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THE CANADIAN JOURNAL.

NEW SERIES.

No. XXVIII.—JULY, 1860.

NOTICE OF A SKULL BROUGHT FROM KERTCH, IN THE CRIMEA.

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Read before the Canadian Institute, January 21st, 1860.

The Anglo-French campaign of 1855 in the Crimea, led to a general familiarity with much concerning that remarkable peninsula, of which we were in ignorance before. Its geography, its ethnology, and its antiquities all attracted attention; and rewarded research by novel disclosures; and its ancient history acquired a fresh interest, and received new illustrations from the investigations of the half obliterated remains of its long extinct past. Among its ancient historical sites, which, owing to peculiar circumstances, received a large share of attention, that of Kertch is, on various accounts, the most remarkable. Built on the site where, some 500 years before Christ, the Greek city of Panticapæum was founded, it was the centre of an area rich with memorials of the strangely chequered past, which has seen the same spot successively occupied by Milesian Greeks, Romans, Huns, Tartars, Genoese, Turks, and Russians. The Russian occupation of the Crimea dates only from a late period in the eighteenth century, but since then, a Museum had been formed in the town of Kertch, in which were preserved many historical antiquities of the Crimean Bosphorus; and especially sepulchral relics recovered from the tumuli which abound on the site of the ancient Milesian colony.

Learning from an old fellow-student that he was about to proceed to the Crimea to join the Army Medical Staff, I wrote to him, drawing his attention to various objects worthy of observation; and in directing his notice to the treasures accumulated in the Museum at Kertch, specially requested him to note for me—should opportunity offer,—the characteristics of an ancient Macrocephalic skull preserved there. It is referred to in Captain Jesse's "Notes of Travel in Circassia and the Crimea," where it is said to have been found in the neighbourhood of the Don. The interest of such cranial remains increases in value, from the evidence they furnish of ancient analogies to the remarkable artificial compression which now we associate almost exclusively with American crania.

It chanced, as is now well known, that, in the fortunes of war, the town of Kertch fell into the hands of the Anglo-French invaders; and some few of its ancient treasures were preserved and transmitted to the British Museum. By far the greater portion of the Museum collections however, were barbarously spoiled by the rude soldiery; and among the rest doubtless perished the little-heeded relic of the Macrocephali of the Crimea, first described by Hippocrates, five centuries before our era. Blumenbach has figured in his first Decade, an imperfect compressed skull, received by him from Russia, which he designates as that of an Asiatic Macrocephalus; and in 1843, Rathke communicated to Müller's "*Archiv für Anatomie,*" the figure of another artificially compressed skull, also very imperfect, but specially marked by the same depression of the frontal bone. This example is described as procured from an ancient burial-place near Kertch in the Crimea; and no doubt other illustrations of the peculiar physical characteristics of the ancient Macrocephali of the Bosphorus will reward future explorers, when the attention of those engaged in such researches, or even in ordinary agricultural labours on the site, is specially directed to the interest now attaching to them.

Meanwhile, however, my hopes of obtaining any further facts from the Macrocephalic cranium seen by Captain Jesse in the Kertch Museum, had been dissipated by the dispersion and wanton destruction of its treasures; and I had ceased to think specially of Crimean crania, when I was gratified by receiving the gift of a skull, including the lower jaw, brought from Kertch, and described by the donor, as that of a Circassian lady. In form it presented no correspondence with the Macrocephalic type to which my inquiries had been previously directed, for the forehead is markedly vertical, and in its general proportions it is strikingly characterised as a brachycephalic cranium of unusual width at the parietal protuberances, while marked by much delicacy and beauty, especially in the facial bones.

A special interest attaches to the evidences of physical form, as well as of philological characteristics, pertaining to the tribes of the Caucasian area, owing to the factitious importance that has been assigned to certain of them in modern Ethnology. It may not, therefore, be altogether valueless to put on record the facts connected with the recovery of the Crimean cranium in question; and to note the peculiarities of its form and measurements; though, from the mixed character of the population of Kertch it would not be safe to assign the crania of its modern cemetery to any absolute ethnological group, or to make them the basis whereon to found data for classification, or for any comprehensive generalization.

Dr. Latham, in his "Varieties of Man," classes the nations and tribes of the area within the range of Mount Caucasus under the generic designation of Dioscurian Mongolidæ, including in its chief divisions: The Georgians; the Lesgians; the Mizjegi; the Irôn; and the Circassians. He derives the term Dioscurian, from the ancient sea-port of Dioscurias, where the chief commerce between the Greeks and Romans and the natives of the Caucasian range took place. According to Pliny, it was carried on by one hundred and thirty interpreters, so numerous were the languages; and one striking characteristic of the locality, still noticeable, is the great multiplicity of mutually unintelligible tongues. This therefore is the idea designed to be conveyed by the term Dioscurian. Caucasian would have been a preferable, because more familiar and precise term, but it has been already appropriated as an Ethnological division, in a way sufficiently confusing and indefinite, without adding thereto by the creation of such a contradictory union of terms, as would arise from such a designation as Caucasian Mongolidæ,—almost equivalent, in popular acceptation, to European Asiatics!

The use of both epithets, Caucasian and Mongolian, is traceable to Blumenbach, and the history of his adoption of the former supplies a curious example of a term, subsequently employed as one of the most comprehensive heads of classification, having its origin from the fewest possible premises. Among the captives taken by the Russians in one of their frequent inroads on the country lying between Mount Caucasus and the Euxine, was a Georgian woman, who was carried prisoner to Moscow, and died suddenly there. The body was made the subject of anatomical examination by Professor Hiltenbrandt, and the skull having been prepared, was subsequently presented to Dr. Asch, of St. Petersburg. From him it passed into the hands of Blumenbach, and its peculiar symmetry and beauty appear to have made a lively impression on his mind. That this was not without good reason appears from the following description of the Georgian cranium by Dr. Lawrence:

"The form of this head is of such distinguished elegance, that it attracts the attention of all who visit the collection in which it is contained. The vertical and frontal regions form a large and smooth convexity, which is a little flattened at the temples; the forehead is high and broad, and carried forward perpendicularly over the face. The cheek-bones are small, descending from the outer side of the orbit, and gently turned back. The superciliary ridges run together at the root of the nose, and are smoothly continued into the bridge of that organ, which forms an elegant and finely turned arch. The alveolar processes are softly rounded, and the chin is full and prominent. In the whole structure there is nothing rough or harsh, nothing disagreeably projecting. Hence it occupies a middle place between the two opposite extremes, of the Mongolian variety, in which the face is flattened, and expanded laterally; and the Ethiopian, in which the forehead is contracted, and the jaws also are narrow and elongated anteriorly."

Little could the poor Georgian captive dream of the posthumous honours and admiration that were to atone to her for her living wrongs. She has avenged herself on her European captors, by introducing uncertainty and confusion into the science for illustrating which Blumenbach regarded her symmetrical cranium as a peculiarly valuable prize. It was in the Third Decade of his anatomical descriptions of skulls, published in 1795, that the skull of the fair Georgian was introduced, accompanied by a glowing description of its elegance and unequalled grace; and a reference to the beauty of the Georgian women, which, as his example proved, lives even in their fleshless bones. A comparison of the skull with a cast of one of the most beautiful classic busts in the Townley collection, seemed to the enthusiastic craniologist as though he had acquired the actual skull of the head from which the ancient marble was copied; and when placed alongside of the only Greek skull in his collection, the Georgian was superior to it, the Greek being next in rank.

Hence it was that Blumenbach adopted his Georgian skull as a typical cranium, for the most perfectly developed division of the human species. In the same decade in which the Georgian skull appears, the term *Caucasian* is introduced in connexion with it; and along with this term of classification appear also those of Mongolian and Ethiopian; and these, with the epithets Malay, and American,—subsequently added,—formed the names of a quinary division of the human species, which he conceived his physical researches to have established. By the term Caucasian, Blumenbach meant no more than the adoption of a convenient name for his highest division of the human species, the typical characteristics of which were most completely epitomised in his symmetrical cranium. But the associations and historical traditions connected with Mount Caucasus, supplied a tempting basis for theory and speculation. The mountain range was assumed by some as the central point for the origin of mankind; and the epithet derived from it is now associated with so many extravagant ideas, and so much loose and confused classification, that the vague uncertainty it has acquired is abundantly sufficient to justify its abandonment. When, however, Dr. Latham substitutes the term Dioscurian for Caucasian, in its limited sense as applicable to the inhabitants of the actual area of Mount Caucasus, he does so not only from different data to those employed by Blumenbach, but even in defiance of such analogies as their ascertained physical conformation seems to suggest. He accordingly admits that he occupies exceedingly debateable ground. "So long has the term Caucasian been considered to denote a type of physical conformation closely akin to that of the Iapetidæ, *i. e.* pre-eminently European, that to place the Georgians and Circassians in the midst of the Mongolidæ, is a paradox. Again, the popular notions founded upon the physical beauty of the tribes under notice, are against such a juxta-position; the typical Mongolians, in this respect, have never been mentioned by either poet or painter, in the language of praise." Perhaps, however, the facts which justify Dr. Latham in saying of

Blumenbach's solitary Georgian skull, "never has a single head done more mischief to science, than was done in the way of posthumous mischief, by the head of this well-shaped female from Georgia," may have had their influence in tempting to the Caucasian paradox of his Dioscurian Mongols. The classification, at any rate, entirely ignores physical conformation, and rests on vocabulary analogies, confirmed by an opinion expressed by Mr. Norris, of the Asiatic Society, that on the surer evidence of grammatical comparison, the closest philological affinity of the Dioscurian languages is with the Aptotic ones, of which the Chinese is generally accepted as the type.

It is scarcely necessary to say, that languages may belong to a different class from the people who speak them. Europe supplies abundant and well authenticated illustrations of this. An Englishman speaking Chinese, does not thereby become a Mongol, nor will the adoption of the English tongue by the Chinese emigrants to Australia and elsewhere, affect their essentially Mongolian physical characteristics. Dr. Latham accordingly refers to the want of sufficient evidence for discussing the physical elements of classification in his Dioscurian Mongols. "Physiological objections," he observes, "based upon the symmetry of shape and delicacy of complexion on the part of the Georgians and Circassians, I am at present unable to meet. I can only indicate our want of osteological data, and remind my readers of the peculiar climatological conditions of the Caucasian range; which is at once temperate, mountainous, wooded, and in the neighbourhood of the sea—in other words the reverse of all Mongol areas hitherto enumerated. Perhaps, too," he adds, "I may limit the extent of such objections as a matter of fact. It is only amongst the chiefs, where the personal beauty of the male portion of the population is at all remarkable. The tillers of the soil are, comparatively speaking, coarse and unshapely."

The latter remark—whatever be its value,—may be made of the tillers of the soil everywhere; but if the Georgian and Circassian mothers are generally as graceful and beautiful in form as the concurrent opinion of travellers affirms them to be, the perpetuation of anything approximating to a Mongol physical type in their sons, would be one of the greatest marvels in physiological ethnology. In the absence, however, of osteological data, the smallest contribution towards the accumulation of the requisite facts may have its value.

The history of the cranium to which I now direct attention, is as *follows*: *Dr. Michael Turner was present in the Crimea, and in* active service on the medical staff, during the Anglo-French Invasion of 1855, and witnessed the capture of Kertch. At that period, its population was estimated at between seven and eight thousand; and was composed of Tartars, Cossacks, Greeks, Russians, and a sprinkling from the tribes bordering on the shores of the Black Sea. More than two-thirds of the whole population of the Crimea are a mixture of the pure Asiatic Mongol Tartar with the modified European Turk; and except among the nobles, or murses, and partially among the population of the northern valleys, they abundantly indicate their Tartar origin in their features.

The antipathies which the mutual wrongs of Russian and Turk have created, have obliterated in the minds of the latter any idea of kindred with the Tartar, or semi-Turkish population of the Crimea; and after the sack and pillage of the town of Kertch, the Turkish troops carried their violence so far, as to open and spoil the graves in the Christian cemeteries; and on finding trinkets and relics in some of the first they opened, a general desecration ensued. The articles found consisted of rings, beads, and amulets, and also of crucifixes, and images of the saints; and these were sought for, and appropriated by the Turkish soldiers, with the utmost indifference to the condition in which they left the ravished occupants of the desecrated graves. Whilst strolling in the neighbourhood of the city where such shameful spoliation had been carried on, Dr. Turner passed through a large cemetery, which he was led to believe had been confined exclusively to members of the Greek Church, from the number of large marble crosses heading the graves. Most of the latter were opened, and rifled of such of their contents as could tempt the cupidity of the spoilers; and the skeletons and partially desiccated remains of their former occupants lay strewed about the ground. On looking into one of the open graves which had been thus despoiled, he was tempted to examine the nature of the sepulture, as the body still remained in its original position; and also to ascertain whether the marauders had left anything of value behind. He accordingly jumped into the grave, and turning over the loose soil with his hands, he was struck, on uncovering the head, by its long black hair and beautiful teeth. The body was not yet returned to the dust, so that the interment was one of no very remote date from that of the disturbance of what cannot properly under such circumstances be called its last resting place. The muscles, which still remained on the forehead, were dry and contracted, and across the forehead, and round the head, was a broad gold fillet, sufficiently indicating that the grave was tenanted by one who had occupied a high social rank. No other ornaments or relics were observed, the whole of those having doubtless been removed by the original riflers of the grave. Dr. Turner did not consider it a very serious aggravation of the desecration to which the dead had already been subjected, to possess himself of the skull, which struck him as one peculiarly marked with indications of former delicacy and beauty; and through the kind intervention of my friend Dr. C. W. Covernton, it has since been transferred to me.

From a comparison with other skulls procured by him, Dr. Turner at first inclined to the opinion that he had acquired the cranium of a Greek lady. The breadth at the parietal protuberances, however, along with other marked features, differ essentially from the Greek type of head; and as there were many Circassians among the wives of the most influential and affluent families in the city, the probabilities he conceives are, *a priori*, in favour of its being ascribed to a people celebrated for the beauty of its females, and for their frequent introduction both to Turkish and Græco-Russian households around the Euxine. An elaborately sculptured, but broken marble cross at the head of the grave, added additional proof that the once loved and lost beauty of some Kertch household, whose remains were subjected to such indignities, had been ranked, during her life-time, among the finest porcelain of human clay. Under the peculiar system which prevails in oriental households, however, and by which Christian as well as Ottoman alliances are influenced, a wide area is embraced within the possible origin of the beauties who adorn such eastern homes; and a comparison of the most strikingly marked characteristics of this head with the varying types of cranium pertaining to what may be regarded, even in some respects philologically, as the European ethnic area, would rather suggest its classification among Armenian than Circassian forms. The materials however, for arriving at any very definite conclusion are limited, and perhaps inadequate for positive generalizations; and it may suffice to put on record such minute descriptions and measurements, as may afford the means of future comparison.

The skull, as already indicated, is that of a female, of fully 30 years of age. The bones of the face are characterised by great delicacy. The zygomata are slight, and inclose a space proportionally small by the zygomatic arch. The face is altogether small for the head, giving the idea of a considerable breadth of forehead; though it will be seen that the parietal diameter is in greater excess than usual when compared with the frontal diameter. The teeth, the beauty and completeness of which attracted the attention of Dr. Turner when first exposed in the cemetery at Kertch, have since mostly fallen out: but with the exception of one decayed molar, such as remain fully accord with his description, and with the delicacy of the superior and inferior maxillaries. The forehead is smooth, with no projection of the frontal sinuses, and no depression above the nasal suture, but with marked frontal protuberances at the upper angles of the forehead. The occipital protuberance is slight, and the profile of the calvaria exhibits a markedly vertical aspect both in its frontal and occipital outlines. The frontal bone passes somewhat abruptly from the forehead to the top of the skull, thereby giving a square form to the profile instead of the more usual arched curvature; so that, with the nearly vertical occiput, the cranium has a singularly compact outline, when viewed in profile. The parietal bones are large, with a gradually increasing protuberance to their greatest diameter, a little behind the line of the mastoid processes. Owing to this the outline of the vertical aspect presents somewhat the form of a truncated wedge, narrowing gradually and with a nearly uniform diminution until abruptly rounded off into the forehead at the frontal protuberances.

The following are the most characteristic measurements of this skull:—

Longitudinal Diameter	6.7
Parietal Diameter	5.7
Frontal Diameter	3.8
Zygomatic Diameter	4.4
Vertical Diameter	4.7
Intermastoid Arch	14.3
Intermastoid Line	3.7
Length of Face	6.2
Horizontal Circumference	19.7

Dr. J. Aitken Meigs has remarked in his "Cranial Characteristics of the Races of Men," chiefly founded on data supplied by the

Morton Collection in the Academy of Sciences at Philadelphia: "The extreme South-eastern section of the European ethnic area, occupying mainly the table-land of Iran, is represented in the Morton Collection by six Armenian, two Persian, and one Affghan skull. A general family resemblance pervades all these crania. They are all, with one exception, remarkable for the smallness of the face, and shortness of head. In the Armenian skull, the forehead is narrow and well formed, the convexity extending upwards and backwards towards the parietal protuberances and laterally towards the temporal bones. The greatest transverse diameter is between the parietal bones. This feature, combined with the flatness of the occiput, gives to the coronal region, an outline resembling a triangle with all three angles truncated, and the base of the triangle looking posteriorly. In fact, the whole form of the calvaria is such as to impress the mind of the observer with a sense of squareness and angularity. The dimensions of the orbits are moderate; the malar bones small, flat, and retreating; zygomatic processes slender, and the general expression of the face resembling that of the Circassians, from which latter it differs in being shorter." On nearly all those points, the Kertch skull closely corresponds to this description of Armenian Cranial characteristics. The only noticable exceptions are in the orbits, which may be described as somewhat large, but with their perpendicular diameter the greatest; and in the length of the face, which has more of the assigned Circassian dimensions. The formation of the lower jaw indicates a delicately pointed and small chin. Viewed altogether, the peculiar features of this skull are well defined, and sufficiently characteristic to enable an experienced craniologist to assign it, with little hesitation, to the Iranian group, with its included Georgians, Lesgians, Circassians, and Armenians. Of those the last named—to which the Kertch cranium seems by its most prominent peculiarities to belong,—possesses some characteristics of peculiar interest. In his "Varieties of Man," Dr. Latham places the Armenians foremost among his "unplaced stocks;" but regarding them from a philological point of view, he seems to consider them as in some respects presenting indications of a link between the Indo-European and the Semitic groups; but he also adds: "it is through the Armenian, that the transition from the Mongolidæ, to the Atlantidæ, is most likely to be recognised." Obtained as the skull now described has been, under peculiar and somewhat unique circumstances, and with a minuteness of evidence relative to the social condition and the vital characteristics originally pertaining to her whose sepulture was involved in the ravages of the Crimean war, which led to its acquisition: the facts recorded in this paper, may possess some slight value as a contribution to data now accumulating from the labours of many independent workers, and destined ultimately to establish physical ethnology on a sure and well-determined basis.

GEOMETRIC PROBLEMS RELATING TO CURVES HAVING DOUBLE CONTACT.

BY J. W. MARTIN, LL.D., TORONTO.

Read before the Canadian Institute, 10th March, 1860.

Given a circle and a point o inside it; if a line passing through o and cutting the circle in the points a and b be divided externally in m, so that $\frac{(ao \times bm)}{am \times bo} = \frac{co}{c'o}$ segments of fixed chord passing through o then tangent to circle from m will be to perpendicular from m on rt the polar of o as secant of angle which cc' makes with diameter of circle passing through o to unity.

If ac bc' be produced, they will meet at p, a point on rt; and if from p we draw a line parallel to cc' it must pass through m, the anharmonic ratio of the pencil $p.\ a\,o\,b\,m$ being as $co{:}c'o$, and as the angle $bpm = bc'o = bap$ $(pm)^2 = am \times bm =$ square of tangent to circle from m, locus of m $\therefore$ is $s - e^2a^2 = o$, $s = o$ being equation of circle, and $a = o$ that of the line rt. In like manner, if p be joined with middle point of cc' joining line meets ab in m'. So that $\frac{ao \times bm'}{am' \times bo} = \frac{co}{c'o}$ and locus of m' is $s + e'^2a^2 = o$, e' being $=$ to cc' divided by sum of perpendiculars on rt from c and c'. The conics $s - e^2a^2 = o$, $s - e'^2a^2 = o$, are polar reciprocals. The lines coc', $fo\,f'$, each of which makes with diameter of circle passing through o, an angle whose secant$=e$ are parallel to the asymptotes of the conic $s - e^2a^2 = o$, and polars of the points where the asymptotes cut (rt), while the line joining their middle points is the polar of the centre. If from any point on (rt) tangents be drawn to circle and the two conics, points of taction lie on a right line passing through o, and anharmonic ratio of any four points on rt is = that of lines drawn from o to points of taction where tangents drawn from the four points on (rt) touch either conic.

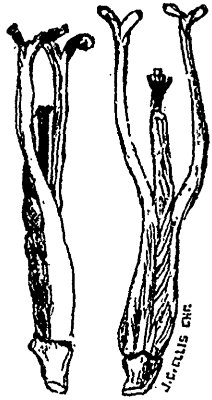
ON SOME QUESTIONS IN RELATION TO THE THEORY OF THE STRUCTURE OF PLANTS OF THE ORDERS BRASSICACEÆ AND PRIMULACEAE.

BY THE REV. W. HINCKS, F.L.S.
PROFESSOR OF NATURAL HISTORY IN UNIVERSITY COLLEGE, TORONTO.

Read before the Canadian Institute, 11th February, 1860.

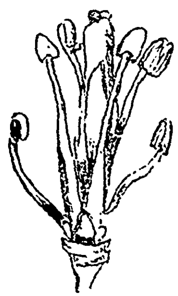
The title of my paper embraces two distinct Botanical notes or topics which would appear interesting to the theoretical Botanist who has had some experience in such studies, but which would hardly at all have engaged the attention of most practical students of the science, and which it may almost seem hopeless to attempt making intelligible to those who do not make Botany a pursuit, yet it appears to me that as we all profess an interest in the advancement of science, and as our society is formed on the plan of social meetings for mutual entertainment and improvement as well as for endeavouring to produce something that may be useful beyond our own circle —it must be right that whilst I only bring before you what I hope may either posess some novelty, or at least contribute something towards a just decision on disputed points—I should endeavour to bring it forward in such a manner that all who desire various information may understand the question under discussion and the opinions proposed for their acceptance—I am afraid indeed that after all many will think the subject little worth their notice; I venture however to assure them, that inquiries of this kind are deemed of some importance as well as curiosity, so that if I were so fortunate as to contribute any thing towards clearing either of the doubtful points about to be examined, I should find many to agree with me, that the labour would not be wasted. I have only reason to fear my being found unequal to the difficulties of the case. I am however, giving you speculations which have occasionally occupied me during a number of years and which are founded on cautious and repeated observations of facts, not without study of the judgments pronounced by writers of authority which I desire to treat with respect whilst I freely examine their merits.

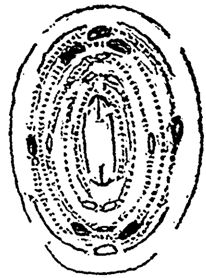
Our first inquiry relates to the real nature of the order of the parts of the flower in a tribe of plants well known as *cruciform flowers*, and familiar from the wall-flower, stock, cabbage, and several common weeds constituting the order *Brassicaceae* of Lindley. Plants of this order are distinguished by a very peculiarly constructed seed-vessel divided into two cells by a partition which is not easily brought into analogy with anything in the ordinary constitution of seed-vessels, and whilst the calyx and corolla consist of four parts each in the usual relative positions, the number of parts in the Gynoecium or ovary, is *apparently* only two, and the androecium shows six stamens in two pairs with a single lower one at each end. Now it is well known to all who have attended to the subject, that every flower consists of circles of leafy organs variously modified in their development, the inner circle consisting of what are now called carpels, of which the apex is the stigma, and the margin usually at least bears the ovules—next follows the circle of stamens, often indeed several circles, each stamen consisting of a filament corresponding to the mid rib of the leaf, and an anther most commonly of two cells formed from its expansion, the parenchyma of one surface being converted into pollen grains. Outside the stamens occur the petals, or inner enveloping circle, and outside all the calyx, consisting of pieces called sepals. Now it is the general rule that these circles alternate one with the other in regular order, the inner circle being indeed peculiarly liable to have its number of parts reduced by pressure, and the others exhibiting occasional anomalies from adherence, irregularity and suppression or abortion, either of a whole circle or some part of it. Every flower is formed on a certain definite plan as to the number of circles and of parts contained in each, and as to their relative position, and when there is any deviation from equal numbers and alternate arrangement we always expect to be able to offer some explanation which shall shew it to be a case naturally arising under the general law. Although five is the natural number of parts in each circle in Exogenous plants, it is by no means unusual to meet with four or even, under pressure, more especially towards the interior of the flower, and rarely in the outer circles, three and two. In cruciform flowers the calyx and corolla have four parts each, the stamens are six, unequal, and there are seemingly but two carpels though with an anomalous connection between their opposite edges, which demands explanation. The late eminent Robert Brown, than whom a higher authority cannot be appealed to, considered the fruit as *really* consisting of two carpels, whose placental edges are at the part where they first touch each other, but the exterior covering of each of which extends until the parts meet in a median line, thus forming a spurious partition. There is nothing impossible in this explanation since the separation of the principal portion of the carpel leaving the placenta in its position, occurs in other instances, and there are probably examples sufficient to justify the notion of the spurious partition though it is something extraordinary. Considering the cases in which a line is observable down the middle of the partition, and others in which there is a partial or even entire separation into two parts, it must I think be agreed that the partition is due to the meeting in the middle of two parts projecting from the placental lines; but I confess I greatly prefer another theory which had occurred to myself many years ago, and which I have since ascertained to have been proposed by Lindley and defended by Kunth. This is the supposition that the fruit is really formed of four carpels, two of which are abortive, their remains forming the partition, whilst the remarkable circumstance of the stigmas being in the line of the placentæ is accounted for by the fact that each stigma is double, formed by the union of one from each carpel, the tip of the carpel dividing into two portions as in some other instances. This explanation is greatly confirmed by the manner in which the alternate circles of cruciferous flowers exhibit increased development in opposite directions, the largest pair in one circle being opposed to the lesser pair in the next. On the whole, though Dr. Gray adheres to Brown's theory, I cannot but consider the other as better explaining all the facts of the case, and it is especially confirmed to my mind, by the consideration of the deviative structure of Parolinia. In vain it is contended by Moquin Tandon and Webb, in their ingenious article on the subject, that the prolongation of the valves into extremities with two horns is an unmeaning and unimportant accident. I cannot look at the figures which are said faithfully to represent the fruit of Parolinia, and which I have copied for your



information, without perceiving the very parts which make up the ordinary fruit in this tribe, the two portions of the stigmas and the styles being kept from adhering as usual by an unusual development of the inner pair of carpels, which is usually only represented by the partition, but here forms an interior style with its stigmas. Occasional monstrosities of the wallflower, in which it has a four-celled fruit, and the genus *Te tracelion* which has one constantly, confirm this explanation. I am not even quite sure that the theory of the four carpels, as maintained by Lindley and Kunth, is identical with that which I am defending, as I have not here access to the works in which it is proposed, but my own theory applies Brown's explanation of the structure of the stigmas with what seems to me a much more satisfactory view of the nature of the partition and the general symmetry of the flower, and I sh[illegible] be disposed to say is liable to no serious objection.

The difficulty, however, which yet remains, [illegible]specting the nature of the peculiar arrangement of the stamens, is probably to be accounted much greater than that which I think has been overcome respecting the structure of the fruit. Dr. Lindley, in his account of the order in the Vegetable Kingdom, if I rightly understand his meaning, (which however is obscurely expressed) takes essentially the same view which I am disposed to favour. His words are: "their stamens are "arranged thus: two stand opposite each of the anterior and "posterior sepals, and one opposite each of the lateral sepals; "there being six stamens to four sepals, instead of either four "or eight as would be normal. Now in which way does this arise? "Is the whorl of stamens to be considered double, one of the series "belonging to the sepals and one to the petals, and of these a part "imperfect? I am not aware of any such explanation having been "offered, nor do I know of a better one. It appears to me that the "outer series is incomplete by the constant abortion of the stamens "usually belonging to the anterior and posterior sepals, the two pairs "that remain belonging in fact to four petals." The obscurity here arises from th[illegible] [illegible]pression "belonging to the sepals and petals," applied to circles of stamens, which is unusual and not very expressive. There is also an absence of any notice of the glands in this connection, though they must be accounted rudimentary stamens and ought unquestionably to be taken into account in any attempt at restoring the true symmetry of the flower. They are found in numbers varying from two to ten in different species. In some genera indeed entirely suppressed, but in others conspicuous enough and offering us assistance, which is surely not to be rejected. The extreme number ten with the four carpels, 6 stamens usually developed, 4 petals and 4 sepals gives 28 parts or 7 circles of 4 parts each. There is a peculiarity in the arrangement of the parts which also affords us important assistance in explaining the appearances, to which sufficient attention has not been given. If we look at the calyx or outer circle, we perceive that the anterior and posterior sepals are exterior to the lateral pair and a





little more developed, in some instances so much as to produce small gibbous protuberances like incipient tails at their bases. The circle of petals is very equal, alternating with the sepals. It is followed by the shorter pair of stamens, which has the appearance of being exterior to the other four, and the circle according to our theory, is completed by two glands, (being rudimentary stamens,) which in many genera are conspicuous in front of each pair of longer stamens and opposite to the anterior and posterior sepals. The four longer stamens form the next circle, which like the petals is equal; within this are to be placed 4 glands, which are manifest in many species at each side of the outer stamens, but whose position is really interior to the longer stamens. There is another set of glands of which two immediately behind the shorter stamens are not unfrequently to be traced, very rarely the least appearance of the whole four, and then we arrive at the carpels of which the most developed pair having their faces to the smaller stamens and lateral sepals, bear the seeds on their edges and unite a stigmatic segment from eac . to form the stigmas immediately over their line of junction; the other pair of carpels lies just within this, and is almost uniformly abortive, the remains forming the partition, but in Parolinia, as we have seen, it produces stigmas.

It is remarkable that whilst analogies for the illustration of the structure of Brassicaceae have been sought—not always judiciously—from Papaveraceae and Fumariaceae, so little use has been made of Capparidaceae the order really most nearly related to Brassicaceae, and belonging to the same alliance. In this we have the same tendency to circles of four parts, but slight irregularity intrudes to a greater extent, and the number of stamens is increased by the development in many instances of those which in Brassicaceae only appear as glands in a rudimentary condition, and of more numerous circles. The carpels are generally supported on a protrusion of the axis, so that the fruit seems elevated on a stalk within the flower, a circumstance not unknown in Brassicaceae, as is seen in the remarkable genus Stanleya. The irregular number of stamens, 6 instead of 8 or 12, is found in many Capparidaceae. In some of them a spurious partition more or less perfect occurs, and has probably the same origin as in Brassicaceae, in others the carpels are reduced to two, and the pod is like one of a cruciform flower without the partition. In others again more carpels than two seem to be developed, perhaps a whole circle of four.

I must now explain the theory of Moquin Tandon and Webb, adopted and defended by Dr. Gray, for explaining the peculiarity of the Androecium in Brassicaceae. They leave the glands out of consideration and reduce the six stamens to a single circle of four primitive parts, by regarding each pair of the longer stamens as one original organ, separated into two by a principle called chorisis or deduplication. This principle, first proposed for the explanation of certain phenomena by Duval, consists in a supposed tendency of parts originally single, and which must be taken as one in explanation of symmetry, to divide themselves either into several layers, one in front of the other, or in several portions standing side by side. This has been extensively applied by some botanical theorists, but Dr. Lindley entirely rejects it, maintaining that there is no sufficient evidence of any single case. I cannot but admit that it affords some very plausible explanations of difficult cases, yet some of those most relied upon, seem to me very doubtful; several obviously to admit of other better explanations, and even if there is some truth in the principle, it is peculiarly liable to abuse in its applications. Dr. Gray follows Brown in believing that the Gynoecium of Brassicaceae consists of only two carpels, a view which has been already sufficiently commented upon. Though particular in describing the glands, and employing them as characters of genera and species, he does not refer to them in judging of the symmetry of the order, and he relies on the arguments of Moquin Tandon and Webb, to prove that the six stamens represent one circle of four. These arguments then I must review:—

1st. In some species, as *Clypeola cyclodontea*, the filaments of the solitary stamens are furnished with two teeth, one on each side, whilst those of the double stamens have but one on their outer side. If we join these two stamens together, so that they form but one, a bidentate filament will result entirely similar to the solitary stamens. This is without doubt plausible, but we must recollect that the two anthers of a stamen represent the two sides of the lamina of the leaf, their presence therefore shows the completeness of the organ, whilst the tooth-like projection on the filament is only representative of a wing to the petiole, or an angle at the bottom of the leaf; since then each of the pairs of stamens has its two anthers, we must conclude that the development of the tooth at the inner side in the pair of stamens is prevented by the two organs being so near to each other, which causes a pressure unfavourable to such development.

2nd. In other species a longer or shorter portion of the filament remains simple, thus in *Sterigma tomentosum* the division takes place as far as the middle; and in Anchonium Billardieri in a third part only of the upper portion of the filament Here the position of the longer stamens, double only in their upper portion, is exactly the same as that of the solitary stamens—these facts I reply afford no argument, because they are easily explained by partial coherence (an exceedingly common occurrence) of organs really distinct, and the two anthers tend to prove this distinctness.

3. In Vella pseudo-cytisus we find in the place of the double stamens, a single one, its filament being frequently rather broader, sometimes divided only at its summit, sometimes entirely undivided, but bearing in that case an anther wholly or partially gemminated. I have not examined this case, but the description indicates a more complete coherence of two organs. Instances however which occur, of only one stamen being found in the place of the pair, are only cases in which that circle, as

well as several of the others, has two of its parts suppressed, and are perfectly consistent with the theory previously explained.

4. Many Cruciferæ become tetrandrous by pelorization; others are normally so. In either case the four stamens are thus equal. This, I answer, is at least as easily explained in our theory as on that of the separation of stamens into two.

5. Finally, certain Cruciferæ instead of returning to the quaternary type recede from it. The single stamens undergo a change analogous or very similar to that of the double pair. One of us has observed flowers of *Matthiola incana*, in which the single stamens were cleft throughout their entire length, each portion being provided *with half an anther and half a filament.* M. Lestiboudois speaks of a Cheiranthus Cheiri in which these stamens were completely geminated, not laterally as the longer pair, but from without inwards. M. Lermye met with a flower of the same species, which had the lower stamens doubled exactly as the upper. Now let these cases be fairly considered: the first appears to show that a stamen may be occasionally slit vertically, but it is acknowledged that there is no increase in the real number of parts, each portion it is expressly stated consisting of half an anther (a single cell,) and half a filament. This may render more probable Dr. Lindley's explanation of Fumariaceæ, destroying an analogy on which Dr. Gray greatly relies, but it supplies no argument in favour of a single primitive organ having become two perfect ones with all their parts. The case observed by Lestiboudois is apparently not one of Chorisis, but of development under the stimulus of cultivation of the gland, which is often noticed *within* the short stamens; that of M. Lermye requires to be more accurately described, but it must not be hastily assumed to have consisted in a division of the single stamen into two perfect ones, it may have been a case like that seen by one of the authors themselves, a mere fissure of the stamen into two parts; or it is perhaps just possible that the single stamen may have been suppressed, and the two glands which often appear at each side of it, developed into a pair of stamens. It is certainly not sufficient without more exact information, to support or overthrow a theory. Dr. Gray relies so completely on the arguments of Messrs. Moquin Tandon and Webb, that I need only farther observe that even if Chorisis furnishes the true explanation of the symmetry of Fumariaceæ, which I hold to be very doubtful, there is no such relation between that order and Brassicaceæ as would oblige

us to extend the principle to this latter, and I cannot but conceive that a more probable explanation has been proposed.

My note on the structure of Primulaceæ relates to one point which I have not seen rightly explained. In this order the stamens are observed to be constantly opposite the petals, a circumstance which always seems to need some explanation. In the present case, I think it evident that it is due to the abortion of a circle of parts belonging to the intermediate position between the petals and stamens and alternating with both. A careful examination of almost any Primula, the Auricula affording an excellent example, shows that the coloured eye of the flower consists of a series of pieces like the petals, as it were fastened on to them, and in such an order that the middle of each arch of the eye is exactly placed between two of the petals. In the genus *Aretia* this is still more evident. In Samolus a set of abortive stamens occurs between the petals, and the same is the case with several species of Lysimachia; in Cyclamen this organ is also easily observed, and in Glaux the proper corolla as well as its double is suppressed. From these examples we are enabled ideally to restore the lost circle, where it is most completely suppressed, and thus to comprehend the true symmetry and the reason of a seeming departure from a general rule. In how many other cases of opposite circles a similar explanation may be justified, I will not presume to say. In respect to this order I think it entirely satisfactory, but it is not the only one conceivable, for any one who has carefully considered a Camelia, in which the numerous circles of petals, instead of alternating as is usual, are forced into regular lines radiating from the centre, will be ready to admit the possibility of parts which are normally alternate becoming opposite by a sort of twist, and what occurs occasionally as a variety, may occur uniformly or nearly so, from a like cause, more constantly operating on a particular tribe, so that we are by no means driven to imagine without evidence an intermediate circle, in every instance of opposite parts, nor is there any necessity for assuming the occurrence of Chorisis where it cannot be distinctly proved.

THE RELATION WHICH CAN BE PROVED TO SUBSIST BETWEEN THE AREA OF A PLANE TRIANGLE AND THE SUM OF THE ANGLES, ON THE HYPOTHESIS THAT EUCLID'S 12TH AXIOM IS FALSE.

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Read before the Canadian Institute, 25th February, 1860.

I propose to prove in the present paper, that, if Euclid's 12th Axiom be supposed to fail in any case, a relation subsists between the area of a plane triangle and the sum of the angles. Call the area A; and the sum of the angles S; a right angle being taken as the unit of measure. Then

$$A = k\,(2-S);$$

k being a constant finite quantity, that is, a finite quantity which remains the same for all triangles. This formula may be considered as holding good even when Euclid's 12th Axiom is assumed to be true; only k is in that case infinite.

Before proceeding with the proof of the law referred to, I would observe, that, while on the one hand Euclid's 12th Axiom is assuredly *not an Axiom* in the proper sense of the term, that is, not a self-evident truth, on the other hand *it has never been demonstrated* to be true. I even feel satisfied, from metaphysical considerations, that a demonstration of its truth is impossible. Legendre's supposed demonstration, which Mathematicians appear to have accepted as valid, was shown by me, in the *Canadian Journal* for November, 1856, to be erroneous.* For the sake of those who may not have the former

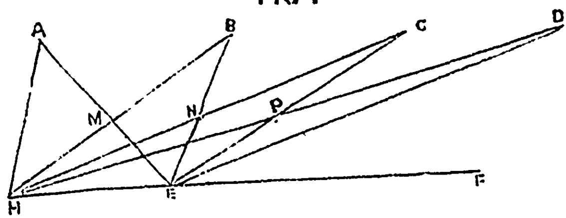
* In an Essay on Mathematical Reasoning, appended to his Mathematical Euclid, Dr. Whewell refers to the attempts which have been made to dispense with Euclid's 12th Axiom. "No one," he writes, "has yet been able to construct a system of Mathematical truth by means of Definitions alone, to the exclusion of Axioms; though attempts having this tendency have been made constantly and earnestly. It is, for instance, well known to most readers, that many mathematicians have endeavoured to get rid of Euclid's Axioms respecting straight lines and parallel lines; but that *none of these essays have been generally considered satisfactory*." The last clause in this statement calls for remark. Sir John Leslie objected to Legendre's reasoning; but on grounds which (as Professor Playfair showed in the *Edinburgh Review*) are altogether frivolous. Playfair maintained that Legendre's proof was satisfactory; and since then, till the publication in the *Canadian Journal* of the article above referred to, mathematicians have—by their silence at least—acquiesced in his verdict. If Legendre's proof has been generally considered unsatisfactory, why did none of those by whom such a view was taken show where the reasoning is defective

numbers of the *Journal* at hand, the substance of my refutation of Legendre is given in an Appendix to the present paper.

Proposition I.

The sum of the angles of a triangle AHE (Fig. 1) is not greater than two right angles.

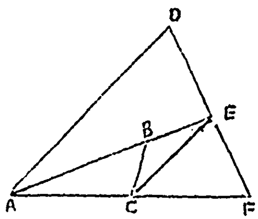
FIG I



For, produce HE to F. Bisect AE in M. Draw HMB, making MB = HM; and join BE. In like manner construct the triangle CHE; N being the middle point of BE; and CN being equal to HN. In like manner construct the triangle DHE; P being the middle point of CE; and DP being equal to PH. And so on indefinitely. Denote by S, S_1, S_2, &c., the sum of the angles of the triangles AHE, BHE, CHE, &c., respectively; and by A_1, A_2, A_3, &c., the angles HBE, HCE, HDE, &c., respectively. Then it is plain that the quantities S, S_1, S_2, &c., are all equal to one another. Also, as the number n becomes indefinitely great, the angle A_n becomes indefinitely small. For, the sum of all the angles in the series, A, A_1, A_2, &c., is less than AEF; and, since the series, A, A_1, &c., may be made to contain an indefinite number of terms, those terms which are ultimately obtained must be indefinitely small, in order that AEF may be a finite angle. But, the exterior angle DEF being greater than the interior and opposite angle DHE, S_3 cannot exceed two right angles by D. And $S_3 = S$. Therefore S cannot exceed two right angles by D or A_3. In like manner it may be proved that S cannot exceed two right angles by A_n, whatever n be. And A_n is ultimately less than any assignable angle. Therefore S cannot exceed two right angles by any finite angle whatsoever.

Cor. 1.—If a line AE (Fig. 2) be drawn from A, an angle of a triangle ADF, to a point in the opposite side; and if the sum of the angles of the triangles DFA and EAF respectively be S and S_1; S is not greater than S_1. For let s be the sum of the angles of the triangle ADE; then

FIG 2



$$S = F + FAE + EAD + D,$$
$$\text{and, } S_1 = F + FAE + AEF.$$
$$\therefore S_1 - S = AEF - (EAD + D),$$
$$= AEF + AED - (AED + EAD + D),$$
$$= 2 - s;$$

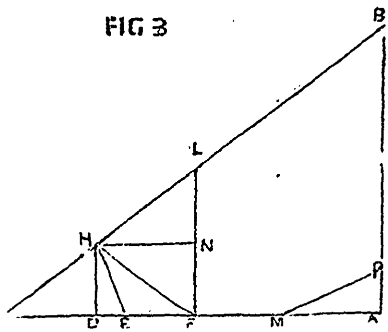
a right angle being taken as the unit of measure. But, by the Proposition, s is not greater than 2. Therefore S is not greater than S_1.

Cor. 2.—From B, a point within the triangle DAF, draw BC to a point C in AF; and let S_2 be the sum of the angles of the triangle ABC. Then S_2 is not less than S. For, produce AB to E; and join EC. Then, by Cor. 1, S_2 is not less than the sum of the angles of the triangle AEC; which sum, again, is not less than S_1, or the sum of the angles of the triangle AEF; and S_1 is not less than S. Therefore S_2 is not less than S.

Proposition II.

If any triangle CHE (Fig. 3) have S, the sum of its angles, equal to two right angles, every triangle has the sum of its angles equal to two right angles.

FIG 3

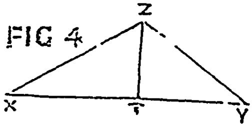


For, CE being a side which is not less than any other side of the triangle CHE, let fall HD perpendicular on CE. Then HD cannot fall without the base CE; else (supposing it to fall beyond E) the angles CEH would be greater than a right angle: hence, because CE is not less than CH, the angle CHE would be greater than a right angle: so that S would be greater than two right angles: which (Prop. I.) is impossible. Produce CD to F; making DF = CD.

Draw FN perpendicular to CF, and equal to HD. Produce it to L, making LN = HD; and join HL and HN. Then the sum of the angles of the triangle CHD is not less (Cor. 1. Prop. I.) than S; that is, it is not less than two right angles. Therefore (Prop. 1) it is equal to two right angles. But (4. I. E.) the triangles CHD and FHD are every way equal. Therefore angle HCD = angle HFD. But the sum of the angles DCH and DHC has been proved to be equal to a right angle. Therefore the angle CHD = the angle DHF = the angle HFN. Therefore (4. I. E.) the triangles DHF and HFN are every way equal; and hence HNF is a right angle. Consequently (4. I. E.) the triangles HNF and HNL are every way equal. Hence

$$\begin{aligned} \angle LHF + \angle CHF &= 2 \angle NHF + 2 \angle CHD \\ &= 2 \angle HFD + 2 \angle CHD \\ &= 2 \angle HCD + 2 \angle CHD \\ &= 2 \text{ right angles.} \end{aligned}$$

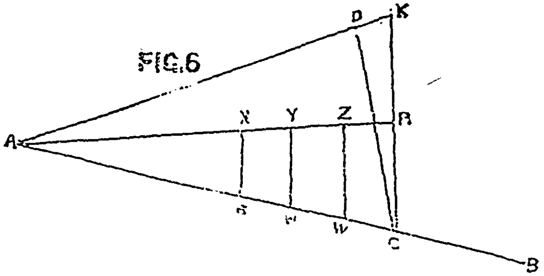
Therefore CHL is a straight line. Also the sum of the angles of the triangle LCF is equal to two right angles. Hence, beginning with the hypothesis that the sum of the angles of the triangle CHD is equal to two right angles, we have found that the sum of the angles of the triangle LCF is equal to two right angles; the sides of the latter triangle being double those of the former. By going on in the same manner, we can find a triangle ABC, with one of its angles BAC a right angle, and the sum of all its angles equal to two right angles; and having each of the sides greater than any given line. Suppose now that $x\,y\,z$ (Fig. 4) is any triangle whatsoever; $x\,y$ being not less than either of the other sides: in which case, as DH (Fig. 3) falls within the base CE of the triangle HCE, the perpendicular $z\,t$ from z (Fig. 4) upon $x\,y$ falls within the line $x\,y$. Then the triangle BAC (Fig. 3) being constructed in the manner above described, so that each



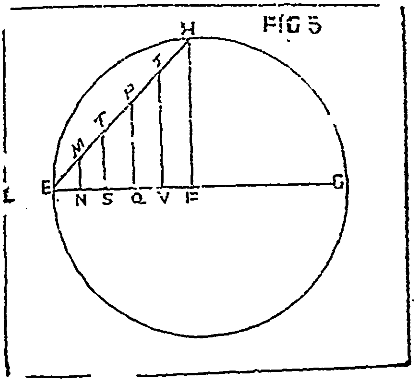
of the sides BA and AC may be greater than any of the lines xz, xy, yz, in Fig. 4, cut off MA equal to xt, and AP to zt. The sum of the angles of the triangle BAC is not greater (Cor. 2, Prop. I.) than the sum of the angles of the triangle PAM or xzt. That is, the sum of the angles of the triangle xzt is not less than two right angles. Hence (Prop. I.) it is equal to two right angles. In like manner the sum of the angles of the triangle *zty* is equal to two right angles. Therefore the sum of the angles of the triangle *xzy* is equal to two right angles.

COR.—Either every triangle has the sum of its angles equal to two right angles, or no triangle has the sum of its angles so great (see Prop. I.) as two right angles.

PROPOSITION III.



If the base CD of a triangle ACD (Fig. 6) be diminished indefinitely according to any law, while neither of the other sides becomes greater than a given line AB, the area of the triangle ACD becomes ultimately less than any finite space L (Fig 5); and the sum of its angles does not ultimately differ from two right angles by any finite angle.



For, within the area L take a point F. Then, by choosing a radius sufficiently small, we can describe, with F as a centre, a circle lying wholly within L, and therefore less than L. Draw a diameter EG, with a radius HF perpendicular to it. Join EH; and from any point M in EH let fall MN perpendicular on EF. By bisecting NF, and again bisecting the parts obtained, and so on, we can divide NF into *n* equal parts; where *n* may be taken greater than any number that can be named. Let NF be so divided into the *n* equal parts, FV, VQ, ..., SN; the number n being taken so great that n times MN is greater than the given line AB. Let TV, PQ, &c., be perpendicular to NF. Suppose then the base DC of the triangle ADC (Fig. 6) to diminish, according to the law of its variation, until CD becomes less than FV; and, if AC be not less than AD, produce AD to K, making AK=AC. Join CK; draw AR perpendicular to CK; and cut off the parts Rz, zy, yx, &c., each equal to MN, until AR is exhausted; the last part being possibly less than MN. At the points of section, z, y, x, &c., raise the perpendiculars zw, yh, xb, &c. Then, because CD is (by hypothesis) less than FV or NS (Figs. 5 and 6), and because it is obviously greater than CR, NS is greater than CR. Also, because n times MN is greater than AB, and AC is (by hypothesis) not greater than AB, n times MN is greater than AC. Much more is n times MN greater than AR. And the parts Rz, zy, &c., were cut off each equal to MN. Hence the number of such parts is not greater than n; and the number of the spaces,

Rw, zh, yb, &c., ... (1)

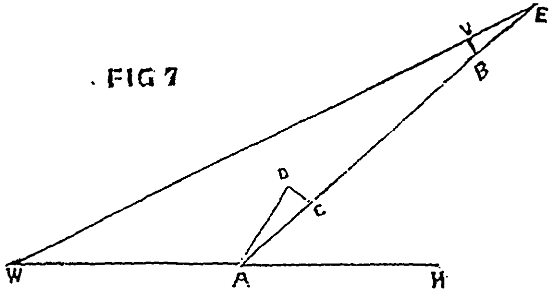
into which the triangle ARC is divided, is not greater than the number of the spaces,

FT, VP, ..., SM, ... (2)

into which the figure MNFH has been divided. But since NS is greater (as we have proved) than RC, and MN is equal to Rz, the space RzwC may be wholly inserted within the space MNSt, and is therefore less than that space. But RzwC is the greatest space in the series (1), and MNSt is the least in the series (2). Hence, since the number of terms in (1) is not greater than the number of terms in (2), the sum of the terms in (2) is greater than that of the terms in (1): that is, the triangle ACR is less than MNFH. Hence the triangle AKC is less than the circle EHG. Much more is the triangle ADC less than the space L.

In the next place, suppose, if possible, that, as CD is indefinitely diminished, the sum of the angles of the triangle ACD ultimately differs from (in which case it must, by Prop. I, be less than) two right angles by more than the finite angle BAH (Fig. 7); BA being, as in the previous case, a given line which neither of the sides, AC, AD, ever exceeds. Produce HA to any point W, and AB to any point E. Join EW; and draw BV perpendicular on EW. Let the base DC (Figs. 6 and 7) be diminished, according to the law of its variation, until DC is less than VB. Then ultimately the triangle ADC may be wholly inserted (as in Fig. 7) within the triangle EWA. For,

FIG 7



since the sum of the angles of the triangle ADC falls short (by hypothesis) of two right angles by more than the angle BAH, the angle DAC must be ultimately less than the angle BAW; and therefore DA falls between BA and WA. Again, the point D cannot lie beyond EW; else DC would be greater than the perpendicular from C upon EW, and consequently (since AC is less than AB) greater than BV: which is contrary to hypothesis. Hence (Cor. 2, Prop. I.) the sum of the angles of the triangle ADC is not less than the sum of the angles of the triangle EWA But the sum of the angles of the triangle ADC is (by hypothesis) less than the angle EAW: which is impossible. Consequently, as DC diminishes indefinitely, neither of the other sides, AD, AC, becoming at any stage greater than AB, the sum of the angles of the triangle ADC cannot ultimately differ from two right angles by any finite angle.

Proposition IV.

If ABC and FCD (Fig. 8) be two triangles of equal areas, and having the angle ACB equal to the angle FCD; and if S be the sum of the angles of the triangle ABC, and *s* the sum of the angles of the triangle FCD; S and *s* are equal to one another.

For, if the sides FC and CD be equal to AC and BC, each to each, the triangles ABC and FCD are equal in every respect. It is therefore only necessary to consider the case in which FC is greater than AC: in which case (in order that the triangle ABC may not be a part of the triangle FCD) CD must be less than BC. Place the triangles so that AC and CF may be in the same straight line; in which case, since the angle ACB is equal to the angle FCD, BC and CD are in the same straight line. Cut off CE equal to CB, and CK equal to CA; and join EK. Then (4. I. E) the triangle ECK is every way equal to the triangle ABC. Therefore triangle ECK=triangle FCD; and consequently triangle EDH=triangle FKH. Cut off HM equal HE, and HP equal to HD; and join MP. Then triangle HMP is every way equal to triangle EDH. Therefore triangle HMP=triangle HKF; and consequently triangle KMN=triangle FNP. The point P cannot fall beyond F, so as to make HP greater than HF; for, if it did, the point M would (in order that the triangle HKF may not be a part of the triangle HMP) fall between K and H; in which case the angle F would be greater than the angle HPM; that is, F would be greater than the angle HDE; whereas, since the exterior angle of a triangle is greater than either of the interior and opposite angles, the angle HDE is greater than F. In like manner it can be proved that the point P does not coincide with F. And therefore P is between H and F; which implies that M is beyond K in the line HKM. Hence, from the two given equal triangles ACB and FCD, with the angles at C equal to one another, we have passed to the equal triangles KMN and FNP, with the angles at N equal to one another. Let S_1 be the sum of the angles of the triangle KMN; and s_1 the sum of the angles of the triangle FNP. Then

$$\begin{aligned} S_1 - s_1 &= M + MKN - (F + FPN) \\ &= E + EKC - (F + FDC) \\ &= B + A \quad - (F + FDC) \\ &= S - s. \end{aligned}$$

Let the same construction that was made with reference to the triangles ABC and FDC be now made with reference to the triangles KMN and FNP; that is to say, cut off NQ equal to NM, and Nr equal to NK. Join Qr. Cut off RL equal to RP, and RT equal to RQ. Join TL, cutting NF in h. Then Q must lie beyond P, on the line NPQ; for, if it did not, the point r would lie beyond F on the line NrF; in which case the angle Q would be greater than the angle NPF; that is, the angle E would be greater than the angle CDF: which is not true. And the point Q lying beyond P, the point r must fall between N and F. Hence, as above, we can prove that the triangles Trh and FLh are equal to one another; and, if S_2 be the sum of the angles of the triangle Trh, and s_2 the sum of the angles of the triangle FLh,

$$S_2 - s_2 = S_1 - s_1 = S - s.$$

We can go on thus indefinitely, forming a series of pairs of equal triangles KMN and PNF, Trh and FLh, &c., to which there is no limit; and, if S_n be the sum of the angles of the first triangle in the n^{th} pair, and s_n the sum of the angles of the second triangle in the n^{th} pair,

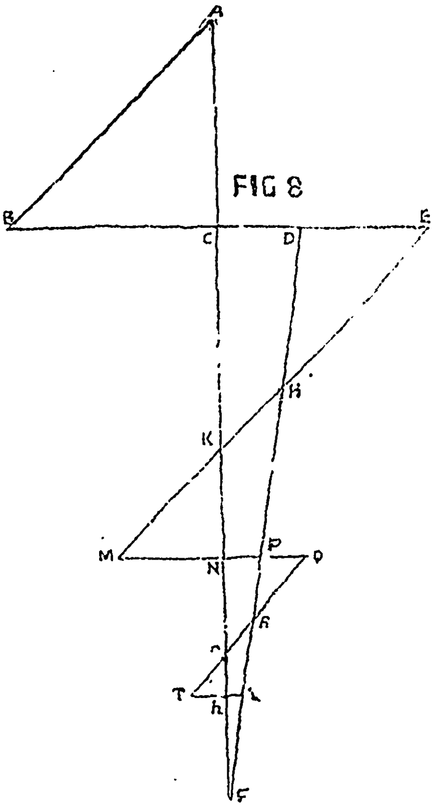


FIG 8

$$S_n - s_n = S - s.$$

But, as the series of triangles, FPN, FLh, &c., is indefinitely increased in number, by a continued repetition of the construction above described, the base (such as hL) of the triangle ultimately obtained becomes indefinitely small. For

$$\begin{aligned} BC &= CD + DE \\ &= CD + NP + MN \\ &= CD + 2NP + hL + Th, \end{aligned}$$

and so on, without limit; so that, if the base (such as hL) of the triangle (such as FLh) ultimately obtained did not become indefinitely small, the finite line BC would be greater than the sum of an indefinite number of lines, none of which was less than a given finite line: which is impossible. Since therefore the base (such as hL) of the triangle (such as FhL) ultimately obtained must become indefinitely small, the sum of the angles of the triangle (such as FLh) ultimately obtained cannot (Prop. III.) differ by any finite angle from two right angles That is, S_n does not continue, as n is indefinitely increased, to differ by any finite angle from two right angles. In like manner, if it be observed that CF is greater than the sum of the lines, AC or CK, KN, rh, &c., it will appear that s_n does not ultimately differ by any finite angle from two right angles. Therefore ultimately the quantity, $S_n - s_n$, is less than any assignable angle. But it was proved that

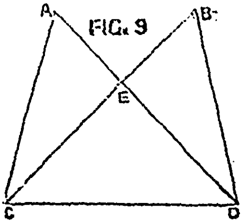
$$S_n - s_n = S - s.$$

Therefore S and s do not differ by any finite angle; that is, they are equal to one another.

Cor. 1.—If two triangles ACB and FCD, having the angle ACB equal to the angle FCD, be unequal; and ACB be the greater; then S, the sum of the angles of the triangle ACB, is not greater than s, the sum of the angles of the triangle FCD. For, the same construction as that described in the Proposition may be made, until a point is reached at which one of the triangles obtained, as Thr, has the sides, Th, hr, either less than Lh and hF respectively, or greater than Lh and hF respectively. The former of these cases cannot occur; because then the triangle Thr would be less than the triangle FhL, and consequently the triangle ACB less than the triangle FCD: which is impossible. Hence the latter case must occur, viz.: that a triangle Thr must be found, having Th greater than hL, and rh greater than hF; and therefore, since the triangle FhL can be wholly inserted in the triangle Thr, the sum of the angles of the triangle Thr is not greater (Cor. 2, Prop. 1.) than the sum of the angles of the triangle FhL. Hence S is not greater than s.

Cor. 2.—If two equal triangles (Fig. 9) ACD and BCD have the common base CD, and if S be the sum of the angles of the former, and s the sum of the angles of the latter, S is equal to s. For the difference between S and s is the same as the difference between the sum of the angles of the triangle ACE and the sum of the angles of the triangle BDE. But, by the Proposition, these latter quantities are equal to one another. Therefore $S = s$.

FIG. 9

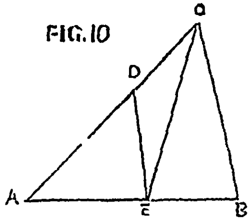


Cor. 3.—Let the two triangles (see fig. to Cor. 2) ACD and BCD, on the common base DC, be unequal. Then, if S be the sum of the angles of the triangle ACD, and s the sum of the angles of the tri-

angle BCD, and if the former triangle be greater than the latter, S cannot be greater than *s*. For the difference between S and *s* is equal to the difference the sum of the angles of the triangle ACE and the sum of the angles of the triangle BED. But the former of these quantities (since the triangle ACE is greater than the triangle BED) is not greater (Cor. 1) than the latter. Therefore S is not greater than *s*.

Cor. 4.—In the case supposed in the previous Corollary, should the assumption be made that the angles of a triangle are not (see Cor. Prop. II.) equal to two right angles, S must be less than *s*. For, by the reasoning in the Proposition and in the foregoing Corollaries, it appears that the difference between S and *s* is equal to the difference between the sum of the angles of a triangle ACB (Fig. 10) and the sum of the angles of a triangle ADE inscribed within the former in the manner shown in the figure. Suppose, if possible, that S=*s*. Then the angles of the triangle ADE are together equal to those of the triangle ACB. Therefore (Cor. 1. Prop. I.) they are equal to those of the triangle ACE. Therefore angle ADE is equal to the sum of the angles DCE and DEC. Therefore the angles of the triangle DEC are together equal to two right angles: which is at variance with the hypothesis on which we are at present proceeding. Hence S is not equal to *s*. But (Cor. 3) S is not greater than *s*. Therefore S is less than *s*.

FIG. 10



Cor. 5.—If the triangle ABG (Fig. 11) be divided by the straight line AC into two parts, of which ACG is the greater, two lines AD

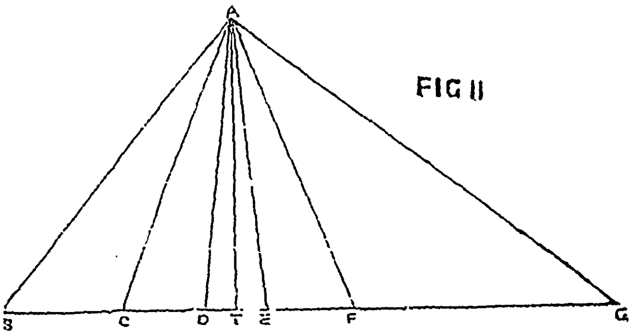


FIG 11

and AE can be drawn, cutting off triangles ADC and AEC, the one less, and the other greater, than ABC, but neither of them differing from the triangle ABC by an area so great as a given area; while at the same time the difference between the sum of the angles of the triangle ABC and the sum of the angles of either of the triangles, ACD, ACE, is less than any given angle.

If the hypothesis be made that the angles of a plane triangle are together (see Cor. Prop. II.) equal to two right angles, the problem can be effected by the methods which Euclid describes.

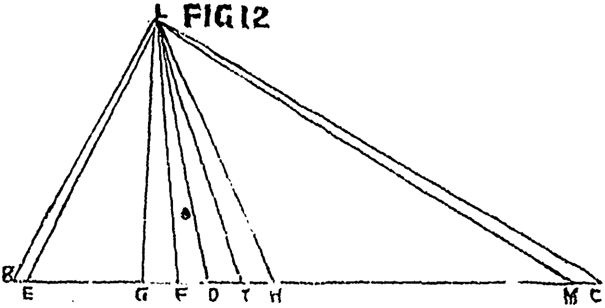
We onlyneed, therefore, to show how it can be performed on the hypothesis that the angles of a plane triangle are not equal to two right angles. Bisect CG in F; and join AF. The triangles ABC and ACF have a comon side AC. Therefore (Cor. 4) the area of the one will (on the hypothesis on which we are now proceeding) be less than, equal to, or greater than, the area of the other, according as the sum of the angles of the former is greater than, equal to, or less than, the sum of the angles of the latter. Nov we can find the sum of the angles of each by construction. Therefore we can tell whether the triangle ACF is less than, equal to, or greater than, the triangle ABC. Should the triangle ACF be greater than the triangle ABC, we may repeat the construction; bisecting CF, and drawing a line from A to the point of section. By repeating this construction sufficiently often, the base (such as CD) of the triangle (such as ACD) ultimately obtained will become less than any assignable line; and hence the area of the triangle will become (Prop. III.) less than any assignable area, and consequently less than the triangle ABC. Let ACD, the triangle obtained by bisecting CE, and joining AD, be less than the triangle ABC; the triangle AEC being greater than ABC. Bisect DE in the point *t*; and join A*t*. Find, as above, whether the triangle AC*t* is less or greater than the triangle ABC, or equal to it. Should it be greater, the triangle ABC lies between the limits, ACD and AC*t*; but should it be less, the triangle ABC lies between the limits AC*t* and ACE. And so on. Ultimately we obtain two limits, which we may suppose to be represented by the triangles ACD and ACE, between which the triangle ABC lies, the base DE of the triangle ADE, which is the difference of the limits, being made as small as we please. Therefore (Prop. III.) the area of the triangle ADE becomes ultimately indefinitely small; so that each of the triangles ACD and ACE becomes indefinitely near in area to the triangle ABC.

At the same time (Prop. III.) the sum of the angles of the triangle ADE becomes indefinitely near to two right angles. Let S be the sum of the angles of the triangle ABC; S_1, the sum of the angles of the triangle ACD; S_2, the sum of those of the triangle ACE; and δ, the difference betwixt two right angles and the sum of the angles of the triangle ADE. Then δ is equal to the difference betwixt S_1 and S_2; so that, since δ ultimately becomes indefinitely small, the difference betwixt S_1 and S_2 ultimately becomes indefinitely small. And (Cor. 4) S is intermediate betwixt S_1 and S_2. Therefore ultimately its difference from either of them becomes indefinitely small.

PROPOSITION V.

If a line LD (Fig. 12) be drawn from L to any point D in the base of a triangle LBC; and if A represent the area, and S the sum of

FIG 12



the angles, of the triangle LBD; and a represent the area, and s the sum of the angles, of the triangle LDC; then, reasoning on the hypothesis that the angles of a plane triangle are (see Cor. Prop. II.) unequal to two right angles, we can prove that $A : a = 2 - S : 2 - s$; a right angle being taken as the unit of measure.

For, by taking FD sufficiently small, the triangle LFD can be made (Prop. III.) smaller than any given space; the sum of its angles also falling short of two right angles by an angle less than any given angle. Having cut off a small triangle LFD from LBD, we can next (Cor. 5, Prop. IV.) draw lines LG, LG_1, LG_2, &c., (only the first of these lines is expressed in the figure), in such a manner that the triangle LGF shall differ from the triangle LFD by a space less than any given space, the sum of its angles at the same time differing from the sum of the angles of the triangle LFD by an angle less than any given angle; and that the triangle LGG_1 shall differ from the triangle LGF by a space less than any given space, the sum of its angles at the same time differing from the sum of the angles of the triangle LGF by an angle less than any giveh angle; and so on, till the whole of the triangle LBD has been exhausted, except a remainder LBE, which is less than the triangle to which it is adjacent. Proceed next to divide the triangle LDC into triangles LDT, LTH, &c., related to the triangle LFD and to one another in the same manner as the triangles LFG, LGG_1, &c.; the remainder LMC being finally left over, less than the triangle to which it is adjacent. Then, since any two adjacent triangles in the series,

$$\text{LDF, LFG, } LGG_1\text{, \&c.,} \qquad (1)$$

which together constitute the triangle LDE, may be made as nearly equal as we please, we can make every one of them as nearly equal to the first as we please. And, from a similar consideration, it appears that we can at the same time make the sum of the angles of any triangle in the series as nearly equal as we please to the sum of the angles of the first. In like manner we can make every one of the triangles in the series,

$$\text{LDT, LTH, \&c.,} \qquad (2)$$

which together constitute the triangle LDM, as nearly equal to LDF as we please; the sum of the angles of each being at the same time made as nearly equal as we please to the sum of the angles of the triangle LDF. Let there be N terms in the series (1), and n in the series (2). Then

$$LED = N \text{ times } LFD \backsim Q; \qquad (3)$$

Q being a quantity which we may arrange to have as small as we please. In like manner,

$$LMD = n \text{ times } LFD \backsim q; \qquad (4)$$

q being a quantity which we may arrange to have as small as we please. Again, if S_1 be the sum of the angles of the triangle LFD, $S_1 \backsim h_1$ the sum of the angles of the triangle LFG, $S_1 \backsim h_2$ the sum of the angles of the triangle LGG_1, and so on, and S_2 the sum of the angles of the triangle LED, we have

$$S_2 = NS_1 - 2(N-1) \backsim h_1 \backsim h_2 \backsim \&c.$$
$$\therefore 2 - S_2 = N(2 - S_1) \backsim h; \qquad (5)$$

where, since we may arrange to have h_1, h_2, &c., as small as we please, we may understand *that h is a quantity which we can arrange* to have as small as we please. In like manner, if S_3 be the sum of the angles of the triangle LDM, we can get

$$2-S_3 = n\,(2-S_1) \backsim k; \;\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (6)$$

k being a quantity which we can arrange to have as small as we please. Hence, from (5) and (6), we can order our construction so as to make the ratio, $2-S_2 : 2-S_3$, as nearly equal as we please to the ratio, $N : n$; the same means by which this is secured having the effect of rendering [see (3) and (4)] the ratio, LED : LMD, as nearly equal as we please to the ratio, $N : n$. Hence we can order our construction so as to make the two ratios,

LED : LMD,

and, $2-S_2 : 2-S_3$,

as nearly equal as we please. This is accomplished by the means above described, whatever be the length of the line FD. It may therefore be still accomplished, though FD be taken indefinitely small. But as FD is indefinitely diminished, the area of the triangle LFD, and therefore that of the triangle LBE is (Prop. III.) indefinitely diminished. Hence, as FD is indefinitely diminished, the ratio of the triangles LED and LBD ultimately becomes indefinitely near to a ratio of equality; the ratio of the triangles LDM and LCM also becoming, under the same circumstances, indefinitely near to a ratio of equality. Consequently, by taking FD small enough, the ratio, LBD : LCD, or, $A : a$, becomes indefinitely near to the ratio, LED : LMD. In like manner it can be proved, that, as FD becomes indefinitely small, the ratio, $2-S_2 : 2-S_3$, approximates indefinitely to the ratio, $2-S : 2-s$. Therefore the ratio, $A : a$, cannot differ *by any finite amount from the ratio, $2-S : 2-s$. That is,*

$$A : a = 2-S : 2-s.$$

Proposition VI.

If BGC and HCF (Fig. 13) be any two plane triangles, S being the sum of the angles of the former, and s the sum of the angles of the latter; then, reasoning on the hypothesis that the angles of a plane *triangle* are not equal (see Cor. Prop. II.) to two right angles, we can prove that

$$\text{tri. BGC} : \text{tri. HCF} = 2 - S : 2 - s :$$

a right angle being taken as the unit of measure.

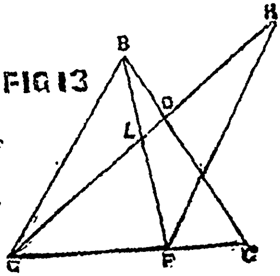


FIG 13

For join BF; and let S_1 be the sum of the angles of the triangle CLF and S_2, the sum of the angles of the triangle BCF. Then (Prop. V.),

$$\begin{aligned} &\text{triangle BCG} : \text{triangle BCF} = 2-S : 2-S_2 ; \\ &\text{and, triangle BCF} : \text{triangle LCF} = 2-S_2 : 2-S_1 ; \\ &\text{and, triangle LCF} : \text{triangle HCF} = 2-S_1 : 2-s. \\ &\therefore \text{triangle BCG} : \text{triangle HCF} = 2-S : 2-s. \end{aligned}$$

COR.—If A be the area of the triangle BCG, we have

$$A = k\,(2-S) ;$$

k being a finite quantity, which remains the same for all triangles.

APPENDIX.

Legendre endeavours to make it appear,* without the assistance of any special Axiom, that C, the third angle of a triangle ABC, is determined from the other two, A and B, independently of the magnitude of *c*, the intervening side. If this be made out, all the properties of parallel lines can easily be deduced. The difficulty is to demonstrate the fundamental position. But here it may be well to quote Legendre's own words: "Soit l'angle droit égal à l'unité, alors les angles A, B, C seront des nombres compris entre 0 et 2; et puisque

* It may be proper to mention that Legendre has treated the subject of parallel lines in two *different* ways, one in the text of his *Elements of Geometry*, and the other in the notes to that work. Playfair considers the former method "quite logical and conclusive," as well as the latter; only objecting to it that it is "long and indirect," and too "subtle" for "those who are only beginning to study the Mathematics." But, as the *admission* of Legendre himself is on record that this method is *not* conclusive; as it is, in fact, palpably the reverse—taking for granted what *requires proof*, as much as *Euclid's* Axiom does; no further attention need be given to it. The proof here criticised—a proof, the *fallacy* of which was for the first time (it is believed) pointed out by the author of the present paper in the *Canadian Journal* for November, 1856—is that advanced by Legendre in the Notes to his Geometry.

$C = \phi$ (A, B, c), je dis que la ligne c ne doit point entrer dans la fonction ϕ. Un effet, on a vu que C doit être entierement determiné par les seules données A, B, c, sans autre angle ou ligne quelconque ; mais la ligne c est heterogene avec les nombres A, B, C ; et si on avait une equation quelconque entre A, B, C et c, on en pourrait tirer la valeur de c en A, B, C, d' où il resulteroit que c est egale a un nombre, ce qui est absurde. Donc c ne peut entrer dans la valeur de C et on a simplement $C = \phi$ (A, B)." Sir John Leslie committed the unaccountable mistake of supposing the argument here stated, to be, "that the line c is of nature heterogeneous to the angles A and B, and therefore cannot be compounded with these quantities"—whereas the argument plainly is that c, which is a line, cannot be expressed in terms solely of A, B, C, which are numbers. "The quantities A, B, C," says Playfair, in his exposition of Legendre's reasoning, are "angles; they are of the same nature with numbers, or mere expressions of ratio, and, according to the language of Algebra, are of no dimension. The quantity c, on the other hand, is the base of a triangle; that is to say, a straight line, or a quantity of one dimension. Of the four quantities, therefore, A, B, C, c, the first three are of no dimensions, and the fourth or last is of one dimension. No equation, therefore, can exist involving all these four quantities and them only: for, if there did, a value of c might be found in terms of A, B, and C; and c therefore would be equal to a quantity of no dimensions: which is impossible."

In this reasoning it is assumed, that, because C is *determined by* A, B, c, therefore C can be *expressed in terms of* A, B, c. Now Legendre does not prove that when a quantity is determined by certain others, it can be expressed in terms of them ; and I affirm that such *a principle, without limitation, is not true.*

For example, consider the angle C of the triangle ABC. And let it be observed that I mean the angle itself, that is, the inclination of a and b to one another, and not the numerical value of the angle, calculated upon the supposition that a right angle, or any other angle, has been assumed as a unit of measure. The angle C is *determined by* the sides, a, b, c; yet it cannot be expressed in terms of these quantities alone ; because *the value of an angle can only be indicated by pointing out its relation to some other angle or angles* ; and therefore cannot be expressed by means simply of lines It is true that *the numerical value* of C may be expressed in terms of a, b, and c :

viz., in an equation where only the ratios of a, b, and c, occur; the ratios being numbers. Thus, if $b=\beta a$, and $c=\gamma a$, we might have

$$\text{numerical value of } C = f(\beta, \gamma).$$

But this is altogether a different thing from saying that C *itself*, the angle properly so called, the inclination of a and b to one another, can be expressed in terms of a, b, and c. Now, if C itself (not its numerical value, but the absolute angle) is determined by a, b, and c; and if, nevertheless, it cannot in the nature of things be expressed in terms of a, b, and c; Legendre's demonstration, the very foundation of which is that a quantity which is determined by certain others, can be expressed *in terms of* them, falls to the ground.

Should it be maintained that C (the angle itself) may be expressed in terms of the numbers β and γ, *a right angle being understood to be the unit of measure*; or more fully thus:

$$C = \text{right angle} \times f(\beta, \gamma);$$

I reply that in the same manner the line c, in Legendre's reasoning, may be expressed in terms of A, B, C, *some line* L being understood to be the unit of linear measure; thus:

$$. = L \times f(A, T, C).$$

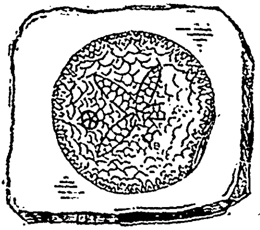
ON A NEW SPECIES OF AGELACRINITES, AND ON THE STRUCTURAL RELATIONS OF THAT GENUS.

BY E. J. CHAPMAN,

PROFESSOR OF MINERALOGY AND GEOLOGY IN UNIVERSITY COLLEGE, TORONTO.

Read before the Canadian Institute, 17th March, 1860.

Introductory Notice.—The accompanying figure represents, on a somewhat enlarged scale, the upper side of the undescribed species of Vanuxem's rare and interesting genus *Agelacrinites*, referred to in a late number of the *Canadian Journal*. As there stated, the species in question was discovered amongst some Lower Silurian fossils, from the Trenton Limestone of Peterborough, Canada West, collected by Mr. W. M. Roger, of the University of Toronto. It is dedicated to the able palæontologist of the Geological Survey of Canada, whose researches have so greatly added to our knowledge of the obscurer organisms of the Silurian age, and who has done so much, in all respects, for the advancement of Canadian Palæontology.



E. J. C. DEL. EWS SC

The present communication is sub-divided into two short sections. The first contains a detailed description of the new species. This description, it should be remarked, however, is founded on a single example. The second section comprises an analytical review of the genus *Agelacrinites* in general, more especially with regard to its structural relations and affinities.

1. *Description of Agelacrinites Billingsii.*—Body, circular, or nearly so. In the specimen on which this description is based, its diameter exactly equals half an inch. It is slightly convex above, and flat, or apparently somewhat concave below. From the centre of the upper side, five rays, composed each of a double series of alternating or interlocking plates, radiate towards the margin of the disc, and terminate in well-defined points at about the twelfth of an inch from this margin. The rays, in the solitary specimen under examination, exhibit no traces of pores, even when strongly magnified. Nevertheless, pores may have been, and probably were, originally present. It is easy to conceive how minute orifices of this kind might become obliterated during fossilization; whilst, on the other hand, the object of the rays is altogether inexplicable, unless we look upon them as really representing ambulacral areas. Moreover, poriferous ray-plates have actually been discovered in certain examples of *Agelacrinites;* and analogy, consequently, would lead us to infer that, in all, they existed originally. These rays, at their origin, leave a small central space covered by larger and somewhat rhombic plates. The latter appear to be five in number, and to constitute the first ray-plates, one being common to two adjacent rays. Very possibly, however, each of these rhombic plates may be divided through the centre, longi-

tudinally; for the specimen is at this spot much broken, and the plates are pressed more or less one over the other. The inter-radial spaces and the margin of the disc are covered by numerous, irregularly disposed, scale-like, and partially imbricating plates. At the margin these are very small, exceedingly numerous, and arranged in three or four irregular rows, with their lo ⸮. .t diameter pointing towards the centre of the disc. To these succeed a series of larger plates, having their greatest diameter in a direction at right angles to that of the border plates, or, in other words, parallel with the circumference of the disc. To these succeed, again, other and somewhat smaller plates, all partially overlapping. This arrangement of the surface plates seems to be an extreme modification of that which obtains in *A. Hamiltonensis* of Vanuxem, and *A. Bohemicus* of F. Roemer; but the larger plates merge gradually, as it were, into the others, and thus there is no defined circle of large plates separating (as in the latter types) the border plates from those of the centre. Finally, in one of the inter-radial spaces, at a distance of about one-sixth of an inch from the centre of the disc, a well-marked "pyramidal orifice" is situated This, in the specimen under examination, is about one-twenty-fourth of an inch in diameter, and is made up, apparently, of ten plates, in two sets of five—one set alternating within the other, as in Hall's *Hemicystites parasitica*. The under side of our species remains unknown, but, in the specimen examined, it is not attached to a shell or other organic body; and hence, as shewn moreover by examples of other species, the genus cannot properly be considered a parasitic one.

Agelacrinites Billingsii differs essentially from our Canadian *A. Dicksoni* of Billings, (and also from the *Edrioaster Bigsbyi* of that palæontologist), by the possession of *short* and *straight* rays, and by its numerous marginal plates. It is also at once distinguished by its straight rays, independently of other characters, from the typical Devonian species, *A. Hamiltonensis* of Vanuxem, and the more recently discovered Carboniferous species, *A. Kaskaskiensis* of Hall. It agrees, on the other hand, somewhat closely with Hall's *Hemicystites parasitica* = *Agelacrinites parasiticus* from the Niagara Limestone of New York; but, in this latter species, the rays are very narrow at their origin, and are connected there (in the centre of the disc) by a small tubercle or rounded plate. In place of becoming narrower also towards the margin (as in *A. Billingsii*) and terminating in well-defined points, they become rapidly broader, "coalesce with the plates of the body," (Professor Hall), and are altogether undefined at their extremities. These characters, as given in the Palæontology of New York (vol. 2, p. 245; and plate 51, figs. 18–20) from an examination of several specimens, are exactly the reverse of those which obtain in our new species. Whilst, also, (although this character is probably somewhat indefinite,) the small border plates in *A. Billingsii* form two or three circles, in *A. parasiticus* they appear to occur only in a single row.

2. *Analytical Review of the Genus Agelacrinites and its included species.*—The generic characters of *Agelacrinites* may be thus defined. Form, circular; stemless; flat or concave below, and somewhat convex above; and covered by numerous small plates, arranged in part irregularly, and in part in regular order. The definitely arranged plates form five rays (ambulacral areas, ?) which originate at the centre of the upper side of the body. These rays are either short and straight, or long and curved. They are also composed of a double series of small polygonal plates, interlocking along the central line of the ray; or, otherwise, of a single (?) series of plates (Roemer's *A. Rhenanus*). The irregularly arranged plates are elliptical or circular, variable in size, very numerous, thin, scale-like, and imbricating; or, imbricating at and around the margin of the disciform body, and joining by their edges in the more central part of the disc. The marginal plates are commonly very small, and, in some species, are separated from the more central plates, by a circle of comparatively large pieces. In the centre of one of these (interambulacral ?) spaces, and about midway between the apex of the body and the margin, is situated an orifice covered by a pyramid of five or more (moveable ?) plates. The apex itself, or centre and origin of the rays, is covered by a single circular plate; or is surrounded by five or ten angular plates—these latter constituting the first plates of the rays. Characters of the under side of the body, position of mouth, &c., not definitely known.

From this definition, it is clear, as, indeed, universally allowed, that *Agelacrinites* belongs to the ECHINODERMATA. In the present state of our knowledge, however, it is impossible to refer it satisfactorily to any one of the admitted Orders or Families of that class. With the Crinoids proper, and the Blastoids, it appears to have only general affinities; but with the Cystideans it is evidently closely connected: more especially by the possession in common of a pyramidal orifice or, so called, anal-pyramid. It differs from the cystidean structure, nevertheless, in many important respects. The peculiar rays, the imbricating plates, the absence of a stem, for example, are essential points of difference. The imbrication of the plates serves to connect it, through the genus *Protaster*, with the Euryales or the Ophiurians; and the conformation of the rays, in certain species, appears to afford another link in support of this view. But is it not equally related to the Echinida? After a careful consideration of the subject, I cannot refrain from hazarding an opinion that the position of the mouth, as usually given, is erroneous. In several species, as in *A. parasiticus* and *A. Kaskaskiensis* of Hall (*Geology of Iowa*, Vol. I., Part II., Plate xxv.) the centre or origin of the rays is a simple disc or rounded tubercle—incontestably, no mouth: and hence we may fairly assume, that, in other species, the mouth must also be situated elsewhere. The question then arises as to the real nature of the pyramidal orifice. This is usually looked upon either as an anal orifice, or as an ovarian aperture. Neither of these views is by any means certain, nor, indeed, apparently susceptible of proof. To consider this orifice as the mouth, however, appears a still less satisfactory conclusion. In the Crinoids proper, the true position of the mouth is still, strictly, unknown. It is considered in some genera to be in the centre of the "vault," or upper surface; and in others to occupy an excentric position, as between two of the arms, &c. This latter view is unsustained by any proof, beyond the mere occurrence of an orifice at the points in question. The excentric orifice may or may not be the mouth. But if we omit these forms from consideration, and turn to those types of Radiata, in which the position of the mouth is no longer doubtful, that organ, it will be seen, is invariably situated in the centre of the body, except in the Family of the *Spatangidæ*, the highest Family or natural group of the entire series. In the other Families of the ECHINIDA, in the ASTERIDA, OPHIURIDA, and other Orders in which the position of the mouth is truly known, the mouth is always central. This is evidently its normal position in the radiated type of structure, and one, consequently, that we should scarcely expect to see departed from, except in the case of those forms which stand at the higher limit of the series. Unless this view be adopted, we must almost necessarily assume, that, in the Radiata, there are certain natural groups (not yet thoroughly worked out) which are perfectly unconnected with each other; and in which, respectively, the higher forms foreshadow an advanced type of structure, whilst the lower forms present the normal type. The higher forms of a low group, however lowly organized as to their entire structure, will be thus in certain respects, in advance of the lower forms of a higher group. Whatever grounds there may be to believe that some law of this kind really holds good in Nature, its application in the present place would be evidently forced. Discarding therefore the idea, that, in the pyramidal orifice of the Cystideans and Agelacrinites, the mouth is represented, this latter organ must be sought for in another place. Reasons have already been stated against this being the centre of the rays. Its true position will be found, I believe, in the centre of the under side of the body. But—it may be urged in objection to this—the genus *Agelacrinites* is sessile: is attached by its under surface to shells and other foreign bodies: and hence the mouth cannot be there situated. Several examples, it is quite true, have been met with attached in this manner to brachiopod shells; but this is by no means a general condition of occurrence; and, rightly considered, is no proof of an original *permanent* attachment. It is just as exceptional a mode of occurrence, indeed, as that from which Vanuxem derived the name of the genus.

This suggestion as to the true position of the mouth, cannot, of course, be satisfactorily adopted, until confirmed by the examination of more perfect specimens than those hitherto discovered; or until the proper functions of the pyramidal orifice, in this genus and in the cystideans, are clearly ascertained. But under any view, it seems obvious, that, without a forced collocation, these peculiar forms cannot be placed in any existing group. In the present restricted state of our knowledge, at least, they must form a group apart. Mr. Billings (Decade III. of Canadian Organic Remains, under description of *Agelacrinites Dicksoni*) appears inclined to regard them as constituting a sub-order of Star-fishes; and he proposes to arrange them in this connection, under the term of *Edrio-asteridæ*. This name seems objectionable, however, on two grounds: first, because the supposed sessile (*id est*, parasitic,) condition of *Agelacrinites* is by no means provĕd; and secondly, because the relations of the genus to the Star-fishes—in so close a way, at least, as the name would imply—is not yet established. For these reasons I would suggest the term THYROIDA, in allusion to the valved aperture, as the name of the special group or order framed for the reception of these forms. The following scheme will then represent the probable relations of the various leading groups belonging to the Echinodermata generally:

Crinoida.
Blastoida.
Cystidea.—
Thyroida—
Asterida.
Ophiurida.
Euryalida.
Echinida.
Holothurida.

In the group THYROIDA, we have, at present, but one Family—that of the AGELACRINITIDÆ, comprising, probably, but one known genus: *Agelacrinites*. The recognised species of this genus are enumerated in the annexed tabular view:

Sub-kingdom RADIATA, Class ECHINODERMATA, Order THYROIDA, Family AGELACRINITIDÆ, Genus AGELACRINITES.

Synopsis of Species.

A.—LOWER SILURIAN SPECIES:

(Rays curved):

1. *A. Buchianus*, E. Forbes.
2. *A. Cincinnatiensis*, Roemer.
3. *A. Dicksoni*, Billings.
4. *A. (Edrioaster) Bigsbyi*, Billings.

(Rays straight):

5. *A. Bohemicus*, Roemer.
6. *A. Billingsii*, Chapman.

B.—UPPER SILURIAN SPECIES:

(Rays straight):

7. *A. parasiticus*, Hall.

C.—Devonian Species:

(Rays curved):

8. *A. Hamiltonensis*, Vanuxem.

9. *A. Rhenanus*, Roemer.

D.—Carboniferous Species:

(Rays curved):

10. *A. Kaskaskiensis*, Hall.

REVIEWS.

Journal de L'Instruction Publique, Vol. III. 1859. *Publié par le Département de l'Instruction Publique. Redigé par l'honorable Pierre J. O. Chauveau, Surintendant de l'instruction publique du Bas-Canada, et par M. Joseph Lenoir, du département de l'instruction publique, assistant rédacteur.* Montreal, Bas-Canada.

The Journal of Education for Lower Canada, Edited by the Honourable P. J. O. Chauveau, Superintendent of Education for Lower Canada, and by James Phelan, Esq., of the Department of Education, Assistant Editor. Vol. III, 1859. Montreal.

The receipt of the completed volumes of the French and English Journals of Education for Lower Canada at an early period of the present year, would have induced us to notice them with the commendations they are so well entitled to, had not an unusual pressure on our very limited space prevented our overtaking this, as well as other intended references to Canadian publications. The primary purpose of both Journals is, we presume, to furnish a vehicle for official and semi-official communications to Trustees, Teachers, and others connected with the various local branches of the educational department. The active and intelligent Superintendent of Education for Lower Canada has, however, availed himself of the existence of such periodicals to render them the mediums of a great deal of interesting and instructive information for both the French and English speaking population of the Lower Province. Along with a judicious selection from French and English periodicals, both Journals are also characterised by original articles and reviews of a very creditable character.

We can conceive of such a Journal materially contributing to popular education in many ways. Standard poems re-appear here, with novel claims to attention and interest. We find such an old and familiar favourite as Gray's Elegy, for example; but it assumes for us new Canadian attractions when read here, accompanied by the anecdote of Wolfe repeating it the night before his death-victory, as he rowed along the St. Lawrence, to visit some of the out-posts; and exclaiming to a companion officer—who heard the beautiful, and then recent poem, for the first time,—that he would rather be the author of that poem, than win the glory of the morrow's victory! What an added charm is thus given, for us, to that beautiful elegy, as we picture to ourselves the youthful general gliding along under the wooded heights of the St. Lawrence, the night before that memorable 13th of September, 1759, on which he fell in the crisis of his triumph, and repeating:—

> "The boast of heraldry, the pomp of power,
> And all that beauty, all that wealth e'er gave,
> Await alike the inevitable hour:—
> The paths of glory lead but to the grave."

In like manner the Centenary Burns Celebration at Montreal, gives occasion for other quotations equally familiar and welcome. Among other fruits of that remarkable recognition of the Scottish peasant bard, are translations of some of his popular verses. His "Caledonia" is thus paraphrased by a native Canadian, M. Joseph Lenoir, the assistant editor of the Journal:—

> "O myrtes embaumés, laissez les autres terres
> Nous vanter à l'envi leurs bosquets solitaires,
> Dont l'été fait jaillir d'enivrantes odeurs.
> J'aime mieux ce vallon, frais et riant asile,
> Où, sur un lit d'argent, coule une onde tranquille,
> Sous la fougère jaune et les genêts en fleurs."

The reader will not estimate the less, this offering from the Canadian to the Scottish muse, from having placed alongside of it, the corresponding stanza in its original homely Scottish guise:—

> "Their groves o' sweet myrtle let foreign lands reckon,
> Where bright-beaming summers exalt the perfume;
> Far dearer to me yon lone glen o' green breckan,
> Wi' the burn stealing under the lang yellow broom."

Properly speaking this quatrain is but half of the true stanza, but it is so rendered in our French Canadian version. Although presenting occasional counterparts such as this, and embracing a good deal of educational information in common, the French and English journals are quite distinct, though each characterized by the same commendable effort to adapt it to the special tastes and sympathies of its readers. Indeed a local interest and a Canadian feeling of a healthful kind pervade both Journals. Bishop Laval, the Hon. James McGill, Generals Brock, Wolfe, and Montcalm; Jacques Cartier, Champlain, and other notable names interestingly associated with the early history of the province, are introduced to the reader in connexion with historical narratives of discoveries made, Colleges founded, or victories won on Canadian soil. The illustrative wood-cuts are also appropriate, and well executed; including views of the most important public buildings of Lower Canada, of its monuments, and some of its most striking city scenes. The Editors also merit the high commendation of aiming at the very difficult achievement of dealing in an impartial and unsectarian spirit with the questions of education, which in the Lower Province are affected by elements of language, race, and creed, very partially felt in Upper Canada.

Feeling as we do, how greatly some means is required for getting hold of the whole population of Lower Canada, and developing among the people feelings of a common sympathy and interest in the spirit of intelligent progress which is at work in the great centres of our public provincial life, we cordially wish success to both Educational Journals, and shall welcome new evidences of improvement, such as we have good reason for anticipating, with each succeeding volume.

D. W.

On the Origin of Species by means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life. By Charles Darwin, M.A., &c. London, John Murray, 1860.

The idea of a *species* as conceived by most minds, is that of a distinct and independent creation, capable of continuing itself unchanged in all its fundamental characters, although subject to partial modification by the influence of external agencies. It is believed, moreover, by those who hold this view, that all our living species having been thus separately created from the beginning of the existing geological age or present condition of things, no real species (*id est*, a type-form capable of continuing itself) has originated, or is capable of being originated, by the intermixture of two distinct types. Such is the general, but not the universal, belief. An opposite view, dating probably from a very distant period, has been brought forward and *maintained*, from time to time, by many philosophic minds. This view is to the effect that what we call *species*, are no independent-creations—at least for the greater part—but are simply *varieties*, *arising from* the modification of a few original types, or, if pushed to its extreme length, of a single originally-existing organism. The *object* of Mr. Darwin's book is to impart an increased vitality and support to this view, by arguments based on a large series of facts, the accumulation of many years of research on his own part and on *that* of other naturalists. The present work purports to be merely a general synopsis of the materials thus gathered together, and of the results to which their consideration tends ; but it is on a sufficiently extended plan to enable us to test, fairly, the relative solidity of the structure which its facts and arguments support.

Although an hypothesis of this kind must naturally seem to those who consider the question seriously for the first time, as one wholly indefensible and preposterous ; it is nevertheless probable, that, few persons have ever made the close *contemplation* of Nature their study for any time, without having experienced, at one period or another, the visitation of sundry *hauntings* of a similar character. When we see, for example, certain forms, at first remarkably distinct, become more and more closely connected by after-discoveries, until the one appears to merge into the other, and our once clear definitions become no longer tenable ; when we see in many species the extraordinary varieties sometimes produced by the crossing and intercrossing of other varieties ; when we consider the transition stages of fœtal development, the homologies of organic structure, the presence of rudimentary organs in many forms, the marked relations which obtain more or less between all living and extinct types of the same series, with other facts of an allied kind—the question becomes forced upon us : why is this ? Why these relations, these homologies, these transition-phases of embryonic development, these rudimentary organs, these closely-connected forms, if all species were separate and distinct creations ? Why, in other words, this recognised unity of plan, amidst this variety of structure, unless by the long-continued modification of an original unit-organism ? Here, however, we merely express our inability *to fathom* the design of the CREATOR in these varied repetitions, so to say, of the CREATIVE THOUGHT; and the transmutation theory, with all Mr. Darwin's ingenious and eloquent reasonings, offers to us no real help in our difficulty. We yield willing homage to the unquestionable ability which his book displays in so many of its details; we go with him most willingly to a certain point, but there our steps are arrested by obstacles that we are altogether unable to surmount. In his introductory observations, for example, we find the following statements:

"Although much remains obscure, and will long remain obscure, I can entertain no doubt, after the most deliberate study and dispassionate judgment of which I am capable, that the view which most naturalists entertain, and which I formerly entertained—namely, that each species has been independently created—is *erroneous. I am fully convinced that species are not immutable*; *but that those* belonging to what are called the same genera are lineal descendants of some other and generally extinct species, in the same manner as the acknowledged varieties of any one species are the descendants of that species."

Now, if the author had confined himself to these limits; if he had sought, by his laborious collection of facts and his skilful deductions, to prove the truth of his opinion as here expressed—using the term *species*, not in its absolute or normal sense, but as limited by our present knowledge—many, we think, who cannot honestly follow him farther, would have become his willing disciples. That various so-called genera have merely the right to rank as species, we firmly believe, and confidently look forward to such researches as those in which Mr. Darwin is engaged, to afford direct proofs of this conclusion * Thus far then we are prepared to listen trustfully to Mr. Darwin's teachings, but when he seeks to carry his applications beyond this, we lose our convictions; certain broad and apparently insurmountable barriers stand up before us; and we find ourselves unable to believe, for example, in the probability of a true transition-link between the carnivorous, retractile-clawed Felidæ, and the four-stomached, hoofed, and herbivorous sheep: and yet this is nothing to what the theory advocated in Mr. Darwin's book would impose upon us.

* It is somewhat remarkable, that, with regard to genera and species, the Inorganic subdivision of Natural History should differ so completely from the Organic branches of that study. That which to the majority of Mineralogists is simply a species, to the Botanist and Zoologist would rank as a genus, and be subdivided into species and varieties. Mineralogy was at one time, in this respect it is true, in unison with these other departments; but notwithstanding various attempts from time to time, to raise its varieties into species, and to bestow upon these latter, "Natural History" names, the broader and more philosophic view has long prevailed.

"It may be asked how far I extend the doctrine of the modification of species. The question is difficult to answer, because the more distinct the forms are which we may consider, by so much the arguments fall away in force. But some arguments of the greatest weight extend very far. All the members of whole classes can be connected together by chains of affinities, and all can be classified on the same principle, in groups subordinate to groups. Fossil remains sometimes tend to fill up very wide intervals between existing orders. Organs in a rudimentary condition plainly show that an early progenitor had the organ in a fully developed state; and this in some instances necessarily implies an enormous amount of modification in the descendants. Throughout whole classes various structures are formed on the same pattern, and at an embryonic age the species closely resemble each other. Therefore I cannot doubt that the theory of descent with modification embraces all the members of the same class. I believe that animals have descended from at most only four or five progenitors, and plants from an equal or lesser number.

Analogy would lead me one step further, namely, to the belief that all animals and plants have descended from some one prototype. But analogy may be a deceitful guide. Nevertheless all living things have much in common, in their chemical composition, their germinal vesicles, their cellular structure, and their laws of growth and reproduction. We see this even in so trifling a circumstance as that the same poison often similarly affects plants and animals; or that the poison secreted by the gall-fly produces monstrous growths on the wild rose or oak-tree. Therefore I should infer from analogy that probably all the organic beings which have ever lived on this earth have descended from some one primordial form, into which life was first breathed."

It is very clear, as already stated, that many of the so-called species of naturalists, are not true species, but simply varieties; and hence, arguments founded merely on closely related forms, are of comparatively little weight as regards the main question here at issue. For the proper acceptation of the theory, it will be necessary to show the passage of one truly distinct type into another, or of these into some common parent-type, so as to render an explanation of the structural homologies and other relations existing between them. If this cannot be effected by reference to existing Nature, let us look back into the rock-preserved annals of the Past, and see if these will lend us any aid. Mr. Darwin is forced to acknowledge that Geology fails, in this respect, to furnish any direct support to his hypothesis. But then, he argues, the geological record is incomplete. In place of a full and connected history, it offers to us only a few isolated leaves of the great book of the Past. Granting this, it must nevertheless be considered highly adverse to his view—as he himself, indeed, has candidly stated—that in these stony annals we find everywhere the same unity of plan with the same distinctness of type as in existing Nature; and that in no part of the world can we glean from them any examples even approaching to a transitional series of forms, in the sense demanded by the theory. But leaving this subject for awhile, let us examine the theory itself, as modified and set forth in Mr. Darwin's Essay, a little more in detail. We will take in succession the more prominent chapters of the book, and attempt respectively, a brief analysis of their contents.

In his first chapter, the author discusses the variations to which species give rise under domestication. He considers more especially and in great detail, the various breeds of the domestic pigeon. He shews, and every one must be familiar with this fact, the extraordinary differences in external aspect, mode of flight, etc., exhibited by many of these. So great is this diversity of character, that Mr. Darwin thinks an Ornithologist would not hesitate to class most of these breeds as distinct species, if he met with them for the first time, and were led to suppose them wild birds; nay, that he would even feel warranted in placing them under several genera. And yet, Mr. Darwin regards all our known breeds as undoubted descendants of the rock pigeon, the *Columba livia*. The strongest fact, perhaps, in favour of this view, is the production from time to time in various breeds, of the normal colours of the supposed parent-type. The question however, is by no means proved. If these pigeons have all sprung from *Columba livia*, should there not be occasionally a more striking reversion to the characters of the original type? Are we moreover authorised to conclude from any *direct evidence*, that a pair of rock pigeons could ever produce the numerous varieties that we now possess? Mr. Darwin shews us that a certain amount of variation does constantly occur amongst pigeons generally, and hence he assumes *by inference* that in course of time, the variation being accumulative, so to say, we *might* obtain the breeds we now possess. It seems, however, as legitimate an inference, notwithstanding Mr. Darwin's able advocacy of the contrary view, that various sub-species or varieties of the pigeon were originally created; just as we believe the leading varieties of the dog and horse have sprung from originally-created varieties. We have certainly no authority to assume that the greyhound and the mastiff were not originally created as such, although capable of breeding together, and producing fertile offspring. We can produce varieties now, because we have varieties from which to produce them; but if we had to breed from a single variety, it seems evident that, in spite of the most judiciously-exercised selection in continuing the breed so as to produce the greatest possible variation, no great success could in this respect be arrived at; and a return to the characters of the original type would be constantly occurring. In the case of the dog, this is apparently allowed by Mr. Darwin, for, whilst expressing his conviction that all our domestic pigeon-breeds have descended from the rock pigeon, he does not regard our various dogs as the descendants of a single wild species. But granting that, in the case of the pigeon, and even in that of the dog, horse, &c., all known varieties have sprung from one existing or extinct type-pair—granting this—what does the admission amount to? Simply to the fact, that certain species are capable of great variation; but, after all, of a variation amounting to no real specific, much less generic, difference. Stay! cry the upholders of this theory: a certain amount of time is required for the production, in this manner, of changes to that extent. We point to the monumental records of Egypt—but these, we are told, are but the works of yesterday. We exhume the dead forms of the geologic Past—and the assumed imperfection of our record is brought against us. On this latter point however, we shall have more to say in the sequel.

In his succeeding chapter, the author discusses some important points connected with "variation under Nature;" but much of his argument is here based rather on the deficiency of our present knowledge, than on absolutely-proved facts. He points out for instance, how greatly certain naturalists differ as to what should be considered species and what varieties, in particular genera, more especially amongst plants and insects; but, rightly considered, although this may go far to prove the unnatural sub-divisions of the systematists, it cannot be looked upon as helping in any material way to explain the origin of true species: *id est*, of God's actual creations as distinguished from the necessarily imperfect conceptions of man. The grand argument of the chapter is founded on the (to a great extent, perhaps, undoubted) fact, that, in large genera, the amount of difference between the included species is often exceedingly small; and that such species present also, as a general rule, more varieties than belong to the species of smaller genera.

"From looking at species as only strongly-marked and well-defined varieties, I was led to anticipate that the species of the larger genera in each country would oftener present varieties than the species of the smaller genera; for wherever many closely related species (*i. e.* species of the same genus) have been formed, many varieties or incipient species ought, as a general rule, to be now forming. Where many large trees grow, we expect to find saplings. Where many species of a genus have been formed through variation, circumstances have been favourable for variation; and hence we might expect that the circumstances would generally be still favourable to variation. On the other hand, if we look at each species as a special act of creation, there is no apparent reason why more varieties should occur in a group having many species, than in one having few.

To test the truth of this anticipation I have arranged the plants of twelve countries, and the coleopterous insects of two districts, into two nearly equal masses, the species of the larger genera on one side, and those of the smaller genera on the other side, and it has invariably proved to be the case that a larger proportion of the species on one side of the larger genera present varieties, than on the side of the smaller genera. Moreover, the species of the large genera which present any varieties, invariably present a larger average number of varieties than do the species of the small genera. Both these results follow when another division is made, and when all the smaller genera, with from only one to four species, are absolutely excluded from the tables. These facts are of plain signification on the view that species are only strongly marked and permanent varieties; for wherever many species of the same genus have been formed, or where, if we may use the expression, the manufactory of species has been active, we ought generally to find the manufactory still in action, more especially as we have every reason to believe the process of manufacturing new species to be a slow one. And this certainly is the case, if varieties be looked at as incipient species; for my tables clearly show as a general rule that, wherever many species of a genus have been formed, the species of that genus present a number of varieties, that is, of incipient species, beyond the average. It is not that all large genera are now varying much, and are thus increasing in the number of their species, or that no small genera are now varying and increasing; for if this had been so, it would have been fatal to my theory; inasmuch as geology plainly tells us that small genera have in the lapse of time often greatly increased in size; and that large genera have often come to their maxima, declined and disappeared. All that we want to show is, that where many species of a genus have been formed, on an average many are still forming; and this holds good.

With regard to the deductions contained in this quotation, as bearing on the origin of actual species, two things have to be observed: first, that many of the so-called species of these large genera may not be, and in many cases decidedly are not, true species; and secondly, as already observed in the case of the dog, &c., many leading varieties in these genera, may be varieties of original creation, or sub-species if we choose to call them so; and thus, a larger amount of material for variation being provided in the one case than in the other, a more extended variation in the former will follow as a natural consequence;

It is just as rational to assume for example, that several pairs of a type or species *A*, differing slightly from one another but capable of fertile intermixture, were created with a single pair, or a smaller number of pairs, of another species *B*—as to suppose that these types with their varieties, and in addition, other types *C*, *D*, *E*, *F*, etc., all sprang from an unknown type-pair, *X*, endowed with an innate plasticity of nature sufficiently accommodating to produce such changes in its descendants, as, gradually branching off in different directions, led eventually to the generation of a whale, a cat, and a sheep—not to mention other and more widely separated forms. This may be a rude, and in the eyes of those who favor Mr. Darwin's view, a coarse and very unphilosophic method of putting the argument; but it is a perfectly legitimate one. Granted, we say, that our system-species, which in many instances are not species at all, are susceptible of a certain amount of variation: there your argument stops. You can go no farther except by the help of blind and gratuitous surmises; of surmises clothed certainly in attractive colours, and in some cases possessing probably the germs of an unseizable truth—but gratuitous, all the same, in the present condition of our knowledge.

Passing over a chapter headed "the Struggle for Existence," in which in brief but graphic terms, the mutual antagonism, and the no less mutual dependency of living forms, throughout the wide range of nature, is forcibly depicted, we arrive at one of the principal topics discussed in Mr. Darwin's volume. This is entitled "Natural Selection," a term employed to express the assumed tendency of Nature to avail itself of any slight change advantageous to a species, in the gradual production of varieties, and through these, of new types. The author appears to claim this principle of natural selection as a doctrine peculiar to the present work; but, in truth—as shown by his own illustration of how a fleet brood of wolves might be produced, in this manner, by the destruction of all but swift-footed prey in their locality—it is essentially identical with the views of the author of the *Vestiges of Creation*. The latter, indeed, goes farther, in recognising also the full claims of climatic and other external causes towards the production of these changes, whilst to such influences, Mr. Darwin is inclined to concede no more than a very secondary importance. Logically considered, however, the first step in this principle of "natural selection," must be more or less dependent, at least in most instances, on the agency of physical conditions. The first slight change, in an accumulative series of changes produced in a plant or animal, can scarcely be effected otherwise than through the direct or indirect influence of external causes. In his introduction, Mr. Darwin alludes to the "Vestiges of Creation," but seeks apparently to mask the mutual affinities of the two works, by assuming, for the earlier one, a theory which certainly does not in any way fairly represent its views. He states, for example:—

> "It is preposterous to attribute to mere external conditions, the structure, for instance, of the woodpecker, with its feet, tail, beak, and tongue, so admirably adapted to catch insects under the bark of trees. In the case of the misseltoe, which draws its nourishment from certain trees, which has seeds that must be transported by certain birds, and which has flowers with separate sexes absolutely requiring the agency of certain insects to bring pollen from one flower to the other; it is equally preposterous to account for the structure of this parasite, with its relations to several distinct organic beings, by the effects of external conditions, or of habit, or of the volition of the plant itself.
>
> The author of the 'Vestiges of Creation' would, I presume, say that, after a certain number of generations, some bird had given birth to a woodpecker, and some plant to the missletoe, and that these had been produced perfect as we now see them."

Now the "Vestiges" theory, really supposes nothing of the kind; but, and in so far at least in accordance with Mr. Darwin's view, that one form is capable of originating another, by a slow and accumulative process of development. The author of "the Vestiges" does not assume, for example, that a bird of an absolutely different kind ever gave birth to a woodpecker "perfect as we now see it;" but that this latter type originated from an older one, by slight, gradual, and long-continued modifications of beak, claws, &c.,—the process giving rise to a complete series of intermediate forms. The two theories are thus essentially alike; although the works themselves stand widely apart. Whilst the one contents itself with broad assumptions, the other seeks to afford proofs of its statements, and honestly brings forward and discusses points apparently hostile to its views. All the proofs it is able to collect, however, are, as we have already attempted to shew, totally inadequate to affect the main question. But—explains Mr. Darwin—although the changes recorded are confessedly slight, they are sufficient to show what would be accomplished, if greater time were called into play; and, in illustration of this, he refers to the agency of present causes in producing, contrary to an earlier belief, geological changes of the greatest magnitude. But the two cases have no true parallelism. One who had never seen the sea, or had never studied its effects, might naturally be inclined to look with incredulity on statements of its wasting powers, and of the results asserted to arise from these. But if he were to reside for a certain time on a sea-coast, where this wasting action were going on, and thus witnessed how, bit by bit, the destruction of the coast took place, he could not shut his eyes to the fact, that, however slight the annual waste, this must amount in a given number of years, to such or such a quantity. In like manner, one residing near an estuary in which rock-sediments were constantly under process of deposition, would be forced to acknowledge by what he saw daily or annually going on, that in course of time (other conditions not interfering) a delta of greater or less extent must necessarily arise. But to make the two cases parallel, we should have to assume that these natural processes would produce, not their obvious and natural results, but some altogether unexpected issue. Natural selection as maintained by Mr. Darwin, is undoubtedly a modifying power or principle of recognised action; and no one can read the section of his book which refers to that subject, without deriving profit and instruction from the perusal. But when the author attempts to establish the sufficiency of this power to effect generic changes, stronger arguments are certainly required, than any he has yet been able to bring forward.

After some additional remarks of an interesting and original character, on the laws influencing variation, but which our comparatively limited space compels us to pass over, we arrive at a distinct portion of the work, in which the author, having stated his views in detail, and advanced facts in support of the theory which these embody, takes up the so-called difficulties of this theory, or the questions which oppose themselves to its reception. Some of these have been already touched upon, and others must have suggested themselves to the reader, but we have forborne to consider them collectively until reaching the present part of the work, in which they are boldly brought forward and combated by the author himself. Mr. Darwin enunciates them as follows:

"Long before having arrived at this part of my work, a crowd of difficulties will have occurred to the reader. Some of them are so grave that to this day I can never reflect on them without being staggered; but, to the best of my judgment, the greater number are only apparent, and those that are real are not, I think, fatal to my theory.

These difficulties and objections may be classed under the following heads:—Firstly, why, if species have descended from other species by insensibly fine gradations, do we not everywhere see innumerable transitional forms? Why is not all nature in confusion instead of the species being, as we see them, well defined?

Secondly, is it possible that an animal having, for instance, the structure and habits of a bat, could have been formed by the modification of some animal with wholly different habits? Can we believe that natural selection could produce, on the one hand, organs of trifling importance, [illegible] as the tail of a giraffe, which serves as a fly-flapper, and, on the other hand, organs of such wonderful structure, as the eye, of which we hardly as yet fully understand the inimitable perfection?

Thirdly, can instincts be acquired and modified through natural selection? What shall we say to so marvellous an instinct as that which leads the bee to make cells, which have practically anticipated the discoveries of profound mathematicians?

Fourthly, how can we account for species, when crossed, being sterile and producing sterile offspring, whereas, when varieties are crossed, their fertility is unimpaired?

The first objection is met on Mr Darwin's part by several pleas, of which we give the author's own summary below, merely stating our personal inability to see clearly the force of his replies. We should remember, in this connection, that our present knowledge is not confined to a few limited areas, but extends over almost the whole surface of the globe; and imperfect as the geological record may be, it is at least exceedingly surprising that neither dead nor existing nature in any part of the world should be capable of affording direct support, however slight, to the author's views. We cannot but think, consequently, that he asks us here to accord him too much. The following are the arguments—as given in a condensed form by the author himself—by which the first of the above most serious objections is attempted to be overcome:—

"To sum up, I believe that species come to be tolerably well-defined objects, and do not at any one period present an inextricable chaos of varying and intermediate links: firstly, because new varieties are very slowly formed, for variation is a very slow process, and natural selection can do nothing until favourable variations chance to occur, and until a place in the natural polity of the country can be better filled by some modification of some one or more of its inhabitants. And such new places will depend on slow change of climate, or on the occasional immigration of new inhabitants, and probably, in a still more important degree, on some of the old inhabitants becoming slowly modified, with the new forms thus produced and the old ones acting and reacting on each other. So that in any one region and at any one time, we ought only to see a few species presenting slight modifications of structure in some degree permanent; and this assuredly we see.

Secondly, areas now continuous must often have existed within the recent period in isolated portions, in which many forms, more especially amongst the classes *which unite for each birth and wander much, may have separately been rendered* sufficiently distinct to rank as representative species. In this case, intermediate varieties between the several representative species and their common parent, must formerly have existed in each broken portion of the land, but these links will have been supplanted and exterminated during the process of natural selection, so that they will no longer exist in a living state.

Thirdly, when two or more varieties which have been formed in different portions of a strictly continuous area, intermediate varieties will, it is probable, at first have been formed in the intermediate zones, but they will generally have had a short duration. For these intermediate varieties will, from reasons already assigned (namely, from what we know of the actual distribution of closely allied or representative species, and likewise of acknowledged varieties), exist in the intermediate zones in lesser numbers than the varieties which they tend to connect. From this cause alone the intermediate varieties will be liable to accidental extermination; and during the process of further modification through natural selection, they will almost certainly be beaten and supplanted by the forms which they connect; for these, from existing in greater numbers will, in the aggregate, present more variation, and thus be further improved through natural selection and gain further advantages.

Lastly, looking not to any one time but to all time, if my theory be true, numberless intermediate varieties, linking most closely all the species of the same group together, must assuredly have existed; but the very process of natural selection constantly tends, as has been so often remarked, to exterminate the parent-forms and the intermediate links. Consequently evidence of their former existence could be found only amongst fossil remains, which are preserved, as we shall in a future chapter attempt to show, in an extremely imperfect and intermittent record."

With regard to the objections placed under the second head, objections of perhaps a still more grave character, the replies, as might be expected, are even still less satisfactory. We have here, indeed, two principal difficulties which it is impossible to set aside except by the aid of entirely gratuitous suppositions. In one of these difficulties, the mode of transition of one generic form into another—of (and Mr. Darwin might have chosen a more startling example) an insectivorous quadruped into a bat, for instance—the author confesses that he can give us no rational explanation. At the same time, *he thinks such difficulties have very little weight.* The arguments here, we trust we do not speak offensively, for nothing is farther from our intention—the arguments here, become painfully akin to those of the "Vestiges." Take the following for example:

> "Seeing that a few members of such water-breathing classes as the Crustacea and Mollusca are adapted to live on the land, and seeing that we have flying birds and mammals, flying insects of the most diversified types, and formerly had flying reptiles, it is conceivable that flying fish, which now glide far through the air, slightly rising and turning by the aid of their fluttering fins, might have been modified into perfectly winged animals. If this had been effected, who would have ever imagined that in an early transitional state they had been inhabitants of the open ocean, and had used their incipient organs of flight exclusively, as far as we know, to escape being devoured by other fish?"

If the author had attempted to show that an imperfectly-flying fish might become gradually modified into a fish possessing more perfect powers of flight, the principle might perhaps be admitted, at least for the sake of discussion: but when "perfectly winged animals" are spoken of, especially in connexion with the context, the argument, if it mean anything, implies the possible transformation of a flying fish into a pterodactyle or some kind of flying reptile; and through this, or without its intervention, into a bird or a bat—a transformation involving most assuredly, greater difficulties, than any examples of petty, subordinate modifications, such as the author's tabular lists may exhibit, will help us to consider one of little weight. Turning now to the second of the grave difficulties referred to above, the formation of a complex organ, like the eye of a vertebrated animal, by the gradual modification of an inferior organ in a lower type, we may again let the author speak for himself: only warning the reader unfamiliar with geological discussions, that where Mr. Darwin speaks of our having to descend far beneath the lowest known fossiliferous stratum to discover the earliest stages by which the eye in the vertebrated class has been perfected, he assumes data altogether denied by the greater number of our most eminent geologists. The lowest sedimentary rocks (containing it should be remarked many beds which retain all their sedimentary characters, and thus agree with higher and fossiliferous strata) are *generally* looked upon as truly azoic formations: as deposits accumulated before the dawn of life upon the globe. The first fish-remains, moreover, the earliest recognised examples of Vertebrata, do not occur at or near the actual base of the fossiliferous strata, but only at the extreme upper limit of the Silurian formation; and in all our earliest fishes the eye exhibits apparently the normal structure. Fishes and other organisms, may, it is true, have lived at earlier periods than Geology indicates; but that view, whether true or false, is purely hypothetical, is opposed to the results of actual observation, and cannot therefore be legitimately introduced into an argument of this kind. But we proceed to our quotation, the last that our decreasing space will allow us to give.

"To suppose that the eye, with all its inimitable contrivances for adjusting the focus to different distances, for admitting different amounts of light, and for the correction of spherical and chromatic aberration could have been formed by natural selection, seems, I freely confess, absurd in the highest possible degree. Yet reason tells me, that if numerous gradations from a perfect and complex eye *to one very imperfect and simple, each grade being useful to its possessor, can be* shown to exist; if, further, the eye does vary ever so slightly, and the variations be inherited, which is certainly the case; and if any variation or modification in the organ be ever useful to an animal under changing conditions of life, then the difficulty of believing that a perfect and complex eye could be formed by natural selection, though insuperable by our imagination, can hardly be considered real. How a nerve comes to be sensitive to light, hardly concerns us more than how life itself first originated; but I remark that several facts make me suspect that any sensitive nerve may be rendered sensitive to light, and likewise to those coarser vibrations of the air which produce sound.

In looking for the gradations by which an organ in any species has been perfected, we ought to look exclusively to its lineal ancestors; but this is scarcely ever possible, and we are forced in each case to look to species of the same group, that is to the collateral descendants from the same original parent-form, in order to see what gradations are possible, and for the chance of some gradations having been transmitted from the earlier stages of descent, in an unaltered or little altered condition. Amongst existing Vertebrata, we find but a small amount of gradation in the structure of the eye, and from fossil species we can learn nothing on this head. In this great class we should probably have to descend far beneath the lowest known fossiliferous stratum to discover the earlier stages, by which the eye has been perfected.

In the Articulata we can commence a series with an optic nerve merely coated with pigment, and without any other mechanism; and from this low stage numerous gradations of structure, branching off in two fundamentally different lines, can be shown to exist, until we reach a moderately high stage of perfection. In certain crustaceans, for instance, there is a double cornea, the inner ones *divided into facets, within reach of which there is a lens-shaped swelling.* In other crustaceans the transparent cones which are coated by pigment, and which properly act only by excluding lateral pencils of light, are convex at their upper ends and must act by convergence; and at their lower ends there seems to be an imperfect vitreous substance. With these facts, here far too briefly and imperfectly given, which show that there is much graduated diversity in the eyes of living crustaceans, and bearing in mind how small the number of living animals is in proportion to those which have become extinct, I can see no very great difficulty (not more than in the case of many other structures) in believing that natural selection has converted the simple apparatus of an optic nerve merely coated with pigment and invested by transparent membrane, into an optical instrument as perfect as is possessed by any member of the great Articulate class.

He who will go thus far, if he finds on finishing this treatise that large bodies of facts, otherwise inexplicable, can be explained by the theory of descent, ought not to hesitate to go further, and to admit that a structure even as perfect as the eye of an eagle might be formed by natural selection, although in this case he does not know any of the transitional grades. His reason ought to conquer his imagination, though I have felt the difficulty far too keenly to be surprised at any degree of hesitation in extending the principle of natural selection to such startling lengths."

An entire chapter, and a most instructive one, in Mr. Darwin's book, is devoted to the subject of *Instinct*, another serious obstacle as all will readily understand, to the reception of the transmutation theory. Mr. Darwin seeks to overcome this obstacle, by establishing two points: first, that a certain amount of judgment or reason enters into the composition of instinct; and secondly, and chiefly, that, instinct can be shewn, in certain remarkable cases, to be a quality of gradation, so to say. In the cell-building instinct of the bees for example, he traces out, as he imagines, a specific connexion between the humble bees and the hive bee—the Mexican *Melipoma domestica* affording a transition-link. But here, we should consider, that, the principle of instinct is perhaps in no case a *simple specific principle*, nor even a generic one; but a principle pervading entire families or groups, and, as such, one that we might naturally infer to offer inherent degrees of variation. To establish the point aimed at by Mr. Darwin, we ought to be able to shew, that the humble-bee could be made to acquire the higher artistic-instinct of the hive-bee. We may be told that this might probably be effected under favourable circumstances, and with sufficient lapse of time; but as this assumption is altogether without proof, we have an equal right to infer that these separate amounts, or rather *kinds* of instinct, were originally bestowed on these different bees at their special creation. The followers of Mr. Darwin's theory, would, of course, ridicule the idea of a separate creation on the part of insects so nearly allied; but as they can offer us nothing to the contrary but inferences and surmises, every one is at liberty, on this point, to entertain his own opinion. Instinct may be legitimately regarded as entirely dependent upon the inherent character of the brain or its representatives, much as the mode and power of flight in birds and other winged animals, depends essentially upon the conformation of the wing. Hence the possession of peculiar instincts in the case of neuter insects incapable of continuing their race (as the neuter bees, neuter ants, &c.,) alluded to by Mr. Darwin as of difficult explanation, becomes, on the older theory, easily explained. Instinct forms, so to say, a portion of the organization of the animal: and thus, if a neuter insect were so organized as to become a fertile one, its instincts would necessarily become modified with the other parts of the organization. If instinct be really capable of improvement or modification, as the transmutation theory is forced to assert, but of which not the slightest proof is afforded, instinct and reason must in a manner be one. But all known facts are opposed to this, although the two principles are sometimes confounded by the unreflective, or by those who are disinclined to allow a certain share of reason to the lower animals. Rightly considered, these principles are not only distinct, but are actually antagonistic elements. The higher the reasoning powers, the feebler or less developed become the manifestations of the instinct principle.

We now come to the fourth great obstacle to the reception of Mr. Darwin's views—the fertility of varieties when crossed, and the sterility of the offspring of separate species in the few cases in which these latter can be made to unite. This subject is discussed by the author at some length, although necessarily under a very limited aspect. His data are chiefly, indeed almost entirely, derived from the Vegetable Kingdom, and hence, are scarcely available as fair test-elements for the proper elucidation of the question. The broad, opposing facts presented by animal hybridism are left, and unavoidably, almost untouched; or are masked under other more or less distinct inquiries: as where the author says—"Laying aside the question of fertility and sterility, in all other respects there seems to be a general and close similarity in the offspring of crossed species and of crossed varieties." Briefly, on this subject, we require to know why separate species (which under Mr. Darwin's view are nothing more than varieties) cannot be made to breed together, or do not breed together in the wild state—or why, in the few instances in which this is effected between closely allied forms, the offspring are sterile—whilst on the other hand, our known varieties breed freely, and produce fertile offspring? This is the real question at issue; and, up to the present time, it has received no definite answer, except on the assumption that true species are separate and distinct creations, and are intended by the CREATOR to remain distinct.

Some of the most striking arguments in opposition to the transmutation theory, are based on geological revelations. These have been already referred to in a previous page, but as Mr. Darwin has devoted a separate chapter to their consideration at the portion of the work to which we have now arrived, we will briefly re-discuss them before closing our review. These geological arguments are two-fold: First, the non-occurrence of intermediate or transitionary forms in rock-strata; and, secondly, the simultaneous occurrence, again and again, at various geological horizons, of entire groups of allied forms, distinct entirely (or for the greater part) from the organisms of lower and consequently earlier formed deposits. To make these points clear to our non-geological readers, we may observe, that, on each side of the Atlantic, we find certain beds entirely destitute of organic remains, underlying other beds in which these remains occur in great numbers. In some places it is difficult to draw an exact line of demarcation between the two, but that in no way affects our argument. At a certain depth all fossils cease. Now, some observers, Mr. Darwin amongst others, believe that organic forms really existed during, and perhaps before, the deposition of these fossil-free strata. Many of these strata, it should be observed, are evidently much altered, by various chemical, igneous, or other agencies, from their original sedimentary condition; and hence, fossils, if ever enclosed in them, may have become obliterated. Other strata of this fossil-free series, however, in various parts of the world, clearly retain their original characters, and do not differ, except in the absence of fossils, from many fossiliferous strata above them. From this fact, combined with the great thickness and extent of the rocks in question, most geologists consider these to be truly *azoic* rocks, formed out of sediments deposited before the actual creation of living things. If this could be absolutely proved, the transmutation theory would receive its death-blow: because in the strata which succeed or lie above these, and which constitute, be it remembered, the first or earliest fossiliferous strata really known, we find various types appearing simultaneously; and amongst these types we meet with various allied forms without any intermediate or truly transitionary links between them. If we cannot absolutely assert, however, that these Silurian forms (using the term Silurian in its extended sense) were the first created forms upon our earth, the weight of evidence is in favour, and strongly in favour, of that view. Hence, in common justice, the contrary hypothesis, resting as it does on purely negative evidence, ought not to be admitted into the discussion. But if we exclude it, what becomes of Mr. Darwin's theory? "If my theory be true," writes Mr. Darwin—"it is indisputable that before the lowest Silurian stratum was deposited, long periods elapsed, as long as, or prol 'ly far longer, than the whole interval from the Silurian age to ..ne present day: and that during these vast yet quite unknown periods of time, the world swarmed with living creatures." But if so, where are the remains of these? Vast thicknesses of rocky strata, formed during some at least of these periods, occur in various parts of the world, but as yet no fossils have been obtained from them; whilst the remains of forms which flourished afterwards, are entombed in thousands in the overlying rocks. It is not sufficient to urge, in refutation, that the lower limit of the fossil-bearing strata has been pushed lower and lower by the discovery of an obscure graptolite. here, and the fragment of a trilobite, there. To substantiate Mr. Darwin's theory, something more than this is clearly required.

But passing over this weighty obstacle, we find in these geological revelations, others not less weighty. Above the Silurian formations, for example, we find another set of strata, to which, collectively, the term Devonian has been applied, and in which the fossils (with very few exceptions) are entirely different. Above the Devonian beds again, we come upon the Carboniferous with another distinct series of organic remains; and so on successively, through various other groups of strata, each representing a certain period of time during which it was under process of deposition in the form of muddy, sandy, or calcareous sediments. In these sediments, moreover, a portion of the flora and fauna of the period (*id est:* of the plants and animals then living) was entombed, and so preserved to us: just as we see, at the present day, the leaves, shells, bones, &c., of existing organisms, enclosed in sediments under process of deposition in seas, lakes, and estuaries. Now, on the hypothesis of distinct acts of creation, there is nothing unaccountable in the sudden appearance, successively, of these distinct sets of forms, and in the want of transitional forms amongst them; but the abrupt appearance in this manner, of numerous, varied, and distinct types; and especially, the abrupt appearance of distinct sets of these, again and again, in geological history, if not absolutely fatal, is, at least, highly adverse to the Lamarckian or transmutation view. The only possible way indeed, in this case, to reconcile fact with theory, is to maintain, with Mr. Darwin, the imperfection of the geological record. But admitting freely the imperfect state of this record, we may legitimately inquire if the imperfection be really sufficient to invalidate the force of our argument. In each of these groups of rocks, we have evidence, according to Mr. Darwin's own shewing, of the lapse of an immense interval of time—and yet, transition-forms are absent. And, again, is it not most remarkable that the annals of this imperfect record, belonging to different and distant ages, and collected from such widely distant localities, should all tell the same tale, should all point to one and the same conclusion, and that an adverse one to Mr. Darwin's view. Assuredly, this cannot be the mere effect of chance. If so, it is as remarkable as would be the case of a hundred coins, thrown at random into the air, all falling with the same face uppermost. It seems impossible therefore, to avoid the conclusion, that, although—by the advancement of organic forms generally, from lower to higher types, which it reveals; by the extinction of entire races, which it plainly announces; by the vast periods of time, which the just explanation of its facts demands—Geology might seem at first thought to favor the transmutation hypothesis: its records, when rightly and fairly read, will be found altogether opposed to that illusive view.

We have not yet reached the end of Mr. Darwin's book: several chapters still remain undiscussed, but the grand argument virtually closes here. The remaining portions of the work are occupied chiefly by additional illustrations, and by a general recapitulatory statement of the subjects brought under review in the earlier chapters of the volume. These illustrations bear principally on the difficulties attached to the commonly received belief, the special-creation theory as this has been termed; and seek to uphold the development view, not by shewing the real strength of this, but by exposing the assumed weakness of the opposing system—in its impossibility, for example, to explain the cause of various striking phenomena connected with the geographical distribution of plants and animals, the embryological development of these, and so forth.

But this is scarcely a logical, certainly not a just method, of meeting the question. The case stands thus. Certain facts are given; certain remarkable phenomena are witnessed everywhere around us. We are asked to explain them. We are forced to confess they transcend our explanation. We are asked how the world comes to be peopled by so very many different plants and animals. We reply, by the act of the CREATOR: these plants and animals being the essentially-unchanged descendants of species separately created at the commencement of the existing state of things. But, say our questioners, if this be the case, if these type-forms were all separately created, is it not most strange that certain points of resemblance should pervade the whole? Even proud Man in his physical organization is but the end-link of the series, differing only in special points of structure from the beast that perisheth. Is it not most remarkable that many forms should have been created with rudimentary organs (as the mammæ of male mammals, the soldered and abortive wings of certain insects, &c.) useless, normally, to themselves, though useful, under an enlarged development, to other forms? *Is it not most startling that the fœtal* forms of various animals should pass through certain stages of development, representing in part the organization of other types? Are not these and other facts that might be adduced, really without obvious explanation on the view that each species has been separately created, and kept distinct?

To these questionings, we have, of course, but one reply: These strange phenomena, we make answer, are regarded by us, as parts of a great plan, conceived and carried out by the ALMIGHTY in his wisdom, for some purpose unfathomable to us at present, and perhaps ever to remain unfathomed by our restricted powers of inquiry. Beyond this, they are as inexplicable to us, as the object of our presence here is inexplicable. They belong to those mysteries of GOD which are kept "on the outside of man's dream." Many have attempted their interpretation, but all, as yet, have failed. Not so, say the supporters of the transmutation theory—these difficulties are met and answered by the principle of "descent with modification" of species from one another. Let us do this theory no injustice. It certainly does afford a rational explanation of the remarkable facts detailed above; but when tested by other facts, it fails entirely. It is comparatively easy to invent a theory in explanation of a particular series of phenomena, provided we be allowed to exclude all collateral facts from consideration. If we look back into the history of any science, how many futile, though at one time universally-accepted theories of this kind, do we not encounter. Many of these, however, though eventually discarded, have helped by their elaboration, to enrich our knowledge; and the wide discussion to which the present work has led, will undoubtedly yield the same good fruits.

In concluding our confessedly-imperfect analysis of this noted Essay, we may perhaps be allowed to state, apologetically, that having been disappointed of a review on the subject, by another pen, we have been forced, at the eleventh hour, to throw thus hastily into form, the thoughts suggested to us by an impartial study of the work when first obtained. If we have been compelled to record our protest against the reception of what we believe to be an unfounded theory, no one, we may safely affirm on the other hand, can lay down Mr. Darwin's book, so remarkable in many points of view, without feeling that a large accession of new thought has been added by it to our common store. E. J. C.

SCIENTIFIC AND LITERARY NOTES.

LIST OF BIRDS OBSERVED IN THE VICINITY OF HAMILTON, C. W. ARRANGED AFTER THE SYSTEM OF AUDUBON

BY THOMAS McILWRAITH, ESQ.

The object of the writer in preparing the following list, has been to afford such information as may be of use, should inquiry at any future period be made regarding the birds frequenting this part of the country. In its present state, the list has been drawn up from observations made during occasional excursions within a period of four years. Those who are acquainted with the subject will see that it is necessarily incomplete; but it will be easy to add the names of such species as may yet be found. In order that the list may be strictly local, no species has been mentioned which has not been found within six miles of the city limits.

Genus Buteo.—Buzzard.

1. B. borealis—*Red-tailed Hawk.* Seen in spring and fall. Not very common.

2. B. lineatus—*Red-shouldered Hawk.* More plentiful than the preceding, which it resembles in appearance and habits.

3. B. lagopus—*Rough-legged Buzzard.* Frequents the marshy shores of the Bay; feeding on mice and wounded birds.

4. B. Pennsylvanicus—*Broad-winged Buzzard.* Abundant during spring. Frequents the meadows near the Lake.

Genus Haliætus.—Sea Eagle.

1. H. leucocephalus—*Bald Eagle.* A few pairs winter round the Bay Shore, feeding on musk-rats, gulls, &c. The young birds are of a uniform brown colour; being more plentiful, and more easily approached than the adult.

Genus Pandion.—Osprey—Fish-Hawk.

1. P. Haliætus—*Fish-Hawk.* Seen fishing in the Bay in spring and fall. Not known to breed here.

Genus Falco.—Falcon.

1. F. peregrinus—*Peregrine Falcon.* Accidental. Has been observed striking down ducks near Burlington Beach.

2. F. palumbarius—*Pigeon Hawk.* Common in autumn, when it attends the flocks of blackbirds which roost in the marsh.

3. F. sparverius—*Sparrow Hawk.* More common than either of the preceding. Breeds near the city.

Genus Astur.—Hawk.

1. A. Cooperi—*Cooper's Hawk.* Seen in spring and fall. Not common.

2. A. fuscus—*Sharp-shinned Hawk.* Seen in spring and fall. Not common.

Genus Circus.—Harrier.

1. C. cyaneus—*Common Harrier.* Often seen sailing over the marshes; particularly during the fall.

Genus Surnia.—Day Owl.

1. S. funerea—*Hawk Owl.* Occasionally met with during severe winters.

2. S. nyctea—*Snowy Owl.* Very plentiful during some winters, at the beach. Between November, 1858, and March, 1859, seventeen specimens were brought to market by fishermen and others. Between November, 1859, and March, 1860, only two individuals were killed.

Genus Ulula.—Night Owl.

1. U. Acadica—*Saw-whet Owl.* Frequently caught during the day, in empty houses, throughout the country. Not seen in winter.

Genus Syrnium.—Hooting Owl.

1. S. nebulosum—*Barred Owl.* The most common species of this family. Seen in spring and fall: not observed in summer.

Genus Otus.—Eared Owl.

1. O. vulgaris—*Long-Eared Owl.* Rather rare. Observed only in the fall.

2. O. brachyotus—*Short-Eared Owl.* More frequently seen than the preceding. Observed to hunt during the day, in cloudy weather.

Genus Bubo.—Horned Owl.

1. B. Virginianus—*Virginian Horned Owl.* Not very rare. No particular haunt.

2. B. Asio—*Mottled Horned Owl.* One shot on the top of a store-house at Cook's Wharf, November, 1859.

Genus Caprimulgus.—GOAT SUCKER.

1. C. vociferus—*Whip-poor-Will.* Generally distributed. Common.

Genus Chordeiles.—NIGHT HAWK.

1. C. Virginianus—*Night Hawk.* Abundant. Breeds in the woods near the Bay.

Genus Chaetura.—SWIFT.

1. C. pelasgia—*Chimney Swallow.* Abundant everywhere.

Genus Hirundo.—SWALLOW.

1. H. purpurea—*Purple Martin.* Quite common in the city.
2. H. bicolor—*White-bellied Swallow.* Abundant. Generally distributed.
3. H. fulva—*Cliff Swallow.* Less common than the preceding. Builds in colonies on the outside of barns, &c.
4. H. rustica—*Barn Swallow.* Quite common. Builds inside of barns, &c.
5. H. riparia—*Bank Swallow.* Abundant. Nests in sand-banks round the Bay Shore and elsewhere.

Genus Musicapa.—*Flycatcher.*

1. M. tyrannus—*Tyrant Flycatcher.* Generally distributed. Not abundant.
2. M. crenita—*Great Crested Flycatcher.* Quite common in the woods.
3. M Cooperi—*Cooper's Flycatcher.* One individual shot in a swamp near the Bay Shore.
4. M. Acadica—*Small Green-crested Flycatcher.* Abundant in the woods.
5. M. fusca—*Pee-wee Flycatcher.* Quite common. Builds in bridges, sheds, &c.
6. M. virens—*Wood Pee-wee Flycatcher.* Less common than the preceding. Frequents dead trees.
7. M. ruticilla—*Redstart.* Common in the woods, in summer.
8. M. flaviventris—*Yellow-bellied Flycatcher.* Only one found. (Not mentioned by Audubon.)

Genus Mylodoctes.—FLYCATCHER WARBLER.

1. M. mitratus—*Hooded Warbler.* Only one specimen found.
2. M. Canadensis—*Canada Flycatcher.* Quite common during spring and early summer.
3. M. Wilsoni—*Wilson's Flycatcher.* Only one specimen found.

Genus Sylvicola.—WOOD WARBLER.

1. S. coronata—*Yellow-crowned Wood Warbler.* Abundant during spring and fall.
2. S. striata—*Black-poll Wood Warbler.* Rather rare. Arrives late and leaves early.
3. S. castanea—*Bay-breasted Wood Warbler.* A regular visitor in spring. Not numerous.
4. S. icterocephala—*Chestnut-sided Wood Warbler.* Rather common. Nests among the briars.
5. S. vinus—*Vine-creeping Wood Warbler.* Quite common. One of the first to arrive.
6. S. Varus—*Hemlock Warbler.* Observed in September only.
7. S. virens—*Black-throated Green Wood Warbler.* Rather common in spring.
8. S. maritima—*Cape May Wood Warbler.* Rare. Two specimens procured.

9. S. cœrulea—*Cœrulean Wood Warbler.* Abundant in some seasons: less so in others.

10. S. Blackburniæ—*Blackburnian Wood Warbler.* A regular visitor, in uncertain numbers.

11. S. æstiva—*Yellow-poll Warbler.* Abundant. Builds in shade trees in the city.

12. S. Petechia—*Yellow Red-poll Warbler.* Common in the fall; rare in spring.

13 S. Americana—*Blue Yellow-back Wood Warbler.* Not very plentiful.

14. S. Canadensis—*Black-throated Blue Wood Warbler.* Plentiful in spring.

15. S. maculosa—*Black and Yellow Wood Warbler.* An irregular spring visitor.

Genus Trichas.—Ground Warbler.

1. T. Marilandica—*Maryland Yellow-throat.* Not common near the city: more so in retired swamps.

2. T. Philadelphia. Rare. One found May 28th, 1860.

Genus Helinaia.—Swamp Warbler.

1. H. celata—*Orange crowned Swamp Warbler.* Only one specimen found.

2. H. rubricapilla—*Nashville Swamp Warbler.* Quite common. Breeds near the city.

3. H. chrysoptera—*Golden-winged Swamp Warbler.* Only one specimen found.

Genus Minotilta.—Creeping Warbler.

1. M. varia—*Black and White Creeping Warbler.* Abundant in the woods.

Genus Certhia.—Creeper.

1. C. familiaris—*Brown Tree Creeper.* Common. Resident.

Genus Troglodytes.—Wren.

1. T. aedon—*House Wren.* A few pairs spend the summer in the gardens of the city.

2. T. hymenalis— *Winter Wren.* Common in spring and fall.

3. T. palustris—*Marsh Wren.* Found in all the marshes round the Bay in summer.

Genus Parus.—Tit.

1. P. atricapillus—*Black-capped Tit.* Abundant. Resident.

Genus Regulus.—Kinglet.

1. R. satrapa—*Gold crested Wren.* Plentiful in spring and fall.

2. R calendula—*Ruby-crowned Wren.* Plentiful in spring and fall.

Genus Sialia.—Blue Bird.

1. S. Wilsoni—*Common Blue Bird.* Plentiful from early spring till late in the fall.

Genus Orpheus.—Mocking Bird.

1. O. Carolinensis—*Cat Bird.* Quite common. Frequents low thickets.

2. O. Rufus—*Brown Thrush.* Less common than the preceding.

Genus Turdus.

1. T. migratorius—*Robin.* Abundant; breeds in the city gardens.

2. T. mustelinus—*Wood Thrush.* Rather rare. Frequents solitary woods.

3. T. Wilsoni—*Tawny Thrush.* Rather common.

4. T. solitarius—*Hermit Thrush.* Rather common. Similar in manner and haunt to the preceding.

Genus Seiurus.—Wood Wagtails.

1. S. aurocapillus—*Golden-crowned Wood Wagtail.* Common in the woods in summer.

2. S. novaeboracensis—*Aquatic Wood Wagtail.* Common; less so than the preceding.

Genus Anthus.—Pipit.

1. A. Ludovicianus—*American Pipit.* Straggling flocks seen in spring and fall.

Genus Alauda.—Lark.

1. A. alpestris—*Shore Lark.* Occasionally seen in company with plectrophanes nivalis.

Genus Plectrophanes.—Lark Bunting.

1. P. Lapponica—*Lapland Lark Bunting.* Occasionally found in company with the succeeding species.

2. P. nivalis—*Snow-flake.* Abundant while snow remains on the ground.

Genus Emberiza.—Bunting.

1. E. graminea—*Bay-winged Bunting.* Found in any grass field in summer.

2. E. Savanna—*Savannah Bunting.* Rather rare. Similar in habits to the preceding.

3. E. pusilla—*Field Sparrow.* Not very numerous; breeds near the city.

4. E. socialis—*Chipping Sparrow.* Quite common. Builds in shade trees in streets.

5. E. Canadensis—*Tree Sparrow.* Small flocks seen during winter.

6. E. passerina—*Yellow winged Bunting.* Rather rare.

Genus Niphœa.—Snow Bird.

1. N. hyemalis—*Common Snow Bird.* Common. Resident.

Genus Spiza.—Painted Bunting.

1. S. cyanea—*Indigo Bird.* Common in the woods from May till September.

Genus Ammodramus.—Shore Finch.

1. A. palustris—*Swamp Sparrow.* Breeds in the reed beds of the Bay.

Genus Linaria.—Linnet.

1. L. minor—*Lesser Redpoll Linnet.* A winter visitor. Plentiful in some seasons; less so in others.

2. L. pinus—*Pine Linnet.* Less numerous than the preceding.

Genus Carduelis.—Gold Finch.

1. C. tristis—*Gold Finch.* Abundant. A few remain during winter.

Genus Fringilla.—Finch.

1. F. Iliaca—*Fox-coloured Sparrow.* Accidental in the fall.

2. F. melodia—*Song Sparrow.* Abundant from March till November.

3. F. Pennsylvanica—*White Throated Sparrow.* Common in spring and fall.

4. F. leucophrys—*White-crowned Sparrow.* Rather rare. Only seen in spring.

Genus Pipilo.—Ground Finch.

1. P. erythropthalmus—*Towhe Bunting.* Not very numerous.

Genus Erythrospiza.—PURPLE FINCH.

1. E. purpurea—*Purple Finch.* Occasional in the woods in winter.

Genus Corythus.—PINE FINCH.

1. C. enucleator—*Pine Grosbeak.* A winter visitor, appearing in considerable numbers in some seasons, and not at all in others. Common during the winter of 1859-60.

Genus Loxia.—CROSSBILL.

1. L. Curvirostra—*Crossbill.* An irregular winter visitor.

Genus Coccoborus.—SONG GROSBEAK.

1. C. Ludovicianus—*Rose-breasted Grosbeak.* Not very numerous. Frequents secluded groves.

Genus Pyranga.—RED BIRD.

1. P. rubra—*Scarlet Tanager.* Common in the woods in summer.

Genus Dolichonyx.—RICE BIRD.

1. D. oryzivora—*Bob-o-link.* Common. Frequents grass fields.

Genus Molothrus.—COW BIRD.

1. M. pecoris—*Common Cow Bird.* Abundant all over the country.

Genus Agelaius.—MARSH BLACKBIRD.

1. A. phoeniceus—*Red-winged Starling.* Abundant in all the marshes.

Genus Icterus.—HANGNEST.

1. I. Baltimore—*Baltimore Oriole.* Common in the woods and orchards.

Genus Quiscalus.—CROW BLACKBIRD.

1. Q. versicolor—*Crow Blackbird.* Seen in spring and fall. Not observed to breed near the city.

2. Q. ferrugineus—*Rusty Grackle.* Abundant in the fall, when they spend the day in the ploughed fields, and root in the reeds of the marsh at night.

Genus Sturnella.—MEADOW STARLING.

S. Ludoviciana—*Meadow Lark.* Common from early spring till late in the fall.

Genus Corvus.—CROW.

1. C. Americanus—*Common Crow.* The main body migratory; a few resident.

Genus Garrulus.—JAY.

1. G. cristatus—*Blue Jay.* Common. A few resident.

Genus Lanius.—SHRIKE.

1. L. borealis—*American Shrike.* A few individuals seen every winter.

2. L. Ludovicianus—*Loggerhead Shrike.** Two individuals shot in April, 1860. Not observed prior to that date.

Genus Vireo.

1. V. flavifrons—*Yellow-throated Vireo.* Not very numerous.

*It is possible that this may prove to be the *Collyrio excur bitoroides* of Baird, as according to that author, *L. Ludovicianus* is found only in the South Atlantic and Gulf States; while *C. excurbitoroides* has been gradually advancing from the west, and might be expected to occur here about this time. Without comparing specimens, it is difficult to distinguish between the two.

2. V. gilvus—*Warbling Greenlet.* Rather common. Visits the shade trees in the city.

3. V. olivaceus—*Red-eyed Greenlet.* Common in the woods in summer.

Genus Bombycilla.—WAXWING.

1. B. garrula—*Bohemian Chatterer.* An irregular winter visitor. Usually seen in company with the pine gro-beak.

2. B. Carolinensis—*Cedar Bird.* Quite common during summer, frequently staying late in the fall.

Genus Sitta.—NUTHATCH.

1. S. Carolinensis—*White-bellied Nuthatch.* Common. Resident.

2. S. Canadensis—*Red bellied Nuthatch.* Common. Not seen in summer.

Genus Trochilus.—HUMMING BIRD.

1. T. colubris—*Ruby-throated Humming Bird.* Common. Seen wherever there are flowers in summer.

Genus Alcedo.—KINGFISHER.

1. A. Alcyon—*Belted Kingfisher.* Common along the Bay shores.

Genus Picus.—WOODPECKER.

1. P. villosus—*Hairy Woodpecker.* Quite common. Resident.

2. P. pubescens—*Downy Woodpecker.* Quite common. Resident.

3. P. varius—*Yellow-bellied Woodpecker.* Common during summer; breeds near the city.

4. P. Arcticus—*Arctic Three-toed Woodpecker.* Rare. Two specimens procured in November, 1859.

5. P. Carolinensis—*Red bellied Woodpecker.* Rather rare. Not seen in winter.

6. P. erythrocephalus—*Red headed Woodpecker.* Common in the country; less so near the city.

7. P. auratus—*Gold-winged Woodpecker.* Quite common. Breeds near the city.

Genus Coccyzus.—AMERICAN CUCKOO.

1. C. erythropthalmus—*Black-billed Cuckoo.* Not very rare.

Genus Ectopistes.—LONG TAILED DOVE.

1. E. migratoria—*Passenger Pigeon.* A regular visitor, in uncertain numbers.

2. E. Carolinensis—*Carolina Dove.* Accidental, in the fall.

Genus Ortyx.—AMERICAN PARTRIDGE.

1. O. Virginiana—*Partridge Quail.* Common in fall and winter.

Genus Tetrao.—GROUSE.

1. T. umbellus—*Ruffled Grouse.* Common. Resident.

Genus Gallinula.—GALLENULE.

1. G. chloropus—*Common Gallenule.* Found in the marshes. Not very numerous.

Genus Fulica.—COOT.

1. F. Americana—*Common Coot.* Found in the marshes. Not plentiful.

Genus Ortygometra.—CROKE GALLENULE.

1. O. Carolinus—*Sora Rail.* Extremely abundant in all the marshes during summer.

Genus Rallus.—RAIL.

1. R. crepitans—*Clapper Rail.* Occasional, in the marsh.
2. R. Virginianus—*Virginian Rail.* More plentiful than the preceding.
3. R. elegans—*Great Red-breasted Rail.* Accidental. One specimen found.

Genus Charadrius.—PLOVER.

1. C. Helveticus—*Black-bellied Plover.* A regular visitor at the Beach in spring and fall.
2. C. marmoratus—*Golden Plover.* More numerous than the preceding.
3. C. vociferus—*Kildeer Plover.* Occasional. Never numerous.
4. C. semipalmatus—*Ring Plover.* Numerous in spring and fall.

Genus Strepsilas.—TURNSTONE.

1. S. interpres—*Turnstone.* Occasional at the beach.

Genus Tringa.—SANDPIPER.

1. T. pectoralis—*Pectoral Sandpiper.* Abundant in the fall.
2. T. alpina—*Red-backed Sandpiper.* Extremely abundant about the 25th of May.
3. T. subarquata—*Curlew Sandpiper.* Occasional. Not numerous.
4. T. himantopus—*Long-legged Sandpiper.* A few seen at the beach every season.
5. T. semipalmata—*Semipalmated Sandpiper.* Very abundant in spring and fall.
6. T. pusilla—*Little Sandpiper.* Not quite so numerous as the preceding, with which it associates.
7. T. arenaria—*Sanderling Sandpiper.* Quite common at the beach.
8. T. islandica—*Red-headed Sandpiper.* Never very numerous.

Genus Lobipes.—LOBEFOOT.

1. L. hyperboreus—*Hyperborean Lobefoot.* Occasionally seen in small ponds near the bay.

Genus Totanus.—TATLER.

1. T. macularius—*Spotted Tatler.* Breeds near all the muddy creeks round the Bay.
2. T. flavipes—*Yellow-shanks Tatler.* Rather common during spring and fall.
3. T. vociferus—*Tell-tale Tatler.* Less numerous than the preceding.

Genus Limosa.—GODWIT.

1. L. fœdoa—*Great Marbled Godwit.* Occasional. Not numerous.
2. L. Hudsonica—*Hudsonian Godwit.* Rather rare.

Genus Scolopax.—SNIPE.

1. S. Wilsoni—*Wilson's Snipe.* Abundant. Migratory.

Genus Numenius.—CURLEW.

1. N. longirostris—*Long-billed Curlew.* Accidental on the Lake Shore.
2. N. Hudsonicus—*Hudsonian Curlew.* Less frequent than the preceding.

Genus Ardea.—HERON.

1. A. nycticorax—*Black-crowned Night Heron.* Accidental. Migratory.
2. A. lentiginosa—*American Bittern.* Abundant in all the marshes.
3. A. exilis—*Least Bittern.* Less numerous than the preceding.
4. A. Herodias—*Great Blue Heron.* Rather common.

Genus Anser.—Goose.

1. A. Canadensis—*Canada Goose.* A few rest on the Bay in their migratory course.

2. A. hyperboreus—*Snow Goose.* Accidental, in the Bay.

Genus Cygnus.—Swan.

1. C. Americanus—*American Swan.* Accidental, in the Bay.

Genus Anas.—Duck.

1. A. boschas—*Mallard.* Common. Migratory.
2. A. obscura—*Dusky Duck.* Common. Migratory.
3. A. strepera—*Gadwall.* Rare. Only two individuals seen.
4. A. Americana—*Widgeon.* Numerous in spring and fall.
5. A. acuta—*Pin-tail Duck.* Occasional. Not numerous.
6. A. sponsa—*Wood Duck.* Quite common. A few breed near the marsh.
7. A. Carolinensis—*Green-winged Teal.* Numerous in spring and fall.
8. A. discors—*Blue-winged Teal.* Less numerous than the preceding.
9. A. clypeata—*Shoveller.* Rather rare.

Genus Fuligula.—Sea Duck.

1. F. valisneriana—*Canvass-back Duck.* Accidental. Only two individuals seen.
2. F. ferina—*Red-head Duck.* Rather common.
3. F. marila—*Scaup Duck.* Abundant in spring and fall.
4. F. marila minor—*Lesser Scaup Duck.* Abundant. Not distinguished by Audubon from the preceding.
5. F. rubida—*Ruddy Duck.* Immense numbers taken with the gill-nets in some seasons: not seen in others.
6. F. fusca—*Velvet Duck.* Occasional, in stormy weather.
7. F. clangula—*Golden-Eye Duck.* Not very numerous.
8. F. albeola—*Dipper.* Abundant in spring and fall.
9. F. glacialis—*Long tailed Duck.* Abundant. Winters in the Lake. Often caught in the gill-nets along with white-fish, twelve miles from shore, and at a depth of 200ft. to 250ft.

Genus Mergus.—Merganser.

1. M. merganser—*Goosander.* Not very plentiful.
2. M. serrator—*Red breasted Merganser.* Not very plentiful.
3. M. cucullatus—*Hooded Merganser.* More numerous than either of the preceding.

Genus Sterna.—Tern.

1. S. hirundo—*Common Tern.* Visits the Bay about the end of May.
2. S. nigra—*Black Tern.* Usually accompanies the preceding.

Genus Larus.—Gull.

1. L. Bonapartii—*Bonaparte's Gull.* Common during fall.
2. L. argentatus—*Herring Gull.* Winters at the beach.
3. L. marinus—*Great Black-backed Gull.* Winters at the beach. Very difficult of approach.

Genus Uria.—Guillemot.

1. U. grylle—*Black Guillemot.* Accidental, after easterly storms.

2. U. Troile—*Foolish Guillemot.* Accidental, after stormy weather.

Genus Colymbus —Diver.

1. C. glacialis—*Loon.* Often seen in the Bay.

2. C. septentrionalis—*Red throated Diver.* Immature specimens frequent; the adult not observed.

Genus Podiceps —Grebe.

1. P. rubricollis—*Red necked Grebe.* Rather rare. Seen only in spring.

2. P. cornutus—*Horned Grebe.* Common during summer.

3. P. Carolinensis—*Pied-bill Dabchick.* Not so numerous as the preceding.

METEOROLOGICAL OBSERVATIONS, 1859, ST. MARY'S, C. W.

To the President of the Canadian Institute.

Sir,—I have herewith forwarded a continuation of the Meteorological Observations made by me in St. Mary's, Canada West, which you received last year. They are in Reduced Tabular form for reference, and I hope may be useful as to our climate, in this the highest portion of the Province, which is about 1090 feet above the level of the ocean, and in Latitude North 43° 17′ 57″ and West Longitude about 81° 13′ 20″, as detailed in my last communication. I have prepared the paper in tabular form. Each month exhibits barometric fluctuations, similar to those in the corresponding months of 1858, indicating I presume some general law, and the mean height of the whole year did not differ more than $\frac{4}{100}$ of an inch from that of 1859. March was again the lowest last year, and had also the greatest number of rainy days.

The amount of rainfall was considerably greater this year, 1859, than in 1858, being 42·71 ins. instead of 35·42 last year; the increase mainly having fallen in the summer and autumnal months.

The direction of the air currents as in last year was mainly from the West, being 136 days in 1859, and 139 days in 1858, and the Easterly winds which invariably bring rain or snow, in this part of the Province, prevailed 85 days in 1859, to 75 days in 1858; which may account for the greater rainfall this year, especially as the increased rate is noticeable in the summer and autumnal months in both cases.

The bright, clear, sunshiny days were as before greatly in excess of the dull cloudy and rainy days ing 217 in 1859, fine &c., to 148 dull and rainy days.

In order to analyse the phenomena of the two years observations 1858 and 1859 more easily, I have divided the tables into seasons, and placed the directions of air currents, and the atmospheric appearance in the form of a percentage on the year for easier comparison, all of which can readily be seen in the accompanying Table —Named Comparative Table of years 1858 and 1859.

In conclusion, I must *apologise for their brevity*, but can vouch for their accuracy, and thus submit them respectfully to the Institution.

W. GRAEME TOMKINS, C.E., P.L.S. &c.

St. Mary's, March 1, 1860.

P.S.—I have appended a Comparative Table of Seasonal Temperatures deduced from my own and other authentic sources.

METEOROLOGICAL OBSERVATIONS, 1859, AT ST. MARY'S, C.W.

SEASONAL TABLE MADE FOR ST. MARY'S, A.D. 1859.

BY W. GRÆME TOMKINS, C.E., P.L.S., ETC.

	Month.	Barometer.	Thermom'r.	Rain in Inches.	Direction of Wind.				Atmospheric Approximation.			
					Nor.	Sou'd.	East.	West	Fine.	Ch'ge.	Dull.	Rain.
Winter.	December ...	28.85	31.45	2.8	6	8	8	9	6	8	10	7
	January	28.86	26.77	3.0	5	17	5	10	10	6	10	5
	February ...	28.77	26.98	1.34	8	4	8	8	8	5	10	5
	Mean	28.83	28.39	T7 27	21.1f.	25.5f.	23.4p.	30p.	26.7f.	21.1p.	33.4f.	18.8p.
Spring.	March	28.62	37 45	4.0	5	7	9	10	11	6	6	8
	April	28 66	41.40	2.52	10	2	10	8	10	7	9	4
	May	28.91	59.48	2.3	4	7	14	6	15	8	5	3
	Mean	28.73	46.11	T8.94	20.7p.	17.4p.	36.0p.	26.0p	39.4p.	22.9p.	21.8p.	16.0p.
Summer.	June..........	28.83	63.00	5.2	2	8	3	17	16	7	4	3
	July	28.88	71.13	2.23	10	6	4	11	16	7	4	4
	August	28.81	66.93	7.8	9	4	3	15	17	5	4	5
	Mean	28.84	67.02	T15.3	22.7p.	19 3p.	11.0p.	47p.	42.7p.	22.9p.	13.2p.	13.2p.
Autumn.	September...	28.83	56.03	1.77	5	11	3	11	9	7	7	7
	October	28.77	39.84	3.1	10	2	8	11	9	9	6	7
	November ..	28.76	35.03	5.2	4	6	10	10	10	5	8	7
	Mean	28.78	43.63	T10.14	21.0p.	21.0p.	23p.	35p.	31p.	23.0p.	23p.	23p.
Annual Mean		28.80	46.29	42.71	21.5p.	17,7p.	23.3p.	37.5p	37.8p.	22p.	22.7p.	17.5p.

Wind from Northward 78 days.
" " Southward 76 "
" " Eastward 85 "
" Westward 136 "

137 Fine days.
80 Changeable.
83 Dull.
65 Rain or Snow.

COMPARATIVE TABLE OF YEARS 1858 AND 1859, AT St. MARY'S, C. W.

BY W. GRÆME TOMKINS, C.E., P.L.S.

Barometric Table.

	Winter.	Spring.	Summer.	Autumn.	Annual.
1858	28.83	28.75	28.92	28.87	28.84
1859	28.83	28.73	28.84	28.78	28.80
Difference—1859	...	—.02	—.08	—.09	—.04

Thermometric.

1858	27.66	42.42	73.69	49.33	48.23
1859	28.39	46.11	67.02	43.63	46.29
Difference—1859	+0.73	+2.89	—6.67	—5.70	—1.94

Rain or Snow in inches.

1858	7.63	10.73	10.99	5.87	35.42
1859	7.27	8.94	15.36	10.14	42.71
Difference—1859	—.36	—1.79	+4.37	+4.27	+7.29

Wind Direction, per centum.

Direction.	1858.	1859.	1858.	1859.	1858.	1859.	1858.	1859.	
North	17.8	21.1	10.0	20.7	33	22.7	37.0	21	+10 in '58
South	26.6	25.5	18.5	17.4	10	19.3	18.5	21	+ 9 in '59
East	24.4	23.4	31.5	36.0	11	11.0	15.2	23	+10 in '59
West	31.2	30.0	40.0	26.0	46	47.0	20.3	35	+ 3 in '58

Atmospheric Appearance, per centum.

Fine	30.0	26.7	39.0	39.4	62.0	42.7	41.5	31	+21 in '58
Change	32.2	21.1	28.4	22.9	17.5	22.9	17.5	23	+ 7 in '58
Dull	21.0	33.4	15.2	21.8	10.5	13.2	24.0	23	+18 in '59
Rain	16.8	18.8	17.4	16.0	10.0	13.2	17.0	23	+10 in '59

CANADIAN INSTITUTE.

SESSION—1859-60.

TWELFTH ORDINARY MEETING—*10th March*, 1860.

Professor WILSON, LL.D., President, in the Chair.

I. *The following Donation for the Library was announced, and the thanks of the Institute voted to the donor:*

From Hon. J. M. Brodhead, Washington.

Explorations for a Railroad route from the Mississippi to the Pacific Ocean. Vol. X.

II. *The following Papers were read:*

1. By the Rev. Prof. Hincks, F.L.S.:
   "On the true aims, foundations, and claims to attention of Political Economy."
2. By W. Martin, LL.D.:
   "On some geometric problems relating to curves having double contact."
3. By J. H. Dumble, Esq. C.E.:
   "On the Expansion and Contraction of Ice."

THIRTEENTH ORDINARY MEETING—*17th March*, 1860.

Prof. DANIEL WILSON, LL.D. President, in the Chair.

I. *The following Donation for the Library was announced, and the thanks of the Institute voted to the donor:*

From Major R. Lachlan, Cincinnatti.

Meteorlogische Waamamingan in Nederland en Zyne Bezittingan, su afeoykingen van Temperateuer en Barometerstand op vele Plaasten en Europa. Uitgeven door het Koninklyk nederlandich Meteorologisch Instituut, 1856 and 1857. Quarto. Two Vols.

Fourth Meteorological Report of Professor James P. Espy, to the United States Government, 27th July, 1854. Quarto. Two Vols.

II. *The following Papers were read:*

1. By Professor Chapman:
   "On the Geological structure of the 'Blue Mountains' near Collingwood." (2.) "On some simple rules for calculating the thickness of Inclined Strata." And (3.) "On a new species of *Agelacrinites* from Peterboro', C. W."
2. By the Rev. Professor G. P. Young, M.A.:
   "Proof of the impossibility of representing the common transcendental functions of a variable, as finite algebraical functions."
3. By Professor Wilson, LL.D.
   "On the origin of Alphabets, in reference to he question of the age of Man."

FOURTEENTH ORDINARY MEETING—24*th March*, 1860.

Professor DANIEL WILSON, LL.D., President, in the Chair.

I. *The following Gentlemen were elected Members:*

U. OGDEN, Esq., M.D., Toronto.

ROBERT CHECKLEY, Esq., M.D., Whitby.

II. *The following Donations for the Library were announced, and the thanks of the Institute voted to the donors:*

From J. H. James, Esq., per Dr. Philbrick.
"Principles of Political Economy." 3rd Edition: by J. S. Mill. Two Vols.

From the Historical Society of Pennsylvania.
"The Record of the Court at Upland Pennsylvania, 1676 to 1681, and Military Journal kept by Major E. Druny, 1781 to 1795. One Vol.

III. *The following Papers were read:*

1. By Professor J. B. Cherriman, M.A.:
"Remarks on Newton's investigation of the Velocity of Sound."

2. By Professor Croft, D.C.L.:
"On a reputed Blue Sand from India."

FIFTEENTH ORDINARY MEETING—31*st March*, 1860.

Professor WILSON, LL.D., President, in the Chair.

I. *The following Gentleman was elected a Member:*

JOHN DE CEW, Provincial Land Surveyor, Cayuga.

II. The President announced that this was the last regular Meeting of the Session, but in consequence of there being several papers yet to read, it was proposed to adjourn to Saturday the 14th April.

Messrs. Spreull and Harman were appointed Auditors of the Treasurer's Accounts for the present year.

III. *The following Papers were read:*

1. By G. R. R. Cockburn, Esq., M.A.:
"On Rent."

2. By Professor J. B. Cherriman, M.A.:
"On a Problem in Substitutions."

3. By S. Fleming, Esq., C.E.:
"On the development of lines of Internal Communication with a view to the future progress of Canada."

SIXTEENTH ORDINARY MEETING—14*th April*, 1860.

Professor H. CROFT, D.C.L., Vice-President, in the Chair.

I. *The following Gentleman was a elected Member:*

Doctor R. W. HILLARY, Whitchurch, C. W.

II. *The following Donations for the Library and Museum were announced, and the thanks of the Institute voted to the donors.*

For the Library.—From the Royal University of Christiana, Norway.

Forhadlinger ved de Skaudenaviske Natuaporskaras Syvende Mode—1— Christiania Don. 12-18, Julie 1856. One Vol.

Generalberatiug fra Gausted Siudssygeasyl for aeret 1858. One Vol.

Tale Cautate bid del &c. for Kong O: . One Vol.

Uber die Geometrische Reprasentation &c. Von C. A. Bjerknes and Dr. O. J. Broch Professor. One Vol.

Karlamagnus Saga ok Kappa Houst. One Vol.

Al-Mutassal Edidct. J. P. Broch. One Vol.

Det Kongelige Noaske Fredericks Unversitets Aarsbereting for oaset 1856-1858. One Vol.

Traces de Buddhisme en Norvège avant l' introduction du Christianisme. par M. E. A. Holmboe. One Vol.

Beretning om en Zoologiske Reise foretagen i sommeren 1857, vad D. C. Danielssan. One Vol.

Fortegnelfe over Modeller of Landhusholdnings-Redskaber, &c. One Vol.

Personalies oplaeste ved Eaus Magestaet Kong Oscar den 1s. One Vol.

Beretning om Gedsfaengslets Verksomhed i aaret 1858. One Vol.

Unbound or in Pamphlet form,—Total 12.

For the Museum.—From John Fleming, Esq.

A collection of Trilobites and other Geological Specimens from Collingwood, Canada West.

III. *The following Papers were read:*

1. By Professor Hind, M.A.:
"On the occurrence of Grasshoppers, (so called) in the North West."

2. By the Rev. Professor Hatch, B.A.:
"On Moral Relations of the Greek Oracles."

MONTHLY METEOROLOGICAL REGISTER, AT THE PROVINCIAL MAGNETICAL OBSERVATORY, TORONTO, CANADA WEST,—APRIL, 1860.

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°. 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 Average	Tens. of Vapour. 6 A.M.	2 P.M.	10 P.M.	M'N
					°	°	°	°	°				
1	29.234	29.285	—	—	36.3	35.6	—	—	—	.193	.110	—	—
2	.572	.526	29.415	29.4993	19.7	30.2	28.7	26.42	—10.22	.058	.061	.100	.085
3	.195	28.993	.070	.0800	32.7	49.9	38.5	40.93	+ 3.92	.169	.202	.191	.191
4	.082	28.962	28.970	.0088	35.6	50.8	40.8	43.70	+ 6.37	.186	.223	.213	.213
5	.030	29.114	29.431	.2107	37.8	46.1	34.7	39.43	+ 1.72	.181	.217	.179	.176
6	.582	.672	—	—	35.2	46.1	—	—	—	.174	.234	—	—
7	.734	.610	.471	.6078	34.5	43.5	43.5	41.42	+ 3.00	.181	.172	.259	.198
8	.[illegible]98	.331	—	—	42.8	55.8	—	—	—	.258	.388	—	—
9	.536	.608	.660	.6128	41.7	44.8	39.2	42.08	+ 3.05	.218	.187	.190	.202
10	.504	.317	.517	.4407	39.9	47.1	41.0	41.85	+ 2.43	.185	.299	.227	.242
11	.543	.631	.729	.6407	35.3	49.1	35.2	39.35	— 0.38	.164	.167	.190	.170
12	.691	.380	.504	.5157	30.6	50.4	38.1	40.53	+ 0.40	.144	.258	.169	.199
13	.608	.603	.433	.5515	34.2	50.4	41.4	40.72	+ 0.28	.153	.245	.221	.200
14	.546	.670	.831	.7047	28.0	29.8	25.9	27.90	—12.83	.130	.147	.078	.111
15	.920	.899	—	—	23.3	32.4	—	—	—	.103	.105	—	—
16	.681	.529	.335	.4955	33.8	38.8	43.0	39.00	— 2.48	.166	.186	.247	.202
17	.277	.732	30.062	.7423	43.5	46.1	32.0	40.53	— 1.22	.250	.196	.121	.175
18	30.228	30.232	30.115	30.1905	30.6	37.6	30.6	33.90	— 8.17	.131	.122	.123	.115
19	30.014	29.892	29.731	29.8603	32.0	47.5	49.0	43.73	+ 1.58	.151	.132	.264	.179
20	29.629	.479	.423	.4895	41.4	49.7	43.9	44.88	+ 2.10	.250	.338	.283	.285
21	.469	.475	.423	.4353	47.5	50.8	47.7	49.42	+ 6.32	.316	.293	.273	.293
22	.496	.526	—	—	38.1	42.1	—	—	—	.204	.204	—	—
23	.643	.612	.574	.6158	34.9	43.5	32.4	36.98	— 6.72	.117	.190	.161	.151
24	.638	.600	.536	.5823	30.2	36.0	32.2	32.77	—11.23	.136	.129	.155	.143
25	.423	.490	.627	.5242	31.3	38.1	30.9	33.85	—10.55	.152	.135	.120	.141
26	.602	.738	.816	.7560	31.8	42.1	34.2	36.70	— 8.03	.116	.170	.160	.151
27	.808	.857	.912	.8805	30.6	47.7	41.0	40.87	— 4.15	.138	.138	.210	.160
28	.913	.847	.782	.8383	36.0	49.7	39.2	41.63	— 3.77	.172	.226	.180	.193
29	.768	.748	—	—	37.8	52.9	—	—	—	.183	.220	—	—
30	.666	.571	.504	.5772	41.0	57.0	48.6	50.72	+ 4.67	.218	.288	.249	.257
31	29.5887	29.5632	29.5783	29.5775	34.73	44.89	37.09	39.55	— 1.44	.170	.198	.191	.185

Day.	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.	Resul't. Direction.	Velocity of Wind. 6 A.M.	2 P.M.	10 P.M.	Result'.	M'N	Rain in inches.	Snow in inches.
								°							
1	.99	.51	—	—	N W b N	N W	N W b N	N 35 W	15.8	29.8	24.0	19.93	20.09	...	...
2	.53	.64	.69	.57	N b W	S E b S	S W b S	S	13.5	10.6	11.0	2.84	9.57	..	0.1
3	.90	.55	.83	.76	S b E	S S W	Calm.	S 31 W	9.5	12.8	0.0	7.05	7.61	...	...
4	.89	.59	.83	.75	N N W	N E b E	W b N	N 69 E	7.0	17.0	24.5	4.17	14.58	0.410	...
5	.80	.69	.88	.72	S W b S	S W	N W b N	N 81 W	9.5	8.5	9.5	6.94	9.26	0.010	...
6	.84	.75	—	—	N W	S	S S W	S 16 W	4.5	11.5	4.0	2.83	5.72	...	...
7	.90	.60	.92	.75	S S W	N E b E	E b N	N 65 E	3.2	8.5	11.2	7.34	8.77	0.052	...
8	.94	.87	—	—	N N E	S E b E	N b W	N 4 E	6.0	4.5	18.0	5.03	7.48	...	...
9	.82	.63	.79	.75	N b W	E b S	N E b N	N 54 E	12.5	11.4	4.5	8.40	9.79	...	...
10	.78	.92	.88	.90	E N E	N E b E	N W b N	N 2 W	18.2	1.5	22.8	6.30	9.15	0.135	...
11	.80	.47	.93	.72	N b W	N b E	N W b N	N 27 W	10.8	5.0	3.2	4.51	6.13	...	...
12	.85	.70	.74	.77	N W b N	S S W	W N W	S 77 W	1.0	17.0	14.0	11.86	15.[illegible]9	0.045	...
13	.77	.67	.85	.79	N W b W	W S W	W b N	N 87 W	22.0	20.4	19.2	14.91	18.[illegible]	0.140	0.2
14	.86	.88	.56	.72	N W b W	N W b W	N N W	N 61 W	12.6	22.2	12.2	14.06	14.63	...	inap.
15	.82	.56	—	—	W b N	S E b E	E	S 86 E	6.2	5.8	11.0	9.24	10.10	...	...
16	.85	.79	.89	.84	E	N E b E	N E b E	N 65 E	14.0	9.4	5.8	6.93	7.79	0.120	...
17	.89	.63	.66	.68	W b S	N N W	W N W	N 73 W	10.0	25.5	12.2	18.77	18.92	...	...
18	.76	.55	.72	.61	N W b W	S E b E	S E	S 85 E	5.0	9.5	3.0	3.80	5.90	...	...
19	.83	.40	.76	.63	N b E	E b N	N N E	N 47 W	6.0	14.5	3.0	6.59	7.05	0.060	...
20	.95	.95	.98	.95	N	N E b E	Calm.	N 25 E	2.5	5.8	0.0	2.33	2.78	0.100	...
21	.96	.78	.83	.84	N W b W	N E b N	N	N 11 E	1.0	5.0	6.8	7.83	8.75	0.140	...
22	.88	.76	—	—	N	N	N W b N	N 25 W	16.2	10.5	11.0	12.88	13.27	0.070	...
23	.57	.67	.87	.68	N b W	S b W	N W b N	N 45 W	7.8	10.2	9.5	8.24	10.17	...	...
24	.80	.61	.85	.77	N W b W	W b N	W b S	N 75 W	13.5	20.5	9.2	15.24	15.83	...	inap.
25	.86	.58	.69	.73	N W b W	N W	N W b N	N 40 W	16.8	19.5	19.5	15.89	16.18	...	inap.
26	.64	.62	.81	.69	N W b W	N W b W	N W b N	N 46 W	14.8	14.0	4.8	10.03	10.22	...	...
27	.80	.41	.81	.64	N W b N	S b W	S W b S	S 31 W	1.0	12.0	7.6	6.12	7.05	...	...
28	.81	.63	.75	.73	N W b W	E	E b N	N 79 E	1.5	7.5	3.8	4.11	5.62	...	...
29	.80	.55	—	—	N N E	E b S	N E b E	N 67 E	2.0	6.2	1.5	3.97	4.50	...	...
30	.85	.60	.72	.70	N N E	E	W b N	N 35 E	1.8	11.0	7.2	4.90	8.42	...	...
31	.81	.64	.80	.74	....	....	....	....	8.87	12.25	9.80	....	10.30	1.282	0.3

REMARKS ON TORONTO METEOROLOGICAL REGISTER FOR APRIL, 1860

Highest Barometer . . . . 30.265 at 8 a. m. on 18th. } Monthly range=
Lowest Barometer . . . . 28.896 at 8.30 p. m. on 4th, } 1.369 inches.

Self-register- ing Ther. { Maximum temperature . . . . 61°8 on p.m. of 30th } Monthly range=
Minimum temperature . . . . 19°5 on a.m. of 2nd } 42°3
Mean maximum temperature . . 47°01 } Mean daily range = 14°84.
Mean minimum temperature . . 32°10 }
Greatest daily range . . . . . . 25°6 from a. m. to p.m. 18rd.
Least daily range . . . . 3.5 from a. m. to p. m. of 3rd.

Warmest day . . 30th ... Mean Temperature . . . 50°72 } Difference=24°30.
Coldest day . . . 2nd ... Mean Temperature . . . 26°42 }

Maximum { Solar . . . . 76°0 on p. m. of 30th } Monthly range=
Radiation { Terrestrial . . . . . . 5.8 on a. m. of 2nd } 70°2.

Aurora observed on 7 nights, viz.: on 9th, 11th, 12th, 14th, 18th, 23rd and 26th; possible to see Aurora on 18 nights; impossible on 12 nights.
Snowing on 5 days; depth 0.3 inches: duration of fall 7.2 hours.
Raining on 11 days; depth, 1·282 inches; duration of fall, 40.6 hours.
Mean of cloudiness=0.50; most cloudy hour observed, 2 p. m., mean=0.67: least cloudy hour observed, 10 p. m.; mean=0.40.

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

North.	South.	East.	West.
3133.17	1065.19	1657.39	3120.57

Resultant direction, N 37° W; Resultant Velocity, 4.10 miles per hour.
Mean velocity of the wind 10.30 miles per hour.
Maximum velocity . . . 34.2 miles per hour, from 8 to 9 a.m. on the 17th.
Most windy day . . 1st—Mean velocity, 20.09 miles per hour. } Difference 17.31
Least windy day . . 20th—Mean velocity, 2.78 do } miles.
Most windy hour, 2 to 3 p. m.—Mean velocity, 12.90 miles per hour. } Difference
Least windy hour, 1 to 2 a. m.—Mean velocity, 7.61 do. } 5.29 miles.

5th. Thin ice on the boards at 5.30 a m.
6th. Thin ice on the pools at 6 a m.
8th. Frogs croaking loudly (first heard this season.)
10th. Distant thunder from 6.40 to 7.20 a. m.
15th. Imperfect Solar Halo from 9 to 10 a. m.
16th. Sheet lightning in E. from 11.30 p. m.
20th. Fog from 11 a. m. Sheet lightning in S. E. from 10.40 p. m.
21st. Dense fog to 8 a. m.
24th. Occultation of Venus from about 8 20 p. m.
27th. Perfect Solar Halo, exhibiting the Prismatic colors, from 7.30 a. m. to 4 p. m.
28th. Hoar frost 6 a. m.

Thin ice on shallow vessels every morning from 24th to 29th inclusive.

The Resultant Direction and Velocity of the Wind for the month of April, from 1848 to 1860 inclusive, were respectively N. 23° W., and 2.10 miles.

The month of April, 1860, was cold, dry and windy—the mean temperature having been 1°43 below the average of 21 years. The depth of Rain recorded was the least during the series, and 1.153 inches less than the average, and the velocity of the wind 2.51 miles per hour above the average of 13 years.

COMPARATIVE TABLE FOR APRIL.

YEAR.	TEMPERATURE.					RAIN.		SNOW.		WIND.		
	Mean.	Difference from Average.	Maximum observed.	Minimum observed.	Range.	No. of days.	Inches.	No. of days.	Inches.	Resultant Direction.	Resultant Velocity.	Mean Velocity
1840	42.4°	+ 1.5°	65.9°	25.3°	40.6°	14	3.420	2	...	° ...	...	...
1841	39.2	— 1.7	62.9	22.1	40.8	3	1.370	3	...	...	...	0.51 lbs
1842	43.1	+ 2.2	59.5	21.6	67.9	8	3.740	2	...	...	...	0.57 "
1843	40.9	0.0	70.0	15.1	54.9	7	3.185	3	0.1	...	...	0.46 "
1844	47.5	+ 6.6	74.5	17.2	57.3	10	1.515	1	Inap	...	...	0.24 "
1845	42.1	+ 1 2	66.0	14.8	51.2	11	3.290	4	1.5	...	...	1.00 "
1846	44.0	+ 3.1	79.4	24.4	55.0	10	1.304	2	1.3	...	...	0.55 "
1847	39.2	— 1.7	65.6	8.4	57.2	8	2.87	2	4.0	...	...	0.59 "
1848	41.3	+ 0.4	65.4	26.5	38.0	5	1.455	1	0.5	N 77 W	1.46	4.89ms.
1849	39.0	— 1.9	70.9	23.2	47.7	10	2.657	2	1.7	N 43 W	3.14	7.50 "
1850	37.9	— 3.0	63.2	18.2	45.0	7	4.72	2	1.1	N 39 W	1.12	7.64 "
1851	41.3	+ 0.4	59.2	25.8	33.4	11	2.29	3	1.2	N 14 E	2.52	8.07 "
1852	38.2	— 2.7	53.8	19.8	34.0	6	1.9	4	9.4	N 23 E	2.41	6.68 "
1853	41.9	+ 1.0	65.7	27.0	38.7	10	2.62	1	1.0	N 12 W	1.95	5.29 "
1854	41.0	+ 0.1	65.1	22.3	42.8	12	2.68	4	2.7	N 50 E	2.57	6.81 "
1855	42.4	+ 1.5	63.8	12.2	51.6	8	2.030	3	1.6	N 36 W	3.99	7.57 "
1856	42.3	+ 1.4	69.8	15.1	54.7	13	2.780	3	0.1	N 29 E	1.64	6.05 "
1857	35.4	— 5.5	51.9	10.0	41.9	10	1.755	11	12.9	N 60 W	4.15	10.24 "
1858	41.5	+ 0.6	61.5	23.8	37.7	13	1.612	2	0.1	N 14 W	1.64	9.57 "
1859	39.5	— 1.4	62.1	23.9	38.2	9	2.527	8	1.2	N 36 W	2.33	10.79 "
1860	39.5	— 1.4	60.7	19.7	41.0	11	1.282	5	0.3	N 37 W	4.10	10.30 "
Mean	40 93	...	66.01	19.83	46.21	9.3	2.435	3.2	2.26	...	...	7.79

MONTHLY METEOROLOGICAL REGISTER, AT THE PROVINCIAL MAGNETICAL OBSERVATORY, TORONTO, CANADA WEST—MAY, 1860.

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

Days.	Barom. at temp. of 32°.				Temp. of the Air.				Excess of mean above Average	Tens. of Vapour.				Humidity of Air.				Direction of Wind.			Resultant 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.	Re-sul't.	ME'N		
1	29.574	29.693	29.811	29.7103	44.6°	42.8°	37.8°	41.30°	− 5.12°	.235	.190	.183	.201	.80	.69	.80	.77	N N W	N W	N W b N	N 36° W	9.4	16.8	8.2	11.65	11.82	0.003	...
2	.871	.848	.800	.8357	35.6	54.7	49.3	46.00	+ 0.18	.186	.337	.270	.260	.89	.78	.77	.79	N b W	S S W	N b W	N 30 W	13.6	6.0	10.0	4.40	7.40	...	...
3	.704	.708	.661	.7140	42.8	59.4	54.7	53.58	+ 6.53	.213	.297	.262	.258	.77	.58	.60	.63	N N W	S b W	N W	S 63 W	0.5	6.5	7.0	1.19	3.50	0.015	...
4	.651	.655	.614	.6397	49.7	65.0	54.7	56.92	+ 9.50	.278	.359	.337	.318	.77	.58	.78	.69	N N W	S S W	W b N	S 37 W	0.2	7.5	3.2	2.20	3.90	...	...
5	.639	.614	.620	.6282	53.5	68.1	48.9	56.13	+ 8.40	.290	.254	.208	.280	.72	.38	.86	.66	Calm.	S	S	S 43 W	0.0	4.6	1.5	1.07	2.03	...	...
6	.616	.530	—	—	49.3	66.3	—	—	—	.293	.419	—	—	.83	.64	—	—	W W	E b N	E b S	N 80 E	0.5	8.2	3.5	4.39	5.34	...	...
7	.515	.497	.506	.5078	51.1	73.2	63.0	62.02	+13.60	.309	.541	.409	.449	.82	.65	.71	.79	N b E	S S W	N W	N 63 W	0.8	3.2	5.2	2.96	3.82	0.080	...
8	.520	.536	.440	.5153	60.5	62.7	59.8	59.78	+11.00	.408	.454	.428	.421	.77	.79	.83	.82	E N E	E	N E b N	N 9 E	6.4	4.0	12.0	3.53	7.86	0.935	...
9	.400	.340	.414	.3832	55.8	60.1	57.2	57.02	+ 7.85	.422	.402	.439	.414	.95	.77	.94	.89	E N E	E b N	S b W	S 68 E	16.5	12.5	8.5	6.40	9.41	0.205	...
10	.486	.552	.612	.5625	52.6	60.5	51.8	54.30	+ 4.80	.386	.408	.363	.381	.97	.77	.94	.90	N E	E	N E b N	N 61 E	3.8	9.0	4.2	4.98	5.77	0.055	...
11	.673	.730	.757	.7248	52.2	61.6	56.5	57.03	+ 7.13	.358	.417	.402	.391	.92	.76	.88	.84	N E b N	E N E	N E	N 49 E	6.5	10.4	10.5	8.89	9.11	...	...
12	.798	.768	.712	.7515	54.7	62.7	56.9	58.45	+ 8.28	.380	.414	.403	.408	.89	.72	.88	.83	N N E	E b N	N E b E	N 52 E	6.2	11.6	10.5	8.61	9.32	...	...
13	.677	.662	—	—	56.5	65.4	—	—	—	.368	.418	—	—	.80	.66	—	—	N N E	E b S	N W	N 59 E	5.5	13.0	4.0	3.65	5.57	...	...
14	.624	.570	.580	.5920	55.4	67.0	54.9	61.18	+10.30	.405	.435	.249	.366	.93	.65	.58	.67	N N W	S E b S	N E b E	S 82 E	2.2	8.0	4.0	1.15	4.70	...	...
15	.654	.653	.542	.6618	55.4	59.8	50.9	56.12	+ 4.83	.371	.281	.200	.285	.85	.54	.53	.63	N N E	E	E S E	N 75 E	4.2	4.6	3.5	2.75	4.80	...	.
16	.653	.646	.695	.6678	55.4	65.9	54.4	59.42	+ 7.83	.265	.312	.287	.295	.67	.49	.67	.59	N N W	S S E	E	N 65 E	5.5	6.2	10.2	5.46	7.32	...	...
17	.757	.731	.687	.7205	50.6	59.0	54.4	55.03	+ 3.05	.215	.291	.385	.296	.57	.57	.91	.68	N E b E	E N E	N W	N 56 E	10.6	18.2	5.2	8.28	9.68	0.052	...
18	.571	.447	.308	.4257	51.1	60.1	50.4	54.05	+ 1.77	.342	.436	.310	.370	.91	.84	.93	.88	N N E	E	N E	N 64 E	3.0	8.8	3.6	4.06	4.97	0.147	...
19	.180	.088	.178	.1600	51.1	60.1	49.0	52.25	− 0.37	.347	.402	.236	.308	.92	.77	.67	.76	N W b N	W b S	N W	N 72 W	4.2	20.5	22.5	15.78	16.52	0.095	...
20	.439	.517	—	—	37.4	49.9	—	—	—	.163	.252	—	—	.72	.70	—	—	N W b N	S b W	N	N 36 W	16.0	12.6	4.0	4.56	10.54	0.015	...
21	.468	.407	.416	.4188	41.4	41.7	42.1	41.50	−11.82	.195	.237	.223	.224	.75	.89	.83	.85	N E b E	N E b E	N E b E	N 55 E	14.5	19.5	7.8	11.49	11.77	0.135	...
22	.466	.565	.674	.5807	44.6	60.1	47.5	51.43	− 2.27	.264	.401	.255	.301	.90	.77	.77	.78	N W	S S W	N b W	S 12 W	1.8	4.2	12.4	5.37	8.22	...	...
23	.700	.802	.742	.7732	43.5	58.0	45.4	50.83	− 3.13	.250	.271	.272	.276	.89	.56	.90	.75	N b W	S b W	S S W	S 27 E	2.5	4.4	1.5	1.14	3.37	...	...
24	.740	.680	.659	.6857	47.2	58.0	54.0	54.58	+ 0.30	.287	.304	.282	.297	.89	.62	.67	.70	N W	E S E	E	N 69 E	1.2	8.0	5.0	3.89	4.89	...	...
25	.627	.550	.445	.5303	56.7	68.8	57.6	61.43	+ 6.75	.278	.304	.428	.328	.60	.42	.90	.62	N E b E	E b N	N E	N 57 E	4.2	12.5	8.5	7.23	7.76	0.013	...
26	.401	.242	.266	.3013	51.8	67.3	61.2	59.40	+ 4.42	.343	.577	.375	.416	.89	.86	.69	.80	N N W	E S E	N W	N 1 W	1.5	9.4	18.5	4.59	8.83	0.010	...
27	.391	.396	—	—	55.4	65.2	—	—	—	.340	.370	—	—	.76	.59	—	—	N W b W	W b N	N	N 55 W	5.2	10.6	1.5	4.78	6.17	...	...
28	.484	.524	.600	.5307	48.6	64.1	53.3	57.02	+ 1.37	.292	.373	.344	.342	.85	.62	.84	.74	N	S S W	W	S 82 W	4.5	6.8	1.0	1.95	4.56	...	...
29	.630	.605	.562	.5725	52.2	66.6	55.4	59.47	+ 3.52	.327	.344	.371	.352	.83	.52	.85	.71	N N W	N E b E	N E b E	N 57 E	2.2	8.2	11.5	8.28	8.97	0.005	...
30	.376	.263	.310	.3193	54.7	69.5	64.1	62.95	+ 6.70	.403	.507	.487	.451	.94	.70	.81	.79	E b N	S	W b N	S 17 W	10.2	12.5	1.0	2.70	6.57	inap.	..
31	.320	.329	.420	.3570	58.0	62.7	57.2	59.15	+ 2.60	.417	.477	.404	.430	.86	.84	.86	.85	S W b W	W S W	N W	N 76 W	5.5	7.5	5.5	5.30	7.79	0.020	...
M	29.6804	29.5585	29.6610	29.5659	50.77	61.39	53.42	55.53	+ 4.05	.315	.371	.331	.338	.83	.67	.79	.76	....	....	....	....	5.45	9.54	6.95	......	7.17	1.815	...

REMARKS ON TORONTO METEOROLOGICAL REGISTER FOR MAY.

Highest Barometer .... 29.886 at 8 a. m., on 2nd } Monthly range=
Lowest Barometer .... 29.088 at 2 p. m. on 19th } 0.798 inches

Self-regist'ing Thermom.
- Maximum Temperature .... 74°5 on p. m. of 30th } Monthly range=
- Minimum Temperature .... 32°5 on a. m. of 2nd } 42°0
- Mean maximum Temperature .... 63°97 } Mean daily range=
- Mean minimum Temperature .... 47°79 } 16°18
- Greatest daily range .... 24°6 from a. m. to p. m. of 6th.
- Least daily range................. 5°2 from a. m. to p. m. of 1st.

Warmest day......... 30th ... Mean temperature......... 62.95 } Difference = 21°65.
Coldest day........... 1st ... Mean temperature......... 41°30 }
Maximum { Solar.............................. 92°0 on p. m. of 7th } Monthly range=
Radiation. { Terrestrial........................ 21°5 on a. m. of 2nd } 70°5.
Aurora observed on 7 nights, viz., on 11th, 12th, 14th, 18th, 23rd, 24th and 27th.
Possible to see Aurora on 10 nights; impossible on 12 nights.
Raining on 16 days,—depth 1.815 inches; duration of fall 36.1 hours.
Mean of cloudiness = 0.57.
Most cloudy hour observed, 2 p. m., mean = 0.64; 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.
2441.17	659.28	2304.80	1441.04

Resultant direction N. 26° E.; Resultant Velocity 2.66 miles per hour.
Mean velocity........................... 7.17 miles per hour.
Maximum velocity .... 29.8 miles, from 11 p. m. to midnight on 19th.
Most windy day .... 19th......Mean velocity 16.52 miles per hour. } Difference=
Least windy day .... 5th......Mean velocity 2.03 ditto. } 14.49 miles.
Most windy hour... 1 to 2 p. m......... .... Mean velocity 9.55 ditto. } Difference
Least windy hour... 4 to 5 a. m. .... Mean velocity 4.94 ditto. } 4.61 miles.

2nd. Hoar Frost at 6 a. m.—4th. Corona round the Moon at 10 p. m. and midnight.—7th. Thunderstorm from 8 to 9 a. m. and 3 to 4 p. m.—8th. Heavy Thunderstorm. Lightning, Rain and large Hailstones, from 9 p. m. to midnight.—10th. Dense Fog from 6 to 8 a. m.—12th. Imperfect Rainbow at 6.50 p. m.—17th. Thunderstorm 8.20 to 10.20 p. m.—18th. Distant Thunder and slight Rain from 3 to 5 p. m.—19th. Sheet Lightning in S. W. at 8.30 p. m.—24th. Distant Thunder 2.30 and 3 p. m.—25th. Thunderstorm from 8.30 to midnight.—26th. Thunderstorm, noon to 3.45 p. m. and Faint Rainbow. 4 to 5 p. m.—28th. Distant Thunder 7.30 p. m.—29th. Thunderstorm, 10 p. m. to midnight.

Heavy Dew recorded on 8 mornings during this month.

The Resultant Direction and Velocity of the Wind for the month of May, from 1848 to 1860 inclusive, were respectively N 6° E, and 1.43 miles.

The month of May, 1860, was warm, dry, and windy.
The Mean Temperature having been 3°95 above the average, was only once previously equalled in 21 years.
The depth of rain recorded is 1.419 inches less than the average. This is only a little more than half the mean amount.
The Mean Velocity of the Wind was 0.75 miles per hour above the average of 13 years.

COMPARATIVE TABLE FOR MAY.

Year.	Temperature.					Rain.		Snow.		Wind.		
	M'n.	Diff. from Aver.	Max ob'd.	Min. ob'd.	Range.	No. of days.	Inch's.	No. of days.	Inch's.	Resultant Direction.	Resultant V'y.	Mean Force or Velocity.
1840	53.8	+2.2	74.5	30.8	43.7	9	4.150	...	not record.	...	...	....
1841	50.5	—1.1	76.2	26.6	49.6	11	2.350	1		...	...	0.35 lbs.
1842	49.1	—2.5	74.3	30.0	44.3	7	1.275	...		...	...	0.53
1843	49.1	—2.5	79.0	28.9	50.7	5	1.570	...	...	...	...	0.52
1844	53.6	+2.0	77.7	29.0	48.7	14	5.670	...	...	...	...	0.30
1845	49.6	—2.0	76.6	29.4	47.2	8	2.300	...	...	...	...	0.55
1846	55.5	+3.9	78.1	34.3	43.8	9	4.375	...	...	...	...	0.46
1847	54.4	+2.8	72.5	27.8	44.7	12	2.040	...	...	...	...	0.29
1848	54.1	+2.5	78.5	31.9	46.6	13	2.520	...	...	N 40° W	1.31	4.93 mls.
1849	48.0	—3.6	72.5	32.7	39.8	16	5.115	...	...	N 51° E	1.07	5.33
1850	47.6	—4.0	76.3	31.1	45.2	7	0.645	1	Inapp	N 64° E	2.05	6.32
1851	51.3	—0.3	73.2	28.7	44.5	12	2.950	1	0.5	N 32° W	1.59	6.34
1852	51.4	—0.2	73.3	34.5	38.8	7	1.125	1	Inapp	S 82° W	0.99	4.00
1853	50.0	—0.7	78.4	38.4	40.0	17	4.420	1	Inapp	N 2° W	0.83	5.16
1854	52.2	+0.6	69.0	27.6	41.4	11	4.630	...	...	E	0.40	5.38
1855	53.1	+1.5	74.8	33.9	40.9	6	2.565	2	0.9	N 1° W	2.76	5.93
1856	50.5	—1.1	80.1	35.5	44.6	14	4.580	1	Inapp	N 4° E	3.09	9.81
1857	48.9	—2.7	72.5	27.9	44.6	15	4.145	1	Inapp	N 23° W	1.14	8.13
1858	48.9	—2.7	66.0	35.0	31.0	17	6.367	...	...	N 42° E	3.33	9.30
1859	55.2	+3.6	76.2	41.5	34.7	11	3.410	...	...	N 72° E	1.59	5.70
1860	55.5	+3.9	73.2	35.6	37.6	16	1.815	...	...	N 26° E	2.66	7.17
M	51.58	...	74.93	31.96	42.97	11	3.234	0.4	0.08	....	...	6.42 Ml.

MONTHLY METEOROLOGICAL REGISTER, ST. MARTIN, ISLE JESUS, CANADA EAST—APRIL, 1860.

(NINE MILES WEST OF MONTREAL.)

BY CHARLES SMALLWOOD, M. D., LL.D.

Latitude—45 deg. 32 min. North. Longitude—73 deg. 36 min. West. Height above the Level of the Sea—118 feet.

Day.	Barom. corrected and reduced to 32°			Temp. of the Air.—F.			Tension of Vapor.			Humidity of Air.			Direction of Wind.			Horizontal Movement in Miles in 24 hours.	Mean of Ozone. (tenths)	Rain in Inches.	Snow in Inches.	WEATHER, &c. A cloudy sky is represented by 10; A cloudless sky by 0.		
	6 A.M.	2 P.M.	10 P.M.	6 A.M.	2 P.M.	10 P.M.	6 A.M.	2 P.M.	10 P.M.	6 A.M.	2 P.M.	10 P.M.	6 A.M.	2 P.M.	10 P.M.					6 A.M.	2 P.M.	10 P.M.
1	29.371	29.367	29.521	34.0	40.0	27.1	.175	.182	.117	.89	.73	82	E S E	N W	N N E	152.20	8.0	...	...	Cu. Str. 10.	Cu. Str. 10.	Cirr. 4. L. Hal.
2	783	650	700	10.0	20.0	17.0	054	065	068	78	62	75	N b W	W	S W	261.30	0.0	...	...	Clear.	Clear.	Clear.
3	574	347	351	20.1	32.1	31.9	081	143	153	77	79	89	S b E	S S E	N E b E	59.70	10.0	...	0.75	C. C. Str. 10.	Cu. Str. 10	Snow.
4	420	367	294	29.0	42.0	36.0	142	199	170	88	74	80	N E b E	N E b E	N E b E	60.70	1.5	...	...	Cu. Str. 4.	Clear.	Clear.
5	227	250	451	34.1	38.2	31.2	180	201	149	90	85	86	N E b E	N E	N E	178.80	7.6	0.500	...	Rain.	Cu. Str. 4.	C. C. Str. 8.
6	670	662	801	30.1	46.7	34.1	145	232	175	89	73	89	N W	N N E	S S W	74.00	3.5	...	1.60	Snow.	Clear.	Clear.
7	958	681	722	30.1	56.8	40.1	136	329	208	82	72	80	S S W	S S W	S E	5.00	0.0	...	...	Clear.	Do.	Do.
8	551	441	516	38.4	40.2	41.0	214	225	235	98	91	91	S S E	S S E	S b W	16.80	10.0	0.583	...	Rain.	Rain.	Cu. Str. 10.
9	654	747	972	38.0	58.0	38.0	203	365	151	83	76	70	N N E	W S W	N N E	17.00	2.0	...	...	Cu. Str. 4.	Cu. Str. 2.	C. Au. Br. Par.
10	930	654	261	28.6	42.0	40.1	129	184	225	82	70	90	N E b E	S S E	S S E	56.40	8.0	0.160	...	Do. 2.	St. 10.	Cu. Str. 10.
11	570	484	647	33.4	49.6	37.7	182	290	193	95	82	87	W b N	N N W	N b W	233.40	5.0	...	...	Clear.	Cu. Str. 4.	Rain.
12	801	402	347	30.1	55.3	42.2	148	321	237	89	74	89	E	S b E	S E	15.00	3.0	0.290	Inp.	Do.	Cir. Str. 4.	Cu. Str. 10.
13	842	479	515	31.2	41.0	40.2	170	169	203	86	65	82	W S W	W	S W	225.30	3.5	...	...	Cu. Str. 10.	C. C. Str. 4.	C. Str. 2. Au. B.
14	492	647	742	24.2	26.4	18.9	083	082	061	66	57	60	W N W	W N W	W N W	379.50	2.5	...	...	Do. 6.	Clear.	Clear. Au. Bor.
15	30.000	011	980	12.1	32.0	27.8	015	112	117	60	63	73	W N W	W S W	S W	402.40	1.5	...	...	Do.	Do.	Clear.
16	002	971	860	32.0	36.4	34.6	143	136	169	79	65	84	S S E	S S E	S S E	31.40	1.0	0.500	...	Do.	Do.	Do.
17	29.301	201	990	40.1	56.1	36.1	225	336	170	91	75	80	S S E	W S W	W N W	423.40	2.5	...	...	R. with thun.	Cu. Str. 4.	Do.
18	30.381	30.224	30.260	25.0	43.2	29.2	110	186	136	75	67	81	N N W	S W b S	S S W	213.30	1.0	...	...	Clear.	Clear.	Do.
19	272	29.914	29.860	30.0	60.1	43.4	130	396	224	78	75	79	W b S	S W	S	13.10	1.0	...	...	Do.	Do.	Clear. Au Bor.
20	29.812	311	417	36.7	64.9	50.2	161	451	303	76	73	88	S S E	S S E	S S W	120.80	1.0	...	...	Cu. Str. 4.	C. C. Str. 8.	Do.
21	437	347	441	43.0	63.7	48.0	251	416	285	91	72	85	E	E b N	N E b E	41.10	1.5	...	...	C. C. Str. 8.	Clear.	Do. 10.
22	591	311	414	36.7	61.7	50.5	177	406	283	85	74	78	N E b E	E S E	S	195.10	1.5	...	...	Do. 4.	Do.	Clear.
23	590	414	574	34.2	54.0	39.0	155	282	167	79	67	68	W N W	W S W	W N W	167.80	1.0	...	...	Clear.	Do.	C. C. Str. 4.
24	630	451	600	31.0	39.8	30.0	136	131	130	77	55	70	W N W	W S W	W N W	151.10	1.5	...	...	Cu. Str. 10.	Cu. Str. 4.	Snow.
25	671	740	711	25.4	46.2	38.3	100	230	182	74	88	79	S S W	S E	S S W	100.90	2.5	...	0.10	Do. 2.	C. C. Str. 2.	Cu. Str. 4.
26	657	561	811	37.2	53.0	42.7	178	269	218	81	67	78	W S W	W	W N W	152.00	1.0	...	...	Clear.	Clear.	Clear.
27	914	991	954	35.2	55.2	40.3	162	218	203	80	50	82	S W b W	W b N	W S W	143.30	1.0	...	...	Do.	Do.	Do.
28	30.001	942	847	32.7	67.4	46.3	137	274	192	74	41	62	S S W	S b E	S E	9.27	1.0	...	...	Do.	Do.	Do.
29	952	900	947	35.4	72.1	55.2	127	270	269	62	32	62	N E b E	S E b E	S E b E	130.23	1.0	...	...	Do.	Do.	Do.
30	976	942	874	43.0	76.2	57.0	209	303	230	75	34	51	E S E	S E	E S E	151.40	0.5	...	...	Do.	Do.	Do.

MONTHLY METEOROLOGICAL REGISTER, ST. MARTIN, ISLE JESUS, CANADA EAST—MAY, 1860.

(NINE MILES WEST OF MONTREAL.)

BY CHARLES SMALLWOOD, M.D., LL.D.

Latitude—45 deg. 32 min. North. Longitude—73 deg. 36 min. West. Height above the Level of the Sea—118 feet.

Day.	Barom. corrected and reduced to 32°			Temp. of the Air.—F.			Tension of Vapour.			Humidity of Air.			Direction of Wind.			Horizontal Movement in Miles in 24 hours.	Mean of Ozone. t'nths	Rain in inches.	Snow in inches.	WEATHER, &c. A Cloudy sky is represented by 10; A cloudless sky by 0.		
	6 A.M.	2 P.M.	10 P.M.	6 A.M.	2 P.M.	10 P.M.	6 A.M.	2 P.M.	10 P.M.	6 A.M.	2 P.M.	10 P.M.	6 A.M.	2 P.M.	10 P.M.					6 A.M.	2 P.M.	10 P.M.
1	29.970	29.900	29.930	49.9	79.5	23.8	.228	.217	.209	.64	.26	.67	S E	S E	S E	125.36	0.0	...	...	Clear.	Clear.	Clear.
2	30.140	890	920	44.9	69.9	53.2	275	360	288	92	50	76	S E	E b S	N E	14.60	0.5	...	...	Hazy.	Do.	Do.
3	29.990	842	725	47.3	74.5	59.0	291	270	318	89	32	63	E	S W b W	S S E	8.50	0.5	...	...	Clear.	Do.	Do.
4	820	714	624	50.0	74.1	59.1	256	263	352	71	31	70	E S E	N E b E	N E b E	131.10	1.5	...	...	Cu. Str. 10.	Do.	Do.
5	860	954	981	57.0	73.2	58.4	262	345	357	55	42	70	N N E	S S E	S b E	0.10	1.0	...	...	Clear.	Do.	Do.
6	30.017	997	30.001	47.0	84.2	62.2	267	545	347	81	47	64	S E	S	S	12.20	1.0	...	...	Do.	Do.	Do.
7	29.920	902	046	54.5	72.1	57.9	383	489	385	90	62	84	S b E	S E	N N E	87.70	1.5	...	...	Hazy.	Do.	Cu. Str. 10.
8	990	941	001	47.0	74.2	60.9	267	532	419	80	63	85	N N E	S S W	S S W	153.40	1.5	...	...	Cu. Str. 4.	Cu. Str. 4.	Clear.
9	30.140	30.050	061	56.1	58.5	56.3	391	452	413	87	94	90	S E	S E	S E	100.10	3.0	Inap.	...	Clear.	Do. 10.	Cu. St. 10. Par.
10	29.947	29.814	29.879	52.2	63.4	57.4	368	543	452	96	94	91	S E	E S E	S S E	249.10	5.0	0.160	...	Cu. Str. 4.	Do. 4.	Slight rain.
11	641	846	30.056	60.3	76.1	63.6	480	541	510	91	60	88	S S E	S S E	S S E	11.16	5.3	...	...	Do. 4.	Do. 4.	C. C. Str. 8.
12	998	30.904	110	64.2	81.6	69.2	529	617	635	89	58	90	S S E	S b W	S S E	2.70	0.0	...	...	Clear.	Clear.	Clear. Au. Br.
13	980	29.894	29.914	59.6	83.1	65.6	403	597	337	79	53	59	S S E	E b S	S E b E	84.50	1.3	...	...	Do.	Do.	Cu. Str. Sr. H.
14	871	747	797	63.8	70.1	51.0	341	300	270	83	30	72	E b N	N E b E	N E b E	311.70	0.0	...	...	Do.	Do.	Cl. Solar Halo.
15	800	831	814	44.1	72.2	66.7	218	327	359	76	42	78	N b E	S S E	W S W	199.70	0.0	...	...	Do.	Do.	Do.
16	771	30.010	30.090	46.0	60.2	47.1	262	216	256	84	43	81	N E b E	E N E	E b S	222.80	0.0	...	...	Do.	Do.	Do.
17	30.140	004	110	45.5	72.4	53.0	198	524	321	63	66	80	N b E	S S E	S S E	44.64	0.0	...	...	Do.	Do.	Do.
18	29.947	29.900	29.799	50.1	64.6	60.3	326	285	426	93	48	82	S S E	S b E	S b E	113.62	6.6	Inap	...	Do.	Do.	Do.
19	489	279	187	61.1	63.0	55.0	480	491	375	90	88	93	S S E	S S E	S W	100.10	5.5	2.670	...	Do.	Cu. Str. 10.	C. St. lig't S. W
20	360	639	847	32.1	38.7	34.1	162	123	155	89	54	79	W	W N W	N b W	255.20	4.0	0.742	0.70	Cu. Str. 10.	Rain.	Rain.
21	997	914	874	35.1	55.5	40.3	127	218	238	62	50	78	W	S S E	S	24.80	1.5	...	...	Snow.	Cu. Str. 6.	Clear.
22	738	819	910	47.0	51.9	48.1	298	315	310	92	83	92	S S W	N W	N W	26.80	3.0	...	...	Clear Frost.	C. C. St. 8.	Cu. Str. 8.
23	997	982	30.004	44.2	71.3	56.9	214	347	413	83	46	90	N W	W b S	S E	24.20	2.5	...	...	Cu. Str. 10.	Cu. Str. 10.	Do. 8.
24	30.052	30.011	002	49.5	71.6	59.0	297	503	410	85	66	82	E S E	S W	S E b E	39.10	1.0	...	...	Clear, sl. frost.	Clear.	Do. 8.
25	29.904	29.879	29.801	52.0	85.7	67.4	354	610	420	90	50	63	S b W	S	E S E	8.80	2.0	...	...	Cu. Str. 8.	Do.	Do. 4.
26	802	800	697	57.2	78.0	63.6	372	323	416	78	35	72	E b S	E b N	S W	228.50	1.5	...	...	Clear.	Do.	Cirr. Str. 4.
27	647	582	711	53.3	63.7	56.1	348	471	300	86	81	86	S E b E	S S E	S S E	217.20	2.0	0.732	...	Do.	Cu. Str. 9.	Do. 9. dis. lig.
28	719	774	871	54.0	63.9	55.7	353	409	370	84	70	84	S	N E b E	S E	30.40	2.5	...	...	Rain, thunder	Do. 8.	Do. 8.
29	927	991	947	51.2	78.2	65.7	245	594	509	65	63	81	S S W	W	W b S	113.80	2.5	...	...	Clear.	Do. 2.	Cir. Str. 4.
30	937	749	607	61.0	73.2	61.7	409	545	383	80	67	71	S S W	S W	S S W	20.50	1.5	Inap.	...	Cu. Str. 10.	Cir. Str. 8.	C. C. Str. 4.
31	642	571	609	63.0	76.3	62.1	478	577	460	83	64	83	S W	W b N	S W b W	206.70	2.5	0.007	...	Do. 8.	C. C. Str. 9.	Do. 8.

REMARKS ON THE ST. MARTIN, ISLE JESUS, METEOROLOGICAL REGISTER FOR APRIL, 1860.

Barometer	Highest, the 18th day	30.381
	Lowest, the 5th day	29.227
	Monthly Mean	29.669
	Monthly Range	1.154
Thermometer	Highest, the 30th day	76°7
	Lowest, the 2nd day	10°0
	Monthly Mean	40°29
	Monthly Range	66°7
Greatest Intensity of the Sun's Rays		90°9
Lowest point of Terrestrial Radiation		7°6
Mean of Humidity		.753

Rain fell on 5 days, amounting to 1.733 inches, it was raining 26 hours and 10 minutes, and was accompanied with thunder on one day.

Snow fell on 4 days, amounting to 245 inches, it was snowing 14 hours and 20 minutes.

Most prevalent wind, the W. N. W.

Least prevalent wind, the E.

Most windy day, the 15th day; mean miles per hour, 17.64.

Least windy day, the 7th day; mean miles per hour, 0.20.

Aurora Borealis visible on 4 nights.

Lunar Halo visible on 1 night.

Parhelia visible on 1 day.

Swallows *(Hirundo bicolor)* first seen on 24th day.

Frogs *(Rana pipiens)* first heard on 20th day.

The electrical state of the atmosphere has indicated high and constant tension of a negative character.

REMARKS ON THE ST. MARTIN, ISLE JESUS, METEOROLOGICAL REGISTER FOR MAY, 1860.

Barometer	Highest, the 9th day	30.146
	Lowest, the 19th day	29.187
	Monthly Mean	29.864
	Monthly Range	0.959
Thermometer	Highest, the 12th day	87°9
	Lowest, the 21st day	26°9
	Monthly Mean	59°85
	Monthly Range	61°0
Greatest intensity of the Sun's rays		99°2
Lowest point of Terrestrial Radiation		15°1
Mean of Humidity		.695
Amount of Evaporation		2°89

Rain fell on 7 days, amounting to 4.310 inches; it was raining 28 hours 52 minutes, and was accompanied by thunder on two days.

Snow fell on 1 day, amounting to 0.07 inches; it was snowing 1 hour 15 minutes.

Most prevalent wind, the S. S. E.

Least prevalent wind, the E.

Most windy day, the 14th day; mean miles per hour, 12.09.

Least windy day, the 5th day; mean miles per hour inappreciable.

Aurora Borealis visible on 1 night.

Slight frost on the mornings of the 17th, 21st and 23rd days.

Parhelia visible 1 day.