rogers, Esq., Kingston; F. Reynolds, Esq., Hamilton; B. Billings, Esq., Prescott; Rev. V. Clementi, B.A., Peterboro'; and E. B. Reed, Esq., London. These gentlemen expressed deep regret at their inability to attend, and pledged themselves to do all in their power to further the interests of the society.

The following resolutions were then unanimously adopted.

1st. That a society be formed to be called the Entomological Society of Canada; consisting of all students and lovers of Entomology, who shall express their desire to join it, and conform to its regulations.

2nd. That its officers shall consist of a President, a Secretary, Treasurer, and a Curator; to be elected annually, at the first general meeting in each year; whose duty it shall be to manage the affairs of the society.

3rd. That the annual contribution of members shall be two dollars, to be paid in advance.

4th. That application be made to the Canadian Institute for the use of a room in their building for the purposes of the society.

5th. That two separate collections be formed, a general one to be the property of the Canadian Institute, and a duplicate one to be the property of the society, and to consist of all surplus specimens contributed to the Society by members; and that all members be at liberty to exchange species for species under the supervision of the Curator.

6th. That meetings be held at 3 p. m., on the first Tuesday in each month, and that special meetings may be called when necessary by the officers.

7th. That Prof. Croft be President for the present year; that Mr. W. Saunders be the Secretary-Treasurer, and Mr. J. Hubbart the Curator.

8th. That the President be authorized to bring the subject before the council of the Canadian Institute at its next meeting.

The following papers were then read to the society :—Insect life in Canada; March and April," by the Rev. C. J. S. Bethune; and a synopsis of Canadian arctiidae" by W. Saunders: the latter illustrated by a complete series of specimens.

A number of interesting insects were brought to the meeting for inspection, chiefly from the collections of Dr. Morris and W. Saunders. Among others, Canadian specimens of the following were much admired. Limenitis ursula, Vanessa cænia, Mellitæa nycteis, M. phaeton, Thecla niphon, T. mopsus, T. laeta, Lycæna neglecta, Polyommatus dorcas, Hesperia mystic, H. wamsutta, and Pamphila numitor. A specimen of Colias eurytheme, though not itself Canadian, was regarded with great interest, from the fact that a specimen had been captured last fall, near St. Catharines, by D. W. Beadle, Esq.

The pretty little moths, Glaucopis semidiaphana and Melanippe propriaria, were duly represented, also beautiful specimens of Arctia dione, and sphinx drupiferarum.

Magnificent specimens of Ceratocampa regalis, and Dryocampa imperialis, were exhibited, and, although not natives, the probability of their being yet found with us, gave them an additional interest.

Among the Coleoptera we observed some rarities; for example, Xyloryctes satyrus, Canthon chalcites, Chlænius lithophilus, Colosoma frigidum, Geotrupes splendidus, Bolbocerus Lazarus, Aphonus frater, and Leptura nitens; all natives of Canada.

After a careful examination of all that was interesting, the meeting adjourned each one highly pleased with the results of the gathering.

The application for the use of a room, in the building of the Canadian Institute, for the purposes of the Society, was brought before the Council, by the President at their meeting, on Saturday, the 18th, when they very liberally granted it, free of expense.

The Society thus formed, will, we trust, be a prosperous one The number of Entomologists in this country is not large, but they are amply sufficient to sustain an organization of this sort. The advantages the Society offers to its members are not by any means small. The general collection will be open to all for purposes of reference and comparison, and will thus afford valuable opportunities to those who wish to name their specimens; while the cabinet of duplicates will offer means of exchange with all parts of Canada. It is intended that duplicate copies of Entomological papers published by those connected with the Society shall be left with the curator for distribution among members. It is probable, also, that as soon as the funds will permit an Entomological library will be added to the other attractions in the Society's rooms: and that a stock of pins will be purchased from which members may obtain supplies at cost price.

That the meetings of the Society may be made as interesting and attractive as possible, it is desirable that members from a distance would furnish short monthly records of interesting captures in their localities, accompanied, where convenient, with specimens of the insects spoken of.

All lovers of Entomology may become members of the Society by remitting the amount of the yearly subscription to the Secretary-Treasurer,

William Saunders, London, C. W.

THE GORILLA.

During the last meeting of the British Association at Cambridge a smart attack was made upon Professor Owen's views respecting the importance of the characters of the brain in man as distinguishing him from the monkeys as well as from inferior animals, by Professor Huxley, supported by Professor Rolleston and Mr. Flowers. It is known to all students in zoology, who attempt to keep up with the times, that Professor Owen some time since proposed an improved arrangement of Mammalia, in which the leading divisions are made to depend on the degree of development of the brain. In this system there are four primary divisions; one of which is occupied by man alone, whilst in the second the Quadrumana (the ape and monkey tribe), with the Carnivora and other important tribes of Mammals, form an extensive group. The tabular view of Professor Owen's plan was given in this *Journal* at the time of its publication, and may be referred to by our readers. Professor Huxley immediately called in question the importance, and, to a certain extent, the reality of the distinctions drawn by Owen, and a controversy has been carried on for several years. On the present occasion Professor Owen brings before the Natural History section of the association a paper entitled, "On the Zoological significance of the Brain and Limb-characters of Man, with remarks on the cast of the Brain of the Gorilla." The main object of this paper is to justify the system previously proposed by a further exposition of the differences between the Human brain and that of the Quadrumana, as seen in its highest form in the Gorilla. In a paper published as an appendix to his lecture on Sir Robert Reade's foundation, delivered before the University of Cambridge in 1859, Owen had fully given his reasons for continuing to place the Ourangs above the long armed apes, and for regarding the Gorilla as the highest known development of Quadrumana. He now, therefore, by means of a cast from the interior of the Gorilla's skull, brings the brain of this animal, which may be taken as the nearest approach to man, into direct comparison with the Human, and he considers the result as confirming his previous conclusions. "In the brain of man the posterior lobes of the cerebrum overlap, to a considerable extent, the cerebellum; whereas in the Gorilla the posterior lobes of the cerebrum do not project beyond the lobes of the cerebellum. The posterior lobes in the one are prominent and well-marked, in the other deficient. He had placed man—owing to the prominence of the posterior lobes of his brain, the existence of a posterior cornu in the lateral ventricles, and the presence of a hippocampus minor in the posterior cornu,—in a distinct subkingdom, which he had called Archencephala, between which and the other members of the class Mammalia the distinctions were very marked, and the rise was a very abrupt one."

We know not whether Professor Owen availed himself of the cast of the Gorilla's brain, not merely to confirm a previous argument, but specially to invite the renewal of an old controversy; however this might be, in the assembly he addressed he must certainly have anticipated opposition, and this was offered with less of moderation and respectful consideration than a sense of decorum seemed to demand We refer especially to the remarks of Professor Rolleston, though Professor Huxley's observations had enough of vehemence. He commenced with a very just remark that "the question was partly one of facts, and partly one of reasoning." The question of fact was, what are the structural differences between man and the highest apes? The question of reasoning, what is the systematic value of those differences?" But there are difficulties here. A large proportion of those who are interested in such inquiries, and know how to appreciate evidence brought before them, have never, or very seldom, had the opportunity of examining the brain of any monkey, or even in favourable cases have seen for themselves a very small variety. They must, therefore, receive the facts from others, and if those on whose knowledge, skill, experience, and intention to make known the truth they most rely, flatly contradict one another on the most essential points, what becomes of the foundations of their belief, or with what advantage can they proceed to reason on the application of facts themselves altogether uncertain?

Here is Professor Huxley's statement as reported: "Professor Owen had made three distinct assertions respecting the differences which obtained between the brain of man and that of the highest apes. He asserted that three structures were 'peculiar to and characteristic' of man's brain—these being the 'posterior lobe,' the 'posterior cornu,' and the 'hippocampus minor.' In a controversy which had lasted for some years, Professor Owen had not qualified these assertions, but had repeatedly reiterated them. He, (Professor Huxley) on the other hand, had controverted these statements; and affirmed, on the contrary, that the three structures mentioned not only exist, but are often better developed than in man, in all the higher apes. He now appealed to the anatomists present in the section, whether the universal voice of continental and British anatomists had not entirely borne out his statements and refuted those of Professor Owen."

This is very strong language. As to certain anatomists present, no doubt the learned professor knew that he could rely upon them to support his views, but we are exceedingly mistaken if their verdict would be confirmed by the great body of those conversant with the facts. At all events, those who are obliged to take their data from others find, in this case, that, respecting matters of fact, two of the very highest authorities are directly opposed; and upon what are they to rely? They may, indeed, remember, as accounting in some degree for the different perceptions of the observers, that Professer Owen believes in the reality and permanence of species, and regards the various degrees of development of the nervous system as likely to furnish the most important of all characters; whilst Professor Huxley warmly defends the Darwinian theory respecting the origin and changes of species, and professes to regard the superiority of man to other animals as independent of structural differences. The former, in studying the brains of the animals nearest to man, would be watchful for good distinguishing characters, and acutely sensible to any which presented themselves: the latter would look at the same objects to search for analogies, and to find out the course of transition from one structure to another. We cannot but think that much of the difference in this case is, not as to what is actually seen, but as to the importance and real meaning of the appearances; and we must add that Professor Owen's view is strikingly confirmed by the gradations in the Mammalian brain throughout the lower forms, the extension of the cerebral hemisphere anteriorly, and still more manifestly posteriorly, being the invariable accompaniment of every elevation of structure.

Professor Rolleston, in professing to specify the real differences between the brain of man and that of the apes, which he accused Owen of neglecting, points out, from Gratiolet, striking particulars of real importance, though not what Owen had taken for the character of his Archencephala, but the assumption that Owen was ignorant of or neglected these particulars because they did not enter into his differential character, for which purpose they were not well suited, is most unfair. The presence of gyrations on the brain is assumed by him as the distinction of his second great division, being thus taken as a sign of more advanced development than when these gyrations are absent, the natural conclusion being that they would be still more fully developed in the higher division. And this was even expressly stated by Owen, as in his Cambridge lecture, in 1859, where he concludes his character of the Archencephala in these words: "The superficial grey matter of the cerebrum, *through the number and depth of the convolutions*, attains its maximum of extent in Man." Besides all this Professor Owen was able to repel the charge of overlooking this peculiarity by referring to his lectures on the convolutions of the brain, delivered "almost at the very time when Leunet wrote his memoir on the subject," and the diagrams of which are still in the Museum of the Royal College of Surgeons. Without neglecting or undervaluing other accompanying signs of higher development, which all confirm his view. Professor Owen took the posterior enlargement of the cerebral hemispheres so as to cover the cerebellum, with the other characters above noted in connexion with this, as forming the best technical distinction of that highest Mammalian group which he named Archencephala. He insists on the reality of the character and contends for its importance. As to the matter of fact, with directly opposing testimony where wilful falsehood cannot be attributed to either party, it is not always easy to find out the truth, but the deliberate assertion of one of the ablest observers and perhaps the most experienced of his time, supported by figures professing to be carefully taken from nature must have great weight, and it seems probable that the counter-statements of some very eminent men are due rather to difference of expression and interpretation than of simple fact.

Taking a general view of the whole subject it is antecedently probable that the comparative development of the brain should furnish the most important characters and no one can reasonably doubt the practical result of Owen's system being a great improvement in the natural arrangement of Mammal'a. In the case of the Lyencephala the peculiar character of the brain corresponds with a lower type of the reproductive system and the section is unquestionably natural. In respect to Gyrencephala and Lissencephala the difference in the character of the brain is very striking and the groups thus associated are felt to be natural, nor are the exceptions more numerous on either side than are always to be expected in the way of special modifications and transition forms where we are obliged to place objects according to their affinities, though departing from technical characters. Now we are none of us perhaps disposed to consider the difference between man and the highest Gyrencephala as less than that between these and the Lissencephala. In some way we feel sure that the human race is elevated above all other creatures, and as in all other known instances superiority is connected with special development of the brain, that is what we have to expect here; as in other instances the enlargement forward, backward, and by gyrations of the cerebral hemispheres is the test of elevation of structure, so here it is what we are led to expect. We know that in the human brain, even in the lowest varieties of our race, the posterior cerebral lobes cover and even pass beyond the cerebellum. We are assured that even in the highest section of the Quadrumana, which must be admitted to be on the whole nearest in structure to man, this is never the case. We have thus a character drawn from the structure of the brain, confirmatory of all other reasons for assigning to man that elevation in a Zoological system which in other ways we know him to possess, and it is really easier to suppose that some ingenious and able men are led away by theoretical prepossessions in their mode of estimating and expressing what they see than to question the direct testimony of one who has done more for the comparative and theoretical anatomy of the Vertebrate sub-kingdom than any of his contemporaries and has himself dissected the brain of at least one species in nearly every genus of Mammals, when what he tells us is probable in itself and agrees with many important statements by others. Until we obtain better evidence we must for ourselves adhere to Owen's views and believe the facts he records. Some of our readers may have smiled at Charles Kingsley's witty application of the Darwinian theory in his Fairy tale of the Water babies. His moral is an excellent one, but neither men passing into apes, nor apes passing into men, accord with our ideas of the position given us by our Creator. We cherish the belief in an essential and permanent structural distinction, and Owen's account of its nature, if not true, is very plausible, and is certainly not yet shewn to be false.

W. H.

AURORAL ARCH OF APRIL 9TH.

A very remarkable auroral phenomenon was recorded at the Magnetic observatory, Toronto, on the night of April 9th.

About 8 P.M. a bright luminous band of extraordinary brilliancy was observed extending from E.S.E. to W.S.W. through or a little to the South of the zenith. The band was at first stationary with an uniform width of about 3° or 4° and with well defined edges.

At 8 30 luminous lines without any apparent movement were observed to fringe the northern edge through about 15° of its length on each side of the zenith. The fringe thus formed tapered off to points at both extremities, the width of the centre being about 7°.

At the same time the southern edge, from its eastern extremity to about 20° west of the zenith, formed the boundary of a fringe of small streamers ascending upwards from the south, and which on reaching the arch were deflected so as to form in appearance the material for the supply of a mass of luminous clouds which rolled tumultuously along the track of the band with enormous rapidity. The motion as far as the eye could judge consisted in a *transfer* of luminous matter and not as on ordinary occasions in undulations or pulsations, a circumstance constituting the chief peculiarity of this display.

From 9 to 9 30 when the arch was contracted in its length about 10° at each extremity, the rays on the northern edge had disappeared, and the streamers were limited to the eastern extremity, but there was no abatement in the mass or velocity of the luminous torrent. The arch then became irregular in its form; by 9 40 it had disappeared, but returned again in a less developed state and continued from about midnight till 2 A.M., when it ceased.

A magnetic disturbance was going on during the earlier part of the display, which ceased with the departure of the arch and recommenced with its reappearance. The extent of the disturbance, in harmony with what commonly occurs during the presence of bands at right angles to the magnetic meridian, was much less than in the case of auroral movements emanating from the north.

The Rev. Vincent Clementi of Peterborough writes that the arch as seen by him appeared first in the north, simultaneously with and apparently forming part of the aurora; that it disconnected itself with the aurora and passed onward with great rapidity until it crossed the zenith, where it remained stationary in the south stretching eastward and westward, streamers or rays breaking from it through the whole extent of its southern edge. The form of the band according to a sketch which he has kindly forwarded was not precisely the same as that seen at Toronto being much wider at the centre and converging to a point at both extremities, with a secondary band with its western extremity coinciding with that of the primary and with its eastern extremity immediately under its centre. He further states that the arch was of such extreme tenuity that stars shone through it with seemingly no diminution of their brilliancy.

The chief points of difference between the phenomena at the two places consist in the tenuity of the luminous matter as seen at Peterborough, the circumstance that there the streamers proceeded *from* instead of *into* the arch, as at Toronto, and that Mr. Clementi makes no mention of the apparent transfer of the luminous matter which here formed the chief peculiarity.

Luminous arches extending through the zenith in a direction perpendicular to the meridian, though not an ordinary accompaniment of the aurora, have been noticed before at Toronto and elsewhere, though rarely attended with the peculiar appearances which marked the display of April 9th.

G. T. K.

NOTE ON THE SPECIES MONOHAMMUS.

To the Editor of the Canadian Naturalist.

In the December number of the *Canadian Naturalist* Mr. Billings has described some of the pine-boring beetles of Canada, of the genus *Monohammus*, and mentions that the *M. titillator* is cited by Mr. Couper and Mr. Ibbetson as occurring at Toronto, but is of opinion that the insect described is the *M. confusor*.

I can confirm this idea of Mr. Billings, as the insects in my own collection and in that of Mr. Ibbetson were named on reference to Harris' work. The description agrees very closely with the reddish brown specimens mentioned by Mr. Billings as having been obtained from Toronto, where from my own observations they seem to be much more common than those of a cinereous tint.

Moreover the drawing of *Monohammus titillator* in Olivier's work agrees very well with these specimens. Those in my collection are mostly of the same size as the *M. confusor* and generally a little more robust, but are probably only a variety. The *M. scutillatus* is moderately common about Toronto, but the *M. marmoratus* quite rare; the latter easily distinguished by its smaller size, its rugosely punctured thorax, and the elytra mottled with brown and grey.

In my collection there is also a crippled specimen very like *M. scutillatus* but the elytra are covered with large white spots, in this respect resembling Leconte's *M. fatuor*, which however is now referred to *M. marmoratus*.

In the recent edition of Harris' work the name *titillator* is still employed.

H. C.

ON GROUND-ICE, OR ANCHOR ICE, IN RIVERS.

BY PROFESSOR JAMES THOMPSON.

In this paper the author described the two principal modes of growth of ice, in still water and in running water. In still or slowly moving water the ice forms itself as a crust on the surface, because, as the water cools from about

40° F. down to the freezing-point, it expands, and therefore becomes lighter, and remains floating at the surface, and then, on freezing there, it expands still further, and therefore still more tends to float. In rapidly-moving river water, on the contrary, and especially at the foot of rapids, ice is often found to grow attaching itself to the rocks or stones forming the bed of the river, as a spongy or porous mass, which, seen in the aggregate and not examined minutely, presents a general appearance not unlike the spawn of frogs. In large rivers in cold climates, as, for instance, in the St. Lawrence, immense quantities of this ice, called ground or anchor ice, are found to accumulate with astonishing rapidity. These accumulations of ice, by damming up the water, cause great floods, and by yielding to the force of the water, and moving down with the current, especially after they have become jammed and heaped up with other ice formed on the surface, act in producing very striking geological effects in disturbing the bottom and banks of the river, and in shoving along huge boulders which otherwise would remain immoveable. The ground and surface-ice also, by their shoving-action, introduce formidable difficulties and dangers in the construction of bridges or other engineering works requiring to be founded on the beds of rivers in cold climates. In the construction of the Great Victoria Bridge across the St. Lawrence at Montreal (the most costly bridge which has ever been executed), these difficulties have been successfully overcome, and a structure has been raised which is likely to stand secure against the much-dreaded forces of the ice. On account of the tendency both of water approaching to the freezing-point and of ice to float, it has long been regarded as rather a singular circumstance that ice should ever be found growing at the bottom of a river. From among the many suggestions which have been offered at various times to account more or less completely for the phenomenon, the author sets out by accepting as quite correct the view that the essential difference between the circumstances of the freezing of lake and river water is, that in the former case the water is left undisturbed to the action of the cold, and is allowed to adjust itself in strata in which the coldest parts, being also the lightest, float to the top; while in rivers the whole water is, by mixing, due to its rapid flow, brought to an uniform temperature at the freezing-point from top to bottom, and is thus brought into a condition in which it is ready to freeze at any part where additional cold may be applied. He is not, however, satisfied with any of the numerous suggestions which have been offered to account for the growth of the masses of spongy ice at the bottom, rather than that the ice should be found at the top, or in a state of mixtnre with the water throughout its depth. Some, for instance, have thought that radiation from the bottom to a cold sky (see paper by the Rev. James Farquharson, Philosophical Transactions, 1835) would cause ice to grow at the bottom of the river much in the same way as hoar-frost grows on land. Arago, having rejected the supposition of radiation being the cause, assigned two other reasons: first, that there might be expected to be a peculiar aptitude to the formation of crystals on the stones and asperities at the bottom, like as there is found to be a special readiness for the formation of crystals on rough bodies in saline solutions; and secondly, he supposed that the existence of less motion of the water at the bottom would favour the growth of the crystals there. As against this view, the author of the present paper states, first, that the water of a rapid river when freezing has abundance of small spicula or fragments of ice floating diffused through it, every one of which offers at least as free a point for the reception of new ice crystallizing from the water as can be presented by asperities on the bottom: and secondly, that the slower motion at the bottom would not favour the occurrence of freezing of new ice there rather than at the top, but that, on the contrary, if effects on the tendency to crystallization are to be sought for in such a slight cause, it should rather be taken that the greater fluid friction at the bottom, and the heavier pressure there, are causes slightly, *but certainly very slightly*, tending to oppose the freezing of new ice at the bottom.

Mr Hodges, the engineer of the contractors for the great bridge across the St. Lawrence at Montreal, in his large and valuable work recently published (in 1860) on the construction of that bridge, desciibes the ice-phenomena of the St. Lawrence, wh'ch he had been obliged during many years to watch and inquire into with anxious care: and in respect to the origin of the ground-ice, he supposes that the water in passing down rapids may become aerated by the rapidity of the current, and that particles or globules of cold air, being whirled by the eddies till they come in contact with the rocky bed of the river, attach themselves to it, and there give out cold which they have brought with them from the very cold atmosphere above, and so induce the freezing of ice around themselves in adhesion to the bottom of the river. As against this speculation, the author of the present paper states that the cold which could be conveyed down into the water by small bubbles would be totally inadequate to produce the results in question, and that any freezing which small bubbles of air could produce would occur during the period of their eddying about through the water, rather than at a later time, when their temperature would be assimilated to that of the water. The author's view, which it was the chief object of the paper to present, is that crystals or small pieces of ice are frozen from the water at any part of the depth of the stream, whether the top, the middle, or the bottom, where cold may be introduced either by contact or radiation, and that they may be supplied in part by snow or otherwise; and that they are whirled about in currents and eddies until they come in contact with any fixed objects to which they can adhere, and which may perhaps be rocks or stones, or may be pieces of ice accidentally caught in crevices of the rocks or stones, or may be ground-ice already grown from such a beginning. The growth of the ice by adhesion of new particles formed elsewhere he attributes to the property of any two pieces of moist ice to adhere when brought into contact, wh ch has been a subject of much discussion of late years, and of which the author's views are to be found in various recent papers in the 'Proceedings of the Royal Society,' and have also been submitted from time to time to the Belfast Natural History and Philosophical Society. He is confident that the anchor ice is not formed by crystallization at the place where it is found adhering. He is aware that the idea has sometimes been mooted, that snow falling into rivers might somehow be converted into anchor ice; but he is not aware that hitherto any explanation has been offered coupling the formation of the anchor ice with the property of ice now commonly designated as "regelation," but which until late years was not very generally known or understood, more especially as a property capable of bringing about the union of small pieces of ice floating freely under water: and the mode of growth of ground-ice is, he believes, as yet commonly regarded as an unsettled point, no opinion offered having received very decisive or general assent.—*Proceedings of the Belfast Natural History and Philosophical Society*, May 7, 1862.

DRY COLLODION PROCESS IN PHOTOGRAPHY.

Mr. Sutton claims to have discovered a process with dry plates which gives all the rapidity and keeping properties of the well-known trade secret of Dr Hill Norris. The following account is extracted from the "Photographic Notes" Oct. 15, 1862.

The problem which has most interested photographers of late years has been the discovery of a dry collodion process, by which plates can be prepared as sensitive as with wet collodion. In the wet process the negative has to be taken and finished upon or near the spot from which the view is taken, and with wet collodion the tourist is therefore obliged to work in a van or tent, and carry a load of paraphernalia about with him, which is of course both expensive and inconvenient. To avoid this he is compelled to work with dry plates, and hitherto no process has been published by which dry plates can be made as sensitive as wet ones. A rapid dry process has therefore been an important subject of investigation to photographers, because during a long exposure of a plate the shadows move, and figures sometimes alter their position. A man or horse, for instance, are likely to remain still for a few seconds, but not for ten minutes.

I have lately solved this problem of rapid dry collodion, and produced dry plates as sensitive as wet ones, which will moreover preserve their sensitiveness and good qualities for several weeks, and perhaps indefinitely. This process, and the principles upon which it is based, I will now briefly describe.

The rapidity of this dry process depends upon the accelerating effect of bromine in dry collodion, and in this respect an anology exists between the Daguerreotype and dry collodion processes. In the former a silver plate simply iodized is extremely insensitive, but when submitted to the fumes of bromine its sensitiveness is increased a hundred-fold. The same thing happens in those collodion processes, wet or dry, in which the free nitrate of silver is washed out of the film. A collodion film simply iodized, and without free nitrate, is as insensitive as an iodized Daguerreotype plate, but a bromo-iodized collodion film without free nitrate may be rendered as sensitive as a bromo-iodized silver plate. In the wet collodion process the most exalted sensibility is conferred upon a simply iodized film by the presence of free nitrate of silver; but you cannot retain free nitrate in a dry collodion film because it not only crystallizes on drying, but by becoming concentrated as the water evaporates, dissolves the iodide of silver, and forms a curious and interesting double salt, the exact properties of which have not yet been fully investigated. You cannot even retain a perceptible trace of free nitrate entangled in a dry collodion film without introducing an element of instability, and consequent uncertainty in your work. The principle therefore of preparing a rapid dry collodion plate consists in using bromo-iodized collodion, and removing all the free nitrate, which is the element of instability.

But the image produced upon a bromo-iodized silver plate, developed with mercury, is extremely thin and superficial, as may be proved by transferring it to a sheet of gelatinized paper. And similarly, the image developed by pyrogallic acid upon a dry bromo-iodized collodion film is thin, and too transparent to yield a good printing negative. It is necessary therefore to apply to the film a coating of some organic substance, in order to give density to the dark parts of the negative. Many substances have been employed for this purpose, viz., gelatine, metagelatine, albumen, various syrups, gum arabic, infusion of malt, tannin, &c, &c.; and experimenters have, almost without exception, exhausted their ingenuity in varying these preservative coatings, as they are called, instead of seeking in the use of bromide for the true accelerating agent. The preservatives named have not all the same effect, and besides affecting the sensitiveness of the film they also determine the color of the finished negative, gelatine and gum giving a black, tannin a red, and albumen a yellowish color to the deposit in the dark parts. Much therefore depends upon the selection of a proper preservative, when the most exalted sensitiveness is required.

One more difficulty remained to be overcome, and it is this. When a collodion film has once been allowed to get dry, and is wetted a second time, it is very liable to split and leave the glass; or if a preservative has been applied to it, it is very liable to rise in blisters, which spoil the negative. But this may be prevented by giving the glass plate a preliminary coating of india-rubber dissolved in Kerosolene.

The operations in the rapid dry process are therefore as follows:—

1. Clean the glass plate, dry it thoroughly, and apply to it a solution composed of 1-gr. of india-rubber dissolved in an ounce of Kerosolene.

2. Coat the plate thus prepared with bromo-iodized collodion containing an equal number of atoms of iodine and bromine, added in combination with cadmium. There should be about 5-grs. of mixed iodide and bromide of cadmium to the ounce of collodion.

3. Excite the film in a bath composed of 30-grs. of pure recrystallized nitrate of silver, slightly acidified with nitric acid.

4. Wash off all the free nitrate of silver, and pour over the film a preservative composed of 25-grs. of gum arabic freshly dissolved in an ounce of water. Let it dry spontaneously, and before putting the plate into the dark slide, dry it again thoroughly before a hot flat iron.

5. Give the same exposure as for wet collodion.

6. Develope the picture by first wetting it with distilled water, and then pouring over it a developer consisting of 1-oz. of distilled water, 2-grs. of pyrogallic acid, 2 scruples of glacial acetic acid, and a few drops of a weak solution of nitrate of silver. The image appears immediately, and very soon acquires the necessary intensity

7. Fix the negative in the usual way with a saturated solution of the hyposulphite of soda or lime, and when dry varnish it with spirit varnish.

Negatives taken in this way are equal in every respect to those taken upon wet collodion plates, and the process is as simple as any of those which are now employed for slow dry plates.

MONTHLY METEOROLOGICAL REGISTER, AT THE PROVINCIAL MAGNETICAL OBSERVATORY, TORONTO, CANADA WEST.—APRIL, 186[illegible].

Latitude—43 deg. 39.4 min. North. Longitude—5 h. 17 min. 33 sec. West. Elevation above Lake Ontario, 108 feet.

Day.	Barom. at temp. of 32°. 6 A.M.	2 P.M.	10 P.M.	MEAN.	Temp. of the Air. 6 A.M	2 P.M	10 P.M	ME'N	Excess of mean above Normal.	Tens. of Vapour. 6 A.M	2 P.M.	10 P.M.	M'N	Humidity of Air. 6 A.M	2 P.M.	10 P.M.	M'N	Direction of Wind. 6 A.M.	2 P.M.	10 P.M.	Resultant Direction.	Velocity of Wind. 6 A.M	2 P.M	10 P.M	Result.	ME'N	Rain in Inches.	Snow in Inches.
1	29.564	29.423	29.153	29.3128	°8.9	°24.8	°30.6	°22.27	°—13.07	.049	.105	.152	.101	.75	.79	.80	.78	N N W	S W	S S W	S 56° W	17.0	13.0	12.5	9.93	12.64	...	0.1
2	28.704	28.903	.293	28.9915	38.8	41.0	32.0	37.17	+ 0.53	.222	.127	.151	.139	.91	.47	.83	.72	W S W	N W b W	N W b W	N 59 W	6.0	29.8	6.4	14.02	15.36	...	0.1
3	29.504	29.750	—	—	29.5	35.2	—	—	—	.136	.135	—	—	.83	.66	—	—	N	N b W	N b W	N 9 W	6.5	11.8	19.0	11.77	12.17	..	...
4	30.041	.994	.857	29.9657	21.2	33.4	27.3	28.30	— 9.03	.092	.089	.127	.103	.81	.47	.84	.68	N N E	S E	N N W	N 19 E	5.0	9.2	3.8	2.66	6.27	...	..
5	29.696	.483	—	—	26.6	46.8	—	—	—	.098	.083	—	—	.68	.25	—	—	N	N W	W b N	N 31 W	12.4	19.8	0.5	10.24	10.46	...	...
6	.384	.442	.592	.4840	34.5	43.5	31.6	36.38	— 1.65	.174	.135	.163	.162	.87	.46	.91	.77	N N W	N E b N	N b E	N 5 E	2.8	6.4	13.0	11.95	12.35	0.060	1.2
7	.676	.789	.927	.8097	26.6	31.5	30.6	29.68	— 8.73	.134	.142	.138	.136	.93	.79	.80	.83	N b E	N b W	N	N 1 W	16.0	11.0	7.2	9.35	9.55	...	0.2
8	.930	.895	.891	.9043	30.2	34.2	32.7	32.60	— 6.12	.149	.152	.165	.150	.89	.77	.88	.81	N b E	S W	N W	N 35 W	4.8	7.8	7.8	4.62	6.96	...	...
9	.901	.882	.845	.8763	31.6	45.7	33.4	37.10	— 1.95	.111	.148	.149	.137	.62	.47	.78	.63	N W b N	S S W	S W	S 67 W	11.0	12.0	3.5	4.38	7.32	...	...
10	.784	.558	.489	.5987	31.3	43.9	41.4	39.52	+ 0.10	.152	.190	.241	.192	.86	.66	.92	.78	S S W	E	Calm.	N 82 E	1.3	5.0	0.0	2.87	3.40	0.035	...
11	.420	.360	.430	.3038	40.7	60.0	48.9	50.18	+10.45	.231	.301	.288	.278	.91	.57	.83	.77	N E b N	S W b S	W	S 59 W	1.5	17.0	3.5	8.69	10.25	0.585	...
12	.471	.662	—	—	43.2	39.9	—	—	—	.209	.163	—	—	.74	.66	—	—	W b N	N N W	N N W	N 29 W	12.3	16.8	8.0	14.29	14.78	...	...
13	.851	.817	.824	.8468	30.2	39.0	31.3	34.28	— 6.15	.136	.167	.136	.143	.80	.68	.77	.72	N b E	S S W	S W b S	S	5.8	5.0	1.0	1.48	4.02	...	...
14	.842	.814	.739	.7968	30.9	47.2	41.7	41.32	+ 0.58	.157	.076	.088	.114	.90	.23	.32	.47	N b W	E b S	E N E	N 80 E	2.5	10.0	8.8	7.50	S 17	...	...
15	.694	.666	.692	.6865	40.3	49.0	43.2	44.33	+ 3.28	.166	.178	.198	.183	.66	.50	.70	.63	E b N	E N E	E	N 77 E	12.2	13.5	5.0	10.31	10.40	0.155	...
16	.678	.646	.610	.6518	40.3	45.0	44.6	43.25	+ 1.77	.219	.278	.264	.251	.87	.93	.90	.89	E N E	E b N	N E	N 51 E	18.0	10.5	13.2	10.67	11.30	0.520	...
17	.611	.627	.661	.6310	42.1	44.6	41.4	42.82	+ 1.07	.268	.283	.241	.261	1.00	.96	.92	.95	N	E S E	W S W	N 74 E	8.5	4.8	1.8	1.60	3.58	0.020	...
18	.700	.704	.682	.6933	39.4	49.7	42.5	44.13	+ 2.07	.230	.255	.236	.243	.95	.71	.86	.84	W S W	S b E	E b S	S 72 E	3.2	3.8	0.5	2.49	4.01	...	...
19	.652	.673	—	—	43.9	52.6	—	—	—	.254	.239	—	—	.89	.60	—	—	E b N	E	E b N	N 84 E	10.5	16.0	7.5	11.94	12.12	...	...
20	.681	.832	.935	.8255	50.4	39.0	39.9	42.67	— 0.12	.248	.222	.233	.231	.67	.94	.95	.85	E b N	E	E b N	N 85 E	14.5	9.2	10.2	10.69	11.07	0.835	...
21	.990	.986	.887	.9575	42.1	45.4	48.2	45.23	+ 2.13	.259	.234	.245	.241	.96	.77	.72	.80	N E b E	E N E	E b N	N 78 E	9.0	12.5	13.0	11.06	11.20	Inp.	...
22	.912	.808	.705	.7937	42.5	54.4	51.5	49.63	+ 6.27	.227	.163	.126	.172	.83	.38	.33	.50	E b S	E b N	E b N	N 77 E	9.0	18.0	11.0	11.77	11.94	...	...
23	.639	.557	.614	.5608	46.4	54.4	50.0	50.87	+ 7.17	.166	.136	.102	.164	.52	.32	.53	.41	E N E	E	N N W	N 70 E	9.0	11.5	4.5	5.26	6.16	...	...
24	.444	.344	.515	.4327	43.5	67.7	46.1	54.07	+10.07	.186	.201	.216	.208	.66	.29	.69	.52	W N W	N N W	N	N 16 W	1.8	22.4	17.0	15.02	15.72	...	...
25	.636	.657	.722	.6818	33.8	47.5	39.6	40.35	— 4.05	.099	.090	.083	.097	.50	.27	.34	.39	N	N N W	N N W	N 20 W	19.0	20.6	13.0	17.49	17.77	...	...
26	.800	.759	—	—	36.0	56.5	—	—	—	.104	.107	—	—	.48	.23	—	—	N N W	N W b N	N W b N	N 27 W	8.5	15.0	3.0	8.18	8.45	...	...
27	.735	.631	.562	.6343	38.5	60.5	41.7	49.67	+ 4.65	.120	.156	.218	.176	.55	.29	.82	.52	N W	S b W	S S W	S 35 W	2.4	8.5	0.5	3.08	4.29	...	...
28	.525	.454	.482	.4827	43.5	61.2	52.2	53.63	+ 8.23	.177	.187	.337	.245	.62	.34	.86	.61	N b W	S	E	S 87 E	1.7	3.0	8.8	3.34	5.01	...	...
29	.518	.455	.486	.4828	43.2	50.8	46.1	47.95	+ 2.22	.209	.222	.225	.212	.74	.59	.72	.63	N b E	S b E	W N W	N 83 E	5.5	5.8	1.5	2.03	4.52	...	...
30	.676	.614	.609	.6013	49.3	57.2	50.0	53.40	+ 7.35	.138	.189	.161	.177	.38	.40	.45	.42	N E b N	S b E	N W b N	S 78 E	7.0	5.2	4.8	1.82	4.89	...	...
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
M	29.6574	29.6359	29.6456	29.6453	36.81	46.85	40.74	42.03	+ 1.04	.173	.177	.191	.181	.77	.56	.74	.68	....	....	....	....	8.16	11.83	7.01	.....	9.20	2.210	1.6

REMARKS ON TORONTO METEOROLOGICAL REGISTER FOR APRIL, 1863.

Highest Barometer..........................30.078 at 8 a. m. on 4th } Monthly range =
Lowest Barometer ....28.704 at 6 a. m. on 2nd } 1.374 inches.

Self-registering Thermom. { Maximum Temperature .... 69°0 on p.m. of 24th } Monthly range =
Minimum Temperature .... 8°6 on a.m. of 1st } 60°4
Mean maximum Temperature .... 49°99 } Mean daily range =
Mean minimum Temperature .... 33°42 } 16°57
Greatest daily range ...... 30°05 from a.m. to p.m. of 27th.
Least daily range............ 4°4 from a.m. to p.m. of 17th.

Warmest day......... 24th .. Mean temperature......... 54°07 } Difference = 31°80.
Coldest day .... 1st .. Mean temperature .... 22°27 }

Maximum } Solar...... .... 93°8 on p.m. of 24th } Monthly range =
Radiation. } Terrestrial .... —10°8 on a.m. of 1st } 104°6

Aurora observed on 5 nights, viz.,—8th, 9th, 12th, 19th, and 21st.
Possible to see Aurora on 19 nights; impossible on 11 nights.
Snowing on 4 days, depth 1.6 inches; duration of fall, 18.7 hours.
Raining on 8 days, depth 2.210 inches; duration of fall 56.5 hours.
Mean of cloudiness = 0.54. Below average 0.04.
Most cloudy hour observed, 2 p.m: mean = 0.58; least cloudy hour observed, 10 p.m.; mean, = 0.50.

Sums of the components of the Atmospheric Current, expressed in miles.

North.	South.	East.	West.
3450.18	827.37	2355.20	1712.09

Resultant direction N. 14° E.; Resultant velocity 3.75 miles per hour.
Mean velocity ... .9.20 miles per hour.
Maximum velocity .... 35.2 miles, from 1 to 2 p.m. of 2nd.
Most windy day..... ..25th .. ..Mean velocity, 17.77 miles per hour. } Difference =
Least windy day......10th .. ..Mean velocity, 3 40 ditto. } 14.37 miles
Most windy hour ...1 p.m. to 2 p.m. .... Mean velocity, 12.26 ditto. } Difference =
Least windy hour...10 p.m. to 11 p.m. ....Mean velocity 6.48 ditto. } 5.78 miles.

1st. Very cold day; slight snow 1 to 4 p.m. Wind high and keen; lunar halo at midnight.—8th. Auroral light 10 p.m. and midnight.—9th. Beautiful arch of auroral light extending across zenith from W.N.W. to E.S.E., from 8 to 9.40 p.m. A few streamers occasionally till 1 a.m. of 10th.—11th.) Sheet lightning 7 to 8 p.m. Thunderstorm and heavy rain 8.30 to 9.30 p.m. (first of season.) Butterflies seen.—12th. Faint Auroral light at midnight.—14th. Solar halo during the evening.—17th. Frogs croaking loudly at night.—18th. Fog 6 to 8 a m.; wild pigeons observed.—19th. Swallows observed; faint auroral light at 9 p.m.—21st. Fog 6 and 8 a.m.; auroral light and streamers 8 and 9 p.m.—25th. Ice on exposed shallow vessels at 6 a.m.; wind high and keen.—26th. Ice on shallow vessels at 27th. Hoar frost at 6 a.m.—28th. Lunar halo at 8 and 10 p.m.—29th. Solar halo 11 a.m. and 2 p.m.; lunar halo 10 p.m.

April, 1863, was comparatively warm, dry, windy and clear.

COMPARATIVE TABLE FOR APRIL.

YEAR.	TEMPERATURE.					RAIN.		SNOW.		WIND.		
	Mean.	Excess above aver'ge (30.1)	Max. observed	Min. observed.	Range.	No. of days.	Inches	No. of days.	Inches	Resultant. Direction.	Resultant. V'y	Mean Force or Velocity.
	°	°	°	°	°							
1840	42.4	+ 1.4	65.9	25 3	40.6	14	3.420	2	...	...	...	....
1841	39.2	— 1.8	62.9	22.1	40.8	3	1.370	3	...	..	...	0 51 lbs.
1842	43.1	+ 2.1	89 5	21.6	67.9	8	3.740	2	...	..	...	0.57
1843	40.9	— 0.1	70.0	15.1	54.9	7	3 185	3	0.1	..	...	0.46
1844	47.5	+ 6.5	74.[illegible]	17.2	57.3	10	1.515	1	Inp.	...	...	0.24
1845	42.1	+ 1.1	66.0	14.8	51.2	11	3.290	4	1.5	..	...	1.00
1846	44.0	+ 3.0	79.4	24.4	55.0	10	1.300	2	1.3		...	0.55
1847	39.2	— 1 8	65.6	8 4	57.2	8	2.870	2	4.0	...	..	0.59
1848	41.3	+ 0.3	65.4	26.5	38.9	5	1.455	1	0.5	N 77° W	1.46	4.89mls.
1849	39.0	— 2.0	70.9	23.2	47.7	10	2.655	2	1.7	N 43° W	3.14	7.50
1850	37.9	— 3.1	63 2	18 2	45.0	7	4 720	2	1.1	N 39° W	1.12	7.64
1851	41.3	+ 0.3	59.2	25.8	33.4	11	2.29[illegible]	3	1.2	N 14° E	2 52	8 07
1852	38.2	— 2.8	53.8	19.8	34.0	6	1.990	4	9.4	N 23° E	2.41	6 68
1853	41.9	+ [illegible].9	65.7	27.0	38.7	10	2.625	1	1 0	N 12° W	1.95	5.20
1854	41 0	0.0	65 1	22.3	42 8	12	2.685	4	2.7	N 50° E	2.57	6 81
1855	42.4	+ 1.4	63.8	12.2	51.6	8	2.030	3	1.6	N 36° W	3.99	7 57
1856	42.3	+ 1.3	69.8	15.1	54.7	13	2.780	3	0.1	N 29° E	1.64	6.05
1857	35 4	— 5.6	51.9	10 0	41.9	10	1.755	11	12.9	N 60° W	4 15	10.24
1858	41.5	+ 0.5	61.5	23.8	37 7	13	1.642	2	0.1	N 14° W	1.64	9.57
1859	39.5	— 1.5	62.1	23.9	38 2	9	2.527	8	1.2	N 36° W	2.33	10.79
1860	39.5	— 1.5	60.7	19.7	41.0	11	1.282	5	0.3	N 37° W	4.10	10.30
1861	42.0	+ 1.0	62.3	26 2	36.1	12	1.619	4	6.9	N 37° E	2.31	8.90
1862	39.6	— 1.4	64.1	20.1	44.0	10	2.235	4	0.2	N 50° E	2.48	9.77
1863	42.0	+ 1.0	67.7	8.9	58.6	8	2.210	4	1.6	N 14° E	3.75	9.20
Results to 1861.	40.98	...	65.87	20.12	45.76	9.5	2.398	3.3	2.51	N 19° W	2.04	7.87
Exc. for 1863.	+ 1.05	...	+ 1.83	— 11.22	+ 13.05	— 1.5	— 0.188	+ 0.7	— 0.91	....	.....	+ 1.33

MONTHLY METEOROLOGICAL REGISTER, AT THE PROVINCIAL MAGNETICAL OBSERVATORY, TORONTO, CANADA WEST,—MAY, 186[illegible].

Latitude—43 deg. 39.4 min. North. Longitude—5 h. 17 m. 33 s. West. Elevation above Lake Ontario, 108 feet.

Day.	Barom. at temp. of 32°.				Temp. of the Air.				Excess of mean above Normal.	Tens. of Vapour.				Humidity of Air.				Direction of Wind.			Result't. Direction.	Velocity of Wind.					Rain in inches.	Snow in inches.
	6 A.M.	2 P.M.	10 P.M.	Mean.	6 A.M.	2 P.M.	10 P.M.	ME'N		6 A.M.	2 P.M.	10 P.M.	M'N	6 A.M.	2 P.M.	10 P.M.	M'N	6 A.M.	2 P.M.	10 P.M.		6 A.M.	2 P.M.	10 P.M.	Result.	ME'N		
					°	°	°	°	°												°							
1	29.641	29.538	29.511	29.5333	43.2	65.7	47.5	52.48	+ 6.07	.154	.207	.235	.202	.54	.35	.71	.52	Calm.	s b w	w b s	s 29 w	0.0	14.8	2.0	6.16	6.77	...	...
2	.541	.655	.690	.6358	50.0	50.0	41.7	45.98	− 0.73	.184	.212	.200	.199	.50	.59	.76	.65	N N W	E S E	N E	N 62 E	7.4	9.5	4.2	5.21	7.53	...	...
3	.724	.692	—	—	38.1	43.9	—	—	—	.137	.137	—	—	.39	.47	—	—	E b N	E	E N E	N 73 E	13.5	9.8	8.0	8.11	8.28	Inp.	...
4	.672	.581	.597	.5813	43.5	49.0	42.8	44.73	− 2.68	.259	.198	.195	.219	.92	.55	.71	.74	N E b E	S E b E	S E	N 81 E	6.8	9.0	0.5	5.45	6.48	...	...
5	.618	.586	.642	.6150	41.7	40.1	38.9	40.08	− 7.65	.175	.153	.203	.171	.65	.60	.86	.69	N E b E	E b N	E	N 73 E	13.0	13.2	7.5	13.59	13.77	0.170	0.1
6	.667	.687	.671	.6783	38.5	42.1	41.4	40.35	− 7.68	.199	.201	.232	.207	.85	.76	.89	.82	E N E	E N E	N E b N	N 69 E	15.5	15.5	4.0	9.67	9.98	0.105	...
7	.627	.563	.579	.5932	40.3	52.2	45.4	46.45	− 1.97	.209	.223	.189	.208	.84	.56	.62	.66	N E b E	s b E	N b E	N 47 E	3.8	8.8	7.6	3.23	6.92	...	...
8	.614	.608	.586	.6025	46.8	57.6	47.2	51.30	+ 2.52	.253	.224	.183	.227	.79	.46	.56	.60	N	s b w	E b N	s 43 E	4.6	5.2	1.2	2.11	3.79	..	...
9	.573	.559	.589	.5762	46.8	64.5	52.6	55.47	+ 6.30	.216	.273	.300	.260	.67	.44	.75	.59	N E b E	S S E	w b s	s 41 w	3.0	3.0	1.0	1.59	2.18	...	...
10	.608	.567	—	—	50.0	58.0	—	—	—	.294	.339	—	—	.81	.74	—	—	Calm.	s	Calm.	s 83 w	0.0	2.5	0.0	0.33	1.35	0.098	...
11	.564	.681	.698	.6537	55.4	52.2	47.2	50.97	+ 1.07	.305	.358	.320	.324	.69	.92	.98	.87	Calm.	E b N	E b N	N 73 E	0.0	6.8	6.0	4.60	5.30	0.825	...
12	.610	.650	.685	.6610	47.6	51.1	46.4	48.93	− 1.23	.316	.352	.296	.323	.96	.94	.92	.93	E b N	E b s	s E b E	N 89 E	0.5	0.5	3.2	1.16	1.74	0.150	...
13	.680	.619	.570	.6213	47.2	59.0	49.3	52.07	+ 1.50	.300	.335	.262	.295	.92	.67	.74	.76	N b w	s	N N W	N 16 E	1.5	9.0	3.2	1.01	5.10	0.242	...
14	.558	.549	.618	.5738	46.1	50.4	47.5	48.42	− 2.47	.273	.308	.207	.259	.87	.81	.62	.76	N	N W b W	N W	N 30 W	3.8	10.5	14.0	8.41	9.65	0.533	...
15	.674	.632	.557	.6080	46.8	59.8	46.1	51.02	− 0.27	.225	.281	.153	.235	.70	.51	.48	.62	N N W	s b w	s w	s 16 w	8.6	10.8	1.5	5.05	6.66	0.030	...
16	.354	.300	.428	.3560	45.0	59.8	50.8	52.52	+ 0.03	.269	.369	.258	.305	.90	.72	.69	.77	E S E	S S W	N b w	N 82 W	7.4	6.4	12.8	3.73	9.01	Inp.	...
17	.466	.503	—	—	45.0	54.0	—	—	—	.221	.232	—	—	.74	.55	—	—	W N W	N	N W b N	N 79 W	9.0	6.6	12.0	6.47	9.75	0.045	...
18	.577	.620	.607	.6352	45.0	55.4	47.5	50.27	− 2.02	.239	.233	.245	.244	.80	.52	.74	.68	N W	W N W	W N W	N 54 W	12.0	19.0	7.2	11.21	11.49	...	...
19	.726	.641	.639	.6648	45.7	71.7	54.7	59.05	+ 6.43	.202	.310	.294	.270	.66	.40	.68	.55	W	S W b W	S W b W	S 56 W	3.4	17.4	6.0	8.93	9.28	...	...
20	.659	.669	.695	.6842	51.5	63.4	56.5	57.53	+ 4.52	.317	.404	.368	.376	.91	.69	.80	.79	S W	W S W	S W	S 54 W	6.6	15.0	3.0	4.39	4.41	0.030	...
21	.707	.845	.840	.8202	55.4	73.1	62.7	64.82	+11.60	.327	.403	.404	.378	.74	.49	.71	.62	S W b W	S S W	S W b W	S 45 W	2.2	13.0	3.8	6.19	6.90	...	...
22	.892	.824	.766	[illegible]	58.7	75.3	63.0	67.78	+14.08	.419	.407	.437	.443	.85	.57	.76	.67	W N W	S W b S	S W	S 60 W	1.8	7.0	0.5	2.68	3.21	...	...
23	.720	.628	.541	.6208	65.9	76.0	62.7	[illegible]	+15.62	.411	.445	.380	.427	.64	.50	.66	.59	N W b W	S S W	S W b S	N 84 W	3.2	4.8	0.5	1.68	2.73	...	...
24	.522	.490	—	—	63.4	73.1	—	—	—	.333	.542	—	—	.57	.65	—	—	N W b N	S S W	N b E	N 85 W	7.5	12.0	6.5	1.00	6.54	...	...
25	.582	.627	.648	.6258	56.9	58.7	54.4	56.30	+ 1.62	.320	.350	.278	.305	.71	.70	.65	.66	E	E S E	E b N	N 86 E	5.1	20.0	8.8	9.45	9.96	...	...
26	.713	.731	.737	.7317	61.5	61.2	56.2	57.02	+ 2.03	.304	.328	.330	.326	.80	.61	.75	.71	E N E	E	E b N	N 68 E	1.0	8.5	4.8	5.02	5.67	...	...
27	.810	.794	.769	.7915	55.8	69.2	52.9	59.83	+ 4.65	.322	.405	.338	.357	.72	.56	.84	.70	N N E	s b w	Calm.	S 17 W	0.5	1.5	0.0	0.19	0.33	...	...
28	.781	.711	.634	.7032	51.1	73.1	63.4	63.20	+ 7.55	.246	.431	.333	.361	.65	.52	.57	.62	Calm.	s w b s	s w b s	S 51 W	0.0	1.2	1.0	0.49	0.64	...	...
29	.604	.501	.384	.4835	56.2	70.2	59.1	64.05	+ 8.10	.360	.431	.323	.380	.80	.58	.64	.64	Calm.	S E	Calm.	N 62 E	0.0	0.5	0.0	0.41	0.51	...	...
30	.264	.148	.063	.1425	59.8	61.6	59.0	61.58	+ 5.33	.370	.519	.439	.469	.72	.95	.83	.86	Calm.	E b s	S W	S 33 W	0.0	2.8	1.8	1.41	3.13	0.210	...
31	.039	.011	—	—	60.1	61.2	—	—	—	.461	.499	—	—	.89	.92	—	—	w b s	W S W	N N W	S 63 W	4.6	4.5	6.5	2.35	3.35	0.925	...
M	29.6324	29.6133	29.6093	29.6170	49.70	60.02	51.42	54.30	+ 2.82	.277	.326	.285	.299	.76	.62	.73	.69	....	... .	....	....	4.72	8.68	4.49	......	5.89	3.303	0.1

REMARKS ON TORONTO METEOROLOGICAL REGISTER FOR MAY, 1863.

Highest Barometer . . . . 29.901 at 8 a.m. on 22nd. } Monthly range=
Lowest Barometer . . . . 29.011 at 2 p.m. on 31st. } 0.890 inches.

Self-registering Ther.:
- Maximum temperature . . . . 79°0 on p.m. of 22nd } Monthly range=
- Minimum temperature . . . . 36°4 on a.m. of 1st } 42°6
- Mean maximum temperature . . 63°42 } Mean daily range=17°15
- Mean minimum temperature . . 46°26 }
- Greatest daily range . . . . . . 34°8 from p.m. to a.m. of 19th.
- Least daily range . . . . 3°4 from a.m. to p.m. of 11th.

Warmest day . . . 23rd...Mean Temperature . . . 69°58 } Difference=29°50.
Coldest day . . . . 5th ...Mean Temperature . . . 40°08 }
Maximum { Solar . . . . 95°5 on p.m of 29th } Monthly range=
Radiation { Terrestrial . . . . 25°5 on a.m. of 17th } 70°.0
Aurora observed on 0 nights; possible to see Aurora on 18 nights; impossible on 13 nights.
Snowing on 1 day; depth 0.1 inches; duration of fall 1.0 hours.
Raining on 14 days; depth, 3.363 inches; duration of fall, 67.7 hours.
Mean of cloudiness=0.48; below average, 0.05. Most cloudy hour observed, 2 p.m.; mean=0.58; least cloudy hour observed, 10 p.m.; mean=0.30.

Sums of the components of the Atmospheric Current, expressed in Miles.

North.	South.	East.	West.
1324.08	1151.42	1674.65	1419.85

Resultant direction, N. 56° E.; Resultant Velocity, 0.41 miles per hour.
Mean velocity 5.89 miles per hour.
Maximum velocity 21.8 miles, from noon to 1 p.m. on 18th.
Most windy day 5th—Mean velocity 13.77 miles per hour. } Difference 13.44
Least windy day 27th—Mean velocity 0.33 miles per hour. }
Most windy hour, 1 to 2 p.m.—Mean velocity, 8.82 miles per hour. } Difference
Least windy hour, 5 to 6 a.m.—Mean velocity, 4.10 miles per hour. } 4.72 miles.

1st. Slight hoar frost, 6 a.m.—5th. Particles of snow, hail, and rain, 10 and 11 a.m.—13th. Distant thunder in W. at 2.45 and 3.30 p.m.—20th. Thunderstorm passing from NW. to E. at 8 a.m.—21st. Sheet lightning in SW. at 9 p.m.—28th. Corona round the moon at 9 p.m.—29th. Solar halo, 8 a.m. to 4 p.m.; very perfect lunar halo at 10 p.m.—30th. Thunderstorm, sheet and forked lightning, and heavy rain in S.W., 11 a.m. to 0.30 p.m.; slight fog 4 p.m.

Great range of temperature from a.m. to p.m. of 10th = 34°8 in 14 hours.
Heavy dew recorded on 14 mornings during the month.
May, 1863, was comparatively mild, wet, calm, and clear.

COMPARATIVE TABLE FOR MAY.

YEAR	TEMPERATURE.					RAIN.		SNOW.		WIND.		
	Mean	Excess above Average (51°4).	Maximum observed.	Minimum observed.	Range.	No. of days.	Inches.	No. of days.	Inches.	Resultant. Direction.	Resultant. Velocity.	Mean Force or Velocity
1840	53.8	+ 2.4	74.5	30.8	43.7	9	4.150	0	...	...	...	...
1841	50.5	— 0.9	76.2	26.6	49.6	11	2.350	1	...	...	...	0.35 lbs
1842	49.1	— 2.3	74.3	30.0	44.3	7	1.275	0	...	...	...	0.53 "
1843	49.1	— 2.3	79.6	28.9	50.7	5	1.570	0	0.0	...	...	0.52 "
1844	53.6	+ 2.2	77.7	29.0	48.7	14	5.670	0	0.0	...	...	0.30 "
1845	49.6	— 1.8	76.6	29.4	47.2	8	2.309	0	0.0	...	...	0.55 "
[illegible]	55.5	+ 4.1	78.1	34.3	43.8	9	4.375	0	0.0	...	...	0.46 "
	51.4	+ 3.0	72.5	27.8	44.7	12	2.010	0	0.0	...	...	0.29 "
1 8	54.1	+ 2.7	78.5	31.9	46.6	13	2.520	0	0.0	N 40 W	1.31	4.93 ms
1849	48.0	— 3.4	72.5	32.7	39.8	16	5.115	0	0.0	N 51 E	1.97	5.33 "
1850	47.6	— 3.8	76.3	31.1	45.2	7	0.545	1	Inp.	N 64 W	2.05	6.32 "
1851	51.3	— 0.1	73.2	28.7	44.5	12	2.950	1	0.5	N 32 W	1.59	6.34 "
1852	51.4	0.0	73.3	34.5	38.8	7	1.125	1	Inp.	S 82 W	0.99	4.00 "
1853	50.9	— 0.5	78.4	38.4	40.0	17	4.420	1	Inp.	N 2 W	0.83	5.16 "
1854	52.2	+ 0.8	69.0	27.6	41.4	11	4.630	0	0.0	E	0.40	5.38 "
1855	53.1	+ 1.7	74.8	33.9	40.9	6	2.565	2	0.9	N 1 W	2.76	5.93 "
1856	50.5	— 0.9	80.1	35.5	44.6	14	4.580	1	Inp.	N 4 E	3.99	9.81 "
1857	48.9	— 2.5	72.5	27.9	44.6	15	4.145	1	Inp.	N 23 W	1.14	8.13 "
1858	48.9	— 2.5	66.0	35.0	31.0	17	6.367	0	0.0	N 42 E	3.33	9.30 "
1859	55.2	+ 3.8	76.2	41.5	34.7	11	3.410	0	0.0	N 72 E	1.59	5.70 "
1860	55.5	+ 4.1	73.2	35.6	37.6	16	1.815	0	0.0	N 26 E	2.66	7.17 "
1861	47.5	— 3.9	72.0	29.1	42.9	12	3.380	1	0.5	N 47 W	3.60	9.17 "
1862	52.2	+ 0.8	77.8	38.1	39.7	8	1.427	0	0.0	N 52 W	2.80	7.87 "
1863	54.3	+ 2.9	77.1	38.1	39.0	14	3.363	1	0.1	N 56 E	0.41	5.89 "
Results to 1861.	51.39	...	74.80	31.83	42.97	11.3	3.241	0.5	0.10	N 2 W	1.49	6.62
Exc. for 1863.	+2.91	...	+ 2.30	+ 6.27	— 3.97	+ 2.7	+ 0.122	+ 0.5	+ 0.00	...	...	—0.73