Technical and Bibliographic Notes / Notes techniques et bibliographiques

The Institute has attempted to obtain the best original copy available for filming. Features of this copy which may be bibliographically unique, which may alter any of the images in the reproduction, or which may significantly change the usual method of filming, are checked below.

L'Institut a microfilmé le meilleur exemplaire qu'il lui a été possible de se procurer. Les détails de cet exemplaire qui sont peut-être uniques du point de vue bibliographique, qui peuvent modifier une image reproduite, ou qui peuvent exiger une modification dans la méthode normale de filmage sont indiqués ci-dessous.

☐ Coloured covers/
Couverture de couleur

☐ Covers damaged/
Couverture endommagée

☐ Covers restored and/or laminated/
Couverture restaurée et/ou pelliculée

☐ Cover title missing/
Le titre de couverture manque

☐ Coloured maps/
Cartes géographiques en couleur

☐ Coloured ink (i.e. other than blue or black)/
Encre de couleur (i.e. autre que bleue ou noire)

☐ Coloured plates and/or illustrations/
Planches et/ou illustrations en couleur

☑ Bound with other material/
Relié avec d'autres documents

☑ Tight binding may cause shadows or distortion along interior margin/
La reliure serrée peut causer de l'ombre ou de la distorsion le long de la marge intérieure

☐ Blank leaves added during restoration may appear within the text. Whenever possible, these have been omitted from filming/
Il se peut que certaines pages blanches ajoutées lors d'une restauration apparaissent dans le texte, mais, lorsque cela était possible, ces pages n'ont pas été filmées.

☐ Coloured pages/
Pages de couleur

☐ Pages damaged/
Pages endommagées

☐ Pages restored and/or laminated/
Pages restaurées et/ou pelliculées

☑ Pages discoloured, stained or foxed/
Pages décolorées, tachetées ou piquées

☐ Pages detached/
Pages détachées

☑ Showthrough/
Transparence

☐ Quality of print varies/
Qualité inégale de l'impression

☑ Continuous pagination/
Pagination continue

☐ Includes index(es)/
Comprend un (des) index

Title on header taken from:/
Le titre de l'en-tête provient:

☐ Title page of issue/
Page de titre de la livraison

☐ Caption of issue/
Titre de départ de la livraison

☐ Masthead/
Générique (périodiques) de la livraison

☐ Additional comments:/
Commentaires supplémentaires:

This item is filmed at the reduction ratio checked below/
Ce document est filmé au taux de réduction indiqué ci-dessous.

10X				14X				18X				22X				26X				30X		
☐	☐	☐	☐	☐	☐	☐	☐	☐	☐	☐	☑	☐	☐	☐	☐	☐	☐	☐	☐	☐	☐	☐
		12X				16X				20X				24X				28X				32X

THE

CANADIAN RECORD

OF SCIENCE.

VOL. II. OCTOBER, 1886. NO. 4.

Presidential Address before the British Association for the Advancement of Science, Sept. 1886.

By Sir J. William Dawson,

C.M.G., M.A., LL.D., F.R.S., F.G.S., Principal and Vice-Chancellor of McGill University, Montreal, Canada.

Twenty-one years have passed away since the last meeting of the British Association in this great central city of England. At the third Birmingham meeting—that of 1865—I had the pleasure of being present, and had the honour of being one of the Vice-Presidents of the Geological Section. At that meeting, my friend John Phillips, one of the founders of the Association, occupied the Presidential chair, and I cannot better introduce what I have to say this evening than by quoting the eloquent words with which he then opened his address :—'Assembled for the third time in this busy centre of industrious England, amid the roar of engines and clang of hammers, where the strongest powers of nature are trained to work in the fairy chains of art, how softly and fittingly falls upon the ear the accent of science, the friend of that art, and the guide of that industry! Here where Priestley analysed the air, and Watt obtained the mastery over steam, it well becomes the students of nature to gather round the standard which they carried so far into the fields of knowledge. And when on other occasions we meet in quiet colleges and academic halls, how gladly welcome is the union of fresh discoveries and new inventions with the solid and venerable truths which are there treasured and taught. Long may such union last; the fair alliance of cultivated thought and practical skill; for by it, labour is dignified and science fertilised, and the condition of human society exalted.' These were the words of a man who, while earnest in the pursuit of science, was full of broad and kindly sympathy for his fellow-men and of hopeful confidence in the future. We have but to turn to the twenty Reports of this Association, issued since 1865, to see the realisation of that union of science and art to which he so confidently looked forward, and to appreciate the stupendous results which it has achieved. In one department alone—that to which my predecessor in this chair so eloquently adverted in Aberdeen, the department of education in science—how much has been accomplished since 1865. Phillips himself lived to see a great revolution in this respect at Oxford. But no one in 1865 could have anticipated that immense development of local schools of science of which your own Mason College and your admirable technical, industrial, and art schools are eminent examples. Based on the general education given by the new system of Board schools, with which the name of the late W. E. Forster will ever be honourably connected, and extending its influence upward to special training, and to the highest university examinations, this new scientific culture is opening paths of honourable ambition to the men and women of England scarcely dreamed of in 1865. I sympathise with the earnest appeal of Sir Lyon Playfair, in his Aberdeen address in favour of scientific education; but visiting England at rare intervals, I am naturally more impressed with the progress that has been made than with the vexatious delays which have occurred, and I am perhaps better able to appreciate the vast strides that have been taken in the direction of that complete and all-pervading culture in science which he has so ably advocated.

No one could have anticipated twenty years ago that a Birmingham manufacturer, in whose youthful days there were no schools of science for the people, was about to endow a college, not only worthy of this great city, but one of its brightest ornaments.[1] Nor could any-one have foreseen the great development of local scientific societies, like your Midland Institute and Philosophical Society, which are now flourishing in every large town and in many of those of less magnitude. The period of twenty-one years that has elapsed since the last Birmingham meeting has also been an era of public museums and laboratories for the teaching of science, from the magnificent national institutions at South Kensington and those of the great universities and their colleges down to those of the schools and field clubs in country towns. It has, besides, been an era of gigantic progress in original work, and in publication,—a progress so rapid that workers in every branch of study have been reluctantly obliged to narrow more and more their range of reading, and of effort to keep abreast of the advance in their several departments. Lastly, these twenty-one years have been characterised as the 'coming of age' of that great system of philosophy with which the names of three Englishmen, Darwin, Spencer, and Wallace, are associated as its founders. Whatever opinions one may entertain as to the sufficiency and finality of that philosophy, there can be no question as to its influence on scientific thought. On the one hand, it is inaccurate to compare it with things so entirely different as the discovery of the chemical elements and of the law of gravitation. On the other, it is scarcely fair to characterise it as a mere 'confused development' of the mind of the age. It is indeed a new attempt of science in its maturer years to

---

[1] It was in 1865 that Sir Josiah Mason was, quietly and without any public note, beginning to lay the foundation of his orphanage at Erdington.

grapple with those mysterious questions of origins which occupied it in the days of its infancy, and it is to be hoped that it may not, like the Titans of ancient fable, be hurled back from heaven, or like the first mother find the knowledge to which it aspires a bitter thing. In any case we should fully understand the responsibility which we incur when in these times of full-grown science we venture to deal with the great problem of origins, and should be prepared to find that in this field, the new philosophy, like those which have preceded it, may meet with very imperfect success. The agitation of these subjects has already brought science into close relations, sometimes friendly, sometimes hostile, it is to be hoped in the end helpful, with those great and awful questions of the ultimate destiny of humanity, and of its relations to its Creator, which must always be nearer to the human heart than any of the achievements of science on its own ground. In entering on such questions, we should proceed with caution and reverence, feeling that we are on holy ground; and that though, like Moses of old, we may be armed with all the learning of our time, we are in the presence of that which, while it burns, is not consumed; a mystery which neither observation, experiment, nor induction can ever fully solve.

In a recent address, the late President of the Royal Society called attention to the fact that within the lifetime of the older men of science of the present day, the greater part of the vast body of knowledge included in the modern sciences of physics, chemistry, biology, and geology, has been accumulated, and the most important advances made in its application to such common and familiar things as the railway, ocean navigation, the electric telegraph, electric lighting, the telephone, the germ theory of disease, the use of anæsthetics, the processes of metallurgy, and the dyeing of fabrics. Even since the last meeting in this city, much of this great work has been done, and has led to general results of the most marvellous kind. What at that time could have appeared more chimerical than the opening up, by the enterprise of one British colony, of a shorter road to the east by way of the extreme west, realizing what was happily called by Milton and Cheadle 'the new North-west Passage,' making Japan the next neighbour of Canada on the west, and offering to Britain a new way to her Eastern possessions; or than the possibility of this Association holding a successful meeting on the other side of the Atlantic? To have ventured to predict such things in 1865, would have appeared quite visionary, yet you are now invited to meet in Australia, and may proceed thither by the Canadian Pacific Railway and its new lines of steamers, returning by the Suez Canal.[1] To-day this is quite as feasible as the Canadian visit would have been in 1865. It is science which has thus brought the once widely separated parts of the world nearer to each other, and which is breaking down those geographical barriers which have separated the different portions of our widely extended British race. Its work in this is not yet complete. Its goal to-day is its starting-point to-morrow. It is as far as at any previous time from seeing the limit of its conquests; and every victory gained is but the opening of the way for a farther advance.

By its visit to Canada, the British Association has asserted its imperial character, and has consolidated the scientific interests of Her Majesty's dominions, in advance of that great gathering of the industrial products of all parts of the empire now on exhibition in London, and in advance of any political plans of imperial federation. There has even been a project before us for an international scientific convention, in which the great English republic of America shall take part, a project, the realisation of which was to some extent anticipated in the fusion of the members of the British and American Associations at Montreal and Philadelphia in 1884. As a Canadian, as a past President of the American Association, and now honoured with the Presidency of this Association, I may be held to represent in my own person the scientific union of the British Islands, of the various Colonies and of the great Republic, which, whatever the difficulties attending its formal accomplishment at present,

---

[1] It is expected that, on the completion of the connections of the Canadian Pacific Railway, the time from ocean to ocean may be reduced to 116 hours, and from London to Hong Kong to twenty-seven days.

is certain to lead to an actual and real co-operation in scientific work. In furtherance of this, I am glad to see here to-day influential representatives of most of the British Colonies, of India, and of the United States. We welcome here, also, delegates from other countries; and though the barrier of language may at present prevent a larger union, we may entertain the hope that Britain, America, India, and the Colonies, working together in the interest of science, may ultimately render our English tongue the most general vehicle of scientific thought and discovery, a consummation of which I think there are, at present, many indications.

But, while science marches on from victory to victory, its path is marked by the resting-places of those who have fought its battles and assured its advance. In looking back to 1865, there rise before me the once familiar countenances of Phillips, Murchison, Lyell, Forbes, Jeffreys, Jukes, Rolleston, Miller, Spottiswoode, Fairbairn, Gassiot, Carpenter, and a host of others, present in full vigor at that meeting, but no more with us. These were veterans of science; but, alas! many then young and rising in fame are also numbered with the dead. It may be that before another Birmingham meeting, many of us, the older members now, will have passed away. But these men have left behind them ineffaceable monuments of their work, in which they still survive, and we rejoice to believe that, though dead to us, they live in the company of the great and good of all ages who have entered into the unseen universe where all that is high and holy and beautiful, must go on accumulating till the time of the restitution of all things. Let us follow their example and carry on their work, as God may give us power and opportunity, gathering precious stores of knowledge and of thought, in the belief that all truth is immortal, and must go on for ever bestowing blessings on mankind. Thus will the memory of the mighty dead remain to us as a power which—

> "Like a star
> Beacons from the abode where the eternal are."

I do not wish, however, to occupy your time longer with general or personal matters, but rather to take the opportunity afforded by this address to invite your attention to some topics of scientific interest. In attempting to do this, I must have before me the warning conveyed by Professor Huxley, in the address to which I have already referred, that in our time, science, like Tarpeia, may be crushed with the weight of the rewards bestowed on her. In other words, it is impossible for any man to keep pace with the progress of more than one limited branch of science; and it is equally impossible to find an audience of scientific men of whom anything more than a mere fraction can be expected to take an interest in any one subject. There is, however, some consolation in the knowledge that a speaker who is sufficiently simple for those who are advanced specialists in other departments, will of necessity be also sufficiently simple to be understood by the general public who are specialists in nothing. On this principle, a geologist of the old school, accustomed to a great variety of work, may hope so to scatter his fire as to reach the greater part of the audience. In endeavouring to secure this end, I have sought inspiration from that ocean which connects rather than separates Britain and America, and may almost be said to be an English sea—the North Atlantic. The geological history of this depression of the earth's crust, and its relation to the continental masses which limit it, may furnish a theme at once generally intelligible and connected with great questions as to the structure and history of the earth, which have excited the attention alike of the physicists, geologists, biologists, geographers and ethnologists. Should I, in treating of these questions, appear to be somewhat abrupt and dogmatic, and to indicate rather than state the evidence of the general views announced, I trust you will kindly attribute this to the exigencies of a short address.

If we imagine an observer contemplating the earth from a convenient distance in space, and scrutinizing its features as it rolls before him, we may suppose him to be struck with the fact that eleven-sixteenths of its surface are covered with water, and that the land is so unequally distributed that from one point of view he would see a hemisphere almost exclusively oceanic, while nearly the whole of the dry land is gathered in the opposite hemisphere. He might observe that the great oceanic area of the Pacific and Antarctic Oceans is dotted with islands—like a shallow pool with stones rising above its surface—as if its general depth were small in comparison with its area. He might also notice that a mass or belt of land surrounds each pole, and that the northern ring sends off to the southward three vast tongues of land and of mountain chains, terminating respectively in South America, South Africa, and Australia, towards which feebler and insular processes are given off by the Antarctic continental mass. This, as some geographers have observed,[1] gives a rudely three-ribbed aspect to the earth, though two of the ribs are crowded together and form the Europ-asian mass or double continent, while the third is isolated in the single continent of America. He might also observe that the northern girdle is cut across, so that the Atlantic opens by a wide space into the Arctic Sea, while the Pacific is contracted toward the north, but confluent with the Antarctic Ocean. The Atlantic is also relatively deeper and less cumbered with islands than the Pacific, which has the higher ridges near its shores, constituting what some visitors to the Pacific coast of America have not inaptly called the 'back of the world,' while the wider slopes face the narrower ocean, into which, for this reason, the greater part of the drainage of the land is poured.[2] The Pacific and Atlantic, though both depressions or flattenings of the earth, are, as we shall find, different in age, character, and conditions; and the Atlantic, though the smaller, is the older, and, from the geological point of view, in some respects, the more important of the two.

If our imaginary observer had the means of knowing anything of the rock formations of the continents, he would notice that those bounding the North Atlantic are, in general, of great age—some belonging to the Laurentian system.

---

[1] Dana, *Manual of Geology*, introductory part. Green, *Vestiges of a Molten Globe*, has summed up these facts.

[2] Mr. Mellard Reade, in two Presidential addresses before the Geological Society of Liverpool, has illustrated this point and its geological consequences.

On the other hand, he would see that many of the mountain ranges along the Pacific are comparatively new, and that modern igneous action occurs in connection with them. Thus he might be led to believe that the Atlantic, though comparatively narrow, is an older feature of the earth's surface; while the Pacific belongs to more modern times. But he would note, in connection with this, that the oldest rocks of the great continental masses are mostly toward their northern ends; and that the borders of the northern ring of land, and certain ridges extending southward from it, constitute the most ancient and permanent elevations of the earth's crust, though now greatly surpassed by mountains of more recent age nearer the equator.

Before leaving this general survey we may make one further remark. An observer, looking at the earth from without, would notice that the margins of the Atlantic and the main lines of direction of its mountain chains are north-east and south-west, and north-west and south-east, as if some early causes had determined the occurrence of elevations along great circles of the earth's surface tangent to the polar circles.

We are invited by the preceding general glance at the surface of the earth to ask certain questions respecting the Atlantic. (1) What has at first determined its position and form? (2) What changes has it experienced in the lapse of geological time? (3) What relations have these changes borne to the development of life on the land and in the water? (4) What is its probable future?

Before attempting to answer these questions, which I shall not take up formally in succession, but rather in connection with each other, it is necessary to state, as briefly as possible, certain general conclusions respecting the interior of the earth. It is popularly supposed that we know nothing of this beyond a superficial crust perhaps averaging 50,000 to 100,000 feet in thickness. It is true we have no means of exploration in the earth's interior, but the conjoined labours of physicists and geologists have now proceeded sufficiently far to throw much inferential light on the subject, and to enable us to make some general affirmations with certainty; and these it is the more necessary to state distinctly, since they are often treated as mere subjects of speculation and fruitless discussion.

(1) Since the dawn of geological science, it has been evident that the crust on which we live must be supported on a plastic or partially liquid mass of heated rock, approximately uniform in quality under the whole of its area. This is a legitimate conclusion from the wide distribution of volcanic phenomena, and from the fact that the ejections of volcanoes, while locally of various kinds, are similar in every part of the world. It led to the old idea of a fluid interior of the earth, but this is now generally abandoned, and this interior heated and plastic layer is regarded as merely an under-crust.

(2) We have reason to believe, as the result of astronomical investigations,[1] that, notwithstanding the plasticity or liquidity of the under-crust, the mass of the earth—its nucleus as we may call it—is practically solid and of great density and hardness. Thus we have the apparent paradox of a solid yet fluid earth; solid in its astronomical relations, liquid or plastic for the purposes of volcanic action and superficial movements.[2]

(3) The plastic sub-crust is not in a state of dry igneous fusion, but in that condition of aqueo-igneous or hydrothermic fusion which arises from the action of heat on moist substances, and which may either be regarded as a fusion or as a species of solution at a very high temperature. This we learn from the phenomena of volcanic action, and from

---

[1] Hopkins, Mallet, Sir William Thomson, and Prof. G. H. Darwin maintain the solidity and rigidity of the earth on astronomical grounds; but different conclusions have been reached by Hennesey, Delaunay, and Airy. In America, it was taught from 1858 by Sterry Hunt, and later by Shaler and Le Conte.

[2] An objection has been taken to the effect that the supposed ellipsoidal form of the equator is inconsistent with a plastic subcrust. But this ellipsoidal form is not absolutely certain, or, if it exists, is very minute. Bonney has, in a recent lecture, suggested the important consideration that a mass may be slowly mobile under long-continued pressure, while yet rigid with reference to more sudden movements.

the composition of the volcanic and plutonic rocks, as well as from such chemical experiments as those of Daubrée and of Tilden and Shenstone.[1]

(4) The interior sub-crust is not perfectly homogeneous, but may be roughly divided into two layers or magmas, as they have been called: an upper, highly siliceous or acidic, of low specific gravity and light-coloured, and corresponding to such kinds of plutonic and volcanic rocks as granite and trachyte; and a lower, less siliceous or more basic, more dense, and more highly charged with iron, and corresponding to such igneous rocks as the dolerites, basalts, and kindred lavas. It is interesting here to note that this conclusion, elaborated by Durocher and von Waltershausen, and usually connected with their names, appears to have been first announced by John Phillips, in his 'Geological Manual,' and as a mere common sense deduction from the observed phenomena of volcanic action and the probable results of the gradual cooling of the earth.[2] It receives striking confirmation from the observed succession of acidic and basic volcanic rocks of all geological periods and in all localities. It would even seem, from recent spectroscopic investigations of Lockyer, that there is evidence of a similar succession of magmas in the heavenly bodies, and the discovery by Nordenskiöld of native iron in Greenland basalts, affords a probability that the inner magma is in part metallic.[3]

(5) Where rents or fissures form in the upper crust, the material of the lower crust is forced upward by the pressure of the less supported portions of the former, giving rise to

[1] *Phil. Trans.* 1884. Also Crosby in *Proc. Boston Soc. Nat. Hist.* 1883.

[2] Phillips, *Manual of Geology*, 1855, p. 493. Dr. Sterry Hunt has kindly directed my attention to the fact of Phillip's right of priority in this matter. Durocher in 1857 elaborated the theory of magmas in the *Annales des Mines*, and we are indebted to Dutton, of the United States Geological Survey, for its detailed application to the remarkable volcanic outflows of Western America.

[3] These basalts occur at Ovifak, Greenland. Andrews has found small particles of iron in British basalts. Prestwich and Judd have referred to the bearing on general geology of these facts, and of Lockyer's suggestions.

volcanic phenomena either of an explosive or quiet character, as may be determined by contact with water. The underlying material may also be carried to the surface by the agency of heated water, producing those quiet discharges which Hunt has named crenitic. It is to be observed here that explosive volcanic phenomena, and the formation of cones, are, as Prestwich has well remarked, characteristic of an old and thickened crust; quiet ejection from fissures and hydro-thermal action may have been more common in earlier periods and with a thinner over-crust.

(6) The contraction of the earth's interior by cooling and by the emission of material from below the over-crust, has caused this crust to press downward, and therefore laterally, and so to effect great bends, folds, and plications; and these, modified subsequently by surface denudation, constitute mountain chains and continental plateaus. As Hall long ago pointed out,[1] such lines of folding have been produced more especially where thick sediments had been laid down on the sea bottom. Thus we have here another apparent paradox, namely, that the elevations of the earth's crust occur in the places where the greatest burden of detritus has been laid down upon it, and where consequently the crust has been softened and depressed. We must beware, in this connection, of exaggerated notions of the extent of contraction and of crumpling required to form mountains. Bonney has well shown, in lectures delivered at the London Institution, that an amount of contraction, almost inappreciable in comparison with the diameter of the earth, would be sufficient; and that as the greatest mountain chains are less than $\frac{1}{600}$th of the earth's radius in height, they would, on an artificial globe a foot in diameter, be no more important than the slight inequalities that might result from the paper gores overlapping each other at the edges.

(7) The crushing and sliding of the over-crust implied in these movements raise some serious questions of a physical

---

[1] Hall, (American Association Address, 1857, subsequently republished, with additions, as *Contributions to the Geological History of the American Continent,*) Mallet, Rogers, Dana, Le Conte, &c.

character. One of these relates to the rapidity or slowness of such movements, and the consequent degree of intensity of the heat developed, as a possible cause of metamorphism of rocks. Another has reference to the possibility of changes in the equilibrium of the earth itself as resulting from local collapse and ridging. These questions in connection with the present dissociation of the axis of rotation from the magnetic poles, and with changes of climate, have attracted some attention,[1] and probably deserve further consideration on the part of physicists. In so far as geological evidence is concerned, it would seem that the general association of crumpling with metamorphism indicates a certain rapidity in the process of mountain-making, and consequent development of heat; and the arrangement of the older rocks around the Arctic basin forbids us from assuming any extensive movement of the axis of rotation, though it does not exclude changes to a limited extent. I hope that Professor Darwin will discuss these points in his address to the Physical Section.

I wish to formulate these principles as distinctly as possible, and as the result of all the long series of observations, calculations, and discussions since the time of Werner and Hutton, and in which a vast number of able physicists and naturalists have borne a part, because they may be considered as certain deductions from our actual knowledge, and because they lie at the foundation of a rational physical geology.

We may popularise these deductions by comparing the earth to a drupe or stone-fruit, such as a plum or peach, somewhat dried up. It has a large and intensely hard stone and kernel, a thin pulp made up of two layers, an inner more dense and dark-coloured, and an outer less dense and lighter-coloured. These constitute the under-crust. On the outside it has a thin membrane or over-crust. In the process of drying it has slightly shrunk, so as to produce ridges and hollows of the outer crust, and this outer crust has

---

[1] See recent papers of Oldham and Fisher, in *Geological Magazine* and *Philosophical Magazine*, July 1886. Also Péroche, *Revol. Polaires.* Paris, 1886.

cracked in some places, allowing portions of the pulp to ooze out—in some of these its lower dark substance, in others its upper and lighter material. The analogy extends no farther, for there is nothing in our withered fruit to represent the oceans occupying the lower parts of the surface or the deposits which they have laid down.

Keeping in view these general conclusions, let us now turn to their bearing on the origin and history of the North Atlantic.

Though the Atlantic is a deep ocean, its basin does not constitute so much a depression of the crust of the earth as a flattening of it, and this, as recent soundings have shown, with a slight ridge or elevation along its middle, and banks or terraces fringing the edges, so that its form is not so much that of a basin as that of a shallow plate with its middle a little raised. Its true permanent margins are composed of portions of the over-crust folded, ridged up and crushed, as if by lateral pressure emanating from the sea itself. We cannot, for example, look at a geological map of America without perceiving that the Appalachian ridges, which intervene between the Atlantic and the St. Lawrence valley, have been driven bodily back by a force acting from the east, and that they have resisted this pressure only where, as in the Gulf of St. Lawrence and the Catskill region of New York, they have been protected by outlying masses of very old rocks, as, for example, by that of the island of Newfoundland and that of the Adirondack Mountains. The admirable work begun by my friend and fellow-student Professor James Nicol, followed up by Hicks, Lapworth, and others, and now, after long controversy, fully confirmed by the recent observations of the geological survey of Scotland, has shown the most intense action of the same kind on the east side of the ocean in the Scottish highlands; and the more widely distributed Eozoic rocks of Scandinavia may be appealed to in further evidence of this.[1]

---

[1] Address to the Geological Section, by Prof. Judd, Aberdeen Meeting, 1885. According to Rogers, the crumpling of the Appalachians has reduced a breadth of 158 miles to about 60.

If we now inquire as to the cause of the Atlantic depression, we must go back to the time when the areas occupied by the Atlantic and its bounding coasts were parts of the shoreless sea in which the earliest gneisses or stratified granites of the Laurentian age were being laid down in vastly extended beds. These ancient crystalline rocks have been the subject of much discussion and controversy, and as they constitute the lowest and probably the firmest part of the Atlantic sea-bed, it is necessary to inquire as to their origin and history. Dr. Bonney, the late President of the Geological Society, in his Anniversary address, and Dr. Sterry Hunt, in an elaborate paper communicated to the Royal Society of Canada, have ably summed up the hypotheses as to the origin of the oldest Laurentian beds. At the basis of these hypotheses lies the admission that the immensely thick beds of orthoclase gneiss, which are the oldest stratified rocks known to us, are substantially the same in composition with the upper or silicious magma or layer of the under-crust. They are, in short, its materials either in their primitive condition or merely re-arranged. One theory considers them as original products of cooling, owing their lamination merely to the successive stages of the process. Another view refers them to the waste and re-arrangement of the materials of a previously massive granite. Still another holds that all our granites really arise from the fusion of old gneisses of originally aqueous origin, while a fourth refers the gneisses themselves to molecular changes effected in granite by pressure. These several views, in so far as they relate to the oldest or fundamental Laurentian gneiss, may be arranged under the following heads: (1) *Endoplutonic*, or that which regards all the old gneisses as molten rocks cooled from without inward, in successive layers.[1] (2) *Exoplutonic*, or that which considers them as made up of matter ejected from below the upper crust in the manner of volcanic action[2]. (3) *Metamorphic*, which supposes the old gneisses to arise from the crystallisation of detrital matter spread over the sea-bottom, and either igneous or derived from the decay of

---

[1] Naumann, Phillips, Durocher, Macfarlane, &c.

[2] Clarence King, Tornebohm, Marr. &c.

igneous rocks.[1] (4). *Chaotic* or *Thermo-chaotic*, or the theory of deposit from the turbid waters of a primeval ocean either with or without the aid of heat. In one form this was the old theory of Werner.[2] (5) *Crenitic* or Hydro-thermic, which supposes the action of heated waters, penetrating below the crust, to be constantly bringing up to the surface mineral matters in solution and depositing these so as to form felspathic and other rocks.[3]

It will be observed, in regard to these theories, that they do not suppose that the old gneiss is an ordinary sediment, but that all regard it as formed in exceptional circumstances, these circumstances being the absence of land and of subaërial decay of rock, and the presence wholly or principally of the material of the upper surface of the recently hardened crust. This being granted, the question arises, ought we not to combine these several theories and to believe that the cooling crust has hardened in successive layers from without inward; that at the same time fissures were locally discharging igneous matter to the surface; that matter held in suspension in the ocean and matter held in solution by heated waters rising from beneath the outer crust were mingling their materials in the deposits of the primitive ocean? It would seem that the combination of all these agencies may safely be evoked as causes of the pre-Atlantic deposits. This is the eclectic position which I endeavoured to maintain in my address before the Minneapolis Meeting of the American Association in 1883, and which I still hold to be in every way probable.

A word here as to metamorphism, a theory which, like many others, has been first run to death and then discredited, but which, owing to the moderate degree in which it was originally held by Lyell, is still valid. Nothing can be more certain than that the composition of the Laurentian gneisses forbids us to suppose that they can be ordinary sediments metamorphosed. They are rocks peculiar in their origin, and not paralleled, unless exceptionally, in

---

[1] Lyell, Kopp, Reusch, Judd, &c.

[2] Scrope, De LaBeche, Daubrée.

[3] Hunt, Transactions Royal Society of Canada, 1885.

later times. On the other hand, they have undoubtedly experienced very important changes, more especially as to crystallization, the state of combination of their ingredients, and the development of disseminated minerals;[1] and while this may in part be attributed to the mechanical pressure to which they have been subjected, it requires also the action of hydrothermic agencies. Any theory which fails to invoke both of these kinds of force must necessarily be partial and imperfect.

But all metamorphic rocks are not of the same character with the gneisses of the Lower Laurentian. Even in the Middle and Upper Laurentian, we have metamorphic rocks, *e. g.* quartzite and limestone, which must originally have been ordinary aqueous deposits. Still more, in the succeeding Huronian and its associated series of beds, and in the Lower Palæozoic, local metamorphic change has been undergone by rocks quite similar to those which in their unaltered state constitute regular sedimentary deposits. In the case of these later rocks it is to be borne in mind that, while some may have been of volcanic origin, others may have been sediments rich in undecomposed fragments of silicates. It is a mistake to suppose that the ordinary decay of stratified siliceous rocks is a process of kaolinization so perfect as to eliminate all alkaline matters. On the contrary, the fact, which Judd has recently well illustrated in the case of the mud of the Nile, applies to a great number of similar deposits in all parts of the world, and shows that the finest sediments have not always been so completely lixiviated as to be destitute of the basic matters necessary for their conversion into gneiss, mica-schist, and similar rocks, when the necessary agencies of metamorphism are applied to them, and this quite independently of any other extraneous matters introduced into them by water or otherwise. Still it must

---

[1] The first of these is what Bonney has called *Metastasis*. The second and third come under the name *Metacrasis*. *Methylosis*, or change of substance, is altogether exceptional, and not to be credited, except on the best evidence, or in cases where volatile matters have been expelled, as in the change of hæmatite into magnetite, or of bituminous coal into anthracite.

be steadily kept in view that many of the old pre-Cambrian crystalline rocks must have been different originally from those succeeding them, and that, consequently, these last, even when metamorphosed, present different characters.

I may remark here that, though a palæontologist rather than a lithologist, it gives me great pleasure to find so much attention now given in this country to the old crystalline rocks, and to their study microscopically and chemically as well as in the field, a work in which Sorby and Allport were pioneers. As a pupil of the late Professor Jameson of Edinburgh, my own attention was early attracted to the study of minerals and rocks as the stable foundations of geological science; and so far back as 1841 I had learnt of the late Mr. Sanderson, of Edinburgh, who worked at Nicol's sections,[1] how to slice rocks and fossils; and since that time I have been in the habit of examining everything with the microscope. The modern developments in this direction are therefore very gratifying to me, even though, as is natural, they sometimes appear to be pushed too far or their value over-estimated.

That the older gneisses were deposited, not only in what is now the bed of the Atlantic, but also on the great continental areas of America and Europe, anyone who considers the wide extent of these rocks represented on the map recently published by Professor Hull can readily understand.[2] It is true that Hull supposes that the basin of the Atlantic itself may have been land at this time, but there is no necessity for holding this view, more especially as the material of the gneiss could not have been detritus derived from sub-aërial decay of rock.

Let us suppose, then, the floor of old ocean covered with a flat pavement of gneiss, or of that material which is now gneiss, the next question is how and when did this original bed become converted into sea and land. Here we have some things certain, others most debateable. That the cooling mass, especially if it was sending out volumes of softened rocky material, either in the exoplutonic or in the crenitic

---

[1] *Trans. Royal Irish Academy.* [2] And I believe at Witham's also.

way, and piling this on the surface, must soon become too small for its shell, is apparent; but when and where would the collapse, crushing, and wrinkling inevitable from this cause begin? When they began is indicated by the lines of mountain-chains which traverse the Laurentian districts; but the reason why is less apparent. The more or less unequal cooling, hardening and conductive power of the outer crust we may readily assume. The driftage unequally of water-borne detritus to the south-west by the bottom currents of the sea is another cause, and, as we shall soon see, most effective. Still another is the greater cooling and hardening of the crust in the polar regions, and the tendency to collapse of the equatorial protuberance from the slackening of the earth's rotation. Besides these, the internal tides of the earth's substance at the times of solstice would exert an oblique pulling force on the crust, which might tend to crack it along diagonal lines. From whichever of these causes or the combination of the whole, we know that, within the Laurentian time, folded portions of the earth's crust began to rise above the general surface, in broad belts running from N.E. to S.W., and from N.W to S.E., where the older mountains of Eastern America and Western Europe now stand, and that the subsidence of the oceanic areas, allowed by this crumpling of the crust, permitted other areas on both sides of the Atlantic to form limited table-lands.[1] This was the commencement of a process repeated again and again in subsequent times,and which began in the middle Laurentian, when for the first time we find beds of quartzite, limestone, and iron ore,and graphitic beds, indicating that there was already land and water, and that the sea, and perhaps the land, swarmed with forms of animal and plant life, unknown, for the most part, now. Independently of the questions as to the animal nature of Eozoon, I hold that we know, as certainly as we can know anything

---

[1] Daubrée's curious experiments on the contraction of caoutchouc balloons, partially hardened by coating with varnish, show how small inequalities of the crust, from whatever cause arising, might effect the formation of wrinkles, and also that transverse as well as longitudinal wrinkling might occur.

inferentially, the existence of these primitive forms of life. If I were to conjecture what were the early forms of plant and animal life, I would suppose that, just as in the Palæozoic, the acrogens culminated in gigantic and complex forest trees, so in the Laurentian, the algæ, the lichens, and the mosses grew to dimensions and assumed complexity of structure unexampled in later times, and that, in the sea, the humbler forms of Protozoa and Hydrozoa were the dominant types, but in gigantic and complex forms. The land of this period was probably limited, for the most part, to high latitudes, and its aspect, though more rugged and abrupt, and of greater elevation, must have been of that character which we still see in the Laurentian hills. The distribution of this ancient land is indicated by the long lines of old Laurentian rock extending from the Labrador coast and the north shore of the St. Lawrence, and along the eastern slopes of the Appalachians in America, and the like rocks of the Hebrides, the Western Highlands, and the Scandinavian mountains. A small but interesting remnant is that in the Malvern Hills, so well described by Holl. It will be well to note here and to fix on our minds, that these ancient ridges of Eastern America and Western Europe have been greatly denuded and wasted since Laurentian times, and that it is along their eastern sides that the greatest sedimentary accumulations have been deposited.

From this time dates the introduction of that dominance of existing causes which forms the basis of uniformitarianism in geology, and which had to go on with various and great modifications of detail, through the successive stages of the geological history, till the land and water of the northern hemisphere attained to their present complex structure.

So soon as we have a circumpolar belt or patches of Eozoic[1] land and ridges running southward from it, we enter on new and more complicated methods of growth of the continents and seas. Portions of the oldest crystalline rocks, raised out of the protecting water, were now eroded by atmospheric agents, and especially by the car-

---

[1] Or Archæan, or pre-Cambrian, if these terms are preferred.

bonic acid, then existing in the atmosphere perhaps more abundantly than at present, under whose influence the hardest of the gneissic rocks gradually decay. The Arctic lands were subjected in addition to the powerful mechanical force of frost and thaw. Thus every shower of rain and every swollen stream would carry into the sea the products of the waste of land, sorting them into fine clays and coarser sands; and the cold currents which cling to the ocean bottom, now determined in their courses, not merely by the earth's rotation, but also by the lines of folding on both sides of the Atlantic, would carry south-westward, and pile up in marginal banks of great thickness, the *débris* produced from the rapid waste of the land already existing in the Arctic regions. The Atlantic, opening widely to the north, and having large rivers pouring into it, was, especially, the ocean characterised, as time advanced, by the prevalence of these phenomena. Thus throughout the geological history it has happened that, while the middle of the Atlantic has received merely organic deposits of shells of Foraminifera and similar organisms, and this probably only to a small amount, its margins have had piled upon them beds of detritus of immense thickness. Professor Hall, of Albany, was the first geologist who pointed out the vast cosmic importance of these deposits, and that the mountains of both sides of the Atlantic owe their origin to these great lines of deposition, along with the fact, afterwards more fully insisted on by Rogers, that the portions of the crust which received these masses of *débris* became thereby weighted down and softened, and were more liable than other parts to lateral crushing.[1]

[1] The connection of accumulation with subsidence was always a familiar consideration with geologists; but Hall seems to have been the first to state its true significance as a geological factor, and to see that those portions of the crust, which are weighted down by great detrital accumulations, are necessarily those which, in succeeding movements, were elevated into mountains. Other American geologists, as Dana, Rogers, Hunt, Le Conte, Crosby, &c., have followed up Hall's primary suggestion, and in England, Hicks, Fisher, Starkie Gardiner, Hull, and others, have brought it under notice, and it enters into the great generalisations of Lyell on these subjects.

Thus in the later Eozoic and early Palæozoic times, which succeeded the first foldings of the oldest Laurentian, great ridges were thrown up, along the edges of which were beds of limestone, and on their summits and sides, thick masses of ejected igneous rocks. In the bed of the central Atlantic, there are no such accumulations. It must have been a flat, or slightly ridged, plate of the ancient gneiss, hard and resisting, though perhaps with a few cracks, through which igneous matter welled up, as in Iceland and the Azores in more modern times. In this condition of things we have causes tending to perpetuate and extend the distinctions of ocean and continent, mountain and plain, already begun; and of these we may more especially note the continued subsidence of the areas of greatest marine deposition. This has long attracted attention, and affords very convincing evidence of the connection of sedimentary deposit as a cause with the subsidence of the crust.[1]

We are indebted to a French physicist, M. Faye,[2] for an important suggestion on this subject. It is that the sediment accumulated along the shores of the ocean presented an obstacle to radiation, and consequently to cooling of the crust, while the ocean floor, unprotected and unweighted, and constantly bathed with currents of cold water having great power of convection of heat, would be more rapidly cooled, and so would become thicker and stronger. This suggestion is complementary to the theory of Professor Hall, that the areas of greatest deposit on the margins of

---

[1] Dutton in *Report of U.S. Geological Survey*, 1881. From facts stated in this report and in my *Acadian Geology*, it is apparent that in the Western States and in the coalfields of Nova Scotia, shallow-water deposits have been laid down, up to thicknesses of 10,000 to 20,000 feet in connection with continuous subsidence. See also a paper by Ricketts in the *Geol. Mag.* 1883. It may be well to add here that this doctrine of the subsidence of wide areas being caused by deposition, does not justify the conclusion of certain glacialists that snow and ice have exercised a like power in glacial periods. In truth, as will appear in the sequel, great accumulations of snow and ice require to be preceded by subsidence, and wide continental areas can never be covered with deep snow, while, of course, ice can cause no addition of weight to submerged areas.

[2] *Revue Scientifique*, 1886.

the ocean are necessarily those of greatest folding and consequent elevation. We have thus a hard, thick, resisting ocean-bottom which, as it settles down toward the interior, under the influence of gravity, squeezes upward and folds and plicates all the soft sediments deposited on its edges. The Atlantic area is almost an unbroken cake of this kind. The Pacific area has cracked in many places, allowing the interior fluid matter to exude in volcanic ejections.

It may be said that all this supposes a permanent continuance of the ocean-basins, whereas many geologists postulate a mid-Atlantic continent[1] to give the thick masses of detritus found in the older formations both in Eastern America and Western Europe, and which thin off in proceeding into the interior of both continents. I prefer, with Hall, to consider these belts of sediment as, in the main, the deposits of northern currents, and derived from Arctic land, and that like the great banks of the American coast at the present day, which are being built up by the present Arctic current, they had little to do with any direct drainage from the adjacent shore. We need not deny, however, that such ridges of land as existed along the Atlantic margins were contributing their quota of river-borne material, just as on a still greater scale the Amazon and Mississippi are doing now, and this especially on the sides toward the present continental plateaus, though the greater part must have been derived from the wide tracts of Laurentian land within the Arctic Circle or near to it. It is further obvious that

---

[1] Among American geologists, Dana and Le Conte, though from somewhat different premises, maintain continental permanence. Crosby has argued on the other side. In Britain, Hull has elaborated the idea of interchange of oceanic and continental areas in his memoir in *Trans. Dublin Society*, and in his work entitled *The Physical History of the British Islands*. Godwin-Austin argues powerfully for the permanence of the Atlantic basin, *Q. J. Geol. Society*, vol. xii. p. 42. Mellard Reade ably advocates the theory of mutation. The two views require, in my judgment, to be combined. More especially it is necessary to take into the account the existence of an Atlantic ridge of Laurentian rock on the west side of Europe, of which the Hebrides and the oldest rocks of Wales, Ireland, Western France, and Portugal are remnants.

the ordinary reasoning respecting the necessity of continental areas in the present ocean basins would actually oblige us to suppose that the whole of the oceans and continents had repeatedly changed places. This consideration opposes enormous physical difficulties to any theory of alternations of the oceanic and continental areas, except locally at their margins. I would, however, refer you for a more full discussion of these points to the address to be delivered to-morrow by the President of the Geological Section.

But the permanence of the Atlantic depression does not exclude the idea of successive submergences of the continental plateaus and marginal slopes, alternating with periods of elevation, when the ocean retreated from the continents and contracted its limits. In this respect, the Atlantic of to-day is much smaller than it was in those times when it spread widely over the continental plains and slopes, and much larger than it has been in times of continental elevation. This leads us to the further consideration that, while the ocean-beds have been sinking, other areas have been better supported, and constitute the continental plateaus; and that it has been at or near the junctions of these sinking and rising areas that the thickest deposits of detritus, the most extensive foldings, and the greatest ejections of volcanic matter have occurred. There has thus been a permanence of the position of the continents and oceans throughout geological time, but with many oscillations of these areas, producing submergences and emergences of the land. In this way, we can reconcile the vast vicissitudes of the continental areas in different geological periods with that continuity of development from north to south, and from the interiors to the margins, which is so marked a feature. We have, for this reason, to formulate another apparent geological paradox, namely, that while, in one sense, the continental and oceanic areas are permanent, in another, they have been in continual movement. Nor does this view exclude extension of the continental borders or of chains of islands beyond their present limits, at certain periods; and indeed the general principle already stated, that subsidence of the ocean-bed has produced elevation of the land, implies in earlier periods a shallower ocean and many possibilities as to volcanic islands, and low continental margins creeping out into the sea; while it is also to be noted that there are, as already stated, bordering shelves, constituting shallows in the ocean, which at certain periods have emerged as land.

We are thus compelled to believe in the contemporaneous existence in all geological periods, except perhaps the earliest of them, of three distinct conditions of areas on the surface of the earth. (1) Oceanic areas of deep sea, which always continued to occupy in whole or in part the bed of the present ocean. (2) Continental plateaus and marginal shelves, existing as low flats or higher table-lands liable to periodical submergence and emergence. (3) Lines of plication and folding, more especially along the borders of the oceans, forming elevated portions of land, rarely altogether submerged and constantly affording the material of sedimentary accumulations, while they were also the seats of powerful volcanic ejections.

In the successive geological periods, the continental plateaus, when submerged, owing to their vast extent of warm and shallow sea, have been the great theatres of the development of marine life and of the deposition of organic limestones, and when elevated, they have furnished the abodes of the noblest land faunas and floras. The mountain belts, especially in the north, have been the refuge and stronghold of land life in periods of submergence; and the deep ocean basins have been the perennial abodes of pelagic and abyssal creatures, and the refuge of multitudes of other marine animals and plants in times of continental elevation. These general facts are full of importance with reference to the question of the succession of formations and of life in the geological history of the earth.

So much time has been occupied with these general views, that it would be impossible to trace the history of the Atlantic in detail through the ages of the Palæozoic, Mesozoic, and Tertiary. We may, however, shortly glance at the changes of the three kinds of surface already referred to. The bed of the ocean seems to have remained, on the whole, abyssal, but there were probably periods when those shallow reaches of the Atlantic which stretch across its most northern portion, and partly separate it from the Arctic basin, presented connecting coasts or continuous chains of islands sufficient to permit animals and plants to pass over.[1] At certain periods also there were, not unlikely, groups of volcanic islands, like the Azores, in the temperate or tropical Atlantic. More especially might this be the case in that early time when it was more like the present Pacific; and the line of the great volcanic belt of the Mediterranean, the mid Atlantic banks, the Azores and the West India Islands point to the possibility of such partial connections. These were stepping-stones, so to speak, over which land organisms might cross, and some of these may be connected with the fabulous or pre-historic Atlantis.[2]

In the Cambrian and Ordovician periods, the distinctions, already referred to, into continental plateaus, mountain ridges, and ocean depths, were first developed, and we find, already, great masses of sediment accumulating on the seaward sides of the old Laurentian ridges, and internal deposits thinning away from these ridges over the submerged continental areas, and presenting dissimilar conditions of sedimentation. It would seem also that, as Hicks has argued for Europe, and Logan and Hall for America, this Cambrian age was one of slow subsidence of the land previously elevated, accompanied with or caused by thick deposits of detritus

---

[1] It would seem, from Geikie's description of the Faroe Islands, that they may be a remnant of such connecting land, dating from the Cretaceous or Eocene period.

[2] Dr. Wilson has recently argued that the Atlantis of tradition was really America, and Mr. Hyde Clarke has associated this idea with the early dominance in western Europe of the Iberian race, which Dawkins connects with the Neolithic and Bronze ages of archæology. My own attention has recently been directed, through specimens presented to the McGill College Museum, by Mr. R. S. Haliburton, to the remarkable resemblance in cranial characters, wampum, and other particulars of the Guanches of the Canaries with aborigines of Eastern America—resemblances which cannot be accidental.

along the borders of the subsiding land, which was probably covered with the decomposing rock arising from long ages of sub-aerial waste.

In the coal-formation age, its characteristic swampy flats stretched in some places far into the shallower parts of the ocean.[1] In the Permian, the great plicated mountain margins were fully developed on both sides of the Atlantic. In the Jurassic, the American continent probably extended further to the sea than at present. In the Wealden age, there was much land to the west and north of Great Britain, and Professor Bonney has directed attention to the evidence of the existence of this land as far back as the Trias, while Mr. Starkie Gardiner has insisted on connecting links to the southward as evidenced by fossil plants. So late as the Post-glacial, or early human period, large tracts, now submerged, formed portions of the continents. On the other hand, the interior plains of America and Europe were often submerged. Such submergences are indicated by the great limestones of the Palæozoic, by the chalk and its representative beds in the Cretaceous, by the Nummulitic formation in the Eocene, and lastly by the great Pleistocene submergence, one of the most remarkable of all, one in which nearly the whole northern hemisphere participated, and which was probably separated from the present time by only a few thousands of years.[2] These submergences and elevations were not always alike on the two sides of the Atlantic. The Salina period of the Silurian, for example, and the Jurassic, show continental elevation in America not shared by Europe. The great subsidences of the Cretaceous and the Eocene were proportionally deeper and wider on the eastern continent, and this and the direction of the land being from north to south, cause more ancient forms of life to survive in America.

---

[1] I have shown the evidence of this in the remnants of Carboniferous districts once more extensive on the Atlantic coast of Nova Scotia and Cape Breton (*Acadian Geology.*)

[2] The recent surveys of the Falls of Niagara coincide with a great many evidences to which I have elsewhere referred in proving that the Pleistocene submergence of America and Europe came to an end not more than ten thousand years ago, and was itself not of very great duration. Thus in Pleistocene times the land must have been submerged and re-elevated in a very rapid manner.

These elevations and submergences of the plateaus alternated with the periods of mountain-making plication, which was going on at intervals at the close of the Eozoic, at the beginning of the Cambrian, at the close of the Siluro-Cambrian, and in Europe and Western America in the Tertiary. The series of changes, however, affecting all these areas was of a highly complex character, and embraces the whole physical history of the geological ages.

We may here note that the unconformities caused by these movements and by subsequent denudation constitute what Le Conte has called 'lost intervals,' and one of the most important of which is supposed to have occurred at the end of the Eozoic. It is to be observed, however, that as every such movement is followed by a gradual subsidence, the seeming loss is caused merely by the overlapping of the successive beds deposited.

(*To be Continued.*)

RELATIONS OF THE EARTH'S ROCKS TO METEORITES.

By H. A. Newton,
Retiring President of the American Association for the Advancement of Science.

[ABSTRACT.]

After briefly recounting the various superstitions and popular views respecting the origin of meteorites and their influence upon the earth, the President reviewed the various views advanced by scientists and reduced them to the following generally accepted propositions:—

1. The luminous meteor tracks are in the upper part of the earth's atmosphere. Few meteors, if any, appear at a height greater than one hundred miles, and few are seen below a height of thirty miles from the earth's surface, except in rare cases, when stones and irons fall to the ground. All these meteor tracks are caused by bodies which come into the air from without.

2. The velocities of the meteors in the air are comparable with that of the earth in its orbit about the sun. It is not easy to determine the exact values of those velocities, yet they may be roughly stated as from fifty to two hundred and

fifty times the velocity of sound in the air, or of a cannon ball.

3. It is a necessary consequence of these velocities that the meteors move about the sun, and not about the earth, as the controlling body.

4. There are four comets related to four periodic star-showers, that have occurred on the dates April 20th, August 10th, November 14th and November 27th. The meteoroides which have given us any of these star-showers constitute a group, each individual of which moves in a path which is like that of the corresponding comet. The bodies are, however, now too far from one another to influence appreciably each other's motions.

5. The ordinary shooting stars in their appearance and phenomena, do not differ essentially from the individuals in star-showers.

6. The meteorites of different falls differ from one another in their chemical composition, in their mineral forms and in their tenacity. Yet through all these differences they have peculiar common properties which distinguish them entirely from all terrestrial rocks..

7. The most delicate researches have failed to detect any trace of organic life in meteorites.

8. These propositions have practically universal acceptance among scientific men. We go on to consider others which have been received with hesitation, or in some cases have been denied.

With a very great degree of confidence we may believe that shooting stars are solid bodies. As we see them they are discrete bodies, separated even in prolific star-showers by large distances one from another. We see them penetrate the air many miles, that is, many hundred times their own diameters at the very least. They are sometimes seen to break in two. They are sometimes seen to glance in the air. There is good reason to believe that they glance before they become visible. Now, these are not the phenomena which may be reasonably expected from a mass of gas.

A spherical mass of gas, at the earth's distance from the sun, must exceed in density air at one-sixth millimetre pressure, or else the sun will scatter it. Such a mass would hardly have a possible existence. The surface of solid meteorites is burned or melted away when brought in contact with the air at a similar velocity, while the experiments of M. Daubrée and the well known effects of dynamite well show the enormous resistance such gaseous bodies would have to encounter, and only a solid body could be conceived of under such conditions as obtain in the flight of a meteorite.

Again, we may reasonably believe that the bodies that cause the shooting stars, the large fireballs and the stone-producing meteor, all belong to one class. They differ in kind of material, in density, in size. But from the faintest shooting star to the largest stone-meteor, we pass by such small gradations that no clear dividing lines can separate them into classes.

See wherein they are alike.

1. Each appears as a ball of fire traversing the apparent heavens, just as a single solid, but glowing or burning mass would do.

2. Each is seen in the same part of the atmosphere and moves through its upper portion. The stones come to the ground, it is true, but the brightly luminous portion of their paths generally ends high up in the air.

3. Each has a velocity which implies an orbit about the sun.

4. The members of each class have apparent motions which imply common relations to the horizon, to the ecliptic, and to the line of the earth's motion.

5. A cloudy train is sometimes left along the track both of the stone-meteor and of the shooting star.

6. "They have like varieties of colors, though in the small meteors the colors are naturally less intense and are not so variously combined as in the large ones.

In short, if the bodies that produce the various kinds of fireballs had just the difference in size and material which we find in meteorites, all the differences in the appearances would be explained; while, on the other hand, a part of the likenesses that characterize the flights, point to something common in the astronomical relations of the bodies that produce them.

This likeness of the several grades of luminous meteors has not been admitted by all scientific men. Especially it was not accepted by your late President, Prof. J. Lawrence Smith, who by his studies added so much to our knowledge of the meteorites.

The only objection of apparent force, that has been urged against the relationship of meteorites and star-showers, is the fact that no meteorites have been secured that are known to have come from star-showers. Within the last one hundred years there have been five or six star-showers of considerable intensity, and the objection assumes that a large number of stones must have come to the ground from them, and have been picked up. But a reasonable estimate of the total number of meteors in all of these five or six star-showers combined, makes it about equal to the number of ordinary meteors which come into the air in six or eight months, and the average annual number of stone-meteors of known date, from which we have secured specimens, has during this hundred years been about two and a half.

Supposing the luminous meteors to be of the same origin and astronomical nature, and that the proportion of those fitted to come through the air without destruction is the same among the star-shower meteors as among the other meteors, a hundred years of experience would lead us to expect two, or perhaps three, stone-falls, from which we secure specimens during the half dozen showers put together. To ask for more than two or three, is to demand of star-shower meteors more than other meteors give us. The failure to get these two or three may have resulted from chance, or from some peculiarity in the nature of the rocks of Biela's and Pempel's comets.

It may be assumed, then, as reasonable, that the shooting stars and the stone-meteors, together with all the intermediate forms of fireballs, are like phenomena. What we know about the one may with due caution be used to teach facts about the other. From the mineral and physical nature of the different meteorites, we may reason to the shooting stars, and from facts established about the shooting stars we may infer something about the origin and history of the meteorites. Thus it is reasonable to suppose that the shooting stars are made of such matter and such varieties of matter as are found in meteorites. On the other hand, since star-showers are surely related to comets, it is reasonable to look for some relation of the meteorites to the astronomical bodies and systems of which the comets form a part.

This common nature of the stone-meteor and the shooting stars enables us to get some idea, indefinite, but yet of great value, about the masses of the shooting stars. Few meteoric stones weigh more than one hundred pounds. The most productive stone-falls have furnished only a few hundred pounds each, though the irons are larger. Allowing for fragments not found, and for portions scattered in the air, such meteors may be regarded as weighing a ton, or it may be several tons, on entering the air. The explosion of such a meteor is heard a hundred miles around, shaking the air and the houses over the whole region like an earthquake. The size and brilliancy of the flame of the ordinary shooting star are so much less than that of the stone-meteor that it is reasonable to regard the ordinary meteoroid as weighing pounds or even ounces, rather than tons.

Determinations of mass have been made by measuring the light and computing the energy needed to produce the light. These are to be regarded as lower limits of size, because a large part of the energy of the meteor is changed into heat and motion of the air. The smaller meteors visible to the naked eye may be thought of without serious error as being of the size of gravel stones, allowing, however, not a little latitude to the meaning of the indefinite word gravel.

These facts about the masses of shooting stars have important consequences.

The meteors, in the first place, are not the fuel of the sun. We can measure and compute within certain limits of error, the energy emitted by the sun. The meteoroides, large enough to give shooting stars visible to the naked eye, are scattered very irregularly through the space which the earth traverses, but in the mean, each is distant two or three hundred miles from its near neighbors. If these meteoroides supply the sun's radiant energy, a simple computation shows that the average shooting star ought to have a mass enormously greater than is obtained from the most prolific stone-fall.

Moreover, if these meteoroides are the source of the solar heat, their direct effect upon the earth's heat by their impact upon our atmosphere ought also to be very great; whereas the November star-showers, in some of which a month's supply of meteroids was received in a few hours, do not appear to have been followed by noticeable increase of heat in the air.

Again, the meteoroides do not cause the acceleration of the moon's mean motion. In various ways the meteors do shorten the month as measured by the day. By falling on the earth and on the moon they increase the masses of both, and so make the moon move faster. They check the moon's motion, and so, bringing it nearer to the earth, shorten the month. They load the earth with matter which has no momentum of rotation, and so lengthen the day. The amount of matter that must fall upon the earth, in order to produce in all these ways the observed acceleration of the moon's motion, has been computed by Prof. Oppolzer. But his result would require for each meteroid an enormous mass, one far too great to be accepted as possible.

The power of such small bodies to break up comets or other heavenly bodies is insignificant, and their effect in producing geologic changes by adding to the earth's strata has been much over-estimated. To assume a sufficient abundancy of meteors in ages past to accomplish any of these purposes, is to reason from hypothetical and not from known causes.

The same may be said of the suggestion that the mountains of the moon are due to the impact of meteorites. Enormously large meteoroides in ages past must be arbitrarily assumed, and, in addition, a very peculiar plastic condition of the lunar substance in order that the impact of a meteoroid can make in the moon depressions ten, or fifty, or a hundred miles in diameter, surrounded by abrupt mountain walls two, and three, and four miles high, and yet the mountain walls not sink down again.

The known visible meteors are not large enough nor numerous enough to do the various kinds of work which I have named. May we not assume that an enormous number of exceedingly small meteoroides are floating in space, are falling into the sun, are coming into our air, are swept up by the moon? May we not assume that some of these various forms of work which cannot be done by meteoroids large enough for us to see them as they enter the air, are done by this finer impalpable cosmic dust? Yes, we may make such an assumption. There exist, no doubt, multitudes of these minute particles travelling in space. But science asks not only for a true cause but a sufficient cause. There must be enough of this matter to do the work assigned to it. At present, we have no evidence that the total existing quantity of such fine material is very large. It is to be hoped that through the collection and examination of meteoric dust, we may soon learn something about the amount which our earth receives. Until that shall be learned we can reason only in general terms. So much matter coming into our atmosphere as these several hypotheses require would, without doubt, make its presence known to us in the appearance of our sunset skies and in a far greater deposit of meteoric dust than has ever yet been proven.

A meteoroid origin has been assigned to the light of the solar corona. It is not unreasonable to suppose that the amount of the meteoroid matter should increase toward the sun, and the illumination of such matter would be much greater as we approach the solar surface. But it is difficult to explain upon such an hypothesis the radial structure, the rifts, and the shape of the curved lines that are marked features of the corona. These seem to be inconsistent with any conceivable arrangement of meteoroids in the vicinity of the sun. If the meteoroids are arranged at random, there should be a uniform shading away of light as we go from the sun. If the meteoroides are in streams along cometary orbits, all lines bounding the light and shade in the coronal light should evidently be approximated by projections of conic sections of which the sun's centre is the focus.

There are curved lines in abundance in the coronal light, but as figured by observers and in the photographs, they seem to be entirely unlike any such projections of conic sections. Only by a violent treatment of the observations can the curves be made to represent such projections. They look more as though they were due to forces at the sun's surface than at his centre. If these complicated lines have any meteoroid origin (which seems very unlikely), they suggest rather the phenomena of comets' tails than meteoroide streams or sporadic meteors.

The hypothesis that the long rays of light which sometimes have been seen to extend several degrees from the sun at the time of the solar eclipse, are meteor streams seen edgewise seems possibly true, but not at all probable.

The observed life of a meteor, with few exceptions, is only a second, or at most a few seconds. Near the beginning of this century, small meteors were looked upon as some form of electricity; while the view that they originate in the earth's volcanoes even gains support from a few men of science at the present day, among whom is the distinguished Astronomer Royal of Ireland. The difficulties of this hypothesis, however, are exceedingly great.

No one claims that the meteors of the star-showers nor that their accompanying comets come from the earth's volcanoes. To ascribe a terrestrial origin to meteorites is then to deny the relationship of the shooting star and the stone-meteor. Every reason for their likeness is an argument against the terrestrial origin of the stones.

To suppose that the meteors came from any planets that have atmospheres, involves difficulties not unlike to, and equally serious with, those of a terrestrial origin.

The solar origin of meteorites has been seriously urged, and deserves a serious answer.

The first difficulty which this hypothesis meets is, that solid bodies should come from the hot sun. Besides this, they must have passed without destruction through an atmosphere of immense thickness, and must have left the sun with an immense velocity.

Then there is a geometric difficulty. The meteorite shot out from the sun would travel under the law of gravitation nearly in a straight line outward and back again into the sun. If in its course it enters the earth's atmosphere, its relative motion, that which we see, should be in a line parallel to the ecliptic, except as slightly modified by the earth's attraction. A large number of these meteors, that is most, if not all, well observed fireballs, have certainly not travelled in such paths. These did not come from the sun.

It has been a favorite hypothesis that the meteorites came from some planet broken in pieces by an internal catastrophe. There is much which mineralogists can say in favor of such a view. The studies of M. Stanislas Meunier and others, into the structure of meteorites have brought out many facts which make their hypothesis plausible. It requires, however, that the stone-meteor be not regarded as of the same nature as the star-shower meteor, for no one now seriously claims that the comets are fragments of a broken planet. The hypothesis of the existence of such a planet is itself arbitrary; and it is not easy to understand how any mass that has become collected by the action of gravity and of other known forces should by internal forces be broken in pieces, and these pieces rent asunder. The disruption of such a planet by internal forces after it has by cooling largely lost its original energy, would be specially difficult to explain.

We cannot then look to the moon, nor to the earth, nor to the sun, nor to any of the large planets, nor to a broken planet as the first home of the meteoroides, without seeing serious if not insuperable objections. But since some of the meteoroides were in time past certainly connected with comets, and since we can draw no line separating shooting stars from stone-meteors, it is most natural to assume that all of them are of a cometary origin.

And if the cometary origin of meteorites is inadmissible, the objections must mainly come from the nature and structure of meteoric stones and iron.

What that structure is, and to some extent what conditions must have existed at the time and place of its first formation and during its subsequent transformations, mineralogists rather than astronomers must tell us. For a long time it was accepted without hesitation that these bodies required great heat for their first consolidation. Their resemblance to the earth's volcanic rocks was insisted on by mineralogists. Professor J. Lawrence Smith in 1855 asserted, without reserve, that "they have all been subject to a more or less prolonged igneous action corresponding to that of terrestrial volcanoes." Director Haidinger, in 1861, said:—"With our present knowledge of natural laws, these characteristically crystalline formations could not possibly have come into existence except under the action of high temperature combined with powerful pressure." The likeness of these stones to the deeper igneous rocks of the earth, as shown by the experiments of M. Daubrée, strengthened this conviction.

Mr. Sorby, in 1877, said:—"It appears to me that the conditions under which meteorites were formed must have been such that the temperature was high enough to fuse stony masses into glass; the particles could exist independently one of the other in an incandescent atmosphere, subject to violent mechanical disturbances; that the force of gravitation was great enough to collect these fine particles together into solid masses, and that these were in such a situation that they could be metamorphosed, further broken up into fragments, and again collected together."

Now if meteorites could come into being only in a heated place, then the body in which they were formed ought, it would seem, to have been a large one. But the comets, on the contrary, appear to have become aggregated in small masses.

The idea that heat was essential to the production of these minerals was at first a natural one. All other known rock formations are the result of processes that involved water or fire or metamorphism. All agree that the meteorites could not have been formed in the presence of water or free oxygen. What conclusion was more reasonable than that heat was present in the form of volcanic or of metamorphic action?

The more recent investigations of the meteorites and kindred stones, especially the discussions of the Greenland native irons and the rocks in which they are imbedded, are leading mineralogists, if I do not mistake, to modify their views. Great heat at the first consolidation of the meteoric matter is not considered so essential. In a late paper, Mr. Daubrée says:—"It is extremely remarkable that in spite of their great tendency to a sharply defined [*nette*] crystallization, the silicate combinations which make up the meteorites are there only in the condition of very small crystals all jumbled together as if they had not passed through fusion. If we may look for something analogous about us, we should say that instead of calling to mind the long needles of ice which liquid water forms as it freezes, the fine grained texture of meteorites resembles rather that of hoar frost and that of snow, which is due, as is known, to the immediate passage of the atmospheric vapor of water into the solid state."

So Dr. Reusch, from the examination of the Scandinavian meteorites, concludes that "there is no need to assume volcanic and other processes taking place upon a large heavenly body formerly existing but since gone to pieces."

The meteorites resemble the lavas and slags on the earth. These lavas and slags are formed in the absence of water, and with a limited supply of oxygen, and heat is present in the process. But is heat necessary for the making of the meteorites? Some crystallizations do take place in the cold; some are direct changes from gaseous to solid forms. We cannot in the laboratory reproduce all the conditions of crystallization in the cold of space. We cannot easily determine whether the mere absence of oxygen will not account fully for the slag-like character of the meteoric minerals.

Wherever crystallization can take place at all, if there are present silicon and magnesium and iron and nickel with a limited supply of oxygen, there silicates ought to be expected in abundance, and the iron and nickel in their metallic form. Except for the heat, the process should be analogous to that of the reduction of iron in the Bessemer cupola, where the limited supply of oxygen combines with the carbon and leaves the iron free. The smallness of the comets should not then be an objection to considering the meteoric stones and irons as pieces of comets. There is no necessity of assuming that they were parts of a large mass in order to provide an intensely heatad birth-place.

But although great heat was not needed at the first formation, there are many facts about these stones which imply that violent forces have in some way acted during the meteorite's history. The brecciated appearance of many specimens, the fact that the fragments in a breccia are themselves a finer breccia, the fractures, infiltrations and apparent faultings seen in microscopic sections and by the naked eye—these all imply the action of force

M. Daubrée supposes that the union of oxygen and silicon furnishes sufficient heat for making these minerals. If this be possible, those transformations may have taken place in their first home. Dr. Reusch argues that the repeated heating and cooling of the comet as it comes down to the sun and goes back again into the cold, is enough to account for all the peculiarities of structure of the meteorites. These two modes of action do not, however, exclude each other.

It has been assumed that the cometic fragments go continuously away from the parent mass so as to form, in due time, a ringlike stream of varying density, but stretched along the entire elliptic orbit of the comet. The epochs of the Leonid star-showers in November, which have been coming at intervals of thirty-three years since the year 902, have led us to believe that this departure of the fragments from Tempel's comet (1866, I) and the formation of the ring was a very slow process. The meteors which we met near 1866 were, therefore, thought to have left the comet many thousand years ago. The extension of the group was presumed to go on in the future, until, perhaps, tens of thousands of years hence, the earth shall meet the stream every year.

Whatever may be the case with Tempel's comet and its meteors, this slow development is not found to be true for the fragments of Biela's comet. It is quite certain that the meteors of the splendid displays of 1872 and 1885 left the immediate vicinity of that comet later than 1840, although at the time of those showers they had become separated two hundred millions of miles from the computed place of the comet. The process then has been an exceedingly rapid one, requiring, if continued at the same rate, only a small part of a millennium for the completion of an entire ring, if a ring is to be the finished form of the group.

It may be thought reasonable, in view of this fact about Biela's comet, established by the star-showers of 1872 and 1885, to revise our conception of the progress of disintegration of Tempel's comet also. The more brilliant of the star-showers from this comet have always occurred very near the end of the thirty-three year period. Instead of there being a slow process which is ultimately to produce a ring along the orbit of the comet, it certainly seems more reasonable to suppose that the compact lines of meteors which we met in 1866, 1867 and 1868 left the comet at a recent date. A thousand years ago this shower occurred in the middle of October. By the precession of the equinoxes and the action of the planets, the shower has moved to the middle of November. One-half of this motion is due to the precession of the equinoxes, the other half to the perturbing action of the planets. Did the planets act upon the comet before the meteoroids left it, or upon the meteoroid stream? Until one has reduced the forces to numerical values, he may not give to this question a positive answer. But I strongly suspect that computations of the forces will show that the perturbations of Jupiter and Saturn upon that group of meteoroids hundreds of millions of miles in length, perturbations strong enough to change the node of the orbit fifteen degrees along the ecliptic, would not leave the group such a compact train as we found it in 1866. If this result is at all possible, it is because the total action is scattered over so many centuries. But it seems more probable that the perturbation was of the comet itself, that the fragments are parting more rapidly from the comet than we have assumed, and that long before the complete ring is formed the groups become so scattered that we do not recognize them, or else are turned away so as not to cross the earth's orbit.

Comets, by their strange behavior and wondrous trains, have given to timid and superstitious men more apprehensions than have any other heavenly bodies. They have been the occasion of an immense amount of vague and wild and worthless speculation by men who knew a very little science. They have furnished a hundred as yet unanswered problems which have puzzled the wisest. A world without water, with a strange and variable envelope which takes the place of an atmosphere, a world that travels repeatedly out into the cold and back to the sun and slowly goes to pieces in the repeated process, has conditions so strange to our experience and so impossible to reproduce by experiment that our physics cannot as yet explain it. Yet we may confidently look forward to the answer of many of these problems in the future. Of those strange bodies, the comets, we shall have far greater means of study than of any other bodies in the heavens. The comets alone give us specimens to handle and analyze. Comets may be studied, like the planets, by the use of the telescope, the polariscope and the spectroscope. The utmost refinements of physical astronomy may be applied to both. But the cometary worlds will also be compelled, through these meteorite fragments with their included gases and peculiar minerals, to give up some additional secrets of their own life and of the physics of space to the blowpipe, the microscope, the test-tube aud the crucible.

ADDITIONAL NOTES UPON THE TENDRILS OF CUCURBITACEÆ.[1]

By D. P. PENHALLOW.

During the past summer, opportunity was presented for the extension of previous observations upon tendril movements in several important particulars, and the results

---

[1] The facts contained in this paper were nearly all obtained at the Botanic Gardens of Harvard University. For the facilities there placed at my disposal, and for many courtesies extended, the author is under deep obligations to Dr. Gray and Dr. Goodale.

obtained are now presented as supplementary to those already recorded.[1] The leading subjects for inquiry, not previously dealt with, concerned (1) a determination of the force developed during the formation of tendril spirals, *i.e.*, to what extent, if any, does the formation of the double spirals involve a direct and measurable strain? Owing to circumstances developed during the progress of the experiments, and to be detailed later, this consideration could not be dealt with in any extended series of observations, and the question had to be approached from another standpoint, the proposition then resolving itself into a consideration of the tensile strength of already formed spirals at different stages of maturity.

Owing to the abundant facilities most courteously placed at our disposal, both in the Physiological Laboratory and the Botanic Garden of Harvard University, it has been possible to collect a large amount of evidence, which goes far to settle the questions involved, of which the following is a condensed summary.

For the determination of the various considerations presented, as large a number of representattves of the order was selected as could conveniently be brought under observation. The genera represented, as also the total number of species observed, are as follows:—

	Species.
Benincasa	1
Citrullus	1
Cucumis	1
Cucurbita	3
Lagenaria	8
Luffa	4
Megarrhiza	1
Rhynchocarpa	1
Sicyos	2
Totals, 9	22

All of these species were employed in determining the tendril type, and most of them were used for observations relating to tensile strength and other considerations. One genus (*Lagenaria*) was studied under glass, while all the genera were studied out of doors, several of them under different conditions of exposure and training.

General considerations seemed to render it highly probable that, if the tendril of *Cucurbita* were not the true type

---

[1] Trans. R. Soc. Can., IV.

of the family, there would at least be but few and unimportant structural differences as observed in the various genera. No exact comparison having been instituted, however, it has been impossible to give definite expression to any settled view on this point, up to the present time. Our recent observations, however, enable us to supply this deficiency. As we have already detailed the structure of the tendril in *Cucurbita,* it may be well to institute our comparisons with it, more especially as we are to demonstrate how far it may be considered the type, and reference should be made to our description for comparison.[1]

All the tendrils of the family, so far as observed, present the same general features of internal structure, at least so far as the more important mechanical elements are concerned, *e.g.,* the distribution of the tissues, particularly with reference to the strong localization of bast and the development of vibrogen bands. Externally, the form varies somewhat, though in most cases in no essential particular. All are more or less well rounded at the base and flattened toward the tip, usually before the middle section is reached, but, with one or two exceptions, the lower or sensitive surface is rounded. In *Sicyos,* an exception appears in the strong flattening of both sides for some distance back from the tip. *Citrullus* and *Lagenaria* are flattened only on the upper side, but show no channel, or mere rudiments of one, while *Cucumis* is not only channelled above, but shows several longitudinal furrows in other parts of the circumference. *Citrullus, Lagenaria, Luffa, Benincasa* and *Cucumis* are more or less hairy all over the base; *Rhynchocarpa* is hairy throughout, while *Sicyos* is equally smooth throughout its entire length. It is a noticeable fact, however, that in nearly all the genera the hairs disappear at a short distance from the base, over the entire region of the collenchyma tissue of the lower side, and even in *Rhynchocarpa,* where the hirsuteness is so strongly defined, there are comparatively few hairs in this region. The same is true of the two collenchyma regions lying laterally

---

[1] Trans. R. Soc. Can., IV.

upon the upper side of the tendril. It thus appears that the hairs are chiefly developed in the regions where the sensitiveness of the tendril is least, that where this latter is most acute the surface is the smoothest.

In the distribution of the vibrogen bands, there is probably greater variation than in any other particular noted. While in *Cucurbita pepo*, we have noted three bands of this tissue, we cannot consider this strictly true of the whole genus, since in *C. pyxidaris* there are four such at the tip. This is also true of other genera. In *Sicyos* and *Cucumis* there are more than four bands at the base, but, by absorption or merging, they become two at the tip. In *Lagenaria* four bands are distinct throughout, but in *Citrullus*, *Cucurbita* and *Megarrhiza* they become reduced from four, at the base, to three or (*Citrullus*) two. On the other hand, in *Luffa*, *Rhynchocarpa*, *Benincasa*, *Megarrhiza* and *Cucurbita*, the three bands at the base become four at the tip. The general tendency appears to be toward the development of three principal bands at the base, together with several subordinate bands which soon disappear. The former become divided into four at about the middle section of the arm. In all such cases, the lateral bands remain intact, while the division occurs in the central band, but in such a way that its halves, though distinct, are not widely separated, and in their relation to the lateral bands still act as one. Before division, the central band is usually equal in width to each of the laterals, a relation, in point of size, which is preserved even after division, so that this band, at its divided extremity, is incapable of exerting a stronger influence over the others than does its undivided base. These facts have an important bearing upon what has elsewhere been stated,[1] with reference to the circumnutations and the relations which the bands bear to them. It may also be noted that, in most of the tendrils observed, free torsion was common and conspicuous.

Tensile Strength.—For the determination of the tensile strength of the tendrils during and after coiling, three

---

[1] Trans. R. Soc. Can., IV.

forms of balances were used. Of these, the first was a torsion balance formed of a brass spring 9.5 c.m. long and 1.6 c.m. diameter, bearing upon the free end a grooved wheel 6.5 c.m. in diameter. This operated against an arbitrarily graduated scale, and carried an index hand. Over the wheel there passed a light, flexible wire, which was drawn through a metal eye fastened to the scale plate, and provided with a suitable loop at the end for the tendril to grasp. The balance was adjusted to an initial tension of 5.31 gr., and a total tension of 254.93 gr. The second balance was constructed of a clock spring with post adjustment, so that the tension could be varied at pleasure. The spring operated upon the face of a board, and from the free end of the former a light wire was led through metal eyes along a horizontal scale, and provided with a suitable loop at the end. The adjustment was made for an initial tension of 120 gr., and a total tension of 900 gr. In both of these instruments the divisions were arbitrary, their value in grains being determined experimentally. The latter was not used at all, and the former to but a limited extent, for reasons soon to appear.

The third and most simple form of instrument, and that which was chiefly relied upon, consisted of a square stick about 30 c.m. long, upon which was fixed a scale in centimetres. Into the end of the stick was firmly fixed a small hook, upon which could be hung the brass spring for direct tension. In this apparatus three springs in all were used, though the two first to be mentioned were employed in nearly all the determinations. Their dimensions were as follows:—

	Length.	Diam. of Spring.	Diam. of Wire.
1 - - - -	9.7 c.m.	0.3 c.m.	0.25 m.m.
2 - - - -	11.7 "	0.6 "	0.50 "
3 - - - -	19.5 "	1.0 "	1.00 "

In using these balances, the spring was hung upon the hook and allowed to run along the scale. To it, the free end of the tendril was then secured by suitable means and the strain exerted by a direct and steady pull. Readings were then taken of the elongation of the spring, and the value in grains, of the corresponding divisions, was afterward determined experimentally.

July 7th, the tension balance was placed in position in the plant house for a tendril of *Lagenaria* to grasp. The first tendril failed to secure a hold, but on the 10th the second tendril grasped the loop firmly, and on the 12th it completed its double spirals. Inasmuch as both balance and vine were firmly secured, there could be no variation of distance between the tendril base and the point of attachment to the balance, without its being indicated on the scale of the latter.

Although the formation of the double spiral began on the 10th, and was completed on the 12th, no evidence of strain was manifested. The tendril continued to mature normally, but up to the 19th of July, during which time the coils became slightly closer, no evidence of strain was apparent, and the connection was therefore severed. It was most evident from this trial that, since the relation of vine and point of attachment remained unchanged, there could have been no shortening of the tendril in the formation of the coils, but that the total increase of tissue was sufficient to compensate for their formation. It had previously been surmised that the increase of parts after coiling would not fully compensate the shortening due to coiling. In order to determine how far this result was the expression of a general law, many measurements were made, all of which proved to be of a confirmatory nature, and we therefore feel justified in asserting that these tendrils, in the formation of their spirals, fail to exert a strain equal to 5 gr. It would also appear that, during the formation of the spiral, while all parts must of necessity elongate somewhat, the special increase of tissue is on the convex side of the tendril, hence in those soft tissues where the bast is known to be least abundant, and where the vibrogen is prominent, this will appear more strikingly later on.

For the reasons already given, resort was had to simple springs for a determination of the strength of already formed spirals in various stages of maturity. The more important results appear in the following table, in which the "tensile strength" is that determined within the limits of elasticity of the spiral:—

TENSILE STRENGTH OF TENDRILS.

Wt. in gr. L. M. = C. M.

No.	Length of Spiral.	Tensile Strength.	Breaking Strain.	No. of Spring.	Remarks.
1	2.3	101.0 gr.	....	1	*Lagenaria vulgaris.*
4	7.1	36.0 "	....	1	*Sicyos.* Coiled one day.
5	2.5	76.0 "	....	1	*Megarrhiza fabacea.*
6	3.3	161.0 "	....	1	" "
7	5.6	156.0 "	....	1	" "
8	3.3	216.0 "	....	1	*Lagenaria maxima.*
9	...	373.0 "	....	2	" "
10	2.0	160.0 "	....	2	" "
"	..	....	488.0 gr.	2	" "
11	3.0	488.0 "	....	2	" "
a	2.5	338.0 "	488.0 gr.	2	*Megarrhiza fabacea.*
b	5.0	323.0 "	423.0 "	2	" "
c	5.0	203.0 "	360.0 "	2	" "
d	8.9	203.0 "	360.0 "	2	" "
e	8.2	101.0 "	....	1	*Sicyos.*
f	6.8	148.0 "	....	2	*Megarrhiza fabacea.*
g	..	203.0 "	....	2	*Lagenaria vulgaris.*
h	12.0	266.1 "	....	2	*Cucurbita pepo.*
i	8.2	301.1 "	366.6 "	2	" "
j	7.6	626.0 "	666.0 "	2	
k	11.5	266.1 "	....	2	Strain of 366. gr. caused separation of tissues, & 446. gr. broke tendril.
l	6.3	396.0 "	....	2	
m	12.7	206.0 "	....	2	
n	..	141.1 "	....	1	*Lagenaria vulgaris.* One day coiled.

As all of the above determinations were made with tendrils which were comparatively fresh and soft, though in different stages of maturity, it would appear evident that the strain which a spring is capable of supporting must vary with maturity of parts, becoming greater with age. It was easily found that, in coiled tendrils, the distinction between the band of bast along the lower side, and the softer tissues of the upper side, was much more prominently marked than before coiling, and when such tendrils, at the proper stage of maturity, were subjected to a carefully augmented strain, it was found possible to cause a complete separation of the bast from the other tissues for a distance of one or more centimetres. Under such conditions of strain, it is also to be noted that the tissues along the lower side of the tendril, or the concave side of the spring, do not elongate, but the softer tissues on the opposite side suffer a strong compression and form a spiral around the straightened and resisting bast as a central axis. The compression of parts is to be observed in the strong, transverse corrugations in the surface, as also in the formation of minute drops of moisture which exude from the surface over the entire area of compression, but more particularly along the lines of vibrogen, where also the corrugations are the most strongly defined.

According to the weight or strain imposed, the spirals will be open or closed. This is conspicuously true where vines hang by their tendrils, or where any resistance is offered to attachment. But it must be kept in mind, as our previous considerations show, that the vine cannot be drawn to a support—it must first approach a support in the natural course of growth, and then become secured to it. In harmony with this, it is found that when the strain is slight, or when the vine in growing advances toward the support, the coils become correspondingly closer.

Rigid support is not essential, nor need the object be large. The requirements are met if it be sufficiently irritating, and possess a very moderate degree of stability. Small pebbles, bits of grass and similar objects lying upon the ground, and even small particles of dirt, hardened by rain, have been found sufficient to meet all requirements and cause the formation of double spirals in a normal manner.

Wilted tendrils may be drawn out without any appreciable resistance, while those which have coiled without the influence of a supporting object, and which rarely if ever coil regularly, may be drawn out with a force which rarely exceeds 40 gr. It thus appears that the strain required to draw out the coils of a tendril which is comparatively fresh, is an expression of the force—or, in other words, the tension—developed in all the softer tissues, and required to maintain the tendril as a spiral; or, to state the case differently, it is a measure of the resistance to compression which the softer tissues on the upper side of the tendril offer. Obviously, the full strength can be maintained only so long as the tension of the tissues is undisturbed, or until this latter is replaced by hardening of all parts, when, as in all old tendrils which have been for some time coiled, a new element of strength is introduced.

To more fully confirm the above results, numerous measurements of the tendrils were made to determine the relative lengths of the inner and outer surfaces of the spirals, or the general relation in length of the bast on the lower side and the softer tissues above. The ratios obtained were necessarily a little high for the true mean position of these tissues, from the fact that all the determinations were made from the inner and outer diameters of the spirals, and therefore relate wholly to the corresponding surfaces. The results, however, will be found to harmonize well with those obtained by a different method. The following will sufficiently explain the results in ratios of the inner to the outer surfaces:—

DIMENSIONS OF SPIRALS.

Ratios of Inner and Outer Surfaces.

Cucurbita pepo.

1	1 : 2.0
2	1 : 2.0
3	1 : 4.0
4	1 : 1.7
5	1 : 2.0
6	1 : 2.7
7	1 : 1.8
8	1 : 1.5
9	1 : 1.7
10	1 : 1.8
11	1 : 2.7
12	1 : 2.0
13	1 : 2.03
14	1 : 1.33
15	1 : 5.2
16	1 : 2.2
	1 : 2.29

In all of these it will be noted that the ratio increases or decreases as the coils become closer or more open—a fact which is most strikingly illustrated in Nos. 3 and 15, which we may almost regard as exceptional cases. If they are excluded from the general results, the mean ratio then falls to 1 : 1.94, which approaches more nearly the mean obtained by direct and separate measurement of the tissues themselves.

Other determinations of the same nature were made by carefully separating the bast from the soft tissues, and measuring each separately. Under such circumstances, both tissues may be straightened out without difficulty. A very large number of such measurements were made, the principal of which are as follows:—

RELATIVE LENGTH OF TISSUES BY SEPARATE MEASUREMENT.

No.	Bast.	Soft Tissue.	Ratios.	Remarks.
1	14.7 c. m.	16.5 c. m.	1 : 1.12	Lagenaria vulgaris,
2	5.0 "	7.0 "	1 : 1.4	Megarrhiza fabacea.
3	11.8 "	13.8 "	1 : 1.17	" "
4	18.0 "	22.3 "	1 : 1.24	Cucurbita pepo.
5	21.6 "	25.2 "	1 : 1.16	" "
6	15.9 "	19.5 "	1 : 1.22	" "
7	12.1 "	13.7 "	1 : 1.13	" "
8	10.0 "	11.7 "	1 : 1 17	
9	15.4 "	19 4 "	1 : 1.26	
10	15.7 "	18.9 "	1 : 1.20	
11	11.2 "	12.8 "	1 : 1.14	
12	16.5 "	20.9 "	1 : 1.26	
13	22.3 "	26.4 "	1 : 1.18	
14	16.3 "	19.8 "	1 : 1.21	
15	13.2 "	15.3 "	1 : 1.16	
			1 : 1.20	

From this it appears that, although the mean ratio is necessarily lower, the relation which the ratios bear to the character of the spirals is the same as in the previous case. The mean ratio in the latter case is 1 : 1.20, which may be taken as fairly representing the true relative lengths of the tissues under consideration in the average tendril.

DISCOVERY OF A PTERASPIDIAN FISH IN THE SILURIAN ROCKS OF NEW BRUNSWICK.

BY G. F. MATTHEW.

Although the existence of fish remains in the highest beds of the Silurian system in England and Russia has been known for many years, there has been until lately, a singular dearth of evidence of the presence of these vertebrates in beds of similar age in America. While the Devonian system has been found to contain abundant remains of fishes both in the Old World and the New, the Silurian system in America, four years ago, was not known to have any authentic remains of the hard parts of fishes.

The fishes of the Silurian age are of two kinds[1]—Pteraspidian ganoids, known by the hard plates that covered the anterior part of the body, and Selachians, whose presence is known by the occurrence of spines of a peculiar kind. The Pteraspidians had a peculiar, fine, but distinct striation of the covering plates of the body, by which their remains are easily recognized. These fossils have, within a few years, been found in the Silurian rocks of Pennsylvania by Prof. E. W. Claypole, to whose acumen and industry we are indebted for many new facts respecting this interesting, and as yet but imperfectly understood family of fishes.

I propose herein to call attention to the discovery in Canada, of fish remains of this peculiar type. The fossils occur in beds of the Silurian system which are found on the southern slope of the Nerepis Hills in King's County, New Brunswick. These beds are of a very fine texture; they are evenly banded, silicious mud-rocks or hardened shales, and pertain to No. 2 of the Mascareen Succession. No. 3 of this series contains marine species such as mark the Lower Helderberg Horizon of New York, or the Ludlow of England.

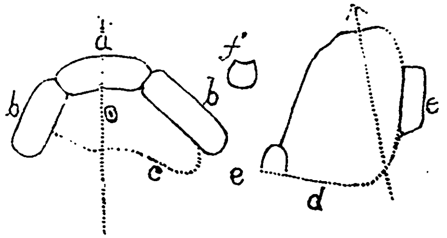
Only one example of the fish is known, and this exhibits several plates belonging to its dermal covering. A part of the rostrum and of the dorsal scute, and two lateral

---

[1] A representative of the tuberculated placoderms (Coccosteus) occurs in Barrande's Etage F in Bohemia, but this family belongs rather to Devonian than Silurian times.

plates ("cornua" ?) are preserved, as well as the front half or third of the ventral shield; the striæ on the ventral scute are symmetrical in relation to a median line passing through it; there is a detached fragment of a plate with concentric striæ like the eye plate of *Pteraspis rostrata.*[1] There is also a large plate more strongly arched than the ventral scute, on which the striæ are not arranged with entire symmetry; it appears to be the dextral half of the dorsal plate; it resembles the shield of Prof. Claypole's *Palæaspis Americdna* in form, and in the linear striæ of the border, but I can hardly think that it belongs to a different individual from that to which the plates already described pertain. On one side of this large plate is a small longitudinally striated side plate, and on the opposite side the fragment of another.

Numbers of carapaces of a small Ceratiocaris occur on the layers with the fish plates. The author is not aware that the existence of a ventral shield has hitherto been observed in any pteraspidian fish, although it is well known to exist in the large Devonian placoderms; the species is evidently different from any other known, and he would propose for it the name *Pteraspis* (?) *Acadica.*



a. The infra-rostral plate.
b, b. Anterior plates of the ventral shield.
c. The ventral scute—a part only is preserved.
d. The large oval plate—dextral half of the dorsal scute, seen from below.
e, e. Lateral plates of the dorsal shield.
f. Part of the eye-plate. (?)

N.B.—The vertical dotted lines represent the median line of the principal ventral plate and the supposed axial line of the half of the dorsal scute.

---

[1] Quart. Jour. Geol. Soc., Feb. 1865, p. 62.

THE AFFINITIES OF THE TENDRILS IN THE VIRGINIAN CREEPER.

BY A. T. DRUMMOND.

The Virginian Creeper (*Ampelopsis quinquefolia*, Michx.) is the most familiar ornamental climbing shrub found in gardens in Canada. It has, in its native state, a wide range, extending from about the vicinity of Quebec westward throughout Ontario to the valley of the Assiniboine in Manitoba.

When recently examining a number of these plants in different gardens, my attention was drawn to certain peculiarities in the growth of the tendrils and to the relation these tendrils bear to the panicles of flowers. The leaves of the Virginian Creeper are alternate, and, to the unobserving, the impression conveyed is that, on the young, growing shoots, the side of the stem opposite to the point of junction of the petiole with the stem is always furnished with a tendril. This, however, is not the case. The first two or three leaves formed on the growing stem or branch have no tendrils to correspond. The reason for this is obvious. The stem, up to the time it has put forth three or four leaves, is both robust and vigorous, and, being at the same time short, can readily retain its position without the aid of supporting arms. With the third or fourth leaf, the tendrils make their appearance. Here, however, is an eccentricity on the part of the plant. Instead of the tendrils occurring in consecutive order, one at the base of each petiole but on the opposite side from the leaf, they appear in regular successions of twos, and between each such set of two tendrils, that is, at every third petiole, a tendril is wanting. This peculiarity is uniform, and is characteristic also of *Vitis cordifolia.* The reason for it is not so clear, unless it is explained in any way by the affinities referred to farther on between the panicles of flowers and the tendrils.

An interesting feature in connection with this absence in the Virginian Creeper of the tendril at every third petiole, is that new, vigorous branchlets chiefly occur at the angle between the stem and leaf where the tendrils are absent, a strong, well-developed branchlet being the rare exception where the tendril is present. These branchlets are, of course, at the base of the petiole and not on the side of the stem where the tendril would be expected, were all the points of junction tendril-bearing. Dr. Balfour's contention is that in the vine and in *Ampelopsis Veitchii*, the tendrils are to be looked upon as the terminations of separate axes or as transformed terminal buds, and he adds that in the vine there are no young buds seen in the angle between the stem and leaves nor between the stem and tendrils. In *Ampelopsis quinquefolia* there are, however, invariably young buds in the angles between the leaves and stem wherever the opposite side of the stem is not tendril-bearing, and also in the similar angle of the leaf next below, but not in the angle of the leaf succeeding that lower on the stem. In other words, at the base of every third petiole—but only where a tendril occurs on the opposite side from the petiole—a young bud or branchlet is wanting. In *Vitis cordifolia*, again, there is always a bud, and, in some cases, two, in the angle of each leaf with the stem. As mentioned already, these young buds sometimes develop into vigorous, healthy branches. There is, therefore, in these two species not sufficient foundation for the theory that the tendrils are terminations of separate axes or modifications of the axes, nor do external appearances in these plants suggest it, though it might possibly apply in the case of the lower tendril of each set of two tendrils in the case of *Ampelopsis*. Dr. Asa Gray's view of the tendrils of these two plants is that they are branches of a very slender kind, similar to runners, but intended for climbing and not for propagation, and therefore destitute of buds or leaves. Two instances I have met with of the Virginian Creeper, gave colour to this view, in that each of the two tendrils bore a solitary trifoliate leaf.

There appear, however, to me, rather to be affinities between the tendrils of *Ampelopsis quinquefolia* and the peduncle and pedicels of its flower.[1] The angulation is somewhat simi-

---

[1] Darwin. Climbing Plants, 136.

lar, but, further, at the base of the petiole, two true stipules are always present, whilst on the tendril at each fork there is —and this also occurs in *Vitis cordifolia*—only one appendage, resembling, in this respect, the similar single appendage at each fork, subsequent to the first, in the peduncle and pedicel of the flower. Again, it will be noticed that in *Ampelopsis quinquefolia* the panicles of flowers possess the same peculiarity as the tendril, in the panicles appearing in successions of twos on the flower-bearing branches, but on the opposite side from the leaves, and in a panicle being wanting opposite each third leaf. The tendrils, further, occur in this plant only on the young leading shoots, and never on the flower-bearing branches which issue from these in the second or succeeding year. We might thus, perhaps, regard the tendrils of the Virginian Creeper as undeveloped panicles, which, appearing this year, serve the important purpose of enabling the parent stem to climb the supporting tree or wall, and thus best attain a position suitable for bearing its fully developed flowers on branchlets, which issue from this stem during next or in succeeding years. Torrey and Gray, in characterizing the order Vitaceæ, to which *Ampelopsis quinquefolia* belongs, refer to the lower leaves as opposite and "the upper alternate, opposite the racemes or thyrsoid panicles, which are sometimes changed into tendrils." Passing over the fact that opposite lower leaves is not a character common to all species in the order, the change from panicles to tendrils—which I have seen in *Vitis cordifolia*, but not in *Ampelopsis quinquefolia*—would seem to confirm the view I have taken of the affinities of the tendril in this plant.

ABSTRACT OF A PAPER ON THE CAMBRIAN FAUNAS OF CAPE BRETON AND NEWFOUNDLAND.

By G. F. Matthew.

In a paper read before the Royal Society of Canada on "The Cambrian Faunas of Cape Breton and Newfoundland," Mr. G. F. Matthew points out that the slates at Mira

River, Cape Breton, contain several species of trilobites, which show that these measures are in the upper part of the Olenus Zone, or Lingula Flags of Great Britain. The species observed were the followiug:—*Peltura scarabeoides*, Wahl; *Sphæropthalmus alatus*, Boeck; and *Agnostus pisiformis*, Lin. There is also a small Lingulella similar to that which characterizes the Upper Flags of the St. John Group, and also an Orthis, similar to *Orthis lenticularis*, Dal.

In a small collection of fossils sent to him by Mr. Howley, of the Geological Survey of Newfoundland, a number of species not heretofore reported from that island were observed. With the aid of these and the description of other species given by the late Mr. Billings and by Mr. Whiteaves, Mr. Matthew is able to classify roughly the Cambrian horizons of that island.

HORIZON OF PARADOXIDES KJERULFI.

The oldest fossils appear to be those of Topsail Head and Brigus in Conception Bay. Mr. Billings describes from these places:—*Agraulos strenuus*, Bill.; *Stenotheca paupera*, Bill.; and *Iphidea*, allied to *I. bella*. To these may be added the following as characterizing the limestones of Topsail Head:—*Paradoxides Kjerulfi*, Linrs.: *Selenopleura*, sp.; *Ptychoparia*. sp.; *Stenotheca*, sp.; *Straparollina?* sp.; *Hyolithes Micmac.*

HORIZON OF THE CONOCORYPHEES.

Manual River, a small stream near Topsail Head, appears to give the next horizon, for Mr. Whiteaves chronicles from this place:—*Microdiscus punctatus*, Salter; *M. Dawsoni*, Hartt; *Agnostus Acadicus*. Hartt; *Conocephalites* (*Liostracus*) *tener*, Hartt; *C.* (*Conocoryphe*) *Baileyi*, Hartt; and *C.* (*Ptychoparia?*) *Orestes*, Hartt. Of these species the second, fourth and fifth do not range as high in the Cambrian beds of Acadia as the others, and it is possible that the collections examined may have been from two horizons. The assemblage of species, however, may be taken to correspond with those of Band C. of the Acadian area. In a fragment of shale from the same locality, the following were found:—*Paradoxides*, sp.; *Agraulos socialis*, Bill.; *Agnostus gibbus?* Linrs; *Hyolithes*, sp.

HORIZON OF PARADOXIDES TESSENI.

A different and probably somewhat higher horizon appears to be indicated by species found at Chapel Arm, Trinity Bay. Mr. Billings describes from this place:—*Paradoxides tenellus*, Bill.; *P. decorus*, Bill.; *Anopolinus venustus*, Bill.; *Obolella* (*Linnarssonia*) *misera*, Bill.; *Solenopleura communis*, Bill.; and *Agraulos socialis*, Bill. In the species from this locality there are also the following:—*Eocystites*, sp.; *Beyrichona?* sp.; *Agnostus lœvigatus*, Dal.; *A. punctuosus*, Ang. var., *Agnosti*, other species and *Microdiscus punctatus*, Salt. There are fragments of a *Paradoxides*, which by its hypostome, suture, pleuræ and pygidium is very like *P. Tesseni* of Europe. This is, perhaps, the *P. decorus* of Billings. The organisms from this locality are evidently a Menevian assemblage, equivalent to Band *d* of Division I. of the St. John Group.

HORIZON OF PARADOXIDES SPINOSUS.

This species was quoted on account of the occurrence of *Paradoxides Bennettii*, Salt., at St. Mary's Bay. Discovered many years ago, it was the first which drew attention to the interesting Primordial Fauna of Newfoundland. The resemblance to *P. Harlani*, Green, from Braintree, Mass., has been pointed out by Mr. Walcott and others. Mr. Billings describes from the same locality *Agraulos affinis*. The corresponding species in the Acadian region is found in Division I., Band *c* (and *d*?). An *Agraulos* and a *Ptychoparia* have been described from the slates in which *P. Harlani* occurs in Massachusetts. If we have regard to the associated species, and also to the suture and eyelobe of *P. Bennettii* (=*P. Harlani?*), it seems probable that the horizon of this species is below that of *P. Forchammeri* and *P. Davidis* of the European Cambrian rocks.

HORIZON OF PARADOXIDES DAVIDIS.

In a black, calcareous rock from Highland's Cove, in Trinity Bay, there are abundant remains of a large *Para-*

doxides. The species, so far as the parts preserved give evidence, is *Paradoxides Davidis,* Salt. Associated with this species were the following:—*Paradoxides Loveni,* Ang? (pygidium); *Agnostus punctousus,* Ang., var.; and *A. brevifrons,* Ang.

In Newfoundland there would, therefore, appear to be a fuller representation of the various forms of the genus Paradoxides than has yet been found in any other part of America.

FAUNAS OF THE OLENUS ZONE.

Of the faunas of the higher part of the Cambrian of Newfoundland, except so far as it is developed in the northern and western part of the island, less is known. In the south-eastern peninsula, the beds above the Paradoxides beds are described as shallow-water deposits—sandstones and flags similar to the Lingula flags of Great Britain.

Mr. Billings has described from these upper measures the following species (locality, Bell Island, in Conception Bay):—*Eophyton Linnœanum,* Tor.; *E. Jukesi,* Bill.; *Arthraria antiquata,* Bill.; *Lingula Murrayi,* Bill.; *Lingulella* (?) *affinis,* Bill.; *L.* (?) *spissa,* Bill.; and *Cruziana similis,* Bill. From Kelly's Island, in Conception Bay, not far from Bell Island, Mr. Whiteaves describes a pretty little Lingula (*Lingula Billingsiana*). These fossils resemble those of the Lingula flags in Great Britain and those found in the St. John Group; but the determination of exact horizons in the upper part of the Cambrian in Newfoundland must await the discovery of fossils in the finer beds of that part of the formation.

In this classification of the various Newfoundland Horizons in the Paradoxides Zone, Mr. Matthew has placed that of *Paradoxides Kjerulfi* first, or oldest, because that is its position in Scandinavia. *P. Kjerulfi* is by some palæontologists classed as an *Olenellus,* but it has not been shown to possess the peculiar pygidium of that genus.

INVAPORATION.

By W. L. GOODWIN.

About sixty years ago, Thomas Graham made some curious experiments with solutions of salts, enclosing them along with water in tin canisters. The solutions increased in weight by condensing water vapour from the saturated atmosphere. This process he called *invaporation*, in contradistinction to *evaporation*. He showed that different salts invaporate at different rates, e.g., common salt having a comparatively strong power of invaporation, and sodic sulphate very weak. One result of this invaporating power of saline solutions is noteworthy. The atmosphere over the ocean must be drier than that over a fresh-water lake. The vapour tensions of saline solutions have since been determined accurately, and confirm Graham's early experiments. The process of invaporation can be watched easily, and it is certainly a most interesting case of the transference of masses of matter by molecular movements. If a small quantity of dry sodium chloride be put in a glass tube open at one end, and sealed up in a larger tube, or in a well stoppered bottle, along with a quantity of water in a second small tube, the salt gradually attracts the moisture, forms a solution, and at length takes to itself the whole of the water, so that the second small tube becomes quite dry. The process is a slow one, requiring months to complete in some cases; but, even where the proportion of water is very large, the salt is not satisfied until it has taken the whole of it.

My colleague, Professor Marshall and myself, have made a series of experiments with the object of measuring the relative forces with which different salts invaporate. Molecular proportions of two salts were enclosed in the same space with a certain quantity of water, for which they were allowed to strive until they had divided it between them. The experiments were made with pairs of chlorides, and different proportions of water were used with the same pair of salts so as to ascertain the effect of dilution. Some very unexpected results were obtained. For example, when lithium and sodium chlorides were allowed to strive for a small quantity of water, the lithium chloride (a deliquescent salt) took the whole of it, the sodium chloride remaining quite dry. This was the result after the salts were enclosed for several months. But with a larger proportion of water, the sodium chloride obtained a small share. When the proportion of water was greatly increased, the result of the contest was different; for it was found, most unexpectedly, that the sodic chloride was now able to seize upon and retain the lion's share. The explanation is to be sought, in all probability, in the formation of a hydrate by the lithium chloride. Sodic chloride does not crystallise with water at ordinary temperatures, while lithium chloride does. It is probable that the lithium chloride attracts water strongly until a definite hydrate is formed, and, thus satisfied, allows the weaker attraction of the sodium chloride to come into play. Further experiments are being made on this point. We have also made experiments with sodium and potassium chlorides, and find that with a small quantity of water, the sodium chloride takes nearly the whole of it. Larger quantities are more evenly divided. Whether this case is reversed or not with increased dilution, we have not yet determined. The experiments are now being extended to cases in which a well known chemical action takes place, the object being to compare solution with chemical action. Molecular proportions of phosphoric and citric acids have been enclosed with aqueous solution of ammonia, insufficient to neutralise both acids. As is well known, solutions of ammonia salts become acid by loss of ammonia to the atmosphere. These two acids will strive for the ammonia just as the salts do or water. The result of the battle is yet to be seen. We hope by these researches to throw some light upon the vexed question of the nature of solution.

QUEEN'S UNIVERSITY, Kingston, Ont.,
September 20th, 1886.

THE LAW OF VOLUMES IN CHEMISTRY.[1]

BY T. STERRY HUNT.

The questions regarding the so-called molecular weights and volumes of liquids and solids, which are now attracting the attention of chemists, can, I think, be better understood if we keep in mind the principles enunciated by the writer in 1853, that "the doctrine of chemical equivalents is that of the equivalency of volumes," and that "the simple relations of volumes which Gay-Lussac pointed out in the chemical changes of gases, apply to all liquid and solid species;" so that "the application of the atomic hypothesis to explain the laws of definite proportions becomes wholly unnecessary." In further illustration of this view it was said, in 1867, that "the gas or vapor of a volatile body constitutes a species distinct from the same body in a liquid or solid state; and the liquid and solid species themselves often [probably always] constitute two distinct species of different equivalent weights." From this it follows that freezing, melting, and vaporization are chemical changes. The union of many volumes of a vapor or gas in a single volume of a liquid or of a solid, is a process of chemical combination, while vaporization is chemical decomposition. Such decomposition is either with or without specific difference, and examples of these two modes are seen respectively in heterogeneous dissociation and in integral volatilization, which latter is the breaking up or dissociation of a polymeric species into simpler forms having the same centesimal composition. Both of these processes are subordinated to the same laws of pressure and temperature, and involve similar thermic changes in the relations of the bodies concerned. In this enlarged conception of the chemical process we find a solution of the problems above named, and an explanation of the distinction which has been made between "the chemical molecule" and "the molecule of the physicist." That the latter has a much less simple constitution than the former, as calculated from the results of chemical

---

[1] Also SCIENCE for September 10, 1886.

analysis and from vapor-density, has been long maintained alike on dynamical and chemical grounds. It is discussed by the writer in 1853 in the essay already quoted, entitled "The Theory of Chemical Changes and Equivalent Volumes,"[1] and again in the late paper of Spencer Pickering in the *Chemical News* for November, 1885.

If, then, as maintained by the writer, the law of volumes is universal, and if the production of liquids and solids by the condensation of vapors is a process of chemical union, giving rise to polymerids, the equivalent weights of which are as much more elevated as their densities are greater than those of the vapors which combine to form them, the hypothesis of atoms and molecules, as applied to explain the law of definite proportions and the chemical process, is not only unnecessary, but misleading. According to this hypothesis, which supposes molecules to be built up of atoms, and masses of molecules, the different ratios in unlike species between the combining weight of the chemical unit or molecule (as deduced from analysis and from vapor-density; $H = 1.0$) and the specific gravity of the mass are supposed to represent the relative dimensions of the molecule. Hence, the values got by dividing these combined weights by the specific gravity have been called "molecular volumes." The number of such molecules required to build up a physical molecule of constant volume would, according to this hypothesis, be inversely as their size. If, however, as all the phenomena of chemistry show, the formation of higher and more complex species is by condensation, or, in other words, by identification of volume, and not by juxtaposition, it follows that the so-called molecular volumes are really the numbers representing the relative amount of contraction of the respective substances in passing from the gaseous to the liquid or solid state, and are the reciprocals of the coefficient of condensation of the assumed chemical units. If steam at 100° C. and 760 millimetres pressure, with a formula, as deduced from its density, of H_2O, and a combining weight of 18, is converted into

---

[1] See the author's "Chemical and Geological Essays," pp. 426-437, and, further, *ibid.*, pp. 453-458.

water at the same temperature, 1,628 volumes of it are condensed into a single volume, having a specific gravity of 0.9588, which at 4° C. becomes 1.0000. Water is thus 1,628 $= (H_2O)$; and the weight of its volume at the temperature of formation, as compared with an equal volume of hydrogen gas or of steam, in other words, its equivalent weight, is $1,628 \times 18 = 29,304$, which thus corresponds to a specific gravity of 1.000; ice, at its temperature of formation, with a specific gravity of 0.9167, being $1,487 = (H_2O)$ with an equivalent weight of 26,766. The hydrocarbon, $C_4H_{10} = 58$, condenses to a liquid having, according to Pelouze and Cahours, a specific gravity of 0.600, which corresponds to an equivalent weight, as compared with that of water, of 17,582, or approximately 303 (C_4H_{10}), with a calculated specific gravity of 0.5997. The reciprocal of the co-efficient of condensation (or so-called molecular volume) of steam is 18, while that of the gaseous hydrocarbon is $600 : 1000 :: 58 : x = 96.66$.

The chemical unit for bodies, which, like these, volatilize integrally, is fixed by the density of their vapors; while for fixed species, like anhydrous oxides and silicates, or for those which by heat undergo heterogeneous dissociation, as for example calcite and hydrous silicates, the unit may be the simplest formula deduced from analysis, or, for greater convenience in calculation in the case of oxides and silicates, may have a value corresponding to $H=1$, or $O=8$. The unit for silica thus becomes $Si\,O_2 \div 4 = 15$; that for alumina, $Al_2O_3 \div 6 = 17$; and that for the magnesian silicate, $Si\,Mg_2\,O_4 \div 8 = 17.5$. Such unit-weights as these have been employed by the writer in his late essay on "A Natural Sytem in Mineralogy," in the tables of which they are represented by P; while the values got by dividing these numbers by the specific gravity of the species have been designated unit-volumes, and represented by V. The writer of that essay, in deference to the general usage of chemists, therein adopted the received terminology of "molecular weights" and "molecular volumes," and, failing at the time to grasp the full significance of his own earlier teachings as to the universality of the law of volumes, spoke of the so-called molecular weight as an unknown quantity, although, in accordance with that principle, this molecular weight, or, properly speaking, this equivalent weight, is simply deduced for any body the specific gravity of which is unknown.

MISCELLANEOUS.

SOLUTION OF STARCH IN LEAVES.—A diastatic ferment can be extracted from green leaves in the following way:—The leaves are bruised in a mortar, and covered with cold water; after 24 hours they are pressed, and 1½ volumes of 90° alcohol added to the juice, which is then filtered. The same quantity of alcohol is again added to the filtrate, and after a few minutes, the clear liquid is filtered off and the precipitate washed once or twice with alcohol of 65°. The diastase is obtained in solution by dissolving the washed precipitate in water and filtering. 10 c.c. of such a solution is added to 0.5 gram. of starch into a paste and kept at 63°, and the formation of sugar is shown by comparison with a similar flask to which a few drops of chloroform have been added. The leaves of the potato, dahlia, artichoke, maize, beet, castor oil plant, and the unripe seeds of the opium poppy, sunflower, and castor oil plant, have all yielded positive results. Microbes have not been found in the solution, and the starch was in all cases transformed into a mixture of reducing sugar and dextrine. To connect this with the formation of sugar in growing plants, the author shows, by a series of experiments that, although diastase will only act on starch paste and not on crude starch at 60°, 57°, and 50°, yet at 42° and 34° it always transforms a little crude starch into sugar. The quantity of sugar produced reaches a limit in twenty-four or thirty six hours; but if it be dialyzed out of the solution as fast as it is formed, the formation is rendered continuous. The same result is produced by diluting the solution, so that it seems to be the accumulation of sugar which puts an end to the diastatic action.

Cuboni's experiment, therefore, in which the disappearance of starch from a vine leaf, placed in the dark, was prevented by an annular incision in the stem above and below the leaf, does not negative the idea that starch is transformed into sugar by a diastatic ferment in the leaf: arrest of sugar formation would, under these circumstances, be brought about by accumulation of sugar in the isolated leaf. When only one incision is made, either above or below the leaf, the starch disappears as usual; and when a grape cluster, either in flower or fruit, is opposite the leaf, the starch disappears, even when the stem is cut through above and below. It appears from this that the demand for fresh supplies of carbohydrates in some centre of growth will drain off the accumulated organ with sufficient rapidity to render its formation continuous.—*Ann. Agronom.*, 12, 200-203.