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SIR WILLIAM HAMILTON'S PHILOSOPHY: AN EXPOSITION AND CRITICISM.

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I. SCOTTISH PHILOSOPHY.

I propose to present in this Journal a series of articles on Sir William Hamilton and his philosophy. Whatever value one may ascribe to the work which Sir William has performed in the world, it cannot be doubted that he is the representative of a very extensive philosophical school at the present day, and that for some time it will be required by friends and foes alike, that that school shall be estimated as it is represented in his writings. The philosophy, of which Hamilton is the most distinguished exponent, he regards as being identical, in its fundamental positions, with that which is known in our histories of philosophy as the *Scottish School*; and it is consequently of importance, if it be not absolutely necessary, in order to the scientific comprehension of Hamilton's philosophy itself, that it should be studied in its relation to the national philosophy of his country, of which it is ostensibly an exposition and defence. I shall accordingly endeavour to give, in the present article, such an outline of the Scottish philosophy in its history and its most prominent characteristics, as seems requisite for the explanation of Sir William Hamilton's speculations; and in doing so, I must of course limit myself exclusively to the most prominent of the problems on which these speculations touch.

The earliest impulse to philosophical speculation is probably to be traced in Scotland, as in most other countries in modern Europe, to the general intellectual revival which mingled, at one time as cause, at another as effect, with the reformation of the church in the 16th century. A powerful influence must have been exerted in the earlier part of the century by John Mair, especially through his opinions on civil and ecclesiastical polity,* which he had probably thought out when, as a student at the University of Paris, he became acquainted with the claims of the Gallican church, and which, it is equally probable, gave a direction to the lives of his pupils, Knox and Buchanan, as well as to the reform which they were the principal means of introducing. But in those departments of philosophy, in which the Scottish school became afterwards famous, Mair attained no emancipation from the traditional forms of thought whose trammels were beginning to be felt througho.. Europe; and accordingly when the last quarter of the century opened, it was still an axiom in St. Andrew's, *Absurdum est dicere errasse Aristotelem,* which could not be questioned without a riot,* and the denial of which by the Principal in the University of Glasgow, was sure to excite, in one of the regents, disrespectful manifestations of ill temper.† The Principal of that University at the time was Andrew Melville. Melville had in earlier life attended the lectures of Ramus at the University of Paris, and not only his immediate assault on the dominant Aristotelianism in the Universities of his native country, but his whole teaching, as far as may be gathered from the text books which he introduced,* seems but the natural issue of the stimulus which he had received from the great leader of the revolt against Aristotelian authority in France. The learning and eloquence and argumentative ability, with which Melville led his successful inroad upon the old routine of thought in

* For these, see McCrie's *Life of Knox.*

* *Autobiography and Diary of Mr. James Melville,* pp. 123-4.

† Ibid, p. 67.

* Ibid, p. 49.

the Scotch Universities, originated a fresh educational power which had begun to attract even foreigners† to the then remote University of Glasgow; and a more intimate acquaintance with the period will only confirm the impression, that for Scotland a brilliant career in letters was being opened up,* such as her Southern sister had then already commenced, and such as she herself entered upon at once, whenever the cause was removed, which soon after this began to operate, and which rendered such a career impossible for her until she had done a century of other work more essential to her own existence, and also, it is believed, to the progress of civilization in the British islands.

It is no part of my task in this place to interpret the development of the Scottish mind in the seventeenth century; but even Mr. Buckle explains the limitation of its range during that period, as arising, not from an inherent impotence, but partly from the compulsory imprisonment of external circumstances, partly from the voluntary concentration of its powers on an unwearying revolt against political and ecclesiastical despotism. That such is the true explanation of the narrow space within which the Scottish mind moved during the century in question, becomes apparent from the results which immediately followed the Revolution of 1688. With the peaceful communication, which by this means was opened, between the north and the south of Britain, began that influence of the two nations on each other, which, after a few years, rendered their legislative union possible and which is now welding them into one. The literature of England thus found its way into Scotland, and the literary language of London soon become that of Edinburgh also. The Scotch, able once more to breathe freely, began to look abroad on what other nations had been doing, while they were absorbed in their long struggle for existence and for what was dearer to them than existence itself. Even in theology a freer range of thought was ventured upon: so conservative a churchman as Wodrow did not shrink from acquainting himself with the writings of Tindal and Collins, while he indicates the change which had come over the spirit of the Scottish

† Ibid., p 50. This work, to which I have referred several times, contains some valuable information regarding the condition of the Scottish Universities during the latter part of the 16th century. The author was a nephew of Andrew Melville, and was the first regent in Scotland who lectured on Aristotle's works, not from Latin translations, but from the original (p. 54.)

* See D. Stewart's *Dissertation*, p. 62, note.

Kirk by his alarm at "the notions getting into the heads of young preachers, that moral duties are preferable to positive, &c."* Already in the earlier years of the century there are not wanting indications of the first beginning of those efforts, which at a later period became more decided, to explain what had been deemed the peculiarities of Christianity in accordance with the natural course of mental and material phenomena. In this reawakening of the nation to questions, which it had been precluded from investigating by the circumstances of its history during the previous century, it was natural that the intensely theological bent, which had been already given to it by these circumstances, should direct its efforts still. It may be owing to this, that, as has been noticed by Cousin,† the most eminent guides of the new intellectual movement were connected professionally with the national church and that the speculations of the Scottish school, especially in moral philosophy, have *uniformly* shewn the high moral influence of the old presbyterianism, or, as Hamilton has expressed it, have been uniformly opposed to all destructive systems.‡

Meanwhile a change took place in the constitution of the Universities, the influence of which in the impulse given to science and philosophy has never, so far as I am aware, been noticed. This was the institution and endowment of professorships, and the consequent abolition of the practice in accordance with which each regent carried his set of pupils through the studies of the entire curriculum in Arts. The change had in fact to some extent been adopted in the University of Glasgow more than a century before, namely in 1576, under the Principalship of Andrew Melville.§ and was subsequently continued, as well as extended; ‖ but its advantages were in a large measure annihilated by the circumstance, that the salaries attached to the several professorships were on a graduated scale, and that when, any of the higher became vacant, the occupants of the less lucrative were advanced.** It was not however till the year 1708 that the old system was abandoned in Edinburgh;†† and the first appointment, under the

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*Wodrow's *Correspondence*, Vol. III, p. 470.

†*Philosophie Ecossaise*, pp. 18–19 (3me. ed.)

‡*Lectures on Metaphysics*, Appendix B. (c.)

§*Autobiography and Diary of J. Melville*, p. 54.

‖*Reid's Account of the University of Glasgow* in Hamilton's edition of his Works, p. 729.

**Ibid, p. 730.

††Bower's *History of the University of Edinburgh*, Vol. II., pp. 71-2.

new system, to the chair of Moral Philosophy did not take place till 1729, to that of Logic and Metaphysics not till the following year. In Aberdeen the old system was continued even in 1752, when Dr. Reid was elected Professor of *Philosophy* and in discharge of its duties required to teach Mathematics and Physics, as well as Logic and Ethics.*

The first professor appointed under the new system to the chair of Logic and Metaphysics in Edinburgh was Dr. John Stevenson, to whom an honourable place should be assigned among the earlier originators of the philosophical inquiry, which the introduction of that system assisted in advancing. It is not indeed for the contributions which his own speculations have given to the philosophy of Scotland, that he is here brought into prominence; but his influence as a teacher in awakening and unfolding the philosophical spirit in others is spoken of by such pupils as Robertson and Stewart so highly, that one cannot but wish to know more of him than is contained in the slender notices which †have come down to us.

In the same year in which Stevenson entered upon his labours in Edinburgh, a man of greater importance both for the results of his speculations, and for his influence as a philosophical teacher, commenced his career as professor of moral philosophy in the University of Glasgow. Francis Hutcheson is rightly regarded by nearly all historians of philosophy as the true originator of the Scottish School. Undoubtedly his claim to this position is founded in a considerable measure on the influence which he exerted in directing inquiry towards mental phenomena in general; but we shall afterwards see how largely the distinctive doctrine of the Scottish school is indebted to the most prominent doctrine of his system,—the theory of internal senses whose affections furnish the mind with ideas as peculiar and indecomposable as those with which we are furnished by the affections of the external or bodily senses.‡

We are now to trace the course through which speculation was led to the position it assumed in the Scottish school. From the opening of intercourse with England, the Scotch professors seem to have kept their students abreast of the most recent English specula-

*Stewart's *Account of Reid* in Hamilton's edition of Stewart's Works, Vol. X., p. 253.

†The fullest information about Stevenson that I have met with is in Bower's *History of the University of Edinburgh*, Vol. II., pp. 269-281.

‡See Reid's *Intellectual Powers*, Essay VI., Chap. 2.

tions. The writings of Hobbes and of his immediate antagonists came too soon to produce any appreciable influence in Scotland, or at least their influence was interrupted by that of a work which has created a more prominent epoch in the history of philosophy. It is from Locke's *Essay concerning Human understanding* and the consequences to which its doctrines were reduced by others, that we must trace the most important philosophical systems which have since prevailed in France and Germany, as well as in Britain. During the earlier part of last century the doctrines of the *Essay* formed the basis of the principal philosophical teaching in the Scottish Universities; the abridgement by Bishop Wynne was a favourite text-book, and the *Elements of Logic* by Professor William Duncan of Aberdeen is also a mere summary of Locke.*

But, in the transition from Locke to the speculations of Scotland, we may not omit a philosopher, who has not, indeed, received the same prominent position in our histories of philosophy, because his doctrines are only now exerting their just influence by being only now interpreted correctly, but who appears to me to have at once displayed keener philosophical insight, and attained more nearly the true theory of knowledge, as well as the true theory of existence. In Berkeley's *New Theory of Vision*, which was published in 1709, if it be carefully read, there will be found rising to explicit statement at times an implied theory of perception, not by sight alone, but by all the senses; the theory, in fact, which was more fully explained in the *Principles of Human Knowledge* (1710), and which received its most perfect form in the *Three Dialogues between Hylas and Philonous* (1713). The received interpretation of this theory, which became afterwards prevalent in the Scottish school, regards it as a reduction of Locke's theory to partial scepticism—to scepticism concerning the reality of material things. I cannot but maintain that few, who read the bishop's writings afresh in the light of more recent speculations, will rise from their perusal with any such interpretation of their drift. What the drift of his teaching is, it must require considerable time, in the face of such long-established misapprehension, to explain; still, in the few sentences which the brevity of this sketch allows me for such a purpose, I must endeavour to indicate, at least in general, the meaning I attach to his theory.

To interpret the theory, especially in so far as the interpretation of

*Veitch's *Memoir of D. Stewart*, p. 25, note.

it depends on the interpretation of the language in which it is delivered, we must go back upon Locke's *Essay*, which determined the terminology and phraseology of philosophical writings for a long time, both in England and in France. The problem of Locke's work, as its title implies, is a scientific explanation of human understanding; and this problem is reduced to the question, What is the origin of human understanding, or, in other words, of human knowledge? In the solution, which the *Essay* gives, of this problem, human knowledge is explained as originated exclusively by the action of the phenomena which are presented to the mind from the period of birth onwards, none of these phenomena being admitted to have had any prior existence involved in the nature of the mind. Now, the phenomena which are presented in human knowledge, and which, therefore, form the immediate objects of the mind when it knows, Locke named *ideas*.* It will thus be seen how the problem of the Essay came to be expressed in the question, What is the origin of our ideas? and this became the form in which the problem of philosophy continued to be studied in the school of Locke. It is not necessary here even to touch upon the detailed analysis of our ideas, into which the *Essay* enters with the view of vindicating its theory regarding their origin; but it is necessary to notice the fact, that ideas, or the immediate objects of knowledge, though, of course, *existing as ideas*, are still regarded as only in some way revealing to us *real existence* which can never itself be known. Now, in the light of this philosophy and its phraseology, the doctrine of Berkeley must be recognised as bearing a very different significance from that which is usually ascribed to it. There are, at least, three points in his doctrine, which I am confident that an examination of the *Dialogues between Hylas and Philonous* will confirm at every page.

1. Berkeley maintained the common belief of men, that sensible things, that is, the things which form the immediate objects of perception, really exist, and are not, as most of philosophers maintain, merely images of a real world, which we do not and cannot perceive.

2. But the question with Berkeley is strictly not whether sensible things really exist, or not; but what is meant by saying that they exist really? Now, according to the common doctrine of philosophers, which Berkeley combats, the real existence, which we ascribe to the material universe, is predicable not of the things which we

* See *Essay*, Book II., chap. 1, sec. 1.

know by the senses, but only of a material substance, which these things represent, though, in itself, it can never be known by the senses or by any other means. These things, however, which we know by the senses, but which merely represent to us real existence, were, as we have already seen, called *ideas* in the philosophy prevalent at Berkeley's time; yet, in spite of this unfortunate fact, it is not difficult to arrive at the conclusion that, regarding the reality of his opposition to the theory of representative perception, there is not a shadow of the doubt from which Sir William Hamilton acknowledges himself unable to clear the language of Reid. "These ideas, as you call them," his language repeatedly and explicitly insists, "these things which we see and touch, you may call them by whatever name you please, are not mere images; they are not the mere show of a world, but the real material world itself, and the only material world that really exists: for that unknown, and unknowable, and unthinkable world, of which you say the world we know is but a phantasm—it is that world which is a phantasm; the result of your own fantastic speculations, with which you puzzle yourselves and your followers." Berkeley, therefore, does not seek to explain the material world, which we know, by supposing the existence of another world, about which we know and can know nothing.

3. What, then, is the explanation which Berkeley gives of the existence which we attribute to material things? According to him, since a thing exists for us only inasmuch as we know it, its very existence, so far as we are concerned, consists in our knowledge of it. The existence of anything independent on me must, therefore, he concludes, be merely the fact that it is known by some other mind; and, consequently, the material universe, as it does not depend for its existence on human, finite minds, must be known by an Universal and Everlasting Mind.

Berkeley brings us, naturally, to the speculations of the Scottish school, not merely because it was necessary to go back upon him to find the originating influence of these speculations, but also because we must go to Scotland to follow the history of the Berkeleyan philosophy. It is fortunate that Dugald Stewart has preserved to us, on the authority of his teacher, Professor Stevenson, the most valuable evidence we possess of the extent to which the doctrines of Berkeley were studied, and studied sympathisingly, among his younger contemporaries in Scotland. The evidence, to which I refer, is the fact, that a number of young men in Edinburgh had formed a club for the purpose of studying Berkeley's *writings*, that they had corresponded with him in order to obtain further explanations regarding his theory, and that he had spoken of them as evincing a more intelligent comprehension of his argument than he had met with anywhere else.* The only person, whom Stewart mentions as having been a member of the club, is the Rev. Dr. Wallace, who is well-known as one of the earliest writers on the theory of population, and is still remembered, in the church of his native country, for the wise application of his economical studies in the origination of the Scottish Ministers' Widows' and Orphans' Fund. While this was going on in the capital, traces more distinct may be discovered of the influence which the Irish bishop's writings were exerting in other parts of the country.

Two or three years before Hutcheson had begun his career as professor in Glasgow, a younger son in the family of the Humes (or Homes), of Ninewells, in Berwickshire, though scarcely over sixteen years of age,† was schooling himself into habits of speculative thought, by which he was to create a new era in the philosophy of Europe. After abandoning, from disinclination, the study of law, and trying, for a few months, a mercantile life in Bristol, he ultimately retired, for about three years, to Rheims, and afterwards to La Fleche, in Anjou, with the view of devoting himself entirely to philosophical and literary pursuits. While he was still but twenty-six years of age, he returned to London, with the *Treatise of Human Nature* ready to be put into the printer's hands. Though the doctrines of the Treatise were afterwards recast and its author objects to their being judged in their earlier form,‡ there can be no doubt it is in this form that they have acquired historical importance and are, therefore, to be considered at present. Moreover, I know none who have not felt disappointment on turning from the Treatise to its revision—none who have not found in the former, rather than in the latter, the power which has revolutionised the speculative opinions of modern Europe.

Hume starts with the same question, with which Locke's *Essay* is mainly occupied, "What is the origin of ideas?" § Hume's answer

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* Stewart's *Dissertation*, pp. 350-1 (Hamilton's edition).

† See the Letter to Michael Ramsay, in Burton's *Life and Correspondence of D. Hume*, Vol. I., pp. 12-16.

‡ See Advertisement to his *Inquiry Concerning Human Understanding*.

§ See the *Treatise*, Book I., Chap. I., Sect. 1.; and the *Inquiry*, Sect. 2.

is, also, in the *main*, identical with that of Locke; but the conclusions which he draws with regard to our most important ideas, as well as with regard to the nature of ideas in general, from his analysis of their origin, diverge as widely as is conceivable from the conclusions of the corresponding analysis in the *Essay Concerning Human Understanding*. Setting out with the theory, that all ideas originate in the experience of each human organism from the commencement of its existence, or at least from the commencement of the consciousness associated with it, he refuses to recognise in any idea a single element which cannot be traced to this origin; and there is no belief exalted to so lofty a height in human reverence, that he fears to direct against it the assaults which logically issue from his theory, nor does he weary in piling argument upon argument if he hopes to succeed in dethroning it from the eminence which he believes it to have usurped. There was much in the character of the man who undertook this Titanic task, which qualified him for carrying it out. The retirement of his early life, and the thoughts with which his early studies constantly occupied his mind, combined probably with the peculiarities of his physical temperament* to create in his very boyhood a wish to "fortify himself with reflections against death, and poverty, and shame, and all the other calamities of life;"† and the result of this may be observed in an inability to appreciate the passionate enthusiasm which has carried many to their noblest deeds, as well as in a distaste, if not an incapacity, for those feverish longings and endeavours which trouble the lives of men who are driven into the struggle of human existence by the tyranny of external circumstances or by the equally resistless tyranny of nervous irritability. With all this there was a native kindliness of disposition, a humility under his own speculative convictions regarding the littleness of human reason and its liability to error, which produced in him such an indifference to varieties of opinion, such an absence of pugnacious dogmatism and even such generosity towards antagonists,‡ as have been reached by few. When such a character was united to an intellect which saw from afar the dim terminations in which all lines of thought inevitably end, which untied with delicate touch the most complicated knots of speculation,

*See the remarkable letter to a physician in Burton's *Life and Correspondence of D. Hume*, Vol. I., pp. 30–38.

† Ibid.

‡See his letter to Reid, with Reid's reply, in Burton's *Life and Correspondence of D. Hume*, Vol. II., pp. 153–6.

which wrought into luminous language the most intractable eccentricities of scepticism, we can understand how the farthest and fullest consequences of the doctrine which traces all ideas to experience were unfolded with a consistency which was deterred by no consideration of human interests, whether esteemed to be petty or lofty alike.

There is of course much in Hume's, as in every creative mind, the origin of which the most elaborate investigation into the circumstances of his life leaves us unable to trace; still it is impossible to avoid recognising the influence of the philosopher who has been mentioned immediately before him and whom we know to have been a power among the thinking young men of Scotland while Hume was still a young man. The evidence, which the *Treatise of Human Nature* contains, of the general "impression that Berkeley's writings left upon Hume," has been noticed by Dugald Stewart;* and we are now to see that the bishop's philosophy furnishes a point of transition to that of the sceptic. The theory of the former, which ascribes real existence to the sensible objects or "ideas" that are immediately presented to the mind, and denies that they represent any unknown and unknowable substance, is adopted likewise by the latter, but whenever they come to define what is implied in *existence*, they diverge into two theories of the universe as hopelessly irreconcilable as could be conceived. For while the bishop maintains that the natural belief in the existence of things, independently of their being perceived by our minds is valid, and explains that as being an existence in the Eternal and Universal Mind who knows all things, one of the most elaborately finished sections† in the whole of the *Treatise* is occupied with an effort to prove that the belief is altogether illusory and to explain the origin of the illusion.

This divergence in the interpretation, which the two speculations severally give to the existence of matter, arose from another difference which reveals more fully the thorough consistency at which Hume unshrinkingly aimed. If *matter* is but a system of "ideas" which have no existence beyond the mind that perceives them, what must follow with regard to *mind?* Is it also "only a system of floating ideas without any substance to support them?" Berkeley was too acute not to see, too honest not to face this question;‡ and his

* *Dissertation*, p. 351 (Hamilton's Edition.)

†Book I, chap. 4, sect 2. Cf, Book I, chap. 2, sect 6.

‡See the third Dialogue in Wright's edition of his works, Vol. I., pp. 203-4. My references are all to this edition.

answer is well worthy of consideration by those who would comprehend his theory. To Hume the same problem presented itself, but met with a very different solution. According to his theory regarding the origin of mental phenomena, these are all, to use his own language, either *impressions* or *ideas*, or, to use language which he might have adopted if he had not been too timid in departing from that of ordinary literature, *presentations* or *representations*. Still further, according to that theory, our representations can never contain any element which has not been first given in a presentation; and therefore any idea or representation which we form of existence must be derived from some impression or presentation. But there is no presentation of existence as an object of knowledge, uniformly accompanying the presentation of those objects to which we attribute existence; and consequently, "the idea of existence is the very same with the idea of what we conceive to be existent. Any idea, therefore, we please to form is the idea of a being, and the idea of a being is any idea we please to form." Accordingly, "we can never conceive any kind of existence, but those perceptions which have appeared within the narrow compass of our own minds."* But our minds themselves? It is evident that "what we call a *mind* is nothing but a heap or collection of different perceptions, united together by certain relations, and supposed, though falsely, to be endowed with a perfect simplicity and identity."† I shall not here anticipate a criticism that will more appropriately arise at a subsequent part of these discussions, when we shall find the similarity between the theory of Hume and the latest form of empiricism in their explanation of all known existence as a series of presentations and representations.

When the *Treatise of Human Nature* appeared in 1739, Thomas Reid, who was a year older than Hume, had been already two years a clergyman of the Scotch church in the parish of New Machar in Aberdeenshire. Descended on the father's side from a family, which for some generations had been distinguished in the literature and in the learned professions, especially in the church, of Scotland; on the mother's side, a nephew of David Gregory, the celebrated Savilian professor of Astronomy at Oxford and personal friend of Sir Isaac Newton, Reid continued to follow his ancestral scientific tastes with

* *Treatise*, Book I., Chap. 2, Sec. 6.

†Ibid, Book I., Chap. 4, Sec. 2.

the modesty, with the reverence for traditional modes of thought and life, which one should expect in the character of a conscientious and benevolent country clergyman. This is not the place to attempt a mediation between the opposite extremes in the estimate of Reid, which have been maintained even in recent times by Hamilton and Cousin on the one hand, by Ferrier and Buckle on the other. In his quiet observation of such phenomena as his range of inquiry brought within his reach, in his unpretending classifications of such as he observed, in his timid groping after inferences which his observations seemed to legitimate, there was no danger of falling into those extravagancies in which the flights of genius are doomed to land, often, like that of Icarus, from the very height to which they rise; but he would probably have accepted, as but a dubious compliment, the ascription to him of those sublime anticipations, which direct the labours of subsequent inquirers till they are established in literal accordance with the rules of scientific induction.*

Dr. Reid, in a well known letter to Dr. Gregory, (20th August, 1790), acknowledges that the discovery of the fundamental and distinctive principle of his philosophy was owing more to Berkeley and Hume than to himself.† From the evidence already adduced of the influence which Berkeley's writings had exerted in Scotland while Reid was still a young man, we are not surprised to learn, as we do from the philosopher himself,‡ that he had at one time adopted the whole of the idealist's theory. According to the same account, it was not till the conclusions of Hume's *Treatise,* "which gave him more uneasiness than the want of a material world," were seen to follow inevitably from the principle on which idealism is built, that he was arrested to question whether that principle is not an unfounded hypothesis.§ The principle referred to is that which Reid supposed to be the universal opinion of philosophers, that "the only objects of thought are *ideas* or *images in the mind*;" and he claims for himself nothing that is strictly his own in philosophy, except his having called this hypothesis in question.* We shall have to consider immediately whether Reid was correct in selecting this as the fundamental peculiarity of his philosophy; but there will be seen to be little

*See *Intellectual Powers*, Essay I., Chap. 3.

†Stewart's *Account of Reid*, p. 22, a (Hamilton's edition of Reid's Works.)

‡Works, p. 283.

§See the above mentioned letter to Dr. Gregory.

room for doubt, that he is mistaken in supposing the doctrine selected to be distinctive of his system even among those of which he intended his own to be a critique, or that, except in one aspect, it is distinguishable from the doctrine of Berkeley, against which he believed it to contain a successful polemic.

To explain, it must be observed that the doctrine referred to may be regarded both as a theory of knowledge and as a theory of existence. As a theory of knowledge, it maintains that the *immediate* objects of perception are not mere "ideas or images in the mind" of objects that exist really or out of the mind, but these really existent objects themselves. The *Three Dialogues* of Berkeley, however, maintain exactly the same theory in the different language enforced by their different point of view. For the idealist denominates the immediate objects of perception by the term current among philosophers; the realist, by the term current among ordinary men, or in the language of *common sense*. But the idealist himself acknowledges the revolt of natural feeling against his theory, arising from the awkwardness enforced by the technical language of philosophers, which obliged him to speak of the immediate objects of *perception* as *ideas*, and not as *things*; * and the statement, that the immediate objects of perception are not the mere images of an unknown existence, but exist really themselves, would undoubtedly have been accepted by both philosophers, as expressing their theory of knowledge in contradistinction from the theories which they opposed.

Though the doctrine of Berkeley and that of Reid, considered as theories of knowledge, may thus be regarded as coincident, as theories of existence they appear, at first thought, to diverge in widely opposite directions; but it is impossible, on second thought, to say how far this apparent divergence would have been found to be real, if the true meaning of Berkeley had been explained to Reid. For I can find no evidence that Reid had ever clearly proposed to himself the question, in answering which his doctrine seems to diverge from that of Berkeley. His polemic against Berkeley consists mainly in an appeal to the natural and necessary belief of mankind, that the objects which we perceive exist really—that they exist beyond the mind which perceives them; but we have already seen that the credibility of that belief is asserted quite as unmistakably by Berkeley—that he only refuses to accept it without a scientific explanation of its mean-

* Berkeley's Works, Vol. I., p. 205.

ing. His explanation, as we have further seen, is that the belief in the real existence of the objects of perception is only the belief that they are really perceived, and that the belief in their existence beyond our minds, is simply the belief that they are perceived by another mind, or by other minds: their existence, therefore, according to him, consists in the perception of them by some mind; and he is consequently content to speak of them as *ideas*, which have no existence but *in* a mind. It is difficult to explain the shock which this language created among Berkeley's antagonists, except by supposing that they understood the preposition *in* as expressing some kind of relation in place; it is more difficult to conceive what mental fact they understood it to denote, and most difficult of all to believe that they had paid any attention to his own explanation, in accordance with which *to exist in a mind* and *to be known by a mind* are convertible phrases * If this explanation had been noticed by Reid, it is scarcely possible to believe that he could have placed himself in the unmitigated antagonism, which he assumed, towards Berkeley; for the faith in a Primordial and Universal Mind involves the admission that nothing exists which is not also known, or, in other words, that everything exists in that Mind. Does the hostility between Berkeley and Reid thus resolve itself wholly into a difference about the meaning of words? There still remains one point at which the two doctrines seem to come into distinct collision; for, while the Scottish philosopher regards the material objects presented to the senses as being the qualities of a substance which is not known by us,† but is, of course, known by the Omniscient, the Irish philosopher protests against the hypothesis of such an unknown substance, as not only unnecessary to explain the phenomena of knowledge, but as contradicting its essential conditions.

I have already hinted the possibility of a doubt whether Reid has hit upon the really fundamental principle of his philosophy, when he elevates to that position his discovery, that the theory of perception by means of ideas is without any ground in fact. I believe the historian of philosophy must decide that such a principle should be recognised in Reid's antagonism, not to the "ideal theory," as he calls it, but to the empirical theory regarding the origin of knowledge. Whatever opinion may be formed of his opposition to the latter theory,

* Works, Vol. I. p. ..

† *Intellectual Powers*, Essay II., chap. 19.

it is that which distinguishes his place in the development of British speculation and gives his philosophy an importance it never could have derived from the principle which he regarded as its distinctive peculiarity. For as the growth of philosophical speculation unfolds into clearer prominence the real meaning of the problems which it has to solve, it will be found that the conclusions of philosophers regarding the principle involved in the "ideal theory" must depend on their conclusions regarding the origin of our knowledge. There is not here space for an explanation and proof of the above statement; but it may be sufficient in the present connection to notice the fact, that in disproving the "ideal theory" Reid himself is obliged to adduce beliefs which he regards as originated by the very constitution of our minds, and as therefore having an origin prior to experience. It is in this connection that the doctrine of Hutcheson, with regard to internal senses, assumes historical importance as having possibly suggested the general name of *common sense* for the source of those beliefs which are common to all mankind and are considered capable of explanation only as original and compulsory issues of intelligence. Moreover the statement I have made regarding the actual fundamental principle of Reid acquires additional confirmation, from the fact that the Scottish philosophy, of which he is regarded as the chief representative, is, when named after its distinctive characteristic, usually designated *the philosophy of common sense.*

While a correct historical estimate of Reid's philosophy thus seems forced to raise into special prominence his assertion, for some of the elements which constitute human knowledge, of an existence independent on experience, it is scarcely possible to avoid surprise at the slender grasp with which he holds this principle and the unskilful manner in which he applies it in his explanation of the mental phenomena. This may indeed be partly accounted for by the fact, already mentioned, that he was ignorant of the prominence due to this doctrine of his system; but it also arose from his never having clearly apprehended any criterion, by which the *a priori* facts in consciousness could be readily recognised. For although Sir William Hamilton gives Reid the credit of having discovered such a criterion of these facts in their *necessity*,* yet not only are Reid's references to this characteristic so incidental as to afford no ground for believing that he recognised it as *the* criterion, but his doctrine of first princi-

* *Reid's Works*, p. 323 *a*, note; and *Lectures on Metaphysics* Vol. II, p. 359.

ples is such as must have led him to deny that necessity is their differentiating attribute. A brief glance at this doctrine may not be useless in enabling us more correctly to interpret the philosophy of Reid.

According to this doctrine,† *first principles* are those which all reasoning in the last appeal implies, inasmuch as the inference of one truth from another cannot have proceeded without a beginning, but must have started from some truth or truths which are not themselves inferred from any prior truth. Such truths, as being prior to all others in human knowledge, are called *first* principles; and since they do not draw their evidence from others, must contain it in themselves. *Self-evidence* is therefore the distinctive characteristic of first principles. There is, however, a difference of opinion among men, as to what truths are self-evident, and accordingly it is necessary to inquire whether there is "no mark or criterion by which first principles that are truly such may be distinguished from those that assume the character without a just title." In answering the question which he thus proposes, we should certainly expect to find what Reid considered to be *the* criterion of first principles; and yet, in the four propositions with their corollaries which form his answer, while there is an enumeration of several tests, some of which are most inapplicable, there is no mention of the criterion which is now recognized. The only passages in which this criterion is explicitly referred to, as far as I can recollect and as far as Sir William Hamilton quotes, are at pp. 455, 459 and 521 in his edition of Reid's works, where, among other evidences, *necessity* is adduced as proving the *non*-empirical character of the two principles, that every beginning of existence must have a cause, and that intelligence in the cause may be inferred from the marks of it in the effect. In these passages undoubtedly Reid sees that a proposition, which we know to be true *necessarily*, and therefore true in all places and at all times, cannot be obtained by an induction, however extensive, of our experiences; but waiving the consideration that he here mis-states a subjective necessity of knowledge as the knowledge of an objective necessity, we must notice, what does not seem to be observed by Hamilton, that Reid's classification of first principles is sufficient to shew that he would have refused to constitute necessity the criterion of them all. For he divides truths into the two classes of *contingent* and *necessary*, while he allocates to each of

† *Intellectual Powers*, Essay VI., Chapters 4–7.

these a separate set of first principles.* Among the first principles of the latter, he enumerates the two which have just been mentioned; and it is not because they are first principles, it is because they are not contingent, but necessary truths, that he regards them as transcending experience.

With this doctrine of first principles, it is not to be wondered that Reid has been so unsuccessful in what ought to have been the most prominent excellence of his system. We have probably in this an explanation of the circumstance, that, although he recognises the importance of an accurate system of the facts which are primal in human knowledge, his detail of them, especially when compared with their exhibition in Kant's *Critique*, appears rather an enumeration at random than even an attempt at systematic classification. It is further remarkable, as possibly traceable to the same source, that, although the analysis of the idea of cause in the *Treatise of Human Nature* led him to the theory of its *a priori* character, he failed to see the conclusion which his own principles should have inferred from the analysis in the same work of the ideas of space and time.

In Reid is included all that is distinctive of Scottish metaphysical philosophy previous to Hamilton. We have indeed contributions of various value from others: in the writings of Dugald Stewart, the whole field traversed in the works of Reid, as well as numerous collateral departments of interest and importance, is illustrated with more elaborate fulness, with the elegance of a wider and more refined æsthetic culture, with a superior command of the English language, and an infinitely superior erudition, if not with a more comprehensive grasp of principles, or any bolder originality in their application; but we have no considerable addition to the substance, no new trait in the character of the philosophy.

We are now better prepared for understanding the exact point at which Sir William Hamilton found the philosophy of his country and the nature of the task which was laid before him. In my next article I shall give an exposition of Sir William's own system; and I shall thereafter proceed to estimate his success in solving the problems which he took in hand.

**Intellectual Powers*, Essay VI., Chapters 5–6.

ON THE VAGARIES OF MEDICINE.

BY C. B. HALL, M.D.

(*Read before the Canadian Institute*).

Mr. President,—In the investigation of any scientific question, our judgment is not to be formed from the number of its advocates, or the individual opinion of its respective supporters, but from the views emanating from the few experimenters and investigators who are acknowledged lights in their particular sphere.

You cannot name the whole range of Animated Nature, without alluding to Buffon and Cuvier, calling on the way upon poor Goldsmith.

In the vast field of Palæontology you recognise an Agassiz, Owen, Buckland, Richardson, and a few others. The Geologist knows Murchison, Ramsay, Lyell and Logan, and remembers with sad reverence the name of Hugh Miller. Numerically, how meagre seem these names to the countless thousands who, in every part of the world, are prosecuting with unyielding ardour these delightful and most useful studies, gathering, as it were, particles of matter from every clime, ascending with a Humboldt to the mountain peak, or diving with a Wallich to the bottom of the sea; but like the streams that pour their ceaseless torrents, never get their full nourishment and strength until they mingle with the ocean's depths. Thus has it ever been with the Science of Medicine—from its earliest record, there have been, through each succeeding era, certain gifted spirits who have ruled its destinies; culling from every busy theorist such parts as bore the test of experience, and rejecting all others—moulding and fashioning, in their proper places, such as fitly joined, and thus keeping together a series of connected truths from the days of Hippocrates to our own.

The early history shows nothing remarkable, other than the ordinary pursuits of learning. We read of each distinguished physician having his class of pupils, and we are told that in two of the schools founded by rival pupils of Pythagoras, human dissection was practised, and whether true or not, leaving the impression that it was upon the living subject. But the first account of any great numbers was in the College of Bagdad, when about the eighth and ninth century there were generally a thousand regular attendants. From this date, as did all learning, the practice of medicine fell into the hands of Monks, who did not add much to its advancement, but retained their hold upon the public mind to a comparatively modern date. In the latter part of the 15th century, when we find the first notice of the most intricate disease as well as the most revolting to humanity—so close was the alliance with these Reverend Gentlemen, (either as prescribers or patients) that the writers of the day gave it the name of Rheume Ecclesiastique. Not to trouble you with the names of such impostors as Valentine Greateacre, who astonished the London world about the middle of the 16th century with his wonderful cures, but which, in more modern times, was called Mesmerism, I will only allude to a few of the vagaries with which science-proper has had to contend. My object being to call your attention to the fact that the Science of Medicine has descended in an unbroken chain, from the earliest ages to our own, separate from the absurdities foisted upon it, receiving in each era additional links or more firm welding, and were I able, like a modern Plutarch, to parallel the other sciences, would be constrained to show the picture much to the advantage of medicine.

As now, so of yore, men sought for specifics, some single principle upon which all cure was to rest—the Alchymist in his universal solvent, 'till a more wise made the grand discovery—that if successful they would have nothing to keep it in. The first great principle was Vitality, its source, power, and influence, but as this was associated as much with Philosophy as Medicine, I need give it only a passing notice.

Phlogiston or Caloric, the principle of heat, was the life-giving power at one time, and many and curious too, were the expedients proposed to impart and regulate its influence as a sole curative. Oxygen, in the last century, claimed a higher state, no less than the source of the former and its vitality.

In like manner Magnetism yielded its place to modern Electricity, which, with all its boasted power of giving life to inanimate objects, must, with its twin sister Spiritualism, yield to the ever-existing truth, that life is solely the gift of the Creator, and goes back to its giver when the created resolves into its elements.

Long and tiresome were the disputes on the classification of disease, which in time were reduced to the synoptic and systematic—the first being dogmatic, and biased by the peculiar mind of each writer, at last gave way to the latter, which, arranged, and re-arranged, according to discoveries of different periods, is now acknowledged by the profession proper.

Brown, a man of great learning, sustained for several years the Brunonian System, wherein medicines were to act according to their degrees of stimulating or exciting. To this followed the Italian system of stimulants and their opposite—depletants or stimulants and counter stimulants.

Brosseau reduced all diseases to inflammation of the stomach and bowels, or Gastro-enterite, and adopted to a great extent this theory of treatment.

Dickson proved, to his own satisfaction, that all changes are periodic, or as Shakespeare makes his victims of the Pontine Marshes say,—"they're all alike the ague."

And Muller, to this day, denies any variation from the normal, to other than chemical causes. A few wonderful means of cure may not be without interest. Cholera received the name of St. Vitus' Dance from the habit of its victims resorting to the Chapel of St. Vitus, in Germany, and dancing away the complaint—it was necessary to keep up the dance 'till the disease gave way or the patient fell from exhaustion. *One woman danced for a month!* "And frequently it was required to hire musicians to play in rotation, as well as various strong sturdy companions to dance with the patients till they could stir neither hand nor foot." So efficacious was flagellation for certain ailments, that it has been suggested as the origin of the physicians cane. We read that "this process was employed to cure Octavius Augustus of Sciatica." Another believes "it has the same effect as Colocynth administered internally." Galen recommended it as a general restorative; others for nervous irritability. One of Queen Elizabeth's physicians found great success in herbs; he says, "with daisy-tea I did recover one Belliser, not only from a spice of Palsie, but also from a quartan ague"—and to show "man's inhumanity to man," he adds, "and afterwards this same Belliser, more unnatural than a viper, sought divers ways to have murdered me, taking part against me with my mortal enemies, accompanied with bloody ruffians for that bloody purpose."

Success did not always attend merit. Sir I. Brown, one of the first physicians who received the honor of knighthood says, "when he commenced his career, he had twenty remedies for one disease, but at the close he had twenty diseases for one remedy."

Scarpa, a distinguished surgeon, says he destroyed a hat full of eyes before he was successful in the operation for cataract; and Dr. Lettsom was satirized for some of his remarks in the *Gentleman's Magazine,* by setting his name to doggrel verse:

"When patients comes to I,
I physics, bleeds, and sweats 'em,
Then, if they choose to die,
What's that to I? (I Letts 'em.")

Though a few men of real worth have been subject to uncalled-for strictures, the profession may be safely said to have held its high position throughout the most of its history. Lady Mary Montagu said-"air, exercise, and company, are the best medicines, and physic and retirement good for nothing but to break hearts and destroy constitutions." And you all remember Macbeth's contemptuous remark to his physician:—

"Can'st thou not minister to a mind diseased;
Pluck from the memory a rooted sorrow;
Raze out the written troubles of the brain,
And with some sweet oblivious antidote,
Cleanse the stuff'd bosom of that perilous stuff
Which weighs upon the heart?
Then—Throw physic to the dogs, I'll none of it."

There have been four great divisions, called the four pathæ, which have been charged upon the profession, but to which they plead not guilty, though each has been acknowledged as having great merits in the cure of different complaints. The first is called the Antipathia, and consists in employing medicines of an opposite effect to the tendency of the disease, having for its motto—"contraria contraribus opponenda," and in all cases giving purgatives for constipation, astringents for looseness, and opium for pain. Though this principle acts correctly in many cases, the profession reject it as unsound, because there are instances in which the opposite would be indicated.

The next is called Allopathia, having for its principle the creating another disease, counter to the one treated—their motto "Ubi irritatio ibi fluxus;" consequently their remedies were called counter-irritants or blisters &c., or such as excited the action of an organ functionally opposed to the diseased, as irritating the stomach to cure inflamation of the throat. By this theory is often produced the curative effect of mercury, the discharge from the salivary glands serving to carry off the over-wrought action of the liver. An interesting case is recorded of an old-standing jaundice being cured in a few days by a suddenly produced ptyalism, the discharge from the salivary glands being of a yellowish brown color, resembling bile, and of a bitter taste. Numerous as are the favorable results of this practice, the profession deny the theory, because it is found efficacious in certain cases, in others it is no good. As the discharge from a blistered surface will not diminish the discharge from an inflamed eye, or remove the excessive secretions in dropsy, the principle is recognized as an adjunct, but rejected as a theory.

The third, Homœopathia has had its rage and struggled hard for supremacy, but its benefits are found too circumscribed for general principles. In this medicines are selected as curatives which would produce similar effects in the normal state, hence "Similia similibus curantur." Like the two preceeding, this theory was known to the ancients, and by some carried to great length. Aristotle prescribed "a hair of the dog that bit you" as a preventive of madness, a now established maxim, and followed faithfully as an antidote for a quite different species of hydrophobia. The two most interesting points in this are the principle of infinitesimal doses recently propounded by Hahneman in Germany, and their being carried along the nervous filaments into the substance of the brain, as stated by Jahr of Paris, who appears to the former what Spurtzeim was to Gall, the philosophiser of his dogma. He asserts positively that in this way melancholy, grief—a cacoethes scribendi,—particularly for poetry, and the more fatal malady of love, have each their globule and each globule is a charm. There have been some cases of lesion of the brain causing a marked change in the person's mind without any subsequent malady. Bœrhaave describes a poet of his time, who, after recovery from an injury of the kind, lost the art and denied his former offspring.

The Reports of one of the London hospitals mention a Welshman twenty-five years of age, who had lived in England for twenty years and spoken their language. After a protracted disease of the brain he could only speak Welsh. I had, myself, the case of a boy five years old, who lost two ounces of the substance of the brain from the kick of a horse—previous to the injury he was unable to speak correctly, and had nicknames for his brothers and sisters. After three weeks quietude he recovered, and to the surprise of his parents spoke distinctly, calling each person by their proper name. On the other hand it is said these results may have happened without any injury to the brain, but were simply suspended memory from rest. The dependence upon a too minute attenuation is discharged by that modern Paul

Pry of nature, the microscope, which detects when the division of particles of gold and sulphur have been carried to their utmost limit, showing particles of mineral in some of the globules and *none in others.*

Like the others it is very good at times, but never can be called a science. It may relieve headache in the morning by taking (as compared with former potations) an infinitesimal dose of the cause of disease, but it can never cure *delirium tremens!* And, besides, I quote from Vol. 15 of the "Medical Gazette," "Homœopathia has been fairly put to the test of experiment by some of the members of the Academy of Medicine in Paris, and the result was a failure. Andral tried it on 130 or 140 patients, in the presence of the homœopathists themselves, adopting every requisite care and precaution; yet, in not one instance was he successful." However, one credit we must award it, and that is—the harmless sugar globules have been the means of stopping the excessive use of those patent medicines that flooded the country, many of them of a most dangerous kind, and I fancy it is generally a transition of patient from one to the other—with them it is "Coelum, non animum, mutant."

The fourth, Hydropathia, I need not remind you is of ancient date, though recently brought forward as a sovereign balm, or the absurdity of its being the only one thing needful, but the incalculable amount of good it has effected in the prevention of disease cannot be named, and if, as we are told, cleanliness be godliness, its advantages have been moral as well as physical. Celsus describes its use in hydrophobia to allay its spasms. "In this hopeless state," he says, "the only remedy is to throw the patient instantly and without warning, into a fish-pond, plunging him under the water that he may drink, then raising his head and forcing him under it and keeping him below till he is filled with water, so that the thirst and water-dread may be relinquished at once." And Von Helmont, at a later date, kept the patient under water till the choir could sing the psalm *Miserere.*

It is here worthy of remark how singularly void of information on all that pertains to medicine are men of learning, men erudite in every other branch, yet seeming incapable of forming a correct opinion on that subject in which their lives are most interested.

Lord Bacon says it is accounted an error to commit a natural body to empiric physicians, which commonly have a few pleasing receipts, whereupon they are confident and adventurous, but know neither the causes of the disease, nor the complexion of patients, nor the peril of accidents, nor the true method of cure.

Still one chooses for his physician an Allopath, another a Homœopath, a third an Eclectic, without, for an instant, giving a thought as to whether the individual man thus chosen be competent to manage the intricacies of disease, or blindly ignorant of the first-most rudimentary principles of any skill—a simple retailer of other men's ideas. Not many years since, a man by the name of John Long, a prototype of a Dr. Tumblety, in this country, commenced the sale of a wonderful specific in the City of London, with plenty of effrontery and a pleasing address, he soon found his sales increasing, and, fired with ambition at such success, issued a card with the name of St. John Long, and "in one year's operations his pass-book at his bankers contained credit to the amount of £13,400." Little more than a century back the British House of Commons passed "an act for the providing a reward to Joanna Stephens, upon proper discovery to be made by her, for the use of the public, of the medicines prepared by her," granting her the enormous sum of £5,000 sterling. A committee of twenty was appointed by the Government for the examination, who reported that she had made the discovery to their satisfaction, and that "we have examined the medicines, and her method of preparing the same, and are convinced, by experiment of the utility, efficacy, and dissolving power thereof."

These medicines, in the words of the lady, "are a powder, a decoction and pills; the powder consists of eggshells and snails, both calcined. The decoction is made by boiling some herbs (together with a ball, which consists of soap, swine-cresses, burnt to blackness, and honey) in water. The pills consist of snails calcined, wild carrot seeds, burdock seeds, ashen keys, hips and haws, all burned to blackness, soap and honey. Preposterous as was this recipe, it was purchased in that enlightened age which a distinguished chronicler said, "produced more men of letters as well as more men of science, than any epoch of similar extent in the literary history of England.

I now give one quotation from Dr. Mason, good in confirmation of my position: "Whilst a few species of diseases are now no longer to be found, which are described by earlier writers, a few seem to have supplied their place which are of modern origin; yet, upon the whole, the march of nature is but little interfered with, and hence the prognostics and aphorisms of Hippocrates, the medical histories of Aretæus and Galen, of Rhages and Avicenna, are transcripts of animal life in our own day. The extensive families of fevers and spasmodic affection are, in the main, the same now as they are represented to us by the most ancient writings that have descended to us,—with, however, this improvement, that cases requiring *then* from three to six months for a cure, *now* take only as many weeks, and three to six weeks *now* get their quietus in as many days. The great lesson which experience has taught has been physiological, and consequently the course of treatment has become more positive and definite, hence the more favorable result. From Hippocrates we have the first link supplied by Galen to the end of the Greek Schools—Celsus then furnishes the Italian. The Schools of Bagdad by Serapion, and through the Monks till about the time of Sydenham, all of whose works are handed down to us and translated into our language, and the whole of the schools represented by these writers were united upon what is called the Humoral Pathology of disease—the same as is taught by the schools of medicine at the present day.

Mr. Jeafferson, in his book De Doctoribus, says the lives of three physicians, Sydenham, Sir Hans Sloane, and Heberden, completely bridge over the uncertain period between old empiricism and modern science. Sydenham was born in 1624, and received the most important part of his education in the University of Oxford. Sir Hans Sloane continued till 1753—his museum was purchased by the British Government and became the nucleus of the present British Museum. Heberden extended the time to the beginning of the present century and was the instigator of the transactions of the College of Physicians, to the first volume of which he was the chief contributor.

I will only refer to one or two instances to show the scientific application of medicines for the relief of disease: First,—Affections of the liver or bilious complaints, as they are called, and peculiar to this country.

A secretion of this organ called hepatine is readily changed into sugar by the disturbance of particular nervous functions, or interruptions of the circulation of the blood through the vena-porta. Now this occurs in all cases of biliary calculi or obstructions of the biliary ducts, rendering the most trifling derangement of the liver a cause of this nonazotic, unfibrous and most abnormal secretion. Now, not one of the theories named, could, as such, directly have the slightest effect in removing or changing this sugar from the blood, yet to the scientific man I have only to name casein or rennet of milk as one substance that would convert it into lactic acid, the natural gastric juice of the system.

The other is Scrofula, or generally known as incipient consumption. All organized bodies are composed of carbon, hydrogen, nitrogen, and oxygen. This disease is produced by excess of hydrogen in the proportion to form water with oxygen, and a deficiency of nitrogen to form fibrin or proper muscular contractility. Again, not to enumerate the particular remedies, I may say that all such as contain nitrogen in excess and are deficient in hydrogen, of these oxalic and tartaric acid, the cyanides, and ammoniacal salts, are familiar examples. Thus, Sir, the great point of separation between the profession and empiricism is on the subject of *education*,—the former holding that whatever theory is followed, the practitioner must understand anatomy, physiology, pathology, chemistry, and the nature and effects of medicines, the action of mental influence upon disease, sufficient knowledge of collateral sciences to judge so far as they bear upon their patients, atmospheric influence, chemical tests, and the use of the microscope; and, in addition, their minds expanded by study, enabling them to comprehend and unravel the intricacies of a complex and difficult complaint, with sufficient moral acquirement to ensure patience, long suffering, forbearance, gentleness, kindness, and unflinching firmness; and a trusting confidence that He who holds the sparrow in its fall, will guide their counsels and direct their skill.

REVIEWS.

A new arrangement of Phanerogamous plants, with especial reference to relative position, including their relations with the Cryptogamous. By Benjamin Clarke, F.L.S., M.R.C.S. London, sold by the author, 2 Mt. Vernon, Hampstead, 1866.

This work, expected for some time, is at length before us; and, though we cannot pretend now to offer such a critical examination as it deserves, we are desirous of introducing it to the notice of those interested in its subject, with such remarks as occur to us on a first perusal. And first, it is quite certain we think that there is room for such a work; and that, if it makes any considerable approach towards accomplishing its object, it will confer a great boon on the lovers of botanical science. Nay, even if its principles should prove unsound or insufficient, and its results should not satisfy the mind, it is still true that the laborious endeavour to promote knowledge deserves grateful acceptance, and that many years cannot have been devoted to such inquiries by an intelligent and patient investigator, so situated as to have access to many rare specimens, without important facts being brought to light, and hints afforded which will assist others in the pursuit of the same object. It must be admitted that the proper limits and true relations of the families of plants are not yet understood; and, if there are real relations at all in nature, which we for our part cannot doubt, their discovery must be an object of the highest interest and importance. We have no sympathy with those who make light of system in Natural Science. It is justly remarked by the present distinguished President of the Linnæan Society, in his annual address, in reference to the sort of contempt with which some seem now to regard system in Zoology and Botany: "This is surely a mistake. Without a good system, clearly identifying the subjects of observation, no biological inquiries can have any practical advantage; and, in all our reviews of the progress of our science, we ought equally to appreciate the labours of the systematist, the physiologist, and the biologist, provided that each in his own department has duly called in aid the results obtained in the others." We should be disposed even to go beyond the learned President, not valuing a good system only or chiefly for its enabling us clearly to identify the subjects of observation, but accounting the relations which it brings to our knowledge as among the most valuable results of our studies; and believing that, as nature can only be usefully examined through the medium of a system, the best system will give us the truest and most practically valuable acquaintance with its wonders and beauties.

Mr. Clarke, in the work before us, deserves the praise of endeavouring to improve system by means of biological knowledge, and that not only what he could collect from others, but what he obtained by patient and varied observation.

The vegetable kingdom is so beautiful, and, for many reasons, so interesting to man, that many persons desire to acquire some knowledge of it. There is no general insensibility to the value of the trees of the forest, the grain, fruits, and vegetables of the field and garden, or to the beauty of the wild flowers of our fields and woods, or those with which culture adorns the parterre or the green-house. Many feel the desire occasionally to make them the subject of scientific study, but they are met by difficulties which discourage and repel all but the most persevering; and which oblige even these to be satisfied with becoming acquainted with a certain number of objects without forming any clear idea of an order of nature among the subjects of their study, or of the well-distinguished groups which make up the whole. Since a professedly natural method has been generally adopted, the genera have undoubtedly been collected into those higher families which among plants are, by an improper application of that term (inconsistent, at least, with its use in other parts of Natural Science), called *Orders;* but, although much is thus gained, these families have not always characters easily recognised by the student; and higher combinations, presenting distinctly to the mind larger associations, are absolutely essential to our enjoying the advantages of good classification. So far as concerns the highest divisions of all, we cannot but think that Jussieu's Acotyledoneæ, Monocotyledoneæ, and Dicotyledoneæ, exhaust the subject, and are so well supported by every part of vegetable structure, that we may rest on them with full satisfaction; but it is, at the same time, clear to us that they are not *classes* in the sense in which it is convenient to use that term in Natural Science, but what in Zoology are called sub-kingdoms or branches, and that the other proposed classes, if good at all, are such sections of these sub-kingdoms as ought to be called classes; but the difficulty is that, between the sub-kingdoms and the great families which it is customary to call *orders,* we need and have not yet obtained such good divisions, conformable to nature and separated by well-marked characters, as might with propriety take this name. The Gymnospermous Exogens may be a good class of Dicotyledoneæ (for the structure is Dicotyledonous, though the cotyledons are more than two); and it is possible, though less clearly made out, that Lindley's Dictyogens may be a good class of Monocotyledoneæ (of its members being within that sub-kingdom there can be no doubt); but there remains in each sub-kingdom a great assemblage of families which must afford means for establishing good divisions of equal importance with these, though such divisions have not yet been detected. In what he calls alliances, Dr. Lindley made an admirable attempt, making the best use of what had been done by others, and exerting to the utmost his own great powers to unite the so-called orders into larger groups, which should be at once natural, because resting on important real characters, and capable of definition. Different opinions will be entertained as to the measure of success attained by him. Of the value of the plan, we think there ought to be but one opinion, and we see with pleasure that, though differently worked, it is adopted by Mr. Clarke. Many of Lindley's alliances—indeed, a very large proportion of them—seem to us good. In a few instances he appears to us to give undue importance to an unsupported technical character; in others we fancy we see artificial separations of what a right estimate of nature would combine; and there are, doubtless, instances of obscure orders placed by a not very happy guess; but, on the whole, the work deserves the highest admiration; and if such alliances, carefully reviewed, were combined into classes, we might, at length, boast of possessing a Natural System.

For assistance, in improving our views, we have to look to such labourers as Mr. Clarke, and we receive his attempt with welcome, and with respectful, and we hope, candid attention. We only wish we had better means in our power, in our remote position, for testing the accuracy and real value of some of the points which he especially relies upon. We will, however, offer a few remarks which occur to us, whilst bearing our humble testimony to the claim of the author, to have his plans examined with care by those whose thoughts are turned to the interesting subject on which he has bestowed so much industry and ingenuity.

We cannot help wishing that Mr. Clarke had not expressed himself in a manner implying the truth of the Darwinian* hypothesis respecting the origin of species, and seeming to make relationship always dependent on a common descent. This hypothesis will, no doubt, for some time be very much regarded by scientific inquirers in all their investigations; but, however strongly

* We refer, here, specially to the transmutation and gradual formation of species, by descent from others. Mr. Clarke may not receive Darwin's special views as to the mode in which species arise, but he appears to hold the doctrine of their gradual growth, one out of another; which, indeed, is by no means peculiar to Darwin and did not originate with him, but is the foundation of his system, and will be resisted by all those who view Nature as a perfect plan, proceeding from Divine intelligence, which it is the object of our efforts to understand and interpret.

it may attract some minds, it is not too much to say that it is not yet satisfactorily proved, and that it labours under some great difficulties and serious objections. A really good classification, independently formed, might greatly assist our judgment on the question, but the assumption of the hypothesis, and the attempt to trace the order of descent of the multiplied forms occurring in nature, can only encourage fanciful analogies, and interfere with our application to the great questions, What are the parts? and, What are the circumstances respecting them? which, in themselves, and from their connection with general structure, have most real importance in determining affinities and leading us to a natural grouping of objects. Mr. Clarke's arrangement is certainly not rendered more plausible by the fancy that Endogens (Monocotyledoneæ) are derived through Riccia and Lemna from Marchantiaceæ; Epigynose Dicotyledoneæ through Balanophoraceæ from Bryaceæ; the Chloranthal division through Gnetaceæ and Lycopodiaceæ from the same source, and so in other instances. We cannot but be struck with the insufficiency of the analogies by which, in one or two cases, it is attempted to justify these speculations. Take for example the relation of Endogens to Marchantiaceæ. The author expresses himself thus:—"I am aware that there is no evidence to show that any near affinity exists between *Lemnaceæ* and *Ricciaceæ*, further than that the habit of some species of *Riccia* is so much like that of *Lemna*, that they are stated to have been mistaken for species of that genus by some authors, while species of the latter genus have in fact been described as belonging to the former; and although habit is often of little value as a character among phanerogamous plants, it is acknowledged to be of more importance among the cryptogamous. But as the present arrangement includes the placing of all the cryptogamous families in relation with the phanerogamous, the Endogens should in all probability be compared to one, at least, of their lowest forms; and as the Ricciaceæ have no affinity with the apetalous forms of Exogens, a negative evidence is afforded of their being the cryptogamous form of the *Lemnaceæ*." What this amounts to is, that granting the truth of the system, and that each race of higher plants is derived from, or immediately related to, some form of the lower, there seems some probability that Ricciaceæ afford the form nearest to Lemnaceæ, there being cases in which the foliage, when the flowers or fruit cannot be found, may be mistaken one for the other. The system being by no means proved, we might leave any reader to judge of the importance to be attached to the analogy; but we may ask whether the resemblance between the smaller water lilies and Brasenia to Hydrocharis and Limnobium in one direction, and to Menyanthes and Limnanthemum in another, is not quite as striking, though it suggests no idea of near relationship. Certainly when the flower and fruit are not procurable, we might well be in doubt of which of these genera we had obtained the foliage. We cannot but strongly censure the practice of some botanists, amongst whom is Mr. Clarke, of speaking of cryptogamous and phanerogamous as two great divisions of the vegetable kingdom, thus disguising the fact that the latter includes two divisions, as well separated from each other, and by as important characters, as either is from cryptogamous; just as in zoology, we know of nothing more misleading than the use of the division into vertebrata and invertebrata, when each great branch of the latter is as distinct from the others as any of them is from vertebrata. Curiously enough it is among Cryptogamous—flowerless or spore-bearing plants, whose growth commences from a primordial cell, without an embryo being formed and preserved in a seed—that we have made the farthest advances towards a true and good classification, the full recognition of which would have been no disadvantage to Mr. Clarke in making his comparisons. The sub-kingdom naturally falls asunder into three good classes, each of which has the same number of alliances, under which all the orders or families are readliy arranged. We have first *Thallogens*, or Thallophytes, with no proper distinction of stem and leaves; with no chlorophyll, no stomata, and the lowest reproductive type, though always, we believe, two cells intermingle their contents to form a spore capable of germination. Here are ranged Fungales, Lichenales, and Algales. Secondly, *Anogens*, with stomata and the green colour of vegetation depending on the presence of chlorophyll, but with no vascular system, and reproduction in a prothallus which is temporary, the plant producing successive ones periodically. Here rae found Charales, Hepaticales, and Muscales. Thirdly, *Acrogens*, with an imperfect vascular system, and reproduction from a prothallus in which the fertilized archegonium developes the spore-producing plant, of which the prothallus usually resembles an initial condition. Here are placed Equisetales, Lycopodales and Filicales. We doubt much whether this exposition of the Acotyledonous or Sporigenous sub-kingdom, on the correctness of which we rely with great confidence, and which affords the best indications we have of what we may expect to find in the higher sub-kingdoms, is quite consistent with Mr. Clarke's attempt to connect the muscal alliance with Epigynose Dicotyledoneæ. Granting that he is right, that Epigynose plants are lower in structure than Hypogynose, from which Perigynose cannot be properly separated, we have still to consider the proper position of Gymnospermæ, and many powerful arguments may be adduced in justification of their usual position below other Dicotyledoneæ, but even passing by this point, what is the true value of Mr. Clarke's analogies? Grant that some Bryaceæ are parasitical, though more are only epiphytical, but parasitism is a mode of nutrition found in various parts of the vegetable system far removed from each other. The involucre of *Jungermanniaceæ is compared with those occurring in Chamælauciaceæ*, Calyceraceæ, and Dipsaceæ. The analogy is surely but a slight one, and other cases may prove it unimportant. In Quercus the female fruit is surrounded by numerous scales, which are leafy organs. The fruit of palms of the section Calameæ is enveloped in such scales, completely combined into one covering, and in both instances the single seed has overpowered the rudiments of other carpels with their *germs; yet there is no relation* between Quercus and the Calameæ; and though leafy organs may give origin to the teeth on the urn-shaped capsule or spore-case of Bryaceæ, their structure is so far removed from that of Epigynose Dicotyledoneæ, and adherence of parts under pressure, is so widely diffused a phenomenon of vegetable growth that no inference can be drawn from the resemblance, such as it is. Assuming that the diclinous and apetalous characters are but of secondary importance, and that there is no clear or useful line between *Hypogynosæ and Perigynosæ, then if this kind of character, founded* on the closeness or separation of the circles of the flower, be really available for leading distinctions, we ought in the sub-kingdom Dicotyledoneæ to place Gymnospermæ as the lowest class, Epigynosæ next, and Hypgynosæ as the highest; but ought we not rather to derive our classes from particulars relating to the embryo? and are we yet prepared to say that this is impossible?

We are far from being satisfied with Mr. Clarke's view of the analogies between the vegetable and animal kingdoms. It is no doubt true that the total absence from plants of the sensory and motive organs limits our opportunities for noting differences amongst them: and with respect to nutrition, whilst animals, living on organized substances, have great variety in the means for securing what they need, plants absorbing their inorganic nutriment on a plan which is nearly uniform in them all deprive us of another series of valuable characters upon which in the animal kingdom we rely much. Still the nutritive system of plants displays very important varieties, some of which deserve more minute study than they have yet received, and their reproductive system whilst essentially corresponding with that of the animal kingdom is so wonderfully varied as of itself alone to be almost sufficient for a good system, although we can by no means admit the wisdom of refusing the aid of organs connected with nutrition where we find them affording clear and constant characters. Mr. Clarke finds an analogy between Actiniadæ and phanerogamous plants, which if it is any thing beyond the superficial resemblances so often observable in nature must be very vague and general. He thinks indeed that a leaf is a true branch and analogous to the limb of an animal, but assuredly there is no analogy between the branch or limb of a plant and the limb of an animal, the former is the growth of a bud attached to, but in every respect resembling the parent stem and having, like it, leaves specially organized for their peculiar function. In truth if we would see any relation between an animal's limb and a leaf it must be, though then but slight, by carefully distinguishing the leaf from the branch: the inference from the monstrosity of a cabbage leaf is very far fetched and unsatisfactory. Actiniadæ belong to a great series of animals made up of a definite number of merosomes each supplied with all the vital organs as if so many separate simple animals had been compressed into complete union round a common axis thus becoming a single individual, their flower like aspect results from this arrangement. A flower is the reproductive system of a plant, formed by a modification of its leafy organs, which by the suppression of the axis are in that case brought into successive circles. Is there any more than a superficial resemblance, unconnected with structural analogies, between the two?

But we have not yet touched upon the really important part of Mr. Clarke's work. We cannot but feel that he has himself given very insufficient explanation of his meaning and of the way in which he arrives at his conclusions in respect to the Procarpous and Heterocarpous characters, and we need also to be better informed as to the theoretical grounds on which he rests the importance of these characters. They may be very important and they form a fit subject for investigation. Thus far we chiefly judge of them by their results which by no means always agree with our feeling of what is natural, which however is only the general effect of previous judgments and impressions occurring without reference to this view of the subject, and is only worth anything so far as our own previous judgments rested on clear and solid characters which it would not be easy to overbalance by other views of the matter. It seems reasonable in such a case that we should await further knowledge and study before we presume to pronounce any judgment, but we think it would have been better if the author had explained and defended his ideas more fully. We should like also a fuller explanation on the subject of the position of the raphe, we mean as to the theoretical grounds for the special importance attached to it, and the proof that can be given of its intimate connection with natural grouping. The elaborate tables given by the author form a most interesting study to any one desiring to understand the affinities of plants. Being the chief part of the work they have been allowed to give the book a peculiarly awkward shape making it a real difficulty to read it. It is however worth reading and studying and we hope it will meet with attention from those who are in a position to form the best estimate of it.

In reference to the table of Monocotyledoneæ or Endogens, we may state that the leading division into Exorhizal and Endorhizal appeared to us *a priori* a good one, since it was once supposed that all Monocotyledoneæ were endorhizal, and when it was observed that many of them were exorhizal, it would naturally be concluded that these latter approached the Dicotyledoneæ, and were more highly developed than the others, and this idea would correspond well with the facts, but for the decision at which our author has ultimately arrived respecting Palmaceæ, which he places among the exorhizals, whilst we feel irresistibly compelled to give it a position near the grasses. Can he be wrong on this point, respecting which he seems to have hesitated long? If not, such an anomaly of structure throws doubt on the value of the character. There are few characters more important than *Venation*, when it affords definite results. Now all palms, whether pinnately or palmately veined, have straight veins, simple with the least possible cross connection. Arals, on the contrary, to which Mr. Clark believes the palms allied, have a complex venation, showing an approach towards the boundary of Monocotyledoneæ, only their lowest forms approximating to simplicity and parrallelism of venation. Comparing palms with grasses, the largest grasses approach to their aspect.

the bractes among the flowers form a striking point of resemblance corresponding with the glumes of Graminaceæ; in Cornucopiæ we have even a grass in which the lowest bracte, almost envelopes the spike of flowers as in palms. Palms have only two fertile carpels, the third being aborted and not a few have but one seed as in grasses where the two styles and stigmas are conspicuous. Altogether we know of no suggested relation of palms which can be compared in probability in every possible view with that to grasses, and we feel sure that botanists will not be persuaded to abandon it. Otherwise, we should accept at once as the classes of Monocotyledoneæ, Endorhizeæ, Exorhizeæ and Dictyogenæ, for we still hold to Lindley's class which in Mr. Clarke's arrangement may be considered as occupying the highest place though only as an alliance of the Exorhizeæ.

In his preliminary remarks in the section on the "value of other floral characters," &c., besides the Epigynous, and Procarpous and Heterocarpous, we meet with the following paragraph relating to Irregularity:—"The occurrence of irregular flowers, where they are irregular in the highest degree, especially if the ovary is reduced to one carpel, and that anterior, and the raphe where the ovule is pendulous is next the placenta, is beyond doubt also a character indicating a comparatively higher degree of development, even though it may not extend through the whole of the family, and it may be a question if there is any material exception to this peculiarity of structure as a guide to affinity, as far as regards subdivisions. On these principles the *Proteaceæ* and *Leguminosæ* and their allies, in which the Apocarpous ovary occurs in conjunction with the irregular flower * * * * will decidedly take their places as the highest developed forms of plants."

We confess that we cannot see the force of this reasoning. Irregularity, as is generally agreed, and is certainly proved by examples of return to regularity from increased nutriment, especially in terminal flowers, is due to unequal distribution of nutriment—that is to say, less development of some organs than of others in the same circle—and how it should mark general increased development we cannot conceive.

There are perhaps few alliances without one or more irregular orders, but are these deemed superior in any sense to the regular ones? Lindley assigns to the Daphnal alliance, in which he places Proteaceæ (which, however, is insulated by Mr. Clarke) a solitary carpel, but Thymelaceæ have certainly two or more carpels, though only one seed; Lauraceæ, as Endlicher perceived, have three; Cassythaceæ, two or three; and the fact of their being so pressed together as to form or perfect only one seed, marks a lower type. The solitary carpel of Proteaceæ is due to great irregularity with close pressure; and even if this order is assumed to have no connection with Daphnales, the single envelope, and the close adherence upon it of the stamens, are characters opposed to high development. Granting that the Perigynose character is not separable from the Hypogynose for any useful purpose, it still indicates partial union of some of the floral circles from near position and some amount of pressure. We presume then, that if the Hypogynose structure is higher than the Epigynose, the highest forms are those which are most completely hypogynose, having all the organs and circles distinct, and that all degrees of union place the plants displaying them somewhat lower, a principle which certainly places Leguminosæ below Ranunculaceæ. Besides, must we not always place full development of all parts rudimentally existing above the partial abortion of certain circles, and will not this compel us to set Spiraea above Leguminosæ, and generally the regular above the nearly related irregular?

We should feel detailed criticism of the tables to be for us at least premature at present, but there are some of the combinations which we have much difficulty in conceiving to be true, and the evidence does not yet come before us with any appreciable force. We want, as a preliminary step, an attempt to estimate the real meaning or relation to vegetable structure of every character that has been much used by any eminent systematic botanist since the triumph of the natural method, with an examination of its comparative value, as drawn both from reasoning on the importance of organs and modes of considering them, and from experience of the results of their use. We should thus know whether the value assigned to a character rests on good scientific principles; and though many points might for a time remain unsettled or liable to question, we could not but feel our foundations to be more solid, and our building more symmetrical and more promising of satisfaction.

In the meantime, nothing can contribute more to clearness of ideas than simple, intelligible, and strictly accurate terminology. It is, we think, quite time that the expressions, *Monopetalous* and *polypetalous*, should give place to better terms. We never liked, as a matter of taste, and from a preference for simplicity in conveying an idea, *gamopetalous* and *dialypetalous*, though they are greatl, to be preferred to the old terms; but why not use *Sympetalous* and *apopetalous*, with the corresponding terms for the several circles of the flower, observing, however, that *Syncarpous*, which is often employed, is only truly applicable to the case of the union of whole fruits, as in our pretty American twin-berry (Mitchella repens). To express union or separation of the portions of the fruit of one flower, we must employ *Syncarpellous*, *apocarpellous*, and let us be rid, except in Linnæan phraseology, of the term pistil, which is so specially liable to misconception and abuse. Again, the plan strongly insisted upon by Lindley, of naming all Orders from a type genus by an adjective ending in *aceæ*, has such obvious and decided advantages, that we are almost disposed to be angry with those botanists who will still obstinately use the other terminations, given accidentally, or from regard to sound only, before this improvement was thought of, and will cling to *Umbelliferæ*, *Cruciferæ*, &c., when the better method is before them. Mr. Clarke is a sinner in these matters, and should not think them beneath his attention. We have last spoken of an improvement in the expression of Botanical affinities, by Dr. Lindley, but that great botanist was, in other matters, an adherent of the terminology which expressed the mistaken ideas of preceding times to an extent which greatly injures his descriptions of plants, and which is very conspicuous in his otherwise useful work, "Descriptive Botany.' We part, for the present, from Mr. Clarke's book, with the observation that we believe there is much good in it, and that it well deserves study; but our own faith is in finding out the leading types of vegetable structure, and subdividing each of these so as to have groups analogous with each of the primary types, and so on in farther subdivision; whilst the attempt to show the derivation of each section, and to trace the various groups to their origin in lower forms, we must declare to be thus far utterly unsatisfactory, and to hold forth no rational promise of better success in future W. H.

Catalogue of Birds known to Inhabit Western Canada, systematically arranged according to the method adopted in the Museum of the University of Toronto. By the Rev. W. Hincks, F.L.S., &c., Professor of Natural History, University College, Toronto.

List of Birds observed near Hamilton, C.W., by Thomas McIlwraith. Extracted from the proceedings of the Essex Institute. Vol. V. 1866.

We have, here, two catalogues of the birds of Western Canada: one as general as it could be made from the information within the author's reach; the other professedly local, and the expression of actual personal knowledge and observation, yet the latter reaches 241 species, including several not found in the more general list, which only numbers 271. It is much to be regretted that Mr. McIlwraith's list was not, like an earlier one, which he communicated to this journal, some years since, among the materials accessible in compiling Professor Hincks' list, which it was hoped might be a useful aid to lovers of ornithology, throughout the country. For their convenience, we will here give the names of the birds added by Mr. McIlwraith, which may be inserted in their places in the more general catalogue. But we must first notice the difficulty of comparing the two lists, from the different order in which the birds are placed, and the great difference in the names employed.

The writer of this article being the compiler of one list, has, of course, no idea of adding to its authority by any expression of approbation here. He performed a very humble labour, at the request of the Board of Arts and Manufactures, to assist in the public object of sending to the Paris Exhibition as good a set as the time would allow of being collected of the feathered inhabitants of Western Canada. He employed the arrangement and nomenclature to which he is accustomed, and which seemed to him most likely to be of general use. In immediate reference to Mr. McIlwraith's list, he adds, now, a few words of explanation. At the head of his own catalogue is a key to its arrangement. To assist comparison, he will here attempt to explain Dr. Baird's plan, which is followed by Mr. McIlwraith. Neither of the two, it will be observed, is that found in ordinary ornithological works. First, Dr. Baird begins with the birds of prey—our 2nd. order—and with the family Vulturidæ—our 3rd. family in this order—which, indeed, does not appear in our catalogue, as we learn, for the first time, from Mr. McIlwraith's present list, that Cathartes aura, the turkey-buzzard (his only Vulture), visits Canada occasionally. We place the Eagles first, as the most powerful and specially Raptorial group; then the Falcons; then the Vultures, and last of all the Owls. There are, also, differences in the order of the sub-families. Dr. Baird, not only like Cuvier, puts Insessores, the perching birds, after the birds of prey; but, also, after the order Scansores, and not receiving Cuvier's Suborders of Insessores; he adopts a new set, which would mislead the student of ordinary ornithological works. We believe it comes very near the system explained by the editor of Orr & Co.'s English edition of Cuvier, in his additions to the text. Thus, we have Humming-birds, Swifts, represented by our chimney-swallow, Night-hawks, King-fishers, and then the general body of perching-birds, amidst which are introduced the swallows. The remaining orders occur in the usual series: Gallinaceous birds, Waders, Swimmers. We cannot now criticise this system, or bring it into comparison with our own. We only wish to give the reader some aid in comparing the different lists. But, the chief difficulty will be found in the names employed. Dr. Baird assumes that species must not be supposed to be common to Europe and America; hence, *Aquila chrysætos* becomes *A. Canadensis*; *Circus cyaneus*, *C. Hudsonius*; *Falco peregrinus*, *Falco anatum*, &c., &c.; besides which, small sections, or subgenera, are all distinguished by generic names, increasing their number in a manner very trying to the memory; and which, in fact, nearly takes away the use of generic groups; besides all which, there are a few instances in which the decisions of Dr. Baird, and Dr. Geo. Gray, respecting the generic names proper to be adopted, differ. It is, then, scarcely to be wondered at that so large a proportion of our birds appear under different names, in lists derived from such different authorities. With great respect for Dr. Baird's scientific character and acquirements, we think a wise discretion is exercised in preferring Dr. Gray's names for our Canadian use. We know that some excellent practical ornithologists amongst us think that even he has carried sub-division too far; but few would be satisfied with the vague generic characters of earlier times; and, it is desirable to follow some widely-recognized authority. Looking around, we can find none better than Dr. Geo. Gray.

We may now give the additions to our general list of the birds of Western Canada, derived from Mr. McIlwraith's latest Hamilton list:

Ord. Insessores. Subord. Dentirostres.

Fam. Laniidæ: 1. Myiobius Traillii. Traill's Fly-catcher.

Subord. Conirostres.

Fam. Sturnidæ: 2. Xanthornis varius. The orchard oriole.

Ord. Raptores. Fam. Aquilidæ.
3. Buteo Bairdii. Baird's buzzard.
4. Buteo elegans.
Fam. Vulturidæ.
5. Cathartes aura. The Turkey-buzzard.
Ord. Grallatores. Fam. Charadridæ.
6. *Charadrius hiaticula.* The piping plover.
Ord. Natatores. Fam. Laridæ.
7. Stercorarius pomarinus.
8. Hydrochelidon fissipes.
Fam. Anatidæ: 9. Anser frontalis.
Fam. Alcidæ: 10. Uria grylle.
11. Uria Troile.

Of these eleven species, Xanthornis varius was known to us as Canadian, but accidentally omitted. We hesitated about the two species of Uria, but did not consider that we had certain evidence. Both species of Buteo, and the Anser, we still regard as uncertain as to their being good species.

We add, here, that in our list, Ibis falcinellus should have been I. guarauna, which is equivalent with I. Ordii, of Mr. McIlwraith's list. Phaleropidæ should have been made a family, and Alcidæ occurs twice, being, in the first instance, a misprint for Colymbidæ Divers. We shall be glad to record any further additions to the list of our Native birds.

Mr. McIlwraith deserves the gratitude of all Canadian ornithologists. W. H.

A short treatise on the Milk-weed, or Silk-weed, and the Canadian nettle, viewed as industrial resources. By Alexander Kirkwood. Read before the Ottawa Natural History Society, 15th. Feb., 1867.

This little pamphlet deserves the attention of all who are interested in the growth and prosperity of this Province; and, especially, of all who are engaged in agricultural pursuits. It is strictly a practical treatise, giving instructions for the culture of the plants, and preparation of the fibre, as well as showing the reasons for believing in their value as textile materials. The subject well deserves attention, and we hope Mr. Kirkwood's treatise will obtain a large circulation, and exercise a useful influence. W. H.

ON THE SOURCE OF MUSCULAR POWER

BY EDWARD FRANKLAND, PH.D., F.R.S.

The following pages comprise the most important parts of a lecture lately delivered, by Professor Frankland, at the Royal Institution. The subject is one which has, for some time past, attracted the attention of chemists and physiologists, as it had become evident that our old ideas on the matter were incorrect.

Mr. Frankland has, it appears, fully proved this by actual experiment; and the paper is so interesting, both to chemists and physiologists, as well as in an economic point of view, that we present a full extract to the readers of the Canadian Journal. H. C.

What is the source of muscular power? Twenty years ago, if this question had been asked, there were but few philosophers who would have hesitated to reply, "The source of muscular power is that peculiar force which is developed by living animals, and which we term the *vital force!*" but the progress of scientific discovery has rendered the view implied in such an answer so utterly untenable that, at the present moment, no one possessing any knowledge of physical science would venture to return such a reply. We now know that an animal, however high its organization may be, can no more *generate* an amount of force capable of moving a grain of sand, than a stone can fall upwards or a locomotive drive a train without fuel. All that such an animal can do is to liberate that store of force, or *potential energy*, which is locked up in its food. It is the *chemical change* which food suffers in the body of an animal that liberates the previously pent-up forces of that food, which now make their appearance in the form of *actual energy*—as heat and mechanical motion.

From food, and food alone, comes the *matter* of which the animal body is built up; and from food alone come all the different kinds of *physical force* which an animal is capable of manifesting.

The two chief forms of force thus manifested are *Heat* and *Muscular motion* or *mechanical work*, and these have been almost universally traced to two distinct sources—the *heat* to the oxidation of the *food*, and the *mechanical work* to the oxidation of the *muscles*.

This doctrine, first promulgated, the speaker believed, by Liebig, occupies a prominent position in that philosopher's justly celebrated 'Chemico-Physiological Essays.'

In his work entitled 'Die organische Chemie in ihrer Anwendung auf Physiologie und Pathologie, Braunschweig, 1842,' Liebig says, "All experience teaches that there is only one source of mechanical power in the organism, and this source is the transformation of the living parts of the body into *lifeless compounds*. . . . *This transformation* occurs in consequence of the combination of oxygen with the substance of the living parts of the body." And again, in his 'Letters on Chemistry, 1851,' p. 366, referring to these living parts of the body, he says, "All these organized tissues, all the parts which in any way manifest force in the body are derived from the albumen of the blood; all the albumen of the blood is derived from the plastic or sanguineous constituents of the food, whether animal or vegetable. It is clear, therefore, that the plastic constituents of food, the ultimate source of which is the vegetable kingdom, are the conditions essential to all production or manifestation of force, to all *these* effects which the animal organism produces by means of its organs of sense, thought, and motion." And again, at page 374, he says, "The sulphurized and nitrogenous constituents of food determine the continuance of the manifestations of force; the non-nitrogenous serve to produce heat. The former are the builders of organs and organized structures, and the producers of force; the latter support the respiratory process, they are *materials for respiration*."

This doctrine has since been treated as an almost self-evident truth in most physiological text-books; it has been quite recently supported by Ranke;* and, in his lecture 'On the Food of Man in relation to his Useful Work, 1865,' Playfair says, page 37, "From the considerations which have preceded, we *consider* Liebig *amply* justified in viewing the non-nitrogenous portions of food as mere heat-givers. . . . While we have been led to the conclusion that the transformation of the tissues is the source of dynamical power in the animal." At page 30 he also says, "I agree with Draper and others in considering the contraction of a muscle due to a disintegration of its particles, and its relaxation to their restoration. . . . All these facts prove that transformation of the muscle through the agency of

* 'Tetanus eine Physiologische Studie.' Leipzig. 1865.

oxygen is the condition of muscular action." Finally, in a masterly review of the present relations of chemistry to animal life, published in March last,* Odling says, page 98, "Seeing, then, that muscular exertion is really dependent upon muscular oxidation, we have to consider what should be the products, aud what the value of this oxidation." . . . And again, page 103, "The slow oxidation of so much carbon and hydrogen in the human body, therefore, will always produce its due amount of heat, or an equivalent in some other form of energy; for while the latent force liberated by the combustion of the carbon and hydrogen of fat is expressed *solely in the form of heat*, the combustion of an equal quantity of the carbon and hydrogen of voluntary muscle is expressed *chiefly in the form of motion*."

Nevertheless, this view of the origin of muscular power has not escaped challenge. Immediately after its first promulgation, Dr. J. R. Mayer wrote,† "A muscle is only an apparatus by means of which the transformation of force is effected, *but it is not the material by the chemical change of which* mechanical work is produced." He showed that the 15 lbs. of dry muscles of a man weighing 150 lbs. would, if their mechanical work were due to their chemical change, be completely oxidized in 80 days, the heart itself in 8 days, and the ventricles of the heart in 2½ days. After endeavouring to prove by physiological arguments that not one per cent. of the oxygen absorbed in the lungs could possibly come into contact with the substance of the muscles, Mayer says, "The fire-place in which this combustion goes on is the interior of the blood-vessels, the *blood* however—a slowly-burning liquid—is the oil in the flame of life. . . . Just as a plant-leaf transforms a given mechanical effect, *light*, into another force, *chemical difference*, so does the muscle produce mechanical work at the cost of the chemical difference consumed in its capillaries. Heat can neither replace the sun's rays for the plant, nor the chemical process in the animal: every act of motion in an animal is attended by the consumption of oxygen and the production of carbonic acid and water; every muscle to which atmospheric oxygen does not gain access ceases to perform its functions."

But Mayer was not the first to conceive this view of muscular action. Nearly two hundred years ago, a Bath physician, Dr. John

* 'Lectures on Animal Chemistry.'

† 'Die organische Bewegung in ihrem Zusammenhange mit dem Stoffwechsel.' 1845.

Mayow,* distinctly stated that for the production of muscular motion two things are necessary—the conveyance of combustible substances to the muscle by the blood, and the access of oxygen by respiration. He concluded that the chief combustible substance so used was fat. A century before Priestley isolated oxygen, Mayow was aware of its existence in the air, in nitre, and in nitric acid; he knew that combustion is supported by the oxygen of the air, and that this gas is absorbed in the lungs by the blood, and is absolutely necessary for muscular activity.

For two decades this doctrine sank into oblivion; and it is only within the last two years that it has been again advanced, chiefly by Haidenhain,† Traube, and, to a limited extent, by Donders.‡

Experimental evidence was, however, still wanting to give permanent vitality to the resuscitated doctrine; for although the laborious and remarkable investigations of Voit|| and of Edward Smith§ point unmistakably in the direction of Mayow and Mayer's hypothesis, yet the results of these physiologists were not sufficiently conclusive to render the opposite view untenable. This want of data of a sufficiently conclusive character has been supplied by a happily conceived experiment undertaken by Fick and Wislicenus in the autumn of last year, and described in the 'Philosophical Magazine,' vol. xxxi. p. 485. In the application of these data, however, to the problem now under consideration, one important link was found to be wanting, *viz.* the amount of actual energy generated by the oxidation of a given weight of muscle in the human body. Fick and Wislicenus refer to this missing link in the following words:—"The question now arises what quantity of heat is generated when muscle is burnt to the products in which its constituent elements leave the human body through the lungs and kidneys? At present, unfortunately, there are not the experimental data required to give an accurate answer to this impor-

* 'De Motu musculari,' 1681. Mayow was born in 1645, and died 1679.

† 'Mechanische Leistung Wärmeentwickelung und Stoffumsatz bei der Muskelthätigkeit,' 1864.

‡ As this is passing through the press, the speaker has become aware that Messrs. Lawes and Gilbert advocated this doctrine in 1852, and repeatedly since; their opinions being founded upon experiments on the feeding of cattle.

|| 'Untersuchungen uber den Einfluss des Kochsalzes, des Kaffeés und der Muskel-bewegungen auf den Stoffwechsel,' p. 150. Munich, 1860.

§ 'Phil Trans.,' 1861, p. 747.

tant question, for neither the heat of combustion of muscle nor of the nitrogenous *residue* (urea) of muscle is known." Owing to the want of these data, the numerical results of the experiment of Fick and Wislicenus are rendered less conclusive against the hypothesis of muscle combustion than they otherwise would have been, whilst similar determinations, which have been made by Edward Smith, Haughton, Playfair, and others, are even liable to a total misinterpretation from the same cause.

The speaker stated that he had supplied this want by the calorimetrical determination of the actual energy evolved by the combustion of muscle and of urea in oxygen. Availing himself of these data he then proceeded to the consideration of the problem to be solved, the present condition of which might be thus summed up:—It is agreed on all hands that muscular power is derived exclusively from the mutual chemical action of the food and atmospheric oxygen; but opinions differ as to whether that food must first be converted into the actual organized substance of the muscle, before its oxidation can give rise to mechanical force, or whether it is not also possible that muscular work may be derived from the oxidation of the food, which has only arrived at the condition of blood and not of organized muscular tissue.

The importance of this problem can scarcely be overrated; it is a corner-stone of the physiological edifice, and the key to the phenomena of the nutrition of animals. For its satisfactory solution the following data require to be determined :—

1st. The amount of force or actual energy generated by the oxidation of a given amount of muscle in the body.

2nd. The amount of mechanical force exerted by the muscles of the body during a given time.

3rd. The quantity of muscle oxidized in the body during the same time.

If the total amount of force involved in muscular action, as measured by the mechanical work performed, be greater than that which could possibly be generated by the quantity of muscle oxidized during the same time, it necessarily follows that the power of the muscles is not derived *exclusively* from the oxidation of their own substance.

As regards the first datum to be determined, it is necessary to agree upon some unit for the measurement of mechanical force. The unit most commonly adopted is that represented by the lifting of a kilogram weight to the height of one metre. The researches of Joule and Mayer have connected this standard unit with heat;—they prove that the force required to elevate this weight 425 times will, when converted into heat, raise the temperature of an equal weight of water 1° C. If this weight were let fall from a height of 425 metres, its collision with the earth would produce an amount of heat sufficient to raise the temperature of 1 kilogram of water 1° C. The same heating effect would also of course be produced by the fall of 425 kilograms through 1 metre. This standard of force is termed a *metrekilogram;** and 425 metrekilograms are equal to that amount of heat which is necessary to raise the temperature of 1 kilogram of water through 1° C. If then it be found that the heat evolved by the combustion of a certain weight of charcoal or muscle, for instance, raises the temperature of 1 kilogram of water through 1° C., this means, when translated into mechanical power, 425 metrekilograms. Again, if a man weighing 64 kilograms climbs to a height of 1,000 metres, the ascent of his body to this height represents 64,000 metrekilograms of work; that is, the labour necessary to raise a kilogram weight to the height of 1 metre 64,000 times.

The author then proceeds to describe the manner in which he determined the actual energy developed by one gram of each the substance in the following list, when *burnt in oxygen*:—

Beef Muscle	2,161	Metrekilograms of Force.
Albumin	2,117	
Beef Fat	3,841	
Hippuric Acid	2,289	
Uric Acid	1,108	
Uria	931	

The heat evolved was determined by means of a calorimeter of peculiar construction; the substance being burnt by means of chlorate of potassa, and various corrections introduced.

It is evident that the above determination of the actual energy developed by the combustion of muscle in oxygen represents more than the amount of actual energy produced by the oxidation of muscle within the body, because, when muscle burns in oxygen its carbon is converted into carbonic acid, and its hydrogen into water; the nitrogen being, to a great extent, evolved in the elementary state;

* I follow the example of the Registrar-General in abbreviating the French word *gramme* to gram.

whereas, when muscle is most completely consumed in the body, the products are carbonic acid water and urea; the whole of the nitrogen passes out of the body as urea—a substance which still retains a considerable amount of potential energy. Dry muscle and pure energy yield, under these circumstances, almost exactly one-third of their weight of urea, and this fact, together with the above determination of the actual energy developed on the combustion of urea, enables us to deduce with certainty the amount of actual energy developed by muscle and albumen respectively when consumed in the human body. It is as follows:—

Actual energy developed by one gram of each substance when consumed in the body.

Name of substance dried at 100° C.	Heat units. (Mean.)	Metrekilograms of force. (Mean.)
Beef Muscle purified by ether . . .	4368	1848
Purified Albumen	4263	1803

The second point, viz., the amount of mechanical force exerted by the muscles of the body during a given time, was ascertained from the experiments of Fick and Wislicenus, during their ascent of the Faulhorn, from the Lake of Brienz. The third point, viz., the amount of muscle oxidized in the body, as ascertained from the experiments of the above-named observers, on the quantity of nitrogen secreted in the urine, both before, during, and after the ascent.

From the above data the following table was constructed:—

	Fick.	Wislicenus.
	Grams.	Grams.
Weight of dry Muscle consumed	37·17	37·00
	Metrekilograms	Metrekilograms
Actual energy capable of being produced by the consumption of 37·17 and 37·00 grams of dry Muscle in the body	68,690	68,376
Measured work performed in the ascent (external work)	129,096	148,656
Calculated circulatory and respiratory work performed during the ascent (internal work) . .	30,541	35,631
Total ascertainable work performed . .	159,637	181,287

It is thus evident that the muscular power expended by these gentlemen in the ascent of the Faulhorn could not be exclusively derived *from the oxidation, either of their muscles, or of other* nitrogenous *constituents* of their bodies, since the maximum of power capable of being *derived* from this source even under very *favourable* assumptions *is, in* both cases, less than one-half of the work actually performed. But the deficiency becomes much greater if we take into consideration the fact, that the actual energy developed by the oxidation or combustion cannot be wholly transformed into mechanical work. In the best constructed steam-engine for instance, only $\frac{1}{10}$ of the actual energy developed by the burning fuel can be obtained in the form of mechanical power; and in the case of man, Helmholtz estimates that not more than $\frac{1}{5}$ of the actual energy developed in the body can be made to appear as external work. The experiments of Haidenhain, however, show that, under favourable circumstances, a muscle may be made to yield in the shape of mechanical work as much as one-half of the actual energy developed within it, the remainder taking the form of heat. Taking then this highest estimate of the proportion of mechanical work capable of being got out of actual energy, it becomes necessary to multiply by 2 the above numbers representing the ascertainable work performed, in order to express the actual energy inve' ed in the production of that work. We then get the following c .nparison of the actual energy capable of being developed by the amount of muscle consumed, with the actual energy necesssary for the performance of the work executed in the ascent of the Faulhorn.

	Fick.	Wislicenus.
	Metrekilograms.	Metrekilograms.
Actual energy capable of being produced by Muscle metamorphosis	68,690	68,376
Actual energy expended in work performed .	319,274	368,574

Thus, taking the average of the two experiments, it is evident that *scarcely $\frac{1}{5}$th of the actual energy required for the work performed could be obtained from the amount of muscle consumed.*

Similar though not quite so conclusive results were obtained from experiments made on prisoners engaged in treadmill labour, on military prisoners engaged in shot drill, and on various kinds of labourers.

We have seen, therefore, in the above four sets of experiments, interpreted by the data afforded by the combustion of muscle and urea in oxygen, that the transformation of tissue alone cannot account for more than a small fraction of the muscular power developed by animals; in fact, this transformation goes on at a rate almost entirely independent of the amount of muscular power developed. If the mechanical work of an animal be doubled or trebled there is no corresponding increase of nitrogen in the secretions; whilst it was proved on the other hand by Lawes and Gilbert, as early as the year 1854, that animals, under the same conditions as regarded exercise, had the amount of nitrogen in their secretions increased twofold by merely doubling the amount of nitrogen in their food. Whence then comes the muscular power of animals? What are the substances which, by their oxidation in the body, furnish the actual energy, whereof a part is converted into muscular work? In the light of the experimental results detailed above, can it be doubted that a large proportion of the muscular power developed in the bodies of animals has its origin in the oxidation of non-nitrogenous substances? For whilst the secretion of nitrogen remains nearly stationary under widely different degrees of muscular exertion, the production of carbonic acid increases most markedly with every augmentation of muscular work, as is shown by the following tabulated results of E. Smith's highly important experiments regarding the amount of carbonic acid evolved from his own lungs under different circumstances.*

Excretion of carbonic acid during rest and muscular exertion:—

	Carbonic acid per hour.
During sleep	19·0 grams.
Lying down and sleep approaching	23·0 "
In a sitting posture	29·0 "
Walking at rate of 2 miles per hour	70·5 "
" " 3 " "	100·6 "
On the treadwheel, ascending at the rate of 28·65 feet per minute	189·6 "

It has been already stated as a proposition upon which all are agreed, that food, and food alone, is the ultimate source from which muscular power is derived; but the above determinations and considerations, the speaker believed, prove conclusively, firstly, that the non-nitrogenous constituents of the food, such as starch, fat, &c., are the chief sources of the actual energy, which becomes partially trans-

* Phil. Trans. for 1859, p. 709.

formed into muscular work; and secondly, that the food does not require to become organized tissue before its metamorphosis can be rendered available for muscular power; its digestion and assimulation into the circulating fluid—the blood—being all that is necessary for this purpose. It is, however, by no means the non-nitrogenous portions of food alone that are capable of being so employed, the nitrogenous also, inasmuch as they are combustible, and consequently capable of furnishing actual energy, might be expected to be available for the same purpose, and such an expectation is confirmed by the experiments of Savoy upon rats,* in which it is proved that these animals can live for weeks in good health upon food consisting almost exclusively of muscular fibre. Even supposing these rats to have performed no external work, nearly the whole of their internal muscular work must have had its source in the actual energy developed by the oxidation of their strictly nitrogenous food.

It can scarcely be doubted, however, that the chief use of the nitrogenous constituents of food is for the renewal of muscular tissue; the latter, like every other part of the body, requiring a continuous change of substance, whilst the chief function of the non-nitrogenous is to furnish by their oxidation the actual energy which is in part transmuted into muscular force.

The combustible food and oxygen coexist in the blood which courses through the muscle, but when the muscle is at rest there is no chemical action between them. A command is sent from the brain to the muscle, the nervous agent determines oxidation. The potential energy becomes active energy, one portion assuming the form of motion, another appearing as heat. *Here is the source of animal heat, here the origin of muscular power!* Like the piston and cylinder of a steam-engine, the muscle itself is only a machine for the transformation of heat into motion; both are subject to wear and tear and require renewal, but neither contributes in any important degree by its own oxidation to the actual production of the mechanical power which it exerts.

From this point of view it is interesting to examine the various articles of food in common use, as to their capabilities for the production of muscular power. The writer therefore made careful estimations of the calorific value of different materials used as food, by

* 'The Lancet,' 1863, pages 381 and 412.

the same apparatus and in the same manner as described above for the determination of the actual energy in muscle, urea, uric acid, and hippuric acid.

The author then appends a series of tables, showing the actual energy developed by one gram of various articles of food, when burnt in oxygen, or when oxidized in the body, together with other tables, showing the weight and cost of various articles of food required to be oxidized in the body in order to raise 140 lbs. to the height of 10,000 feet. From the first table we make the following short extract, to elucidate the concluding remarks :—

	Metrekilograms of force.
Cheese	1,969
Potatoes	429
Oatmeal	1,696
Bread Crumb	945
Beef (lean)	664
White of Eggs	284
Milk	280
Beef Fat	3,841
Butter	3,077
Cabbage	184
Pea Meal	1,667

These results are in many instances fully borne out by experience. The food of the agricultural labourers in Lancashire contains a large proportion of fat. Besides the very fat bacon which constitutes their animal food proper, they consume large quantities of so-called apple dumplings, the chief portion of which consists of paste in which dripping and suet are large ingredients, in fact these dumplings freqently contain no fruit at all. Egg and bacon pies and potato pies are also very common *pièces de résistance* during harvest-time, and whenever very hard work is required from the men. The speaker well remembers being profoundly impressed with the dinners of the navigators employed in the construction of the Lancaster and Preston Railway: they consisted of thick slices of bread surmounted with massive blocks of bacon, in which mere streaks of lean were visible. Dr. Piccard states that the Chamois hunters of Western Switzerland are accustomed, when starting on long and fatiguing expeditions, to take with them, as provisions, nothing but bacon-fat and sugar, because, as they say, these substances are more nourishing than meat. They doubtless find that in fat and sugar they can most conveniently carry with them a store of force-producing matter. The above tables affirm the same thing. They show that ·55 lb. of fat will perform the work of 1·15 lb. cheese, 5 lbs. potatoes, 1·3 lb. of flour or pea-meal or of 3½ lbs. of lean beef. Donders, in his admirable pamphlet 'On the Constituents of Food and their Relation to Muscular Work and Animal Heat,' mentions the observations of Dr. M. C. Verloren on the food of insects. The latter remarks, "Many insects use during a period in which very little muscular work is performed food containing chiefly albuminous matter; on the contrary, at a time when the muscular work is very considerable, they live exclusively, or almost exclusively, on food free from nitrogen." He also mentions bees and butterflies as instances of insects performing enormous muscular work, and subsisting upon a diet containing but the merest traces of nitrogen.

We thus arrive at the following conclusions :—

1. The muscle is a machine for the conversion of potential energy into mechanical force.

2. The mechanical force of the muscles is derived chiefly, if not entirely, from the oxidation of matters contained in the blood, and not from the oxidation of the muscles themselves.

3. In man the chief materials used for the production of muscular power are non-nitrogenous; but nitrogenous matters can also be employed for the same purpose, and hence the greatly increased evolution of nitrogen under the influence of a flesh diet, even with no greater muscular exertion.

4. Like every other part of the body, the muscles are constantly being renewed; but this renewal is not perceptibly more rapid during great muscular activity than during comparative quiescence.

5. After the supply of sufficient albuminized matters in the food of man to provide for the necessary renewal of the tissues, the best materials for the production, both of internal and external work, are non-nitrogenous matters, such as oil, fat, sugar, starch, gum, &c.

6. The non-nitrogenous matters of food, which find their way into the blood, yield up all their potential energy as actual energy; the nitrogenous matters, on the other hand, leave the body with a portion (one-seventh) of their potential energy unexpended.

7. The tranformation of potential energy into muscular power is neccesarily accompanied by the production of heat within the body, even when the muscular power is exerted externally. This is, doubtless, the chief and, probably, the only source of animal heat.

[E. F.]

A STUDY OF THE CEPHALIC DISK OF THE REMORA. [ECHENEIS.]

From the "Comptes Rendues."

Abstract by the author of a memoir by M. E. Bandelot, presented to the Academy by M. E. Blanchard.

The disk on the head of the Remora has been, from the most remote times, an object of interest to observers. Among modern Naturalists, some, as Voigt, and Stannius, have advanced the opinion that the disk may be regarded as the equivalent of a dorsal fin; but this mode of viewing the subject has not been supported by a rigorous demonstration, since there are certain interior portions of the disk whose relations have not been determined; moreover the mechanism by means of which the attachment of the disk is accomplished, has not yet been analyzed and explained in a satisfactory manner. The researches which I have the honor to submit to the Academy, have for their object the solution of these still obscure problems. The disk of the Remoras occupies, as is well known, the upper surface of the head. Its figure is that of a much elongated oval, of which the border, a little elevated, consists of a fold of the skin so disposed as to form around the organ a sort of flexible case. The upper surface of the disk is level, it presents on each side of the median line, a series of little transverse laminæ, nearly parallel, and slightly inclined backward, so as partly to cover each other like the laths of a Venetian blind. Between these folds are as many corresponding empty spaces.

Excepting the border, the disk is supported by an internal frame-work formed by a considerable number of small bones, disposed in a series of similar segments regularly succeeding one another from behind forward. Each segment consists of the following pieces, four in number: one interspinal bone, two radial bones, and an articular bong element.

The interspinal bone is a small unpaired piece, occupying the median line on the lower face of the disk, of the form of a sharp spine with its point downwards, its aspect in every respect bringing to our minds the interspinal bones which support the rays of the fins; it is of the same nature with them.

The rays are represented by two little bony stems set across in a horizontal plane and articulated at their base on the level of the median line, with the corresponding interspinal bone. Each of these stems, taken alone, corresponds with a half-ray of a fin, this half, instead of remaining closely attached to its fellow in a vertical plane, being withdrawn from it so as to lie down sideways.

The articular bone element is an unpaired, symmetrical bone, extended across the disk, of which it occupies the whole width. It consists of a very narrow middle portion, and of two lateral portions, enlarged into laminæ, or quadrilateral plates. From the upper surface of these latter, protrudes a little lamellose apophysis directed backwards (the articular apophysis) under which is fixed the extremity of the ray belonging to the same segment.

This bony element, the nature of which has been hitherto misunderstood, must be regarded, according to my view, as the homologue of the little bony nodule found in the fin at the separation of the bases of the two parts of each ray.

As to the mechanism by means of which the attachment of the disk is accomplished, it is easily apprehended when one has considered the arrangement of the parts of this little apparatus. Each ray (of the transformed fin) in fact serves as a support for one lamina of the disk. It is capable of moving upon its anterior extremity as if upon a hinge, and consequently of inclining forward or backward, the lamina to which it belongs. This double movement is secured by means of little muscles which are inserted, at one extremity, on an apophysis at the base of the rays projecting at the lower face of the disk, at the other on the interspinous bones of the neighbouring segments. These muscular bundles correspond with the elevators and depressors of the rays of the fins.

It is easy to demonstrate by a very simple geometrical construction, that when the lamellæ of the disk are erected, the space which they enclose is increased; the air included is consequently rarified within this space, and as all communication with the exterior is stopped by the cutaneous fold which borders the disk an effect of suction is produced which may be, in every respect, compared with that of the cupping glass.

ENTOMOLOGICAL SOCIETY OF CANADA.

An ordinary meeting of this society was held at the residence of the President, J H. Sangster, Esq., M.D., Yorkville, on Friday, March 1st, at 7 o'clock, p.m. Frequent showers of rain, accompanied by lightning—an unusual circumstance so early in the year—rendered the attendance very limited indeed. The President took the chair, and the minutes of the Annual General Meeting of 1866, and a Field Meeting held on the 1st of June, were read and adopted; the Fenian raid, which called away many of the members to their duty as volunteers, and the threatened attack of cholera, which engrossed the attention of others, prevented any subsequent meetings being held.

A communication was read from the Hon. James Cockburn, Solicitor General West, in reply to an application for a grant from Government in aid of the funds of the Society, stating that the Finance Minister could not recommend any fresh grants for scientific objects, as the country was on the eve of Confederation. It was resolved that further application should be made as soon as the Confederation of the Provinces has taken place.

Mr. W. H. Ellis, of University College, Toronto, was proposed and unanimously elected an ordinary member of the Society.

To the Library,

From the Author,—"Synopsis of the Diptera of the Eastern Archipelago," and "Characters of undescribed species of Smicra (*Chalcidites*)," by F. Walker, Esq., F.L.S.

From the QUEBEC BRANCH:—The Annual Report, President's Address, By-laws, etc, of the Branch.

To the Cabinet,

From F. Walker, Esq., British Museum, London, Eng.:—A box of British and Exotic Lepidoptera, Coleoptera, and a few other insects, received through the Smithsonian Institution, Washington, D.C.

The Secretary stated that he had procured, by purchase, for the Library of the Society, the first five volumes of the Proceedings of the Entomological Society of Philadelphia, and that as soon as they came from the binder's hands they would be placed in the rooms of the Society; he also stated that Mr. Saunders, Curator of the London Branch, has preparing, and expected shortly to have published for the Society, a list of Canadian Coleoptera, containing about 800 species.

The announcement of the death, by typhoid fever, of Dr. Brackenridge Clemens, of Eastern Penn, on the 11th of January last, was received with much regret. Dr. Clemens was one of the best American Entomologists of the day, and had attained a wide reputation beyond the limits of his own country: he was the author of a most beautiful Monograph of North American Sphingidæ many "contributions of American Lepidopterology," &c., and was the only authority on the Micro-Lepidoptera on this continent.

Mr. Bethune mentioned a few rare Lepidoptera, new to Canada, that had been captured by members during the past year; among others *Thecla strigosa*, Harris, and *Lycæna pembina*, Edw. by Mr. Saunders; *Erebus odora*, lines, by Dr. Sangster; and *Philampelus sattelitia*, lines. by the Rev. Clements. The meeting then proceeded to the examination and discussion of Sphingidæ, the appointed subject for the evening. The Rev. Prof. Hincks made some interesting remarks upon their classification and that of insects in general, based upon a "quinary system;" Dr. Sangster exhibited a large number of rare and beautiful specimens and Mr. Bethune, a specimen of an undetermined Sphinx, captured by Mr. Pettit at Grimsby, C. W. Twenty-eight species of this family are now known to inhabit this country.

Before the close of the meeting, it was resolved that for the future ordinary meetings of the Society be held on the *First Friday* in each month, from September to May inclusive, at 7 p m; and that from May to August inclusive, Field meetings be held at 9, a.m., on each *second* and *last Saturday* of the month: notice of the place of meeting to be duly announced beforehand. The next meeting will be held at Professor Croft's, Yorkville, on Friday, April 5th, at 7, p.m.

After partaking of Dr. Sangster's kind hospitality, the meeting adjourned.

C. J. S. B.

OBITUARY.

THE REV. EDWARD HINCKS, D.D.

The influential positions occupied in Canada by more than one member of the family of the late Rev. Thomas Dix Hincks, LL.D. Professor of Hebrew and Oriental languages in the Belfast Academical Institution, confers an additional interest here on the death of the most distinguished among his sons. Dr. Edward Hincks obtained a fellowship at Trinity College, Dublin, with distinction rarely, if ever equalled, before he was twenty one. In the first honors thus obtained, he chiefly displayed his mastery of mathematical science. His later triumphs turned mainly on his no less thorough command both of the classical and oriental languages. His early training amid all the special advantages of the paternal roof, no doubt tended to give this peculiar bias to his tastes, and to direct him to the field of his later successful labours.

Dr Edward Hincks was born at Cork, in Aug. 1791. Soon after obtaining his fellowship he took orders in the Church of England, and was presented by his College to the Rectory of Ardtrea, which he subsequently exchanged for that of Killyleagh in the Diocese of Down. .here the last forty one years of his life were spent; and those labours carried out which have won for him a European reputation as one of the most profound and original philologists of the 19th century. The period in which he lived was one presenting peculiar facilities and inducements to his favourite investigations. The discovery of the famous Rosetta Stone took place in his early youth; but he had already obtained distinction as a philologist before the labours of Young and Champollion furnished the long sought key to the mystery of Egyptian hieroglyphics. To this interesting department of philological research he now applied his extensive knowledge: and frequently gave evidence of a rare talent for deciphering its novel characters and unknown language.

But the labours of Professor Grotefend of Gottingen had, so early as 1802, accomplished for the cuneiform alphabet of the Persepolitan inscriptions, what Dr. Thomas Young subsequently did for the ancient characters of Egypt; and the alphabet thus partially deciphered, was augmented by the ingenious researches of Rask of Denmark. To those discoveries, the later explorations of Botta and Layard gave a new interest; and the name of Dr. Edward Hincks will ever be associated with those of Rawlinson, Oppert, and others of the most profound European philologists who have devoted themselves to the deciphering of the cuneiform inscriptions of Persepolis, Nineveh, and other ancient seats of Asiatic civilisation. He laboured with unwearied perseverance in this novel field of research; and won a reputation, especially among German scholars for great acuteness and sagacity, combined with caution and patient conscientiousness. A writer in the *Athenæum* speaks from personal knowledge, of the high terms in which he was referred to by such continental scholars as Rœdiger and Ewald; and adds: "His talent for deciphering texts in unknown characters and languages was wonderful. It was applied to the study of Egyptian hieroglyphics, and to the inscriptions in the cuneiform character. In this field especially he laboured for years with great perseverance and success, having been the first to ascertain the numeral system, and the power and form of its signs, by means of the inscriptions at Van. He was one of the chief restorers of Assyrian learning throwing great light on the linguistic character and grammatical structure of the languages represented on the Assyrian monuments." His interpretations of these inscriptions were disputed for a time by men of the first class, such as Rawlinson and Grotefend, who had already committed themselves to other views; but we believe the principles of interpretation which he was the first to discover and explain, are now generally accepted as true and indisputable.

It is not to be overlooked, when estimating the value of Dr. Hincks' labours, that they had to be carried on, for the most part in a remote Irish village, hampered with inadequate means, and dependent wholly on indirect resources for the study of the ancient inscriptions of Egypt and Assyria.

An Irish writer in the *Northern Whig* complains that men have been advanced to the highest offices and honours of the Church; to bishoprics and archbishoprics, some of whom could not translate a verse of the Hebrew bible *ad aperturam libri*: while incomparably the most learned man in the Church, and inferior to none in personal and moral qualifications was left to die in the possession of the moderate living he had received from his College nearly half a century before. We cannot think that it would have been a wise use of the patronage of the Crown to have hampered a scholar devoted to such engrossing researches, with the onerous duties of a bishopric But so long as Deaneries and prebendal stalls are reserved for men like Buckland or Stanley; no fitter occupant of such could have been found than the deceased Irish Scholar. In London within reach of the British Museum, or placed in charge of its Egyptian and Assyrian treasures, the nation would have been amply repaid by the results to which such facilities would have given birth. As it is his literary remains are by no means slight. Many valuable papers are printed in the *Transactions of the Royal Irish Academy*, the *Royal Society of Literature*, and the *Asiatic Society*: others were communicated to the British Association; in the sections of which the present writer has repeatedly met him. His profound learning seemed almost to disqualify him from dealing with a popular audience; and it was sometimes amusing to observe the simplicity and *naivete* with which he would solve the difficulty suggested by some tyro, in reference to the interpretation of a Nimroud cylinder or a cuneiform inscription, by a Hebrew or Arabic quotation or an appeal to Zend or Sanscrit roots. Nevertheless when occasion required, Dr. Hincks could forsake his study for the arena of public life; and was known as a moderate, but consistent liberal in the political questions which have of late years assumed such grave significance in Ireland, in reference to education, the franchise, and the Church itself. The courage and independence he manifested in dealing with some of those vexed questions, is believed to have been a hindrance to his promotion in the Church. He was, however, in receipt of a small literary pension bestowed on him in acknowledgement of his labours as a scholar. The King of Prussia manifested the estimation in which he was held by the philologists of Germany by conferring on him an order of Knighthood; and the foremost literary societies of Europe had bestowed their chief distinctions on him.

Dr. Hincks was in his seventy-sixth year at the time of his death. Throughout his long life he had laboriously devoted his rare learning to cope with the most obstruse problems in epigraphy and philology. But with all his great attainments he was modest, simple-hearted, and kind; and has left behind many who affectionately mourn his loss on private, as well as on public grounds.

MONTHLY METEOROLOGICAL REGISTER, AT THE PROVINCIAL MAGNETICAL OBSERVATORY, TORONTO, CANADA WEST.—JULY, 1866.

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

Day.	Barom. at temp. of 32°. 6 A.M.	2 P.M.	10 P.M.	MEAN.	Temp. of the Air. 6 A.M.	2 P.M.	10 P.M.	ME'N	Excess of mean above Normal.	Tens. of Vapour. 6 A.M.	2 P.M.	10 P.M.	M'N	Humidity of Air. 6 A.M.	2 P.M.	10 P.M.	M'N	Direction of Wind. 6 A.M.	2 P.M.	10 P.M.	Resultant Direction.	Velocity of Wind. 6 A.M.	2 P.M.	10 P.M.	Resul't.	ME'N	Rain in Inches.	Snow in Inches.
1	29.847	29.845	—	—	58.0°	73.5°	—°	—°	—°	.382	.45	—	—	79	55	—	—	S b E	S	Calm.	S 14° W	0.5	9.0	0.0	3.28	3.36	0.00	...
2	.833	.751	29.705	29.7565	61.6	71.7	63.7	65.73	+ 0.73	.456	.501	.479	.487	84	64	81	77	S b E	S S W	Calm.	S 22 W	2.0	10.0	0.0	3.41	3.63	Inp	...
3	.636	.532	.478	.5352	62.7	78.5	67.7	69.57	+ 4.42	.504	.411	.503	.497	88	45	75	71	Calm.	S b W	SW b W	S 24 W	0.0	14.0	8.8	6.20	6.40	.13	...
4	.343	.305	.403	.3503	61.2	72.4	64.5	66.22	+ 0.92	.493	.495	.507	.498	92	62	83	79	S S W	W	S W b S	S 68 W	3.8	18.0	2.2	3.21	4.03	.660	...
5	.510	.507	.524	.5305	63.7	76.7	63.4	67.83	+ 2.45	.504	.640	.533	.576	85	69	91	85	Calm.	S S E	S W	S 28 W	0.0	8.5	2.0	3.17	3.42	.110	...
6	.567	.554	.532	.5502	68.8	81.4	69.9	73.60	+ 8.00	.640	.773	.584	.659	79	75	91	81	Calm.	S S W	Calm.	S 32 W	0.0	10.0	0.0	4.15	4.17	..	...
7	.533	.514	.502	.5093	69.9	77.8	69.2	73.23	+ 7.53	.584	.723	.650	.661	79	75	91	81	S W b S	S S W	SW b W	S 39 W	4.0	3.0	3.0	2.19	2.24	.410	...
8	.464	.514	—	—	67.4	78.5	...	—	—	.645	.571	—	—	93	59	—	—	Calm.	NW b N	Calm.	N 45 W	0.0	8.0	0.0	5.07	6.09	.480	...
9	.761	.838	.850	.8237	61.6	69.5	60.1	64.33	— 1.65	.393	.420	.366	.390	71	59	70	65	N	S	N N W	N 25 E	5.0	3.0	0.5	0.96	2.79	...	...
10	.888	.829	.799	.8438	63.4	72.0	58.7	65.78	— 0.30	.369	.354	.396	.376	63	45	80	61	Calm.	E S E	Calm.	S 72 E	0.0	5.0	0.0	1.49	1.83	...	...
11	.822	.781	.708	.7660	65.5	79.3	66.6	70.83	+ 4.65	.429	.515	.501	.498	68	52	77	67	N N W	S S W	S W	S 34 W	2.0	6.0	1.5	3.46	3.94	...	...
12	.719	.646	.618	.6587	63.0	83.9	70.6	74.18	+ 7.88	.475	.68	.561	.595	82	59	75	70	W b S	S W b S	SW b W	S 48 W	1.5	7.0	2.2	5.11	5.39	..	...
13	.608	.498	.499	.5362	73.1	91.5	76.4	81.10	+14.72	.643	.687	.742	.686	79	47	81	67	Calm.	W b S	W N W	S 79 W	0.0	15.8	4.5	4.60	6.26	.525	...
14	.613	.626	.613	.6172	67.0	74.6	66.6	69.88	+ 3.47	.594	.150	.479	.493	90	52	73	68	N E b E	E	Calm.	N 85 E	3.2	10.0	0.0	3.85	3.88	.010	...
15	.620	.588	—	—	68.1	88.3	—	—	—	.511	.698	—	—	75	52	—	—	Calm.	S W b S	SW b W	S 48 W	0.0	5.0	1.3	3.96	4.67	...	...
16	.657	.628	.635	.6372	77.1	89.7	75.3	80.52	+13.93	.749	.717	.741	.740	80	53	84	72	W S W	W b S	Calm.	S 36 W	1.5	9.8	0.0	3.21	3.43	Inp.	...
17	.651	.567	.586	.6022	74.6	87.9	67.4	75.18	+ 8.50	.702	.785	.6.7	.689	82	60	91	79	Calm.	S W b S	W b N	S 53 W	0.0	9.0	2.8	3.09	5.62	2.345	...
18	.530	.478	.601	.5365	67.0	65.9	61.2	65.00	— 1.68	.608	.527	.512	.518	92	83	95	89	W b N	N E	NE b N	N 20 E	1.0	11.0	1.5	2.85	3.76	.165	...
19	.679	.722	.742	.7162	59.8	69.9	60.3	64.37	— 2.42	.455	.408	.419	.425	89	55	80	71	NE b N	S S E	Calm.	S 83 E	1.5	4.2	0.0	1.80	3.18	...	...
20	.744	.696	.635	.6877	60.5	69.1	61.9	64.82	— 1.95	.360	.381	.451	.409	69	54	82	68	NE b N	E	N E	N 61 E	2.8	8.0	3.0	4.76	5.05	...	...
21	.603	.540	.503	.5417	65.2	66.3	65.5	66.38	— 0.38	.523	.56	.516	.546	85	87	87	85	NE b N	E N E	N E	N 64 E	1.0	7.8	4.0	3.98	4.37	.145	...
22	.457	.417	—	—	66.3	73.1	—	—	—	.562	.628	—	—	87	78	—	—	Calm.	S b W	Calm	N 69 W	0.0	3.0	0.0	1.63	4.35	.365	...
23	.438	.503	.559	.5058	61.6	75.3	65.6	68.08	+ 1.20	.481	.513	.518	.516	87	58	82	75	N W	N W	N N W	N 32 W	5.0	6.0	3.5	4.87	5.50	...	...
24	.616	.604	.588	.6022	62.7	77.8	69.5	71.73	+ 4.87	.466	.550	.532	.536	82	57	74	69	Calm.	S S W	Calm.	S 35 W	0.0	4.5	0.0	1.55	2.15	...	...
25	.586	.606	.659	.6178	68.8	78.5	68.8	72.77	+ 5.80	.528	.571	.515	.521	75	59	74	65	N W	S S W	N b W	N 23 W	1.5	1.0	9.2	4.30	5.89	...	...
26	.677	.688	.638	.6688	66.6	77.4	69.9	72.34	+ 5.37	.531	.614	.670	.619	81	65	92	79	N	S E b S	Calm.	S 49 E	6.0	2.5	0.0	0.11	1.77	...	...
27	.651	.620	.564	.6063	66.3	80.3	71.7	74.03	+ 7.05	.535	.656	.691	.631	83	63	89	76	Calm.	S b W	Calm.	N 84 W	0.0	3.4	0.0	0.57	2.11	...	...
28	.524	.448	.466	.4762	69.9	84.3	71.0	75.05	+ 8.08	.541	.493	.556	.538	74	41	73	63	N b W	N W b N	Calm.	N 18 W	1.0	10.0	0.0	2.75	4.13	...	...
29	.470	.436	—	—	68.4	77.5	—	—	—	.467	.483	—	—	67	50	—	—	N b W	S b W	N b W	N 27 W	4.5	5.8	0.8	2.48	3.93	Inp.	...
30	.473	.503	.563	.5187	65.9	77.5	63.0	69.35	+ 2.40	.489	.296	.424	.385	76	31	74	56	N	N	N N W	N 15 W	2.5	8.2	11.0	9.12	9.37	...	...
31	.609	.596	.502	.5600	62.3	74.9	68.1	69.40	+ 2.45	.303	.342	.172	.386	53	39	68	54	N b W	S b W	S	S 5 W	3.0	3.5	3.5	1.42	2.48	.035	...
M	29.6257	29.5092	29.5951	29.6059	65.76	77.08	66.80	70.43	+ 4.18	.514	.544	.530	.535	80	58	81	72	....	....	....	....	1.72	7.42	2.11	.. ...	4.17	5.390	...

REMARKS ON TORONTO METEOROLOGICAL REGISTER FOR JULY, 1866.

Note.—The monthly means do not include Sunday observations. The daily means, excepting those that relate to the wind, are derived from six observations daily, namely at 6 A.M., 8 A.M., 2 P.M., 4 P.M., 10 P.M., and midnight. The means and resultants for the wind are from hourly observations.

Highest Barometer	29.915 at 8 a.m. on 10th	Monthly range = 0.610 inches.
Lowest Barometer	29.305 at 2 p.m. on 4th	
Self-regist'ing Thermom. Maximum Temperature	94°0 on 13th	Monthly range = 46°2
Self-regist'ing Thermom. Minimum Temperature	47°8 on 1st	
Self-regist'ing Thermom. Mean maximum Temperature	79°60	Mean daily range = 18°96
Self-regist'ing Thermom. Mean minimum Temperature	60°64	
Self-regist'ing Thermom. Greatest daily range	35°0 from a.m. to p.m. of 13th.	
Self-regist'ing Thermom. Least daily range	7°9 from a.m. to p.m. of 18th.	
Warmest day 13th	Mean temperature 81°10	Difference = 16°77
Coldest day 9th	Mean temperature 64°33	
Maximum Radiation. Solar	136°2 on 13th	Monthly range = 96°2
Maximum Radiation. Terrestrial	40°0 on 1st	

Aurora observed on 3 nights, viz:—12th, 14, and 25th
Possible to see Aurora on 25 nights; Impossible on 6 nights.
Snowing on 0 days; depth .....; duration of fall 0.0 hours.
Raining on 16 days; depth 5.390 inches; duration of fall 33.5 hours.
Mean of cloudiness = 0.50.
Most cloudy hour observed, 2 p.m.; mean = 0.64; least cloudy hour observed, 10 p.m.; mean = 0.38.

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

North.	South.	East.	West.
1062.79	1191.14	506.20	1190.80

Resultant direction S. 79° W.; Resultant velocity 0.94 miles per hour.
Mean velocity ....4.17 miles per hour.
Maximum velocity..........18.0 miles, from 1.30 to 2.30 p.m. of 4th.

Most windy day ... 30th	Mean velocity, 9.37 miles per hour.		Difference = 7.60 miles
Least windy day ... 26th	Mean velocity, 1.77	ditto	
Most windy hour ... 1 p.m.	Mean velocity, 7.84	ditto	Difference = 5.82 miles.
Least windy hour ... 6 a.m.	Mean velocity, 2.02	ditto	

July 8th. Dense fog a.m. 13th. Very hot day. 14th. Heavy rain, 5.30 a.m., over half an inch in 30 minutes. 15th. Very warm day. 17th. Heavy thunder with very heavy rain 4 to 8 p.m., 1.4 fell during the hour 4 to 5 p.m. 19th. Perfect Solar halo 20th. Solar halo. 23d. Perfect Rainbow. 31st. Solar halo at noon.
July, 1866, was very warm, the mean temperature shewing an excess above the average never before reached except in July, 1854.
This month, with one exception, was the most rainy July on record.

COMPARATIVE TABLE FOR JULY.

YEAR.	TEMPERATURE. Mean.	Excess above aver'ge	Max. observed.	Min. observed.	Range.	RAIN. No. of days.	Inches	SNOW. No. of days.	Inches	WIND. Resultant. Direction.	V'y.	Mean Force or Velocity.
	°	°	°	°	°							
1840	65.8	− 1.2	79.4	48.2	31.2	6	5.270	...	...	...	...	....
1841	65.0	− 2.0	86.3	43.2	43.1	10	8.150	..	...	...	...	0.27 lbs.
1842	64.7	− 2.3	90.5	42.0	48.5	4	3.050	...	...	...	...	0.33
1843	64.5	− 2.5	86.1	40.2	45.9	8	4.605	...	...	...	...	0.44
1844	66.0	− 1.0	86.1	40.5	45.6	12	2.815	...	...	...	...	0.19
1845	66.2	− 0.8	94.6	45.6	49.0	7	2.195	...	...	...	...	0.30
1846	68.0	+ 1.0	94.0	44.9	49.1	9	2.895	...	...	...	...	0.29
1847	68.0	+ 1.0	87.5	43.8	43.7	8	3.355	...	...	...	...	0.19
1848	65.5	− 1.5	82.7	46.7	36.0	10	1.890	...	...	N 14° W	0.18	4.94mls.
1849	68.4	+ 1.4	89.1	51.0	38.1	4	3.415	...	...	S 5° W	0.75	3.52
1850	68.9	+ 1.9	84.9	52.8	32.1	12	5.270	...	...	N 81° E	0.59	4.56
1851	65.0	− 2.0	82.7	52.1	30.6	12	3.625	...	...	N 60° W	0.88	4.13
1852	66.8	− 0.2	90.1	49.5	40.6	8	4.025	...	...	N 43° W	0.93	3.33
1853	65.6	− 1.4	85.4	49.4	36.0	10	0.915	...	...	S 58° E	0.24	3.69
1854	72.5	+ 5.5	93.6	53.0	40.6	9	4.505	...	...	S 49° W	0.37	4.03
1855	77.9	+ 0.9	88.4	53.1	35.3	13	3.245	...	...	S 19° W	0.73	6.47
1856	69.9	+ 2.9	92.0	51.4	40.6	8	1.120	...	...	N 79° W	1.57	5.84
1857	67.8	+ 0.8	85.4	52.4	33.0	15	3.475	...	...	S 68° E	0.81	4.74
1858	67.9	+ 0.9	83.4	55.9	27.5	13	3.075	...	...	N 15° E	1.13	5.73
1859	66.9	− 0.1	87.7	50.5	37.2	12	2.611	...	...	N 56° W	1.48	5.81
1860	63.9	− 3.1	85.8	47.5	38.3	13	4.336	...	...	N 60° W	2.15	7.29
1861	65.4	− 1.6	82.9	49.4	33.5	16	2.635	...	...	N 74° W	1.43	4.66
1862	66.7	− 0.3	88.6	52.6	36.0	15	5.344	...	...	S 89° W	1.42	5.80
1863	67.6	+ 0.6	82.3	49.3	33.0	15	3.408	...	...	N 18° W	0.40	3.89
1864	69.7	+ 2.7	87.9	52.9	35.0	8	1.332	...	...	N 61° W	2.23	6.00
1865	65.0	− 2.0	83.0	45.8	37.2	11	2.470	...	...	N 86° W	2.28	5.34
1866	70.4	+ 3.4	92.2	57.2	35.0	16	5.390	...	...	S 79° W	0.94	4.17
Results to 1861.	66.98	...	87.10	48.72	38.38	10.3	3.474	...	...	N 66° W	0.63	4.97
Excess for 1866.	+ 3.45	...	+ 5.10	+ 8.48	− 3.38	+ 5.7	+ 1.916	...	...	....	..	− 0.80

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Latitude—43 deg. 39.4 min. North. Longitude—5 h. 17.33 min. West. Elevation above Lake Ontario, 108 feet.

Day.	Barom. at temp. of 32°.				Temp. of the Air.				Excess of mean above Normal	Tens. of Vapour.				Humidity of Air.				Direction of Wind.			Resul't Direction.	Velocity of Wind.					Rain in inches.	Snow in inches.
	6 A.M.	2 P.M.	10 P.M.	Mean	6 A.M.	2 P.M.	10 P.M.	ME'N		6 A.M.	2 P.M.	10 P.M.	M'N	6 A.M.	2 P.M.	10 P.M.	M'N	6 A.M.	2 P.M.	10 P.M.		6 A.M.	2 P.M.	10 P.M.	Result'.	ME'N		
1	29.365	29.258	29.300	29.3018	67.7°	76.7°	65.2°	69.27°	+ 2.30°	.599	.738	523	.610	88	80	85	85	s b w	s s w	Calm.	s 25° w	5.5	7.0	0.0	5.16	5.18	0.165	...
2	.300	.412	.530	.427	63.7	71.7	60.2	64.17	— 2.6	428	.398	.364	.381	72	52	70	64	sw b w	w	w	n 89 w	2.5	17.8	8.2	10.44	10.5	...	...
3	.315	.577	.521	.565	55.5	68.8	58.0	61.30	— 5.5	.337	.411	452	.414	76	58	94	77	w n w	s w b w	sw b w	s 68 w	6.0	3 4	3.0	2.14	3.2	1.100	...
4	.446	.504	.591	.523	59.0	71.0	58.7	61.52	— 5.2	.476	.355	.339	.398	95	47	69	74	Calm.	n w b n	n w b w	n 35 w	0.0	14.2	12.0	10.18	10.8	.295	...
5	.620	.591	—	—	58.0	68.8	—	—	—	.359	.309	—	—	74	44	—	—	n w b w	n w b n	w b n	n 59 w	12.0	18.0	1.8	10.09	10.4	...	...
6	.631	.624	.655	.636	56.5	74.6	56.2	63.45	— 3.32	.356	.233	.328	.294	78	27	72	55	w b n	n w b w	w n w	n 66 w	5 6	14.2	2.0	10.48	10.5	...	...
7	.681	.623	.649	.649	57.3	74.6	63.0	66.10	— 0.58	302	347	.293	.347	64	39	50	55	w n w	n w b w	n n w	n 44 w	11.8	9 4	6.0	4.95	5.3	...	...
8	.646	.547	.400	.514	52.9	67.4	60.1	62.55	— 4.12	349	.392	.489	.426	87	58	94	76	Calm.	E	n e b n	n 48 E	0.0	2.8	3.6	3 52	3.9	.085	...
9	.390	.466	.538	.475	57.3	71.7	59.8	62.32	— 4.2	.390	.295	.359	.343	83	37	69	63	n	n w	n w	n 45 w	8 8	15.0	4.8	9.07	9.3	...	...
10	.606	.657	.698	.661	56.9	68.4	61.2	62.90	— 3.6	374	.416	317	.373	80	60	58	65	n w	s s w	n w b w	n 54 w	5.0	9.6	1.5	2.90	4.5	...	...
11	.772	.770	.755	.762	55.1	69 5	59.0	62.48	— 3.9	331	388	.380	.362	76	54	76	66	n b w	s b E	Calm	s 27 E	0.5	2.4	0.0	1.17	1 4	...	...
12	.742	.661	—	—	59 0	62.7	—	—	—	.403	.492	—	—	81	84	—	—	n E b E	n E b E	E	n 79 E	2.6	3.2	7 8	5.47	5.7	2.145	...
13	.459	.442	.426	.439	62.3	64.5	64.8	63 45	— 2.78	.549	.560	.555	550	98	92	90	94	E n E	n E b E	n b E	n 47 E	6.0	8.8	2.8	4.84	4.9	inap.	...
14	.458	.483	.533	.499	64.5	72.4	65.5	67.30	+ 1.07	573	651	.586	.592	94	83	93	89	Calm.	s w b s	n w	n 35 w	0.0	1.8	1.2	1.06	1.9	.010	...
15	.640	.776	.872	.779	63.4	63.7	54.0	59 85	— 6.30	469	.284	.260	.320	80	47	62	61	n	n b w	n b w	n 13 w	11.0	13.6	8 8	9.62	9 97	...	...
16	.949	.909	.869	.904	49 0	61.6	51.5	54.87	—11.17	.225	.311	.243	.268	65	56	64	61	n b E	s s w	s w b s	s 79 w	5.5	5.2	1.2	0.74	2.4	...	...
17	.856	.742	.712	.761	47.1	69.5	57.6	59.93	— 6.0	250	401	.387	.355	77	55	81	68	Calm.	s b w	Calm	s 31 w	0.0	8.2	0.0	2.05	2 6	..	...
18	.679	.570	.516	.575	58.0	71.3	63.0	64.05	— 0.8	.416	496	.513	.482	86	61	89	78	Calm.	Calm.	n n E	n 1 w	0.0	0.0	0.8	1.00	1 7	.180	...
19	.457	.402	—	—	58 3	66.6	—	—	—	418	49	—	—	92	75	—	—	Calm.	E b s	n n E	n	0.0	3.0	4.1	2.53	3.4	.355	...
20	.541	.534	.506	.5257	50.4	63.4	58.3	59.10	— 6.4	340	370	.367	.373	93	63	75	74	s w b s	s s w	s s w	s 66 w	2.8	8.8	1.0	1.23	3.0	.035	...
21	.455	.426	.406	.422	52 0	68.1	54.7	60.07	— 5.3	381	.334	.358	.358	95	48	84	72	Calm.	s b w	Calm	s 60 w	0.0	10.0	0 0	3.14	4 0	.065	...
22	.350	.450	.490	.438	53.6	61 2	51.1	55.73	— 9.6	.362	.230	.299	.294	87	42	80	67	n w	w	w n w	n 73 w	10.6	14.0	0.6	8.82	8.8	...	...
23	.483	.416	.408	.452	46.4	60.1	47.9	52.50	—12.6	.212	.319	.230	.258	67	62	69	65	w n w	s b w	Calm.	n 72 w	1.4	1.0	0.0	3 02	3 7	...	...
24	.500	.500	.577	.529	46.4	56.7	47.2	51.13	—13 8	278	.290	.268	.270	88	62	82	73	w n w	n w b w	w n w	n 76 w	3.0	12.2	1.5	5.77	5.9	inap.	...
25	.601	.593	.594	.598	48.4	61.9	49.7	53.65	—11.0	269	.389	.237	.306	85	69	81	74	w n w	w b s	Calm.	n 89 w	3 2	9.5	0.0	4.16	4 2	...	...
26	.541	.410	—	—	48.6	66.3	—	—	—	.302	.355	—	—	88	51	—	—	Calm	s w b s	Calm.	s 40 w	0.0	11.5	0.0	5.52	5.7	007	...
27	.482	.489	.546	.505	57.6	63 0	59.0	61.27	— 3 0	.434	526	.439	.466	92	91	88	86	s w b s	s s w	Calm.	s 31 w	0.8	9.0	0.0	3.62	3 64	...	...
28	.529	.510	.504	.516	53.6	72 0	62.3	62.88	— 1.25	384	.528	.471	.456	93	67	84	80	Calm.	s	n b w	n 39 E	0.0	2.5	1.0	0.12	2 3	...	...
29	.559	.554	.568	.558	59.0	69.2	56.5	60.18	— 3.68	462	.328	.368	.375	93	45	80	73	n n w	n	Calm	n 44 w	7.8	5.8	0.0	2.25	2.7	...	...
30	.571	.517	.524	.536	53.3	61.6	57.6	57.92	— 5.7	366	381	.410	393	90	69	86	82	Calm.	E b s	Calm.	s 50 E	0.0	4.0	0 0	1.04	1.6	.015	...
31	.602	.592	.560	.579	52.9	63.7	63.0	60 90	— 2.42	372	.452	.487	.450	92	76	85	84	Calm.	E b n	E n E	n 69 E	0.0	6.0	6.8	5.90	5 9	...	...
M	29.5617	29.5534	29.5673	29.560	55 51	67.34	57.97	60.80	— 4.90	.381	401	.384	.390	84	59	78	73	.....	....	...		3.63	8 13	2.60	...	5.1	4.457	

REMARKS ON TORONTO METEOROLOGICAL REGISTER FOR AUGUST, 1866.

NOTE.—The monthly means do not include Sunday observations. The daily means, excepting those that relate to the wind, are derived from six observations daily, namely at 6 A.M., 8 A.M., 2 P.M., 4 P.M., 10 P.M., and midnight. The means and resultants for the wind are from hourly observations.

Highest Barometer . . . . 29.977 at 8 a.m. on 16th. } Monthly range=
Lowest Barometer . . . . 29.258 at 2 p.m. on 1st. } 0.719 inches.

Self-registering Ther.
- Maximum Temperature . . . . . 77°.0 on 1st and 7th. } Monthly range=
- Minimum Temperature . . . . . 42°.4 on 24th. } 34°.6
- Mean Maximum Temperature . . 69°64 } Mean daily range=16°92
- Mean Minimum Temperature . . 52°72 }
- Greatest daily range . . . . . . 27°1 from a.m. to p.m. of 17th.
- Least daily range . . . . 4°6 from a.m. to p.m. of 13th.

Warmest day . . . . 1st ...Mean Temperature . . . 69°27 } Difference=18°.14
Coldest day . . . . . 24th ..Mean Temperature . . . 51°13 }

Maximum { Solar . . . . 127°.0 on 7th } Monthly range=
Radiation. { Terrestrial . . . . 32°.0 on 25th } 95°.0

Aurora observed on 4 nights, viz.—9th, 11th, 23rd and 30th.
Possible to see Aurora on 17 nights; impossible on 14 nights.
Snowing on days; depth inches; duration of fall hours.
Raining on 14 days; depth 4.457 inches; duration of fall 60.7 hours.
Mean of cloudiness=0.56; most cloudy hour observed, 4 p.m.; mean=0.65; least cloudy hour observed, 12 p.m.; mean=0.49.

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

North.	South.	East.	West.
1615.77	636.06	503.17	2151.25

Resultant Direction, N. 59° W.; Resultant Velocity, 2.58 miles per hour.
Mean Velocity, 5.16 miles per hour.
Maximum Velocity, 24.1 miles, from noon to 1 p.m. of 5th.

Most windy day, 4th—Mean velocity 10.81 miles per hour. } Difference,
Least windy day, 11th—Mean velocity 1.43 miles per hour. } 9.38 miles

Most windy hour, 1 p.m.—Mean velocity 8.64 miles per hour. } Difference,
Least windy hour, 10 p.m.—Mean velocity 2.47 miles per hour. } 6.17 miles

14th. Dense fog during morning. 19th. Rainbow at 7 p.m.

August, 1866, was both cold and wet. The mean temperature was upwards of 5° below the average—a depression more than twice as great as any that had before occurred.

COMPARATIVE TABLE FOR AUGUST.

YEAR.	Temperature: Mean.	Excess above Average.	Maximum observed	Minimum observed	Range.	Rain: No. of days	Inches.	Snow: No. of days.	Inches.	Wind: Resultant Direction.	Resultant Velocity.	Mean Force or Velocity
1840	64.7	— 1.5	80.1	47.4	32.7	12	2.905	...	...	...	...	...
1841	64.4	— 1.8	83.5	46.7	36.8	9	6.176	...	...	...	...	0.19 lbs
1842	65.7	— 0.5	80.7	45.3	35.4	6	2.500	...	...	...	...	0.30 "
1843	66.4	+ 0.2	85.5	44.4	41.1	4	4.850	...	...	...	...	0.12 "
1844	64.3	— 1.9	82.5	44.3	38.2	17	Imp	...	...	...	...	0.16 "
1845	67.9	+ 1.7	82.5	44.4	38.1	9	1.725	...	...	...	...	0.19 "
1846	68.4	+ 2.2	86.3	50.4	35.9	9	1.770	...	...	...	...	0.17 "
1847	65.1	— 1.1	83.1	44.9	38.2	10	2.140	...	...	...	...	0.19 "
1848	69.2	+ 3.0	87.5	49.3	38.2	8	0.857	...	...	S 21 E	0.98	4.55 ms
1849	66.3	+ 0.1	79.5	51.4	28.1	10	4.970	...	...	N 71 W	0.66	3.76 "
1850	66.8	+ 0.6	84.2	43.0	41.2	13	4.355	...	...	N 15 E	0.57	4.46 "
1851	63.6	— 2.6	79.8	43.6	36.2	10	1.360	...	...	N 63 W	0.40	4.63 "
1852	65.9	— 0.3	81.2	46.7	34.5	9	2.697	...	...	N 70 W	0.5	[illegible] "
1853	68.6	+ 2.4	91.6	47.6	44.0	11	2.577	...	...	S 36 E	0.36	4.26 "
1854	68.0	+ 1.8	98.1	47.0	51.1	5	0.457	...	...	N 64 W	1.70	4.60 "
1855	64.1	— 2.1	82.1	44.9	37.2	7	1.455	...	...	N 63 W	1.04	6.97 "
1856	63.6	— 2.6	81.3	44.0	37.3	12	1.680	...	...	N 50 W	2.88	7.03 "
1857	65.3	— 0.9	85.3	50.1	35.2	13	5.267	...	...	N 77 W	1.51	6.36 "
1858	67.6	+ 1.4	83.4	45.4	38.0	11	3.894	...	...	N 69 W	1.57	6.50 "
1859	66.6	+ 0.4	81.4	46.2	35.2	11	3.906	...	...	N 36 W	1.62	5.96 "
1860	64.5	— 1.7	81.8	47.1	34.7	14	3.405	...	...	N 70 W	1.8	5.80 "
1861	65.5	— 0.7	82.5	48.2	34.3	15	2.975	...	...	N S E	0.46	4.21 "
1862	67.6	+ 1.4	87.6	47.7	39.9	15	3.185	...	...	N 78 W	1.67	5.96 "
1863	66.6	+ 0.4	87.2	45.9	43.3	12	2.208	...	...	S 61 W	1.80	4.89 "
1864	68.6	+ 2.4	92.6	48.6	44.0	16	5.660	...	...	N 70 W	1.38	4.75 "
1865	65.2	— 1.0	86.1	46.8	39.3	8	1.990	...	...	N 60 W	1.57	5.07 "
1866	60.8	— 5.4	76.7	45.7	31.0	14	4.457	...	...	N 59 W	2.58	5.16 "
Results to 1864.	66.21	...	84.45	46.50	37.95	10.7	3.026	...	...	N 67 W	0.93	5.18
Exc. for 1866.	—5.41	...	—7.75	—0.80	—6.95	+3.3	+1.431	...	...	...	...	—0.02

MONTHLY METEOROLOGICAL REGISTER, AT THE PROVINCIAL MAGNETICAL OBSERVATORY, TORONTO CANADA WEST,—SEPTEMBER, 1866.

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 Normal.	Tens. of Vapour. 6 A.M.	2 P.M.	10 P.M.	M'N	Humidity of Air. 6 A.M.	2 P.M.	10 P.M.	M'N	Direction of Wind. 6 A.M.	10 P.M.	2 P.M.	Resul't. Direction.	Velocity of Wind. 6 A.M.	2 P.M.	10 P.M.	Resul't.	ME'N	Rain in inches.	Snow in inches.
1	29.498	29.421	29.342	29.4032	60.9	67.0	63.7	64.40	+ 1.37	.478	.594	.570	.550	89	89	96	91	N E b E	E b S	Calm.	S 89 E	6.5	2.2	0.0	2.10	2.57	0.080	...
2	.358	.347	—	—	65.5	78.2	—	—	—	.586	.670	—	—	93	69	—	—	S W b W	S S W	Calm.	N 79 W	0.5	0.2	0.0	3.18	4.59	...	...
3	.687	.656	.662	.6610	59.4	71.0	58.3	62.73	+ 0.30	.422	.512	.367	.436	83	67	75	76	Calm.	Calm.	E	N 28 E	0.0	0.0	2.0	1.84	2.36	...	...
4	.652	.563	.452	.5423	56.9	68.8	65.6	64.00	+ 1.87	.384	.411	.391	.403	85	58	62	69	N	E b N	E N E	N 78 E	2.6	8.0	7.5	3.52	3.78	0.470	...
5	.416	.512	.622	.5250	60.9	70.2	54.4	62.07	+ 0.23	.503	.355	.330	.401	94	48	78	74	Calm.	N W b W	W N W	N 69 W	0.0	16.4	0.6	5.32	5.48	0.055	...
6	.619	.656	.697	.6657	51.5	71.0	61.2	61.70	+ 0.27	.336	.420	.385	.391	88	55	71	71	W N W	S S W	N b E	S 63 W	4.5	7.6	6.2	0.71	3.75	...	...
7	.661	.462	.352	.4688	57.6	59.0	58.3	58.57	— 2.58	.354	.451	.425	.422	76	90	87	86	N E b N	N E b E	N W b N	N 32 E	2.2	10.6	6.2	5.06	6.17	0.860	...
8	.389	.570	.695	.5680	56.2	62.7	53.6	58.15	— 2.58	.417	.343	.329	.362	93	60	80	76	N W b W	N W	N W b N	N 45 W	8.2	14.0	1.6	7.15	7.19	inap.	...
9	.776	.809	—	—	48.6	67.0	—	—	—	.302	.434	—	—	88	66	—	—	Calm.	N N W	N b W	N 33 W	0.0	8.7	6.4	5.12	5.42	...	...
10	.841	.740	.650	.7285	49.3	63.4	56.5	57.43	— 2.62	.313	.369	.413	.377	89	63	91	80	N b W	S b W	Calm.	S 4 E	1.0	2.0	0.0	0.58	1.84	inap.	...
11	.499	.266	.148	.2910	59.8	60.9	61.6	61.05	+ 1.40	.452	.516	.532	.509	88	96	97	94	N E b E	E N E	S	S 72 E	5.8	8.6	2.2	2.68	6.40	0.675	...
12	.169	.176	.319	.2232	60.9	68.1	59.0	62.55	+ 3.32	.479	.525	.336	.437	89	77	67	76	S W b S	S W b W	W N W	S 53 W	7.4	12.8	9.5	9.92	10.27	0.077	...
13	.436	.537	.591	.5332	52.6	64.5	51.1	56.60	— 2.25	.310	.240	.293	.275	78	37	80	63	W S W	W S W	Calm.	S 77 W	5.5	14.0	0.0	6.50	6.53	...	...
14	.537	.395	.643	.5352	45.7	64.8	46.8	51.60	— 6.73	.273	.339	.20[illegible]	.280	90	55	64	72	Calm.	S W	W b N	S 88 W	0.0	8.4	19.0	9.30	9.98	0.170	...
15	.837	.911	.936	.9018	35.3	52.6	43.5	45.90	—12.02	.181	.169	.205	.208	88	41	72	67	W b S	N W	Calm.	N 85 W	4.0	8.5	0.0	2.09	3.45	...	...
16	.857	.702	—	—	49.3	54.0	—	—	—	.262	.324	—	—	74	78	—	—	S E b E	E b N	N N E	S 78 E	5.2	2.0	0.9	1.15	3.55	0.780	...
17	.524	.601	.632	.5957	59.4	60.9	50.0	56.05	— 0.95	.446	.321	.304	.349	88	59	84	77	N W	N W b N	N N W	N 32 W	9.4	8.6	1.5	5.26	5.35	0.020	...
18	.607	.573	.593	.5873	49.0	53.3	50.8	51.08	— 5.53	.325	.291	.293	.294	94	71	78	78	N N W	N E b E	N E	N 28 E	0.8	1.0	1.0	1.70	1.86	...	...
19	.554	.594	.702	.6220	49.7	55.1	50.4	51.25	— 4.87	.247	.247	.308	.267	69	57	81	71	N	E b S	Calm.	N 40 E	4.6	7.2	0.0	2.58	3.16	inap.	...
20	.685	.517	.361	.4982	48.2	47.5	49.0	48.47	— 7.13	.255	.306	.320	.296	75	93	94	87	N E	N E b N	N E	N 25 E	5.5	9.0	9.7	7.49	9.41	0.560	...
21	.403	.587	.691	.5728	46.4	52.2	41.4	46.20	— 8.92	.290	.231	.171	.232	92	60	64	73	N W b W	N W	N W b W	N 56 W	5.0	10.8	3.0	6.84	7.05	...	...
22	.765	.850	.892	.8392	36.3	52.2	42.5	44.63	—10.09	.193	.254	.236	.226	90	65	86	77	N W	S b W	S	S 5 W	2.2	8.2	2.0	1.12	3.02	...	...
23	.928	.890	—	—	40.3	52.6	—	—	—	.249	.239	—	—	87	69	—	—	N E b N	E b N	E N E	N 82 E	4.2	14.0	5.2	7.28	7.57	...	...
24	.808	.721	.682	.7322	44.3	56.2	58.0	53.90	+ 0.15	.239	.339	.416	.335	82	75	86	79	N E b E	E	Calm.	S 87 E	0.8	8.4	0.0	3.31	3.35	inap.	...
25	.704	.605	.682	.6628	56.2	53.6	48.6	52.57	— 0.67	.383	.384	.282	.351	85	93	82	88	N	N b W	N	N 3 W	7.8	7.0	20.0	9.09	9.20	1.910	...
26	.798	.779	.777	.7858	46.4	56.9	46.1	49.68	— 3.45	.298	.222	.254	.278	94	48	81	79	N	N E b N	Calm.	N 27 W	0.5	3.0	0.0	0.59	1.05	...	...
27	.786	.831	.860	.8257	42.8	61.9	48.2	51.95	— 0.27	.240	.376	.287	.308	87	68	85	79	Calm.	S S W	Calm.	S 31 W	0.0	6.2	0.0	1.59	1.64	...	...
28	.899	.895	.883	.8902	44.4	63.7	52.9	53.75	+ 1.92	.241	.465	.384	.367	92	78	95	87	Calm.	Calm.	Calm.	S 45 W	0.0	0.0	0.0	0.51	0.70	...	...
29	.900	.868	.832	.8595	47.5	62.3	59.4	54.12	+ 2.80	.316	.419	.340	.359	95	74	93	86	Calm.	Calm.	Calm.	S 29 W	0.0	0.0	0.0	0.08	0.20	...	...
30	.800	.775	—	—	47.9	66.3	—	—	—	.321	.444	—	—	96	68	—	—	Calm.	Calm.	N W b N	N 8 E	0.0	0.0	3.5	1.66	2.38	...	...
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	.	...	...
M	29.6270	29.6114	29.6278	29.6207	50.98	60.79	52.88	55.22	— 2.27	.336	.364	.335	.349	87	67	81	73	.....	...	...	.....	3.14	6.91	3.60	.	4.63	[illegible].657	...

REMARKS ON TORONTO METEOROLOGICAL REGISTER FOR SEPTEMBER, 1866.

NOTE.—The monthly means do not include Sunday observations. The daily means, excepting those that relate to the wind, are derived from six observations daily, namely, at 6 A.M., 8 A.M., 2 P.M., 4 P.M., 10 P.M., and midnight. The means and resultants for the wind are from hourly observations.

Highest Barometer . . . . 29 936 at 10 p.m. on 15th. } Monthly range=
Lowest Barometer . . . . 29.142 at midnight on 11th. } 0.794 inches.

Self-registering Ther.:
Maximum temperature . . . 80°0 on 2nd. } Monthly range=
Minimum temperature . . . 34°.4 on 22nd. } 45°6
Mean maximum temperature . . 64°00 } Mean daily range=15°27
Mean minimum temperature . . 48°73 }
Greatest daily range . . . . . . 21°5 from a.m. to p.m. of 28th.
Least daily range . . . . 5°0 from a.m. to p.m. of 25th.

Warmest day . . . . 1st...Mean Temperature . . . 64°40 } Difference=19°.77
Coldest day . . . . 22nd...Mean Temperature . . . 44°63 }

Maximum Radiation { Solar . . . . 130°5 on 27th } Monthly range=
{ Terrestrial . . . . 26°0 on 22nd } 104°.5

Aurora observed on 3 nights, viz.:—on 3rd, 12th, and 13th.
Possible to see Aurora on 19 nights; impossible on 11 nights.
Snowing on 00 days; depth 0.0 inches; duration of fall 0.0 hours.
Raining on 15 days; depth 5 657 inches; duration of fall, 89.4 hours.
Mean of cloudiness=0.57; Most cloudy hour observed, 2 p.m; mean=0.73; least cloudy hour observed, 10 p.m; mean=0.44.

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

North.	South.	East.	West.
1404.42	523.93	869.18	1433.20

Resultant direction, N. 33° W.; Resultant Velocity, 1.45 miles per hour.
Mean velocity 4.63 miles per hour.
Maximum velocity 25.4 miles, from 8 to 9 p.m. of 14th.
Most windy day 12th—Mean velocity 10 27 miles per hour. } Difference 10.07
Least windy day 29th—Mean velocity 0 20 miles per hour. }
Most windy hour, 11 a.m.—Mean velocity, 7 83 miles per hour. } Difference
Least windy hour, 2 a.m.—Mean velocity, 2.18 miles per hour. } 5.65 miles.

1st. Thunderstorm.—3rd. Solar halo.—11th. Foggy during day; lightning at night.—12th. Rainbow at 6 p.m.—14th. Thunderstorm during day, with violent gusts of wind.—15th. Thin ice, a m. (first of season)—16th. Thunderstorm at night.—20th. Fog during day.—22nd. Hoar frost, a.m.—23rd. Hoar frost, a.m.—25th. Very heavy rain storm.—27th. Hoar frost, a.m.—28th. Hoar frost; fog at night. 29th. Foggy at night. 30th. Solar halo.

September, 1866, was cold and calm. The amount of rain is the greatest since 1847.

COMPARATIVE TABLE FOR SEPTEMBER.

YEAR.	TEMPERATURE. Mean	Excess above Average	Maximum observed.	Minimum observed.	Range.	RAIN. No. of days.	Inches.	SNOW. No. of days.	Inches.	WIND. Resultant. Direction.	Velocity.	Mean Force or Velocity
	°		°	°	°							
1840	54.0	— 3.8	70 2	29.4	40.8	4	1.380	...	...	...	...	...
1841	61 3	+ 3.5	79.9	37.5	42.4	9	3.340	...	...	...	...	0.26 lbs
1842	55.7	— 2.1	85 5	28.3	5 .2	12	6.160	...	...	...	...	0.45 "
1843	59.1	+ 1.3	87.8	33.1	54.7	10	9.760	...	...	...	...	0.57 "
1844	58 6	+ 0.8	81.5	29.6	51.9	4	Imp.	...	...	...	...	0 26 "
1845	56.0	— 1.8	78.8	35.3	43.5	16	6.245	...	...	...	...	0.34 "
1846	63 6	+ 5 8	84 0	39.0	45.0	11	4.595	...	...	...	...	0.33 "
1847	55.6	— 2.2	74.8	38.1	36.7	15	6 665	...	...	..	...	0 33 "
1848	54 2	— 3.6	80.9	29.5	51.4	11	3.115	...	...	N 71 W	2.38	5.81 ms
1849	58.2	+ 0.4	80.6	33.5	47.1	9	1.480	...	...	N 75 W	0.69	4.23 "
1850	56.5	— 1.3	76.0	31.7	44.3	11	1.735	...	...	S 65 W	1.0[illegible]	4.78 "
1851	60.0	+ 2 2	86.3	33.4	52.9	9	2.665	...	...	N 14 E	1 05	5.45 "
1852	57.5	— 0.3	81.8	36.1	45.7	10	3 630	...	...	N 77 W	0.5[illegible]	4 60 "
1853	58.8	+ 1.0	85 4	36.1	49.3	12	5.140	...	...	N	1.06	4.33 "
1854	61.0	+ 3 2	93.1	36.3	56.8	14	5.375	...	...	N 22 W	1.33	4.04 "
1855	59.5	+ 1.7	81.7	36.1	45.6	12	5.585	...	...	N 20 E	1.29	7.61 "
1856	57.1	— 0.7	77.3	37.4	39.9	13	4.105	...	...	S 79 W	1.98	6.53 "
1857	58.6	+ 0.8	81.4	34.1	47.3	11	2.640	...	...	N 68 W	1.61	5.55 "
1858	59.1	+ 1.3	80.1	36.8	43.3	8	0.735	...	...	S 74 W	1.53	5.69 "
1859	55.2	— 2.6	73.8	35.7	38.1	15	3 525	...	...	N 44 W	1.60	6.36 "
1860	55.3	— 2.5	74.2	28.7	45.5	14	1.959	...	...	N 71 W	2.63	5 [illegible]9 "
1861	59.1	+ 1.3	78 2	37.1	41.1	17	3.607	...	...	N 71 W	1.39	4 [illegible]1 "
1862	59.6	+ 1.8	78.9	41.0	37.9	9	2.344	...	...	N 59 W	1.07	5 11 "
1863	55.9	— 1 9	78.2	31 6	46.6	8	1.235	...	...	N 16 W	0.92	6.46 "
1864	56.4	— 1.4	72.4	41.0	31.4	11	2 508	...	...	N 38 W	1.89	7 06 "
1865	64.5	+ 6.7	87.2	43.2	44.0	12	2.450	.	...	S 56 E	0 47	4.12 "
1866	55.2	— 2.6	78 2	35.3	42.9	15	5.657	...	...	N 33 W	1.45	4.63 "
Results to 1864	57.84	...	80.03	34.66	45.38	11.0	3.730	...	...	N 57 W	1.15	5 54
Exc. for 1866.	—2.62	...	—1 83	+0 64	—2.48	+4.0	+1.927	...	...	...	...	—0 91